Intra- and Intergenerational Perspectives on Health and Long-Term Care: The Role of Education

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Inaugural Dissertation

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Chapter 1

Introduction

1.1 Motivation

This dissertation provides empirical evidence on the causal effect of education on health and related outcomes from an intragenerational and intergenerational perspective. In particular, the thesis investigates whether the association between education and health within the same generation and the intergenerational relationship between children's education and their parents' long-term care dependency as a very important health outcome can be interpreted as causal. Moreover, the thesis analyses whether adult children's education causally affects the provision of informal care to their ageing parents. From a policy perspective, all three questions are highly relevant. In light of population ageing and ever-increasing numbers of older people in the population, questions on whether education affects own health and on whether health returns to education spill over also from the child to the parent generation are particularly important given that health is the most important predictor of the need for long-term care in old age. Provided that the intra- and intergenerational associations between education and health are causal, education policies could be cost-effective tools for improving population health. Such policies aimed at improving population health through educational interventions are regularly proposed by international organizations (OECD 2010, WHO 2015). Given that the bulk of long-term care in Europe is provided by unpaid informal caregivers, and mostly by adult children (Norton 2016, pp. 960-961), an increase in education may reduce the availability of informal caregivers through increased opportunity costs (due to forgone labour market opportunities or geographic mobility). To the extent that more education reduces the supply of informal care, this could increase the demand for formal care services and thus long-term care spending. In turn, this would urge policymakers to address the challenges faced by the formal care sector due to the unattractiveness of the care profession, and by informal caregivers due to difficulties in reconciling work, family and and caregiving.

A large literature has shown that the socioeconomic status, measured by income, occupation and education is strongly related to health outcomes (Deaton 2003, Mackenbach et al. 2008, Cutler et al. 2011). A special focus in the literature is on education, not least because education is likely to determine the other factors. It is well established that individuals with higher levels of education are healthier and live longer compared to those with lower education. The so-called education-health gradient has been observed in many countries, regardless of their level of development, in several time periods, at different levels of education and for various health outcomes (Grossman 2006, Cutler and Lleras-Muney 2008). The literature offers at least three possible explanations why more education can improve health. First, education may raise the efficiency in health production, referred to as "productive efficiency" (Grossman 1972). Put differently, better-educated individuals might obtain better health outcomes from given quantities of health inputs. Second, more education increases an individual's ability to acquire and process health information, thereby improving the "allocative efficiency" of health inputs, e.g. by choosing healthier lifestyles (Rosenzweig and Schultz 1982). Third, education may affect health indirectly through other channels such as higher income, safer and physically less demanding occupations, healthier peers, and better housing and environmental conditions (Lochner 2011).

More recently, the scope of the analysis of the education-health gradient has been extended by considering also spill-over effects of education on health. A large body of research found that investments in education may be advantageous not only for the health of the individuals themselves but also for the health of their peers (De Neve and Kawachi 2017). For instance, health benefits of education may also spill over to siblings (Kravdal 2008) or partners and spouses (Monden et al. 2003, Skalická and Kunst 2008). Moreover, intergenerational spill-over effects of education on health have moved into the center of attention. Starting in 2005, there is a growing literature on intergenerational health benefits of education, suggesting that better-educated parents have healthier children (Grossman 2006, Huebener 2020) and that parents of better-educated children are healthier (Yahirun et al. 2017, Lee 2017, Lee 2018, Peng et al. 2019, Thoma et al. 2021, Torres et al. 2021, Yahirun et al. 2022) and live longer (Zimmer et al. 2007, Torssander 2013, Friedman and Mare 2014, De Neve and Harling 2017, Elo et al. 2018, Smith-Greenaway et al. 2018, Sabater et al. 2020). Recent literature on intergenerational benefits of education has also suggested that adult children's education may affect their willingness to provide informal care to their ageing parents (Jiang and Kaushal 2020).

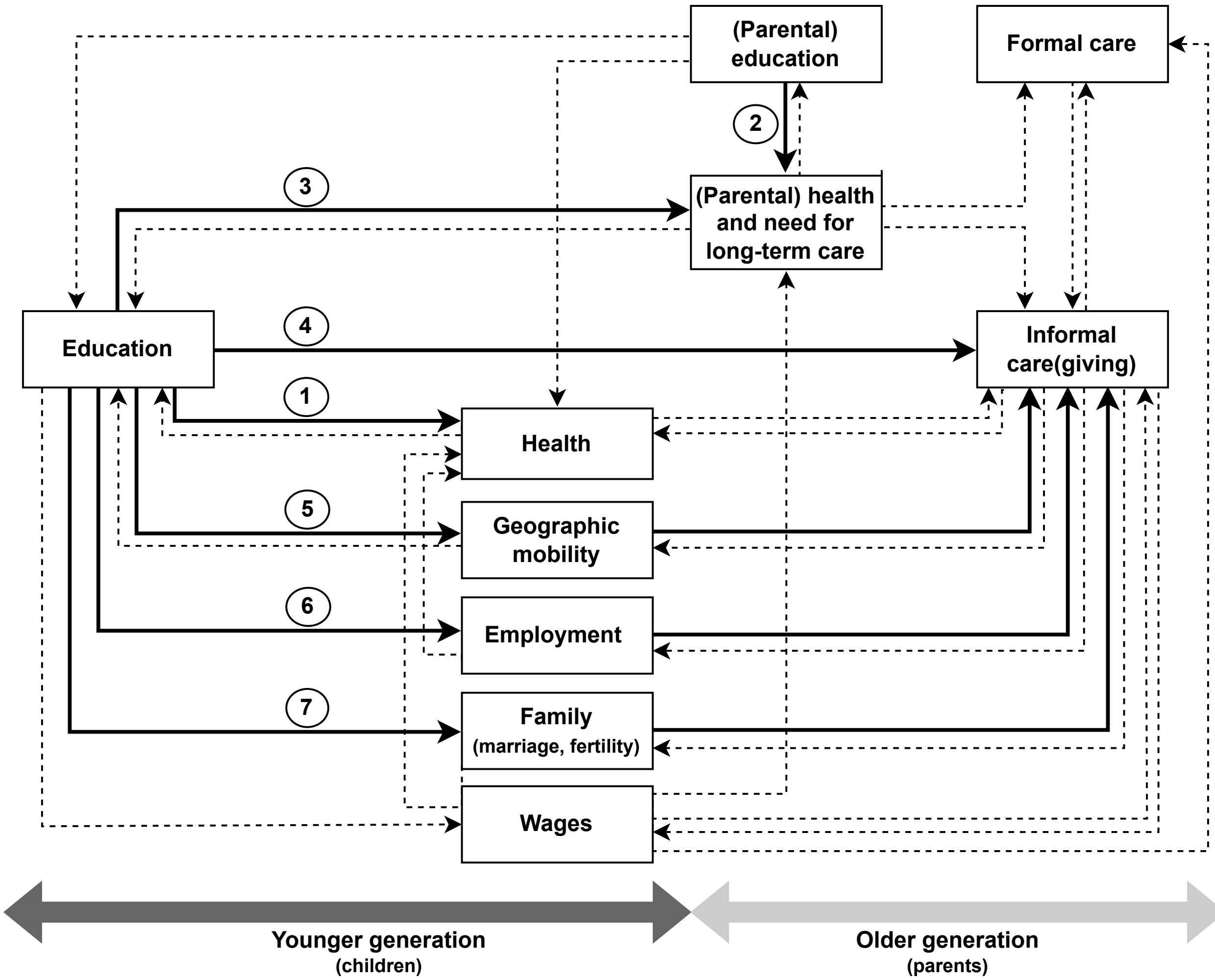
The associations between education and health, both within and across generations, do not necessarily reflect causal effects of education on health. The problem in identifying causal effects is the potential endogeneity of education. First, healthier people, who are likely to need less support and care in old age, usually have higher education and better-educated children (reversed causality problem). Second, unobserved factors such as ability might drive both good health and better education of parents and their children (omitted variables problem). The endogeneity problem also exists in the relationship between education and informal care provision because unobserved ability (e.g. time management skills) might simultaneously increase education and the likelihood to engage in informal caregiving. The most widely used approach in the literature to address this endogeneity and to establish causality in the relationship between education and health is to exploit exogenous variation in education induced by compulsory schooling reforms (Hamad et al. 2018). However, no consensus has been reached in the previous literature on whether or not the association between education and health within the same generation is causal as the previous literature has produced mixed results (Galama et al. 2018, Xue et al. 2021). The literature on intergenerational causal effects of children's education on parental health is very scarce and comes largely from developing or emerging countries (De Neve and Fink 2018, Ma 2019, Ma et al. 2021, Cui et al. 2021, Xie et al. 2021). To the best of my knowledge, there is no evidence so far on causal effects of adult children's education on the likelihood of informal care provision to older parents.

1.2 Aims and Contribution

The main aim of this thesis is to contribute to the growing literature on education as social determinant of health and related literature on interdependencies between parents and their offspring embedded in a social network. In particular, the thesis focusses on the causal effects of education on health, long-term care dependency and long-term care arrangements from an intragenerational and intergenerational perspective. The conceptual framework of the thesis is illustrated in Figure 1.1. The relationships that are directly investigated in this thesis are presented by solid arrows. The dashed arrows mirror interactions, which are also important for the understanding of the complex intragenerational and intergenerational relationship between education and health and related outcomes but that are not directly explored in this thesis.

The first relationship analysed in this thesis is the causal link running from own education to own health, as denoted by the solid arrows 1 and 2. A large literature has documented that education is strongly associated with better health and longer lives (Grossman 2006, Cutler and Lleras-Muney 2008). However, no consensus has been reached on whether more education really *causes* better health and longevity (Hamad et al. 2018, Xue et al. 2021). Chapter 2 relates to this ongoing debate and approaches the following question: "Can the relationship between education and health be interpreted as causal?". In particular, the chapter draws on ambulatory claims data on more than 23 million statutorily insured in Germany including information on a wide range of doctor-diagnosed health conditions to investigate the causal effect of schooling on measures of health that are "more objective" compared to self-reports. The analysis is complemented by using survey data from the German Socio-Economic Panel (SOEP), which is a large representative longitudinal survey of private households in Germany, that is conducted annually since 1984 and interviews around 30,000 respondents in nearly 14,000 households every year (Goebel et al. 2019).

Figure 1.1: Conceptional framework of the thesis



Notes: The figure shows the conceptual framework of the thesis. Dashed arrows present relationships that are not directly investigated in this thesis.
Source: Own illustration.

However, health status may not only be affected by own education but also by child's education, as indicated by the solid arrow 3. Children are an essential part of parents' social network and thus they might be an important source of support for parents in old age. For instance, better-educated children might be able to generate higher wages, allowing them to financially support their parents, by buying access to better housing and cleaner living environment, or by purchasing more and better health care. This financial pathway might, however, be more relevant in poorer countries, where access to health care largely depends on financial resources. Moreover, children can influence parental health by sharing health knowledge and influencing them to adopt healthier behaviours (Friedman and Mare 2014, pp. 1273-1274). In Chapter 3, the thesis relates to this "upward" intergenerational perspective on the causal link between education and health. The chapter tries to answer the following question: "Does adult children's education affect parents' need for long-term care?". The analyses are based on the Survey of Health, Ageing and Retirement in Europe (SHARE) - a multidisciplinary and cross-national panel database of micro data on health, socioeconomic status and social and family networks of more than 140,000 people aged 50 and over from 28 European countries and Israel (Börsch-Supan et al. 2013). The data set is very well suited for the purpose of this study as it is one of very few, if not the only data set that offers the possibility to obtain a relatively large sample with detailed information on two generations.

Apart from understanding the determinants of long-term care dependency, organizing long-term care for older people is particularly important given recent trends in population ageing. Since the largest share of long-term care in Europe is provided informally by family members and in particular by adult children (Norton 2016, pp. 960-961), one important question that arises is whether adult children's educational attainment causally affects the provision of informal care to their ageing parents (Jiang and Kaushal 2020), as indicated by the solid arrow 4. From a theoretical perspective, to the extent that education affects health, better-educated children might be more capable of providing informal care to parents (solid arrow 1). However, education may also raise children's opportunity cost of care, e.g. through geographic mobility (solid arrow 5), labour force participation (solid arrow 6), or family composition (solid arrow 7), thereby reducing the willingness to provide informal care to older parents. Moreover, as indicated by the dashed arrow at the bottom of the figure, education may positively affect children's wages, which increases their ability to finance formal care services, and decreases their dependency on inter-vivos transfers and bequests from parents that are often provided by parents in exchange for support in old age (Cox 1987, Bernheim et al. 1986). In turn, higher wages may reduce the willingness to provide informal care and increase the demand for formal care. Chapter 4 draws on this theoretical consideration by answering the following question: "Does adult children's education affect the provision of informal care to their ageing parents?". A particular focus in this chapter is on heterogeneous effects by adult children's gender and the potential underlying mechanisms. The analyses are also based on data from the Survey of Health, Ageing and Retirement in Europe (SHARE).

There are three common contributions across the three studies. First, all studies go beyond estimating correlations between education and health (or related outcomes) and attempt to establish causality. Over the last three decades, identifying and estimating causal effects with observational data has gained increasing attention in the applied economics literature. This trend is a likely result of the so-called "credibility revolution" in empirical economics that has shifted the focus to studies that use quasi-experimental methods such as difference-in-differences (DiD), instrumental variables (IV) and regression discontinuity (RD) designs for causal identification, thereby increasing the policy relevance and the scientific impact of empirical work (Angrist and Pischke 2010). In 2021, the Nobel Prize in Economics was awarded to David Card, Joshua Angrist, and Guido Imbens for their seminal work promoting the "credibility revolution". Therefore, a special attention in this thesis is on the identification of causal effects in order to derive policy implications from the results.

Second, this thesis uses the same identification strategy to estimate causal effects throughout all three chapters. In particular, compulsory schooling laws are exploited as source of exogenous variation in education, which is the most widely used and accepted approach in the quasi-experimental literature when estimating returns to education (Hamad et al. 2018). Compulsory schooling laws regulate the number of years children are required to attend school. Changes in compulsory schooling generate exogenous variation in years of schooling by birth cohort. Such reforms were implemented in different countries around the world during the 20th century. In Chapter 2, the empirical analysis follows Pischke and von Wachter (2008) and Kemptner et al. (2011) and builds on a reform in West Germany that increased compulsory schooling from 8 to 9 years in the basic track of secondary school (Hauptschule). During the 1940s to 1960s, this change was introduced gradually over time in the different federal states, which generates exogenous variation in years of schooling across states and over time. In Chapter 3 and Chapter 4, the analyses rely on a multi-country framework by exploiting compulsory schooling reforms implemented in seven European countries from the late 1960s to the late 1990s.

Third, all three studies employ an RD design for causal inference. Among the quasi-experimental methods, the RD approach, which was first suggested by Thistlethwaite and Campbell (1960), is one of the most credible methods for estimating causal effects from observational data as it requires seemingly mild assumptions compared to other approaches (DiNardo and Lee 2011, pp. 501-502). In the case of changes in compulsory schooling, the discontinuity is generated by increasing the compulsory years of schooling for cohorts born after a certain cut-off date. Outcomes of individuals born in the close neighbourhood of the discontinuity are then compared as those individuals are expected to be very similar in observed and unobserved characteristics. Hence, treatment assignment can be considered as good as random within a narrow window around the threshold (Cunningham 2021, pp. 243-245, Huntington-Klein 2021, pp. 505-514). Given that the treatment is not solely assigned on the basis of the assignment variable year or month of birth, a fuzzy RD design is appropriate in all chapters, which is numerically equivalent to an IV approach, where the discontinuity is used as an instrumental variable for treatment status (Cunningham 2021, pp. 279-282). In particular, individuals left school at an older age even before the reforms were implemented, e.g. because they attended a different school type that was not subject to the reforms. For instance, in case of the West German compulsory schooling reform, the treatment group does not only consist of basic track students, who were affected by the change in compulsory years of schooling but also of intermediate and academic track students, who actually do not receive the treatment.

Besides these commonalities in terms of causality, identification strategy and empirical method, the three studies differ in terms of their specific contribution to the literature. The specific contribution of each study is presented in the following chapters in detail.

1.3 Outline

The thesis contains three self-contained chapters, which deal with the intragenerational and intergenerational effects of education on health and related outcomes. The following subsections briefly summarize the three chapters and discuss the main findings.

Chapter 2: Revisiting the Causal Effect of Education on Health

The study in Chapter 2 is joint work with Hendrik Jürges and analysed the causal effect of education on health in Germany[1]. The study aimed at contributing to the rapidly growing literature on the causal effect of eduction on health by exploiting changes in compulsory schooling as source of exogenous variation in education. While some studies in the previous literature found significant positive effects of education on health and longevity (e.g. Lleras-Muney 2005, Silles 2009, Powdthavee 2010, Van Kippersluis et al. 2011, Kemptner et al. 2011, Gathmann et al. 2015, Fonseca et al. 2020), some other studies reported no significant effects (e.g. Albouy and Lequien 2009, Clark and Royer 2013, Jürges et al. 2013, Meghir et al. 2018, Courtin et al. 2019, Avendano et al. 2020, Albarrán et al. 2020, Malamud et al. 2021, Dilmaghani 2021). Hence, the previous literature has produced mixed results. For Germany, the results from quasi-experimental studies using instruments for education such as the abolition of fees in academic track schools, the construction of academic track schools, and changes in compulsory schooling also reached different conclusions (Reinhold and Jürges 2010, Jürges et al. 2011, Kemptner et al. 2011).

Using ambulatory claims data on more than 23 million statutorily insured and thus almost 90 percent of the German population in the relevant cohorts, we related to this ongoing debate and investigated the causal effect of schooling on health in the largest and

[1] A shortened version of the study is published in the European Journal of Health Economics. doi: `https://doi.org/10.1007/s10198-021-01404-y`.

most comprehensive analysis for Germany to date. The data set was provided by the National Association of Statutory Health Insurance Physicians (Kassenärztliche Bundesvereinigung, in short: KBV)[2] and contains information on a wide range of doctor-diagnosed health conditions coded according to the 10th revision of the International Classification of Diseases (ICD-10). The health conditions used in the study include ischemic heart disease, diabetes mellitus, obesity, depression, chronic lower respiratory diseases, cancer, diseases of the musculoskeletal system, back pain and diseases of the genitourinary system. Since the health conditions were doctor-diagnosed, the data set allowed us to estimate the causal effect of education on health measures, which are "more objective" compared to self-reported health measures that are often used in the previous literature and likely to be susceptible to reporting bias (Cleary 1997). In an RD approach with month-year of birth as assignment variable, we exploited changes in compulsory schooling from 8 to 9 years in West Germany during the 1940s to 1960s to estimate the reduced form effect of the reforms on doctor diagnoses. To assess the robustness of results, we followed a novel analytic technique suggested by Simonsohn et al. (2020) and estimated in total 240 alternative model specifications in a so-called specification curve analysis.

Our results suggested that the compulsory schooling reforms had, at best, very small impacts on the examined doctor diagnoses. In most of the specifications, we estimated insignificant zero effects. Since the KBV claims data only contain information on the federal state of residence in 2009 rather than information on the federal state of school graduation, which raises concerns about imprecise assignment of the instrument, we complemented our analysis by using the German Socio-Economic Panel (SOEP) to test for potential effects of measurement error in the instrument due to cross-state mobility in Germany. We found that geographic mobility between federal states is quite low, suggesting that it is unproblematic to use the federal state of residence to proxy the federal state of last school attendance. Using the SOEP, we also estimated the first stage and found that the compulsory schooling reforms in West Germany raised average years of schooling by about half a year. Moreover, in line with our findings for doctor diagnoses, the complementary analyses with SOEP data provided insufficient evidence for an effect of years of schooling on self-reported health. Therefore, our study confirmed what seems to be a growing consensus in the recent literature: the lack of a causal effect of education on health and mortality (Clark and Royer 2013, Albarrán et al. 2020, Avendano et al. 2020, Dilmaghani 2021, Malamud et al. 2021). Moreover, in line with our results, Xue et al. (2021) recently found in a meta-analysis including 99 published studies that the causal effect of education on health is basically

[2]The KBV is a coordinating body of the about 165,000 office-based physicians and psychotherapists in Germany.

zero after correcting for a moderate positive publication bias in the existing literature. In any case, our study questioned the large positive effects of education on health and longevity that have been found in the previous literature, e.g. by Lleras-Muney (2005) or Powdthavee (2010).

Chapter 3: Adult Children's Education and Parental Long-Term Care Dependency

The study in Chapter 3 is single-authored and analysed the intergenerational causal effect of adult children's education on parental long-term care dependency. The study aimed at contributing to the large and growing literature on the causal effect of own education on own health and to the emerging literature on intergenerational spill-over effects of education on health. In the literature on intergenerational health returns to education, most studies to date examined the "downward" effect of parental education on children's health, while the literature on "upward" effects of children's education on parental health and longevity has been scarce, especially in terms of causality (De Neve and Kawachi 2017). Although it is important to know if health returns to education spill over also from the child to the parent generation, given the demographic change and the ever-increasing numbers of older people in the population, researchers have only recently begun to exploit exogenous variation in education caused by changes in compulsory schooling to examine the causal link between children's education and parental health outcomes. These few studies found evidence for protective effects of children's education on maternal and paternal survival in Tanzania (De Neve and Fink 2018), on paternal survival in China (Cui et al. 2021), on parental cognitive function and lung function in China (Ma 2019), on parental cognitive abilities in Mexico (Ma et al. 2021) and on parental smoking cessation in China (Xie et al. 2021). Lundborg and Majlesi (2018) reported no overall effect on parental mortality in Sweden but some heterogeneity by gender, suggesting that daughters' schooling decreases fathers' mortality, especially among fathers from low socio-economic background. Since the existing literature has been mostly based on low- and middle-income countries, which are characterised by a relatively low level of public welfare provision and high level of intergenerational co-residence, the main aim of the study in Chapter 3 was to provide causal evidence on the intergenerational effect of children's education on parental health in a more developed context by analysing European countries. The focus in the study was on parents' long-term care dependency as very important health outcome with manifold consequences for families and the society as a whole. Based on data from five waves of the Survey of Health, Ageing and Retirement in Europe (SHARE), parents' long-term care dependency was measured by an overall disability score, which was constructed by conducting a principal component analysis on variables related to physical health and cognitive health (limitations in the activities of daily living, limitations in the instrumental activities of daily living, mobility limitations, grip strength, verbal fluency, time orientation, immediate and delayed word recall). In order to address the potential endogeneity of children's education, exogenous variation induced by compulsory schooling reforms across seven European countries was exploited within a fuzzy RD approach.

Ordinary least squares (OLS) results showed that children's years of schooling were negatively associated with parental long-term care dependency, measured by disability. However, when the potential endogeneity of children's education was taken into account in the fuzzy RD approach, the estimates became positive and statistically insignificant. Several sensitivity analyses confirmed the robustness of the findings. Moreover, heterogeneity analyses revealed no significant effects by gender, by number of children or by groups of countries with similar welfare and family systems according to the European north-south health gradient that has been observed in the previous literature (Ahrenfeldt et al. 2019). This suggested that the simple correlation between children's education and parents' health and long-term care dependency is likely to be confounded by unobservable factors that are correlated with both children's education and parents' health and long-term care dependency such as innate ability. For instance, smarter children are likely to acquire a better health knowledge through higher levels of education, that can be shared with parents to improve parents' health behaviours and health outcomes, thereby reducing parents' long-term care dependency. The study concluded that the absence of an intergenerational causal effect of children's education on parental health outcomes in Europe might be explained by the generosity of health care and long-term care programs in Europe that might weaken the importance of children's support and resources for parental health. Moreover, the low number of intergenerational households in Europe might explain why the study did not find a causal effect in Europe.

Chapter 4: Adult Children's Education and Informal Care Provision

The study in Chapter 4 is single-authored and aimed to add to the literature by providing, to the best of my knowledge, the first causal evidence on the effect of adult children's education on the provision of informal care to older parents. Against the background of the demographic change in European societies, the demand for long-term care is expected to increase during the next decades. In Europe, informal caregivers, and in particular adult children, meet a large part of long-term care needs (Norton 2016, pp. 960-961). One important determinant of adult children's willingness to provide care to their ageing parents might be education as it is likely to increase wages, to improve health, and to raise opportunity costs of care due to increased labour force participation and geographic mobility, which are plausible channels explaining a causal relationship between education and informal caregiving. Moreover, the distinction between adult daughters and sons might be important as daughters have traditionally been more often engaged in caregiving than sons (Schmid et al. 2012, pp. 39-40). In light of recent trends in population ageing, the question whether adult children's education causally affects the provision of informal care to older parents is very important from a policy perspective. To the extent that education affects the supply of informal care, it could have an impact on the demand for formal care services and thus long-term care expenditure.

As in Chapter 3, the study in Chapter 4 used data from the Survey of Health, Ageing and Retirement in Europe (SHARE) and exploited compulsory schooling reforms implemented in seven European countries to account for the potential endogeneity of education in a fuzzy RD approach. The sample has been restricted to children aged 18 years and over and parents aged 65 years and older, which is a pivotal age for being at risk of using long-term care. Descriptive OLS results showed that one additional year of schooling is associated with a 0.3 percentage points lower probability to provide care to older parents among daughters (relative to a mean of 12 percent) and with a 0.1 percentage points lower probability to provide care among sons (relative to a mean of 8 percent). Accounting for the potential endogeneity of education, the fuzzy RD estimates suggested that one more year of daughters' schooling significantly decreases the probability of providing care to parents by about 2 percentage points (i.e. 19 percent), while no significant effects were found for sons. Several sensitivity analyses demonstrated the robustness of the results to various bandwidths, functional forms and sample selection choices as well as to an alternative estimation method that accounts for rare outcomes. Moreover, the study provided some suggestive evidence that the effect is driven by daughters from Southern Europe providing care to parents with a high degree of long-term care dependency. Finally, the study investigated potential mechanisms and found that better-educated daughters' reduced probability to provide care might be explained by increased opportunity costs of care due to increased female labour force participation, while geographic distance to parents and changing family patterns did not seem to play an important role in explaining the results. Overall, the findings in Chapter 4 suggested that the demand for formal care services is expected to increase as a result of a reduced availability of informal caregivers, which in turn might increase long-term care expenditure. The study concluded that these trends combined with the growing number of older people in the population and the shortage of skilled workers in the formal care sector pose a major challenge for policy makers regarding the structure and organization of long-term care in Europe in the future.

Chapter 2

Revisiting the Causal Effect of Education on Health

Joint work with Hendrik Jürges

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2.1 Introduction

This chapter aims at contributing to the growing literature on the causal effect of education on health. Although there is consistent evidence that more educated people are healthier and live longer (Grossman 2006, Cutler and Lleras-Muney 2008), it is still unclear to what extent the education-health relationship is causal. The question whether it is causal is, however, particularly relevant for the formation of education and health policies. Against the background of the demographic change in most European populations characterized by a decline in fertility and an increase in life expectancy, and the huge healthcare costs that are involved, the health of a population should certainly be one of the highest priorities for policy makers. Provided that there is a causal effect of education on health, education policies might be a cost-effective tool to improve population health. Such policy interventions aimed at promoting health through educational initiatives are regularly proposed by international organisations such as the World Health Organization (WHO 2015), and the Organisation for Economic Cooperation and Development (OECD 2010).

In the theoretical literature, several mechanisms have been suggested to explain the link between education and health. The first theoretical explanation is that education raises the efficiency in health production by increasing the marginal productivity of health inputs, referred to as "productive efficiency" (Grossman 1972). Moreover, "allocative efficiency" can explain the link between education and health. That is, education can improve health through information by choosing better health inputs (Rosenzweig and Schultz 1982). Finally, education may affect health indirectly through other channels, such as higher earnings, safer and physically less demanding jobs, healthier living environments or the interaction with healthier peers (Lochner 2011, p. 248). However, the interpretation of the association as causal is difficult because education is most likely endogenous. First, the correlation may come from unobserved confounding variables that affect education and health simultaneously, such as genetic endowments (Behrman et al. 2011), cognitive ability (Angrist and Krueger 2001) or time preferences (Fuchs 1982). Second, the relationship may be driven by reverse causality if unhealthy children obtain less education (Case et al. 2005).

Following Angrist and Krueger (1991), a growing number of studies has exploited exogenous variation in education caused by changes in compulsory schooling laws using instrumental variables (IV) or regression discontinuity (RD) techniques to estimate causal effects of education on various outcomes, including health and mortality. However, the findings of these studies are mixed. Some studies find that education improves health and health behaviours (Mazumder 2008, Silles 2009, Powdthavee 2010, Kemptner et al. 2011, Brunello et al. 2013, Crespo et al. 2014, Mazzonna 2014, Fletcher 2015, Li and Powdthavee 2015, Silles 2015, Brunello et al. 2016, Dursun et al. 2018, Fonseca et al. 2020, Janke et al. 2020, Ye et al. 2022) or reduces mortality (Lleras-Muney 2005, Van Kippersluis et al. 2011, Fischer et al. 2013, Gathmann et al. 2015, Davies et al. 2018, Grytten et al. 2020), while other studies do not find evidence for a causal effect on health and health behaviours (Arendt 2005, Oreopoulos 2008, Clark and Royer 2013, Jürges et al. 2013, Courtin et al. 2019, Baltagi et al. 2019, Dahmann and Schnitzlein 2019, Avendano et al. 2020, Albarrán et al. 2020, Malamud et al. 2021, Dilmaghani 2021) or mortality (Mazumder 2008, Albouy and Lequien 2009, Lager and Torssander 2012, Clark and Royer 2013, Fletcher 2015, Meghir et al. 2018, Malamud et al. 2021). Hence, the existing literature does not come to a consensus and the debate on whether the correlation between education and health can be interpreted as causal is still ongoing, as a considerable amount of recently published studies have shown (Grytten et al. 2020, Albarrán et al. 2020, Janke et al. 2020, Avendano et al. 2020, Fonseca et al. 2020, Dilmaghani 2021, Malamud et al. 2021, Albarrán et al. 2022, Ye et al. 2022). Lately, Xue et al. (2021) performed a meta-analysis and found that the exiting literature on the causal effect of education on health suffers from publication bias in favour of positive results and that the effect of education on health is close to zero after correcting for this publication bias.

We contribute to the existing literature in five important ways. First, we perform the largest and most comprehensive analysis on the causal effect of education on health for Germany to date. Our main data set has been provided by the National Association of Statutory Health Insurance Physicians (Kassenärztliche Bundesvereinigung, in short: KBV) and contains ambulatory care insurance claims of overall 23.6 million statutorily insured and thus almost 90 percent of the German population in the relevant cohorts (Federal Statistical Office 2022). To the best of our knowledge, this is the largest data set that has ever been used to study the causal effect of education on health. Second, our data set allows us to study the causal effect of education on a wide range of doctor-diagnosed diseases coded according to the 10th revision of the International Classification of Diseases (ICD-10) that cover physical health and mental health conditions. So far, researchers have mostly used self-reported health measures. However, it is often discussed that these measures are subject to reporting bias as people may under-, over-, or misreport their health for certain reasons (e.g. understanding of questions, financial incentives). This is problematic because these factors tend to vary with socioeconomic characteristics such as education. Therefore, objective health measures are often preferred over self-reported measures (Cleary 1997, Bago d'Uva et al. 2008, Johnston et al. 2009, Dowd and Todd 2011). Since our data set contains doctor-diagnosed conditions we are able to study the causal effect of education on health measures that are "more objective" compared to self-reported health measures. Third, we use an

RD approach to estimate the causal effect of education on health, which is probably the most credible approach to identify causal effects as it is the closest empirical strategy to the gold standard of randomized experiments and needs the fewest assumptions for identification (DiNardo and Lee 2011, p. 501). Fourth, our data set allows us to use month of birth instead of year or quarter of birth to assign the treatment status, which leads to more precise estimates[1]. Finally, we perform a specification curve analysis to assess the robustness of findings to various model specifications in order to account for subjective analytic decisions by the researcher that might affect the empirical results (Simonsohn et al. 2020).

Following Pischke and von Wachter (2008) and Kemptner et al. (2011), we exploit compulsory schooling reforms that were implemented in West Germany between 1946 and 1969 and raised compulsory schooling from 8 to 9 years in the basic track of secondary schooling. Using the claims data, we identify the reduced form effect of the compulsory schooling reforms on doctor-diagnosed conditions in an RD approach. The results indicate that the compulsory schooling reforms have, at best, very small impacts on doctor diagnoses, including ischemic heart disease, diabetes mellitus, obesity, chronic lower respiratory disease, cancer and urogenital diseases. In most of the specifications we estimate insignificant effects that are close to zero and often of the "wrong" sign. Since the claims data only contain information on the federal state of residence in 2009 rather than information on the federal state of school graduation, we complement our analysis by using the German Socio-Economic Panel (SOEP) to test for potential effects of measurement error in the instrument due to cross-state mobility in Germany. We find that mobility between federal states is quite low and conclude that it is not a major problem to proxy the state of last school attendance with the state of residence. Using the SOEP, we also estimate the first stage effect of the West German compulsory schooling reforms on average years of schooling and find that the reforms led to an average increase of about 0.5 years in school. Moreover, in line with our findings for doctor diagnoses, we find insufficient evidence for an effect of years of schooling on self-reported health using the SOEP data. Therefore, our study questions the presence of the sometimes quite large positive effects of education on health that are found in the previous literature, e.g. by Lleras-Muney (2005) or Powdthavee (2010), and casts doubt on the effectiveness of policies aimed at promoting population health through educational interventions.

The remainder of the chapter is organized as follows. Section 2.2 reviews the literature on the causal effect of education on health by focussing on theoretical mechanisms and empirical

[1]According to Mazumder (2012), we thus fulfil all methodological aspects that are needed for convincing study when exploiting compulsory schooling laws for identification: adopting an RD framework, exploiting month of birth for the assignment of the treatment status and using sufficiently large samples.

evidence. Section 2.3 outlines the empirical approach adopted in this study, including a description of the institutional background, the identification strategy and the specification curve method. In Section 2.4, we describe the two data sets that we use for the analyses. Section 2.5 reports and discusses the findings and the robustness of results, while Section 2.6 summarizes and concludes.

2.2 Literature Review

2.2.1 Theoretical Mechanisms

In the theoretical literature, several mechanisms have been suggested to explain the link between education and health. One often-discussed channel is "productive efficiency", that is, education enters as a factor in the health production function and raises the efficiency in health production. Thus, more educated people are able to produce better health outputs from given quantities of health inputs (Grossman 1972). For example, they are able to understand and follow the doctors' instructions, which raises the benefits of doctor visits and results in a more effective treatment. Furthermore, education can improve health through a better choice of health inputs by improving the knowledge on the relationship between health behaviours and outcomes, referred to as "allocative efficiency" (Rosenzweig and Schultz 1982). Put differently, more educated people are better at obtaining, evaluating and processing health information and may therefore be able to improve their health by using a more efficient mix of health inputs. For instance, more educated individuals are likely to be better informed about the adverse effects of smoking and consequently they are more likely to smoke less or to quit smoking. Finally, in addition to the two direct pathways, education may affect health indirectly through other channels. For example, better education may increase earnings, allowing people to buy healthier food, to purchase a gym membership or to pay higher housing prices to live in healthier regions with less traffic and pollution. Moreover, better-educated people tend to work in safer and healthier jobs and to interact with healthier peers, who promote good health behaviours (Lochner 2011, pp. 247-248).

Despite the theoretical pathways discussed above, the interpretation of the association between education and health as causal is difficult because education is most likely endogenous. First, the relationship may be driven by reverse causality since poor health in childhood can lead to lower educational attainment (Case et al. 2005, Cornaglia et al. 2015). Second, the correlation may come from unobserved confounding variables that affect both education and health but that are omitted from the regression equation because they cannot be measured or are simply not available in the data set. The confounder that is most often mentioned in the economic literature is cognitive ability. That is, smarter individuals are more likely to obtain more education and also to have a more favourable health behaviour and thus better health status (Angrist and Krueger 2001). Another unobserved confounder is time preference since individuals with a low discount rate, i.e. a strong preference for future outcomes over present outcomes, are more likely to invest in both education and health because such investments give rise to long-run benefits (Fuchs 1982). If unobserved confounders and reverse causality are not controlled for, the estimate of the causal effect of education on health is likely to be biased and hence, exogenous variation in education is essentially needed to establish causality (Grytten 2017, pp. 486-487).

2.2.2 Empirical Evidence

Researchers have exploited different sources of exogenous variation in education in order to identify the causal effect of education on health, such as exemptions from military service (e.g. the Vietnam draft lottery), distance to college, abolition of secondary school fees, educational expansions (through the opening of additional schools or colleges), or changes in school year length. Some of these identification strategies have been successfully used in the German context (Jürges et al. 2009, Reinhold and Jürges 2010, Riphahn 2012). Starting with Angrist and Krueger (1991), a large and growing body of literature examines the causal effect of education on health exploiting compulsory schooling laws. Compulsory schooling laws regulate the minimum years individuals have to spend in school or the minimum school leaving age. During the 20th century many countries changed minimum schooling legislation towards more schooling, typically in the middle to lower parts of the educational distribution. In this section, we review the empirical literature on the causal link between education and health outcomes exploiting compulsory schooling reforms as source of exogenous variation in education. According to the examined health measures, we grouped the existing studies into three categories: mortality, self-reported health and health behaviours and "more objective" health. Table 2.A.3 in the Appendix summarizes the findings of the studies.

Lleras-Muney (2005) was one of the first to estimate the causal impact of education on mortality exploiting compulsory schooling laws. Her results suggest a very large reduction on the probability of dying in the next 10 years in the United States, that are, however, questioned by Mazumder (2008) who shows that her results are not robust to the inclusion of state-specific cohort trends. Much smaller effects of education on mortality have been found by Van Kippersluis et al. (2011) for the Netherlands and by Davies et al. (2018) for the UK. In another study, Fischer et al. (2013) report negative effects on mortality for men in

Sweden. Gathmann et al. (2015) evaluate several compulsory schooling reforms implemented in Europe during the 20th century and find small negative effects on mortality for men but no effects for women. Furthermore, it appears that reforms implemented in the early 20th century have a stronger impact on mortality than later reforms. Recently, Grytten et al. (2020) provide evidence, for Norway, suggesting that education has a strong causal effect on mortality for men - to a large part due to fewer accidental deaths. Other studies, however, find no support for a causal effect of education on mortality, e.g. Albouy and Lequien (2009) for France, Lager and Torssander (2012) and Meghir et al. (2018) for Sweden, Clark and Royer (2013) for the UK and Fletcher (2015) for the United States. The latter findings are in line with those of a recent study by Malamud et al. (2021), for Romania, suggesting no mortality reductions due to additional education.

Another set of studies estimates causal effects of education on self-rated health and health-behaviours. The findings of these studies also offer contradictory evidence. Mazumder (2008) and Fletcher (2015) provide evidence for the US, suggesting that education improves self-rated health, which is in line with findings by Silles (2009) for the UK and Li and Powdthavee (2015) for Australia. These findings are also consistent with those by Mazzonna (2014) and Brunello et al. (2016) in several European countries in a multi-country framework. For West Germany, Kemptner et al. (2011) find protective effects of education on self-reported health for men but not for women. Heterogeneity by gender is also found by Dursun et al. (2018) for Turkey. In addition, Kemptner et al. (2011) show that additional schooling reduces the probability of having weight problems for both sexes, while Dursun et al. (2018) report protective effects on the probability of being in the healthy weight range for females in Turkey. The latter finding is in line with a study by Brunello et al. (2013), providing evidence for a positive effect of education on the BMI of females in nine European countries. Silles (2015) examines the effect on smoking behaviour and finds that additional education lowers the probability of smoking for males in Northern Ireland. Moreover, two studies suggest that education also improves mental health (Mazzonna 2014, Crespo et al. 2014). In contrast, other studies provide no support for a causal effect of education on self-reported health (Arendt 2005, Oreopoulos 2008, Clark and Royer 2013, Jürges et al. 2013, Baltagi et al. 2019), BMI and other weight-related outcomes (Arendt 2005, Clark and Royer 2013, Li and Powdthavee 2015, Baltagi et al. 2019), smoking behaviour (Arendt 2005, Kemptner et al. 2011, Clark and Royer 2013, Li and Powdthavee 2015, Dursun et al. 2018, Baltagi et al. 2019) or mental health (Dahmann and Schnitzlein 2019, Avendano et al. 2020). Recent evidence on the causal effect of education on self-reported health is provided by Fonseca et al. (2020), Janke et al. (2020), Albarrán et al. (2020), Dilmaghani (2021) and Malamud et al. (2021). Fonseca et al. (2020) find that education has a positive effect on a wide range of self-reported health measures including functional status, instrumental functional status and chronic conditions when evaluating several reforms in the US, the UK and Continental Europe with SHARE, HRS and ELSA data[2]. In contrast, Janke et al. (2020) report that education has no effect on a number of self-rated chronic conditions, except for diabetes, when analysing two UK education policy reforms with data from the Quarterly Labour Force Survey. Dilmaghani (2021) and Malamud et al. (2021) find no support for an effect on self-rated health for Canada or Romania, respectively, which is consistent with findings by Albarrán et al. (2020) for several European countries.

The literature on the causal effect of education on more objective health measures such as biomarkers is relatively scarce. To the best of our knowledge, only five studies so far have focused on biomarkers of health, and these studies have also generated mixed results. Powdthavee (2010) exploits two compulsory schooling laws in the UK in 1947 and 1973 to examine the effect of education on hypertension as important predictor of heart disease and finds a reduction in hypertension for the first law but no effect for the second law. However, exploiting the same reforms, Clark and Royer (2013) and Jürges et al. (2013) report no effects on hypertension and blood pressure, or blood fibrinogen and C-reactive protein levels, respectively. More recently, Courtin et al. (2019) evaluate the 1959 Berthoin compulsory schooling law in France and find that education has no effect on 16 biomarkers of cardiovascular, immune, metabolic and organ function. Lately, Ye et al. (2022) analyse the effect of the 1986 Chinese nine-year compulsory education law on allostatic load calculated based on a wide range of biomarkers, including the cardiovascular system, the metabolic system, the inflammation system and the urinary system, and find heterogeneous effects by socioeconomic background. More specifically, the authors show that reform eligibility reduced the metabolic risk and total allostatic load among individuals from communities with the middle third per capita income, while it had no effect on allostatic risks for the overall sample and for individuals from communities with the lowest or highest third of per capita income.

Overall, the literature does not come to a consensus to what extent the link between education and health and longevity is causal, which is also the conclusion of a systematic review and meta-analysis by Hamad et al. (2018). Moreover, in a recently published meta-analysis on the literature on the causal effect of education on health, including 99 published papers, Xue et al. (2021) show that the exiting literature suffers from publication bias in favour of positive results and that the effect of education on health outcomes is close to zero

[2]Lately, Albarrán et al. (2022) question the causal findings and replicate the work by Fonseca et al. (2020). Using the same data sets and the same countries and reforms, the authors are not able to find a causal effect of education on any of the health outcomes. Albarrán et al. (2022) emphasize the importance of including country-specific trends in the regression models.

after correcting for this publication bias. However, the debate on whether the correlation between education and health can be interpreted causally is still ongoing as a considerable amount of recently published studies shows (Grytten et al. 2020, Albarrán et al. 2020, Janke et al. 2020, Avendano et al. 2020, Fonseca et al. 2020, Dilmaghani 2021, Malamud et al. 2021, Albarrán et al. 2022, Ye et al. 2022).

2.3 Empirical Approach

In this section, we outline the most salient aspects of the German school system and compulsory schooling reforms in West Germany, which is followed by the identification strategy. Finally, we describe the specification curve analysis method used to graphically assess the robustness of results.

2.3.1 Institutional Background

School System in Germany

In Germany, education policy is regulated on the federal state level, but nevertheless the school systems are almost identical across federal states (Dustmann 2004, p. 212). After voluntary pre-school, children enter primary school at the age of six and attend it usually for four years. Then they continue schooling in secondary schools. The German secondary school system is tripartite and consists of basic track (Hauptschule), intermediate track (Realschule) and academic track (Gymnasium) that are taught at separate schools, differ by duration and curriculum and lead to different leaving certificates. The basic track is the lowest level of secondary school that provides general basic education and leads to a leaving certificate after eight or nine years. Students in the intermediate track obtain more extensive general education and graduate after 10 years. After finishing basic track or intermediate track school, students usually continue their education starting an apprenticeship or a school-based vocational training. The academic track is the most demanding and academic-orientated track, leading to an university-entrance diploma (Abitur) after grade 13[3]. A lower-level qualification, the technical school degree (Fachhochschulreife), which allows students to attend a polytechnic (Fachhochschule) can be obtained after finishing grade 12 (Pischke and von Wachter 2008, p. 593). In addition, there is a fourth type of

[3]Starting in 2001, a school reform was introduced in most federal states that decreased the length of academic track schooling from 13 to 12 years (Hofmann and Mühlenweg 2018). As we consider only individuals that attended academic track schools in the 1940s to 1970s, this reform is not relevant for our analysis. Thus, we assume 13 years of schooling in the academic track when constructing the schooling variable.

secondary school, the so-called comprehensive school (Gesamtschule). This type of secondary school combines all three secondary school tracks at one and the same school. All secondary school leaving certificates can be obtained. However, comprehensive schools do not exist in all states and are rather unimportant with only about 10 percent of all children in Germany attending it (Jürges and Schneider 2011, p. 375). Since the first comprehensive schools were only introduced between 1973 and 1982 on a trial basis (Mühlenweg 2008, p. 356), this type of school is also relatively new and therefore not relevant for our study as we restrict our analysis to individuals born between 1930 and 1959, who finished secondary school in the late 1970s at the latest.

The allocation to one of the secondary school tracks after primary school depends to a great extent on the students' grades in primary school and thus on students' ability. On that basis, the class teacher of the primary school usually gives a recommendation for the secondary school type he or she thinks the child should attend after grade four. In 10 out of 16 federal states this recommendation is binding, while the final decision is taken by the parents in the remaining six federal states (Jürges and Schneider 2011, p. 375, Dustmann et al. 2017, p. 1351). Since students in Germany are allocated to one of the secondary school tracks very early at the age of ten, when information about the students' learning potential is likely to be incomplete, there is a high risk of misallocating students to tracks. In general, there are possibilities for correcting initial track decisions at a later point, when more information about students' ability is available. In principle, switching between secondary school tracks is possible at any grade, but in practice this happens rarely. Dustmann et al. (2017) report that only about 2 percent of students switch between tracks throughout secondary school in Germany. As a consequence, after allocating students to one of the secondary school tracks, they generally stay in that track until they complete it. However, upgrading and downgrading of students between school tracks at later stages of the educational career is very common. Dustmann et al. (2017) show that there is a substantial movement from the basic track and intermediate track to the academic track after graduating from the tracks. Moreover, there is a large amount of downgrading in the form of not enrolling into university after graduating from the academic track because many students with university-entrance diploma (Abitur) start an apprenticeship instead of starting university. Moreover, Dustmann et al. (2017) find that, due to this possibility of upgrading and downgrading, the assignment to a particular secondary school track at the end of the primary school has only little effect on the highest degree and long-term labour market outcomes.

Compulsory Schooling Reforms in Germany

After World War II, West Germany experienced a trend towards higher education due to several educational reforms. One of these reforms was the abolition of school fees in West German secondary schools (Reinhold and Jürges 2010, Riphahn 2012). Moreover, more academic track schools were built in the postwar period in West Germany in order to increase the proportion of academic track graduates (Jürges et al. 2011). The reform we exploit in this paper led also to an educational expansion in West Germany by prolonging compulsory basic track schooling from 8 to 9 years. The reform has already been used to estimate causal effects of schooling on earnings (Pischke and von Wachter 2008, Kamhöfer and Schmitz 2016, Cygan-Rehm 2018, 2022)[4], on fertility (Cygan-Rehm and Maeder 2013), on intergenerational transmission of education (Piopiunik 2014), on political behaviour (Siedler 2010) and on pro-immigration attitudes (Margaryan et al. 2021).

According to Petzold (1981), the introduction of the reform had three main objectives. First, there were pedagogical and psychological arguments, mainly presented by pedagogues, psychologist and physicians, who argued that students were too young and immature for the labour market after eight years of schooling. Sending 14-year old students at the beginning of puberty to the labour market was further considered to be dangerous for their mental and physical development (Petzold 1981, p. 84). The additional ninth grade also aimed at preparing students for their working life. The idea was to provide students guidance in choosing an occupation in the additional school year, since they were considered not mature enough to choose an occupation after eight years in school (Petzold 1981, p. 85). Second, political parties, associations and institutions expected a mitigation of youth unemployment and the social changes involved. The period after World War II was characterized by a high level of youth unemployment and a shortage of vacant apprenticeships. Therefore, the additional school year was sometimes seen as an "institutional storage" of students, who would have been unemployed otherwise (Petzold 1981, p. 87). Third, arguments with respect to educational economics became prevalent at the end of the 1950s. Due to technical progress it became necessary to have more intellectual instead of manual workers. Therefore, the additional school year aimed at guiding youths away from manual to more intellectually demanding jobs by providing longer and more academic education (Petzold 1981, p. 93).

Since the federal states are responsible for education policy in Germany, the ninth year of compulsory schooling was introduced at different points in time in the federal states. Table 2.1 reports, by federal state, the year and month of implementation and the first month-year

[4] Pischke and von Wachter (2008) and Kamhöfer and Schmitz (2016) estimate zero effects of compulsory schooling on earnings in Germany. However, recently, Cygan-Rehm (2018, 2022) finds that there are positive wage returns to education when re-analysing the data from Pischke and von Wachter (2008).

of birth cohort of individuals affected by the reforms. While the two northern states Hamburg and Schleswig-Holstein already introduced the reform in the 1940s, the other states followed in the late 1950s and 1960s. In Saarland, the compulsory ninth grade was implemented in 1958 (Leschinsky and Roeder 1980, p. 332, Backhaus 1963, pp. 43-44). Bremen decided to introduce it in 1959 after having several years of a voluntary ninth grade (Bremen 1957, Backhaus 1963, p. 44, Leschinsky and Roeder 1980, p. 332). In Lower-Saxony, the additional school year was already determined by law in 1954, but the overall introduction was in April 1962 (Lower Saxony 1954, Backhaus 1963, pp. 40-41, Leschinsky and Roeder 1980, p. 332). The remaining five states introduced the compulsory ninth grade due to the so-called Hamburg Accord ("Hamburger Abkommen") in 1964, in which the prime ministers of the federal states agreed on the introduction by 1967 at the latest (Pischke and von Wachter 2008, p. 593). As a consequence, North Rhine-Westphalia introduced it in 1966, followed by Baden-Wuerttemberg and Rhineland-Palatinate in 1967 (North Rhine-Westphalia 1966, Baden-Wuerttemberg 1964, Rhineland-Palatinate 1966). In Hesse, some schools that already fulfilled the organisational requirements started to introduce it in 1962, but the overall implementation followed in 1966 (Hesse 1961, Hesse 1965, Helbig and Nikolai 2015, p. 63). Bavaria was the last state that implemented the ninth grade in 1969 (Bavaria 1969, Helbig and Nikolai 2015, p. 63).

Table 2.1: Compulsory schooling reforms in West Germany

Federal state	**Reform implementation**	**First birth cohort affected**
Hamburg	April 1946	April 1931
Schleswig-Holstein	April 1947	April 1932
Saarland	April 1958	April 1943
Bremen	April 1959	April 1944
Lower Saxony	April 1962	April 1947
Hesse	April 1966	April 1951
North Rhine-Westphalia	April 1966	April 1951
Rhineland-Palatinate	April 1967	April 1952
Baden-Wuerttemberg	April 1967	April 1952
Bavaria	August 1969	August 1954

Notes: The table reports, by federal state, the year and month of implementation and the first month-year of birth cohort of individuals affected by the compulsory schooling reforms (i.e. the pivotal cohort). The pivotal birth cohort is calculated by assuming that children enter school with the beginning of the school year after turning six years old.
Source: Backhaus (1963), Leschinsky and Roeder (1980), Helbig and Nikolai (2015), Bremen (1957), Lower Saxony (1954), North Rhine-Westphalia (1966), Baden-Wuerttemberg (1964), Rhineland-Palatinate (1966), Hesse (1961), Hesse (1965), Bavaria (1969), Federal Statistical Office (1964) and respective volumes of the following years.

For some federal states, the reform introduction dates differ from the ones reported in previous studies that use the same German compulsory schooling reforms. Most of these studies refer to Pischke and von Wachter (2008), who were the first to exploit the West German reforms in order to estimate returns to education. For some federal states there is only a minor difference in the reported dates, for others, e.g. for Saarland and Schleswig-Holstein, the difference is quite large (see Table 2.A.1 in the Appendix). For Saarland, Pischke and von Wachter (2008) report the introduction of the reform in 1964, while we state that it was already in 1958. Moreover, according to Pischke and von Wachter (2008), the compulsory ninth grade was introduced in 1956 in Schleswig-Holstein, but we report that it was already implemented in 1947. One reason for these differences may be that it was possible in some federal states for municipalities to have a voluntary ninth grade before the overall implementation was stipulated by law. Our implementation dates refer to the date when the compulsory ninth grade was implemented in the whole federal state. Pischke and von Wachter (2008) do not provide information, which references they use. We took the information from contemporaneous literature on the German school system by Leschinsky and Roeder (1980) and Backhaus (1963) and from respective federal state laws (Bremen 1957, Lower Saxony 1954, North Rhine-Westphalia 1966, Baden-Wuerttemberg 1964, Rhineland-Palatinate 1966, Hesse 1961, Hesse 1965, Bavaria 1969). Moreover, we reviewed data from the Federal Statistical Office of Germany on the ninth grade attendance in the basic track between 1957 and 1973 (Federal Statistical Office 1964 and respective volumes of the following years). Taken together, these references suggest that the implementation dates reported by Pischke and von Wachter (2008) are sometimes incorrect. Recent work by Cygan-Rehm (2018, 2022) questions the reform dates in Pischke and von Wachter (2008) as well. Referring to Leschinsky and Roeder and data from the Federal Statistical Office of Germany as well, she reports reform dates that match ours (see Table 2.A.1 in the Appendix). Two exceptions are Hesse and North Rhine-Westphalia with a difference of one year each.

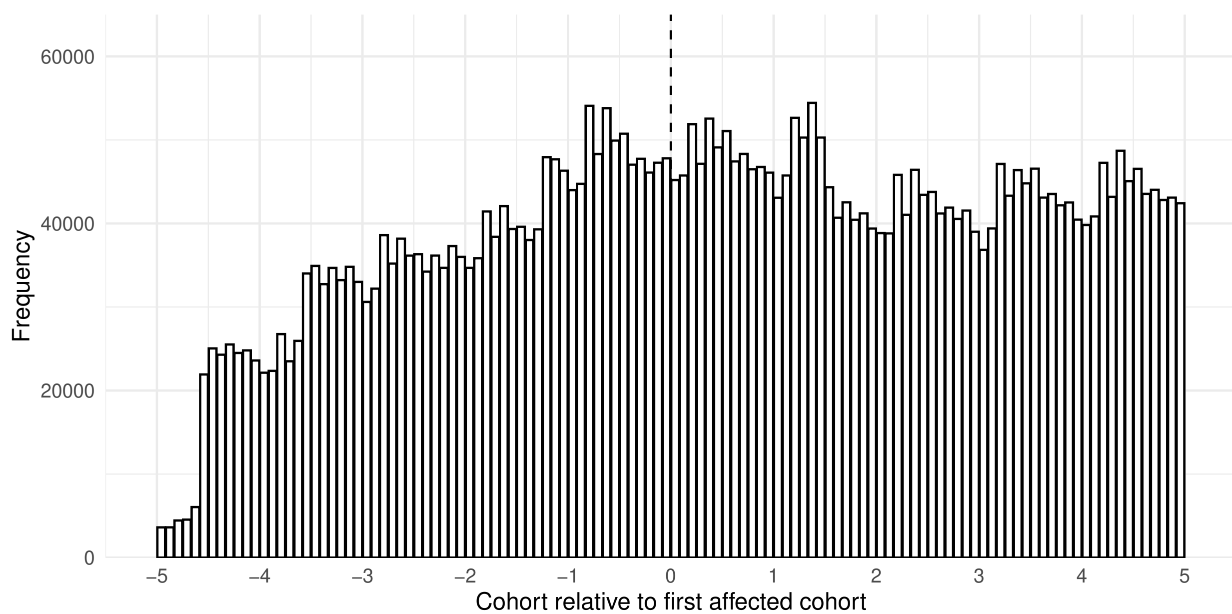
In Figure 2.A.1 in the Appendix, we plot the number of basic track students in the ninth grade in the school year t as a fraction of the number of basic track students attending the eighth grade in the previous school year t-1 based on data of the Federal Statistical Office of Germany. Looking at Saarland, for example, we observe that the reform became effective in 1958 as the ninth grade attendance jumps from 0 percent in 1957 to 99 percent in 1958. This matches exactly the timing reported by Leschinsky and Roeder (1980) and Backhaus (1963) and clearly contradicts the date reported by Pischke and von Wachter (2008). Unfortunately, we do not have early enough data for Schleswig-Holstein to be sure that our reported date is correct. According to Leschinsky and Roeder (1980) and Backhaus (1963), the reform introduction was already in 1947, but data is only available from 1957 onwards. However, we are quite confident that the date that was reported by Pischke and von Wachter (2008) has to be incorrect because the compulsory ninth grade was already stipulated by law in 1947 (Schleswig-Holstein 1947). For the remaining federal states, our reported reform implementation dates also seem to fit with the data as there are jumps in the ninth grade attendance exactly when the additional school year was fully implemented.

In some federal states, the compulsory schooling reform coincided with another reform that shifted the start of the school year from April to August in all federal states except for Bavaria, which started the school year in August already. This transition had to be performed by the beginning of the school year 1967. As a consequence, eight of the 10 states decided to have two short school years of eight months each. Hamburg implemented a long school year (Pischke 2007). Baden-Wuerttemberg, Hesse, North Rhine-Westphalia and Rhineland-Palatinate were affected by these simultaneous implementations since they introduced the additional year of compulsory schooling with the beginning of the school year 1966. A potential problem with the introduction of the short school years is that the first cohorts after the introduction of compulsory schooling reform formally completed nine instead of eight school years although they did not spent much more time in school compared to the previous cohorts who were subject to eight years of compulsory schooling. Thus, the true amount of schooling of the affected cohorts is over-estimated. However, Kemptner et al. (2011) address this issue and show that their estimates are robust to recoding the education variable, indicating that the short school years should not present a major problem for our analysis.

2.3.2 Identification Strategy

In order to estimate the causal effect of schooling on health, we use an regression discontinuity (RD) approach. This method, which was first introduced by Thistlethwaite and Campbell (1960) as a way of estimating treatment effects in a non-experimental setting, is appropriate for the purpose of this study because receiving the treatment (i.e. nine instead of eight years of compulsory schooling) depends on an observed covariate (month-year of birth), which is referred to as treatment-determining or assignment variable. The basic idea of an RD approach is that individuals with values of the assignment variable above some cut-off receive the treatment, while individuals with values below the cut-off do not receive it. Outcomes for individuals in a small range above and below the cut-off are then compared as these individuals are expected not to differ systematically in their observable and unobservable characteristics but only by being treated or not treated. The key identifying assumption for an RD design to be valid is that all observable and unobservable factors are continuously related to the assignment variable, that is, there are no discrete jumps at the cut-off. In case of unobservable factors, this assumption cannot be tested, but it is common practice to provide evidence on its plausibility by showing that no observable covariates discontinuously change around the cut-off (Lee and Lemieux 2010, p. 283). However, this is not possible in our case since the claims data only contain year and month of birth, federal state, gender and doctor diagnoses but no potential covariates. In addition, the continuity assumption may not be plausible if individuals are able to manipulate their treatment status to be on one particular side of the cut-off. Although is reasonable to assume that parents were unable to manipulate their children's month-year of birth because the birth date is determined considerably before the announcement of the policy, we show a histogram of the assignment variable for observations above and below the cut-off to assess the potential manipulation of the assignment variable, as suggested by Lee and Lemieux (2010). The distribution of month-year of birth in Figure 2.1 shows some seasonal variations[5] but apparently no jump at the cut-off. Hence, there is no sign of manipulation of the assignment variable.

Figure 2.1: Histogram of the assignment variable



Notes: The figure shows a histogram of the assignment variable (month-year of birth) for cohorts born five years before and after the first birth cohort affected by the reforms. There is no evidence for bunching at the cut-off and therefore no sign of manipulation of the assignment variable.
Source: Own calculations based on KVB claims data.

[5]Figure 2.1 shows a regular pattern. In each year, the third bar shows a jump, reflecting a sharp increase in the number of observations in March. These seasonal variations could be explained by the seasonality of births. During the first half of the 20th century, the number of births in European countries usually showed a major peak in the spring (March to April) and a minor peak in the autumn (in September). In late autumn and early winter (October to December), birth rates were lowest (James 1990).

In the literature, two types of RD designs are considered: the sharp design and the fuzzy design. In the sharp RD design, the treatment status is a deterministic function of the assignment variable, that is, the treatment status jumps from 0 to 1 when the assignment variable exceeds the cut-off. In the fuzzy RD design, the treatment status is a probabilistic function of the assignment variable, leading to a discontinuous change in the treatment probability or treatment intensity at the cut-off (Imbens and Lemieux 2008, Lee and Lemieux 2010). In our context, the fuzzy RD design is appropriate due to two reasons. First, some students were already attending school for more than nine years before the implementation of the reform. This is especially true for intermediate track and academic track students who were not affected by the introduction of the compulsory ninth year in the basic track. As a result, the treatment group consists not only of basic track students but also of intermediate track and academic track students. These individuals represent treatment group members, who actually do not receive the treatment. Second, the above-mentioned data on the ninth grade attendance in the basic track between 1956 and 1973 by the Federal Statistical Office of Germany suggest that the introduction of the compulsory ninth grade was a gradual process in some federal states (see Figure 2.A.1 in the Appendix). Due to organisational reasons (e.g. not enough teachers, small schools and classrooms, not enough work equipment) not all basic track schools were able to provide the compulsory ninth grade directly after it was implemented. Moreover, some basic schools already offered a voluntary ninth grade before it was officially introduced.

A fuzzy RD approach is basically a standard instrumental variables (IV) approach with the discontinuity being an instrumental variable for the treatment status. The coefficient of interest can be obtained by dividing the reduced form coefficient of the regression of the outcome on the instrument by the first stage coefficient of the regression of the explanatory variable on the instrument (Angrist and Pischke 2009, pp. 259-267, Cunningham 2021, pp. 279-282). Hence, we estimate the following two equations:

$$E_c = \alpha + \beta D_c + f(R_c) + \gamma X_c + \epsilon_c \tag{2.1}$$

$$H_c = \delta + \eta D_c + f(R_c) + \theta X_c + \sigma_c \tag{2.2}$$

First, we use equation (2.1) to estimate the first stage effect of the compulsory schooling reforms on educational attainment, measured by years of schooling. This estimation is based on survey data from the German Socio-Economic Panel (SOEP), which is used as complementary data set. In a second step, we use equation (2.2) to obtain reduced form estimates of the effect of the compulsory schooling reforms on doctor-diagnosed health conditions using the claims data in the main analysis[6] and on self-reported health in a complementary analysis using SOEP data. In equation (2.1), the dependent variable E_c represents years of schooling of individuals in month-year of birth cohort c. D_c is an indicator variable, which takes the value 1 if month-year of birth cohort c is affected by the reform and therefore required to stay nine years in school and 0 otherwise. We follow the recommendations for RD designs and center the assignment variable around the cut-off (Lee and Lemieux 2010, p. 318). Thus, R_c measures the birth cohort relative to the relevant cut-off and is positive for cohorts who are affected by the reform and negative for cohorts who are not affected by it. In the basic specification, X_c includes year of birth, month of birth and federal state fixed effects to account for differences across birth years, birth months and federal states. Moreover, we include a quadratic age term to address potential non-linear age effects. ϵ_c denotes the error term. The coefficient of interest is β, which represents the first stage effect of the change in compulsory schooling on years of schooling. In equation (2.2), the dependent variable H_c denotes the health status of individuals in month-year of birth cohort c, measured by doctor diagnoses or self-reports, respectively. D_c and R_c are again reform indicator and birth cohort relative to the cut-off. X_c captures the same covariates that are included in equation (2.1). σ_c represents the error term. The coefficient η represents the reduced form effect of the reforms on health outcomes. Since the claims data are aggregated according to gender, year and month birth and federal state, we run regressions that are weighted by the number of individuals in each cell. Both equations are estimated separately for men and women and with standard errors clustered by federal state $\times$ month-year of birth.

The function $f(\cdot)$ in equations (2.1) and (2.2) captures the relationship between the assignment variable and the outcome and needs to be correctly specified. Since a misspecification of the functional form can generate biased estimates of the treatment effect, it is recommended by Lee and Lemieux (2010) not to rely on one specification. More precisely, it should be demonstrated that the estimates are not sensitive to the chosen bandwidth by presenting estimates for various bandwidths. Furthermore, it is advisable to report estimates for various trend specifications (Lee and Lemieux 2010, pp. 314-318). We follow these recommendations and show results of local linear and local quadratic regressions as well as global regressions with first-order to fourth-order polynomials in month-year of birth cohort

[6]The claims data do not contain education, and therefore we cannot obtain fuzzy RD estimates of the effect of education on doctor-diagnosed health conditions. However, since we find that the compulsory schooling changes have basically zero effects on these outcomes, this is not a major problem because the fuzzy RD estimates would also be zero. Therefore we report reduced form estimates of the effect of the compulsory schooling changes on doctor-diagnoses in Section 2.6 rather than two-sample instrumental variable (TSIV) estimates (Angrist and Krueger 1992).

based on samples of five different bandwidths. Following a novel approach suggested by Simonsohn et al. (2020), the results are presented graphically in specification curves.

2.3.3 Specification Curve Analysis

Specification curve analysis was first suggested by Simonsohn et al. (2020) as a tool to graphically assess the robustness of results to various model specifications in order to solve the problem that empirical results often depend on subjective analytic decisions made by the researcher. When specifying a causal model researchers have various options. For instance, they can decide on the control variables to use, the observations to exclude and the functional form to assume. In economics, it is therefore standard practice to test a few different model specifications in order to demonstrate the robustness of results. Regression results are usually shown in tables with multiple columns capturing different specifications, which can be very complex and confusing when lots of specifications are reported. This standard practice might be problematic because the reported results depend on an arbitrary choice made by the researcher. Probably even more problematic is that researchers often have a conflict of interest and selectively report evidence from specifications that yield the desired results. Undesirable or unexpected results are often not reported, which is referred to as reporting bias. Consequently, it is not surprising that analyses on the same research question and same data often lead to contradictory results. Building on that, the so-called specification curve method offers a possibility to provide a more comprehensive analysis by assessing the robustness of findings for all "reasonable" model specifications, that is, all specifications that the researcher considers as non-redundant and valid. The basic idea of a specification curve analysis is to define a set of reasonable specifications, to estimate all of them and to report the results graphically in a descriptive specification curve that displays the range of estimates that are obtained through alternative specifications (Simonsohn et al. 2020). Hence, specification curve analysis reduces the problem of selective reporting by researchers but does not eliminate it entirely.

In order to answer the question whether education affects health in an RD approach, we have identified three major analytic decisions: first, the functional form of the relationship between the outcome and the assignment variable to assume (non-parametric/local versus parametric/global estimation), second, the bandwidth around the cut-off to use, and third, the covariates to include. In terms of functional form, we find it reasonable to use six different specifications. In particular, we estimate local linear and local quadratic models as well as global regressions including first-order to fourth-order polynomials in month-year of birth. With respect to the bandwidth, we decided to use a bandwidth of 5 years before and after the cut-off in our main specification. As part of the specification curve analysis, we also use narrower bandwidths of ±1 and ±3 and wider bandwidths of ±7 and ±9 years to assess the sensitivity of our results to different bandwidths. Although there is a growing literature on the optimal bandwidth choice for an RD estimator (Imbens and Kalyanaraman 2012), we consider it as sufficient to run models on samples of five different bandwidths instead of figuring out the "optimal" bandwidth, which is itself subject to random influences[7]. The third analytic decision is related to the inclusion of covariates. Our basic model includes year of birth, month of birth and federal state fixed effects and a quadratic age term. However, it is also reasonable to assume that some federal states have flatter or steeper increases in average educational attainment or health than others. Hence, it might be useful to include also state-specific trends in order to control for state-specific deviations from the nationwide trend captured by the cohort fixed effects. Accordingly, we decided to run models controlling for linear, quadratic and cubic state-specific cohort trends in addition to the control variables mentioned above as well as models without controlling for state-specific trends at all. Moreover, we run separate analyses for men and women since health benefits of education may differ by gender. The combination of these analytic decisions results in 240 different specifications to estimate for each outcome (6 functional forms × 5 bandwidths × 4 state-specific trends × 2 sexes).

2.4 Data

To explore the causal effect of education on health, we make use of two different German data sets: administrative ambulatory care claims data and 26 pooled cross-sections of the German Socio-Economic Panel (SOEP). In this section, we introduce the data sets and provide summary statistics for both samples.

The KBV Claims Data

Our main analysis is based on ambulatory claims data that has been provided by the National Association of Statutory Health Insurance Physicians (Kassenärztliche Bundesvereinigung, in short: KBV). On the federal level, the KBV coordinates the activities of 17 Local Associations of Statutory Health Insurance Physicians (Kassenärztliche Vereinigung, in short: KV) in Germany. There are 17 KVs in Germany, corresponding to the federal

[7] However, we also calculated "optimal" bandwidths using the *rdrobust* package in R (Calonico et al. 2015). Depending on the outcome measure, the mean squared error optimal bandwidths suggested by the *rdrobust* package vary between 3.8 and 5.1 years. Hence, the 5-year bandwidth used in the main specification is consistent with the optimal bandwidth suggested by the *rdrobust* package.

states, with the exception of North Rhine-Westphalia, which is divided in two regional associations. One of the most important responsibilities of the KVs is to ensure a high quality of health care by making health services available for all statutorily insured and by improving the services continuously. All office-based physicians and psychotherapists are mandatory members of one of the 17 KVs, depending on their region of work (KBV 2022a). In Germany, physician fees for ambulatory health care services provided to statutorily insured patients are not paid by the patients themselves making advanced payments but directly by the statutory health insurance through the KVs, which are responsible for distributing the fees to the physicians. For this purpose, a physician sends an invoice to the statutory health insurance, which contains, amongst others, information on the patient' year and month of birth and place of residence. Moreover, the invoice includes the diagnoses and the medical serviced performed by the physician (KBV 2022b). Diagnoses are coded according to International Classification of Diseases (ICD), which is a comprehensive medical classification system created by the World Health Organization containing codes for diseases and their signs and symptoms (Harrison et al. 2021). A copy of the invoice is forwarded to the KBV.

The ambulatory claims data set provided by the KBV includes all statutorily insured German inhabitants born between January 1930 and December 1959 with at least one ambulatory care claim recorded in 2009 (N=23.6 million), which is equivalent to about 90 percent of the overall German population in the relevant cohorts in 2009 (Federal Statistical Office 2022). The data contains information on gender, year of birth, month of birth, federal state of residence and health conditions of the statutorily insured. In particular, individual claims contain information on primary and secondary diagnoses in ICD-10 format[8] and are aggregated into grouped data containing information on the number of individuals having a condition on record and the total number of individuals. Based on that, we calculated the proportion of patients who generated a claim to the statutory health insurance related to a specific ICD-10 code, which we refer to as morbidity rates.

Table 2.2 shows the nine health conditions with the corresponding ICD-10 codes that were selected for the analysis. The selection of the diseases was largely guided by the literature. First, we include ischemic heart disease (ICD I20-I25), diabetes mellitus (ICD E10-E15), chronic lower respiratory diseases (ICD J40-J47) and obesity (ICD E66) because the risk of developing chronic diseases has been found to be higher for people with lower socioeconomic status (Dalstra et al. 2005). Moreover, chronic conditions are related to

[8]ICD-10 is the 10th revision of the International Classification of Diseases, which had been released in 1990. The 11th revision officially came into effect on 1 January 2022 and replaced the 10th revision (Harrison et al. 2021).

risky health behaviours such as smoking, drinking alcohol, physical inactivity and poor nutrition (WHO 2021), which are in turn associated with low socioeconomic status (Cutler and Lleras-Muney 2010). Second, we include cancer (ICD C00-C97) as it is known that a higher socioeconomic status is associated with a lower risk for various types of cancer (Faggiano et al. 1997). Third, musculoskeletal diseases (ICD M00-M99) in general and lower back pain (ICD M54) in particular are interesting to study since they are negatively related to the socioeconomic status. The negative link can be explained by occupational characteristics as better-educated people tend to work in non-manual occupations with safer and healthier working conditions, which may reduce the risk of musculoskeletal diseases, including lower back pain (Croft and Rigby 1994). Fourth, we select depression (ICD F30-F39) as the probability to develop a depression is known to be lower for people with a higher socioeconomic status, probably because it increases the access to higher occupations involving factors such as direction, control, and planning, which may protect against depression (Link et al. 1993). Finally, we use urogenital diseases (ICD N00-N99). These conditions have not been studied so far in the education-health literature, but they are interesting to look at because some types of urogenital disorders may be the result of unhygienic conditions and practices that are likely to be more common in lower socioeconomic groups (Dalstra et al. 2005).

Table 2.2: Selection of diagnoses with corresponding ICD-10 codes

ICD-10 code	**Diagnosis**
I20-I25	Ischemic heart disease
E10-E15	Diabetes mellitus
E66	Obesity
F30-F39	Depression
J40-J47	Chronic lower respiratory diseases
C00-C97	Malignant neoplasms
M00-M99	Diseases of the musculoskeletal system
M54	Low back pain
N00-N99	Diseases of the genitourinary system

Notes: The table shows the selection of diseases with the corresponding ICD-10 codes.

We restrict our sample to German inhabitants living in the 10 West German federal states, excluding Berlin. The reason for excluding former East Germany is that, to the best of our knowledge, there was no comparable compulsory schooling reform. Berlin is excluded because it is difficult to identify whether an individual attended school in West or East Berlin. For the analysis, we only consider individuals with valid information on year of birth, month of birth, federal state of residence and doctor diagnoses. For our baseline specification, we select individuals born five years before or after the first affected cohort of each compulsory school reform (N=6,202,659).

The German Socio-Economic Panel

We use the SOEP for complementary analyses. The SOEP is a large representative longitudinal survey of private households in Germany that is conducted annually since 1984 and interviews around 30,000 respondents in nearly 11,000 households every year (Goebel et al. 2019). In order to obtain a sufficiently large sample, we pool the waves 1992 and 1994 to 2018[9].

The SOEP contains information on an individual's highest secondary school degree and thus, we are able to estimate the first stage effect of the West German compulsory schooling reforms on years of schooling. Following Pischke and von Wachter (2008), we combine information on the highest secondary school degree and the school years usually required for this degree to construct our schooling variable. Consequently, 13 years are assigned to academic track graduates, if they have university entrance certificate (Abitur) and 12 years, if they have an advanced technical college certificate (Fachhochschulreife). For intermediate track graduates the standard duration of 10 years of schooling is taken. For individuals with basic track degree and drop-outs with no school leaving certificate information on the federal state of residence, month and year of birth and the timing of the reform is used to determine whether an individual should have graduated after eight or nine years from the basic track. Hence, nine years are assigned to individuals who were affected by the reform and eight years to individuals who were not affected by it.

Furthermore, using the SOEP as complementary data set, we are able to estimate the causal effect of years of schooling on self-reported health status in a fuzzy RD design. Since 1992, the SOEP contains information on the self-reported health status, which measures health by a five-point scale with the possible answer categories being "very good", "good", "satisfactory, "poor" or "bad"[10]. For the analysis, we recode the original five-category variable into a dichotomous variable, which is equal to one if an individual reports to be in poor or bad health (in German: in "weniger guter" oder "schlechter" Gesundheit) and zero otherwise.

[9]We use SOEP, version 35 (doi:10.5684/soep-core.v35), containing the survey years from 1984 to 2018. For the purpose of this study, the waves from 1984 to 1991 and 1993 cannot be used because self-reported health status, which we use as additional health outcome, was not asked in the questionnaires in these years.

[10]The wording of the question in German was: "Wie würden Sie Ihren gegenwärtigen Gesundheitszustand beschreiben?". The answer categories were: "sehr gut", "gut", "zufriedenstellend", "weniger gut", "schlecht".

We impose the same sample restrictions on the SOEP data as on the ambulatory claims data. In particular, we consider only German inhabitants of the birth cohorts 1930 to 1959 who are statutorily insured and live in the 10 West German federal states excluding Berlin. To avoid that some individuals enter the sample more than once and to ensure the comparability between the SOEP and the KBV claims data, we use for each individual only the observation from 2009 or the observation closest to 2009. Moreover, we restrict our sample to individuals with valid information on school degree, year of birth, month of birth, federal state of residence and self-reported health status, and consider cohorts born five years before and after each compulsory schooling change (N = 2,457).

Finally, we use the SOEP to test for the potential effect of measurement error in the instrument due to cross-state mobility in Germany. SOEP started collecting information on the federal state of last school attendance in 2001. Ideally, we would need this information also in the KBV claims data to ensure a precise assignment of the instrument. However, the claims data only contain information on the federal state of residence in 2009. Using the federal state of residence as a proxy for the federal state of last school attendance, we implicitly assume that individuals attended school in the federal state where they lived in the year of the interview and hence, the instrument might be imprecisely assigned if cross-state mobility in Germany exists. This measurement error in the instrument would then downward bias our estimates of the reduced form effect. Using SOEP data, we perform two tests to assess whether measurement error in the instrument is likely to affect our results (see Section 2.5.3). First, we cross-tabulate the instrument based on the federal state of residence and the instrument based on the federal state of last school attendance in order to assess if cross-state mobility in Germany exists. Second, we examine whether our fuzzy RD estimates of the effect of years of schooling on self-reported health change if we use information on the state of last school attendance to construct the instrument. Information on the federal state of last school attendance is known for about 55 percent of our SOEP estimation sample. The analyses are restricted to individuals for whom information on the federal state of residence and the federal state of last school attendance are available (N=1,351).

Descriptive Statistics

Table 2.3 reports descriptive statistics for the KBV sample in Panel A and the SOEP sample in Panel B. We present mean values of all variables and standard deviations of continuous variables in parentheses. The table demonstrates that the KBV sample and the SOEP sample are comparable in terms of age and gender composition. Specifically, both samples consists of little less men then women. On average, individuals in the KBV sample are 58.4 years old, while individuals in the SOEP sample have a mean age of 56.6 years. The difference between the two samples in terms of average age results from the fact that the average person in the SOEP sample was interviewed in 2007 (see Table 2.3, Panel B), while we observe individuals in 2009 in the KBV claims data. Moreover, Panel A of Table 2.3 shows the morbidity rates for the selected diagnoses in ICD-10 format in the KBV sample. The morbidity rate for diseases of the musculoskeletal system is highest (67.6 percent), followed by diseases of the genitourinary system (44.0 percent). The lowest morbidity rates are found for ischemic heart disease (12.5 percent) and cancer (10.1 percent)[11]. The morbidity rates for the selected diagnoses are in the same range for men and women, but there are three exceptions. First, ischemic heart diseases are more than twice as prevalent among men (17.1 percent versus 7.9 percent). This finding is generally in line with the literature, reporting that men develop ischemic heart disease earlier in life than women, although they share many cardiovascular risk factors. One important reason for the lower age-specific risk in women is that estrogen has a positive effect on the cardiovascular system that can protect women from ischemic heart disease before the menopause (Gao et al. 2019). Second, the table shows that depression is about twice as prevalent among women (22.8 percent versus 12.1 percent). This is also consistent with the literature, suggesting that depression is more common in women, which has typically been explained by hormonal changes during puberty, pregnancy, postpartum or during the transition to menopause. Moreover, socially-driven risk factors may contribute to the increased prevalence of depression in women (Albert 2015). Third, urogenital diseases are roughly 60 percent more likely among women (56.4 percent versus 31.5 percent). This is also in line with the literature on risk factors for genitourinary diseases in general and urinary tract infections in particular, reporting that urinary tract infections are more common among women. Typical risk factors explaining the increased prevalence in women are anatomic factors as well as sexual intercourse and the use of spermicidal contraceptives (Harrington and Hooton 2000).

As shown in Panel B of Table 2.3, the average person in the SOEP has 9.7 years of schooling, with women having 0.2 years less than men. The mean probability of self-reported poor or bad health is about the same for men and women: 24.4 percent of men and 24.0 percent of women report to be in poor or bad health. The average probability of having an academic school degree is almost twice as large for men (24 percent versus 15 percent). In contrast, much more women than men have an intermediate school degree (31 percent versus 21 percent). The share of basic school graduates is about the same size for men and women: 54 percent of men and women report to have a basic school degree, on average.

[11] It should be mentioned that the numbers reported in Table 2.3 could be affected by coding errors as physicians may misreport diagnoses to boost profits. We address this limitation of the data in more detail in the discussion in Section 2.6.

Table 2.3: Descriptive statistics

	Full sample (1)	**Men** (2)	**Women** (3)
Panel A: KBV			
Demographics			
Age	58.4 (5.1)	58.4 (5.0)	58.4 (5.1)
Year of birth	1,950.6 (5.1)	1,950.6 (5.0)	1,950.6 (5.1)
Male (%)	45.0		
Health status			
Ischemic heart disease (%)	12.5	17.1	7.9
Diabetes mellitus (%)	18.1	21.5	14.8
Obesity (%)	14.1	12.6	15.6
Depression (%)	17.5	12.1	22.8
COPD (%)	20.5	20.8	20.1
Cancer (%)	10.1	10.4	9.8
Musculoskeletal diseases (%)	67.6	64.6	70.6
Back pain (%)	36.5	33.9	39.0
Urogenital diseases (%)	44.0	31.5	56.4
Observations	6,202,659	2,788,562	3,414,097
Panel B: SOEP			
Demographics			
Age	56.6 (5.7)	56.6 (5.8)	56.5 (5.7)
Year of birth	1,950.9 (5.0)	1,950.7 (5.1)	1,951.0 (5.0)
Male (%)	45.4		
Health status			
Self-reported poor or bad health (%)	24.2	24.4	24.0
Education			
Years of schooling	9.7 (1.6)	9.8 (1.8)	9.6 (1.5)
Basic track (%)	54.3	54.4	54.3
Intermediate track (%)	26.4	21.3	30.7
Academic track (%)	19.3	24.3	15.0
Survey information			
Survey year	2,007.4 (2.6)	2,007.3 (2.6)	2,007.5 (2.6)
Observations	2,457	1,115	1,342

Notes: The table reports mean values of all variables as well as standard deviations of all continuous variables in parentheses for the pooled sample of men and women in column (1), the subsample of men in column (2) and the subsample of women in column (3). In the SOEP, age refers to the survey year in which the respondent reported his or her health status, school type and school degree.
Source: Own calculations based on KBV claims data and SOEP.

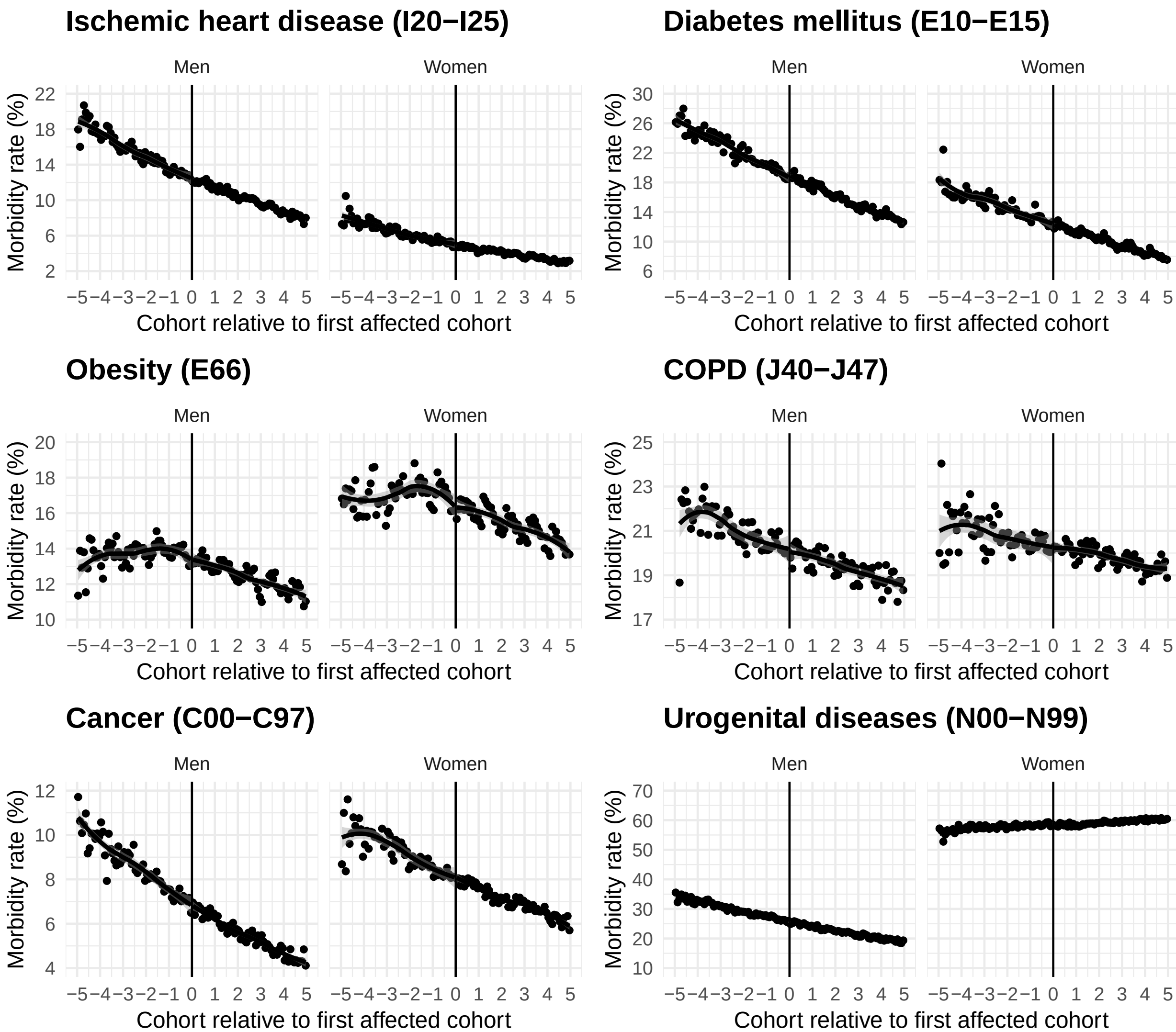
2.5 Results

The results are reported in three sections. First, we present reduced form regression results for the effect of the compulsory schooling reforms on doctor diagnosed conditions, based on the KBV claims data. Second, we show first stage results for the effect of the reforms on years of schooling, reduced form results for the effect of the reforms on self-reported health and fuzzy RD results for the effect of years of schooling on self-reported health, based on SOEP data. Finally, using SOEP data, we test whether measurement error in the assignment of our instrument due to regional mobility in Germany is a potential source of bias for our estimates.

2.5.1 Effect of Compulsory Schooling on Doctor Diagnoses

In this section, we present reduced form results for the effect of the compulsory schooling reforms on doctor diagnoses in ICD-10 format. Figure 2.2 plots morbidity rates in percent by month-year of birth for five cohorts before and after the first birth cohort affected, separately for men and women. It gives a first graphical impression on whether the reforms had an effect on doctor-diagnosed health conditions. The figure shows morbidity rates that decrease in month-year of birth (i.e. increase in age) but do not change discretely at the cut-off. Apart from different prevalences across health conditions, we find no considerable differences between men and women. It is important to mention that depression (ICD F30-F39), musculoskeletal diseases (ICD M00-M99) and back pain (ICD M54) are not part of the figure. The reason is that we found an increase in the probability to be diagnosed with these conditions before 1950 and a sudden decrease after 1950 (see Figure 2.A.2 in the Appendix), which represents individuals at age 59 who gradually start to retire. Therefore, the observed trend possibly reflects a "retirement effect" as musculoskeletal diseases including back pain and depression are more likely to be diagnosed and treated when individuals are still in the labour market because a medical certificate is needed to take sickness leave. Retired individuals do not need this any more. Moreover, back pain and depression are among the most important drivers of early retirement in Germany (Börsch-Supan and Jürges 2012). As the sudden decrease in the percentage treated after age 59 is not random and therefore threatens our identification strategy, we decided to exclude these health conditions from further analyses. This resulted in the six doctor diagnoses shown in Figure 2.2, including ischemic heart disease (I20-I25), diabetes mellitus (E10-E15), obesity (E66), COPD (J40-J47), cancer (C00-C97) and urogenital diseases (N00-N99).

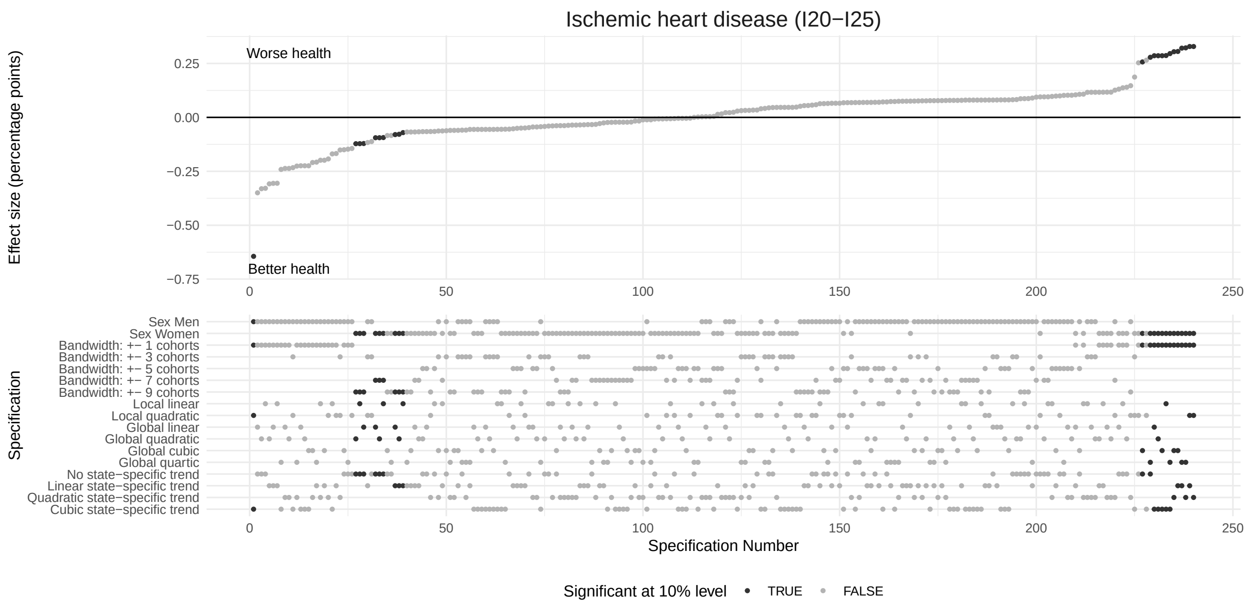
Figure 2.2: Effect of the reforms on doctor diagnoses



Notes: The figure plots morbidity rates of ischemic heart disease (I20-I25), diabetes mellitus (E10-E15), obesity (E66), COPD (J40-J47), cancer (C00-C97) and urogenital diseases (N00-N99) in percent by month-year of birth for five cohorts before and after the first birth cohort affected, separately for men and women. All data points in the figure present averages by month-year of birth. The vertical line denotes the first birth cohort affected by the law changes. The regression line is a smooth curve computed by the loess method. The grey shaded area represents the 95% confidence intervals associated with the regression line.
Source: Own calculations based on KVB claims data.

As described above, we performed specification curve analyses to report the reduced form regression results of the effect of the compulsory schooling reforms on doctor diagnoses in ICD-10 format. The specification curves for ischemic heart disease (ICD I20-I25), diabetes mellitus (ICD E10-E15), obesity (ICD E66), COPD (ICD J40-J47), cancer (ICD C00-C97) and urogenital diseases (ICD N00-N99) are presented in Figure 2.3 to Figure 2.8. In the figures, each dot in the top part represents a point estimate from a different model specification. The 240 different specifications that are estimated are arranged by effect size in ascending order. The dots vertically aligned below indicate the combination of analytic decisions behind those estimates. Black dots represent estimates that are significant at the 10 percent level, while dots in grey show insignificant estimates.

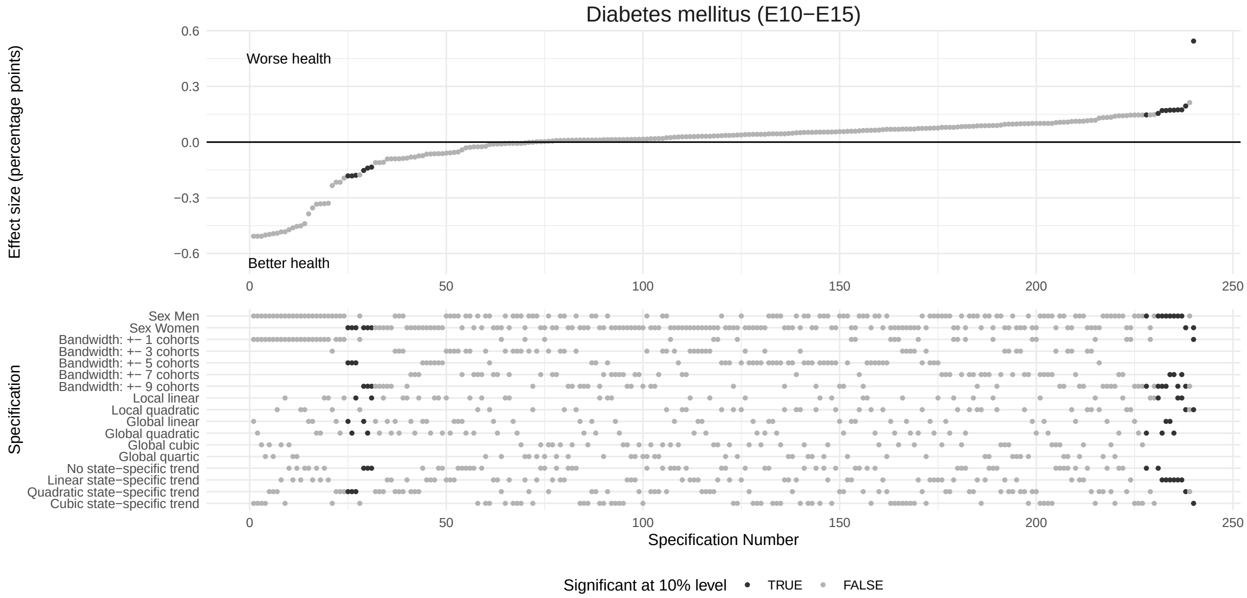
Figure 2.3: Specification curve for ischemic heart disease (I20-I25)



Notes: The figure shows the results of the specification curve analysis of the effect of the reforms on the probability to suffer from ischemic heart disease. Each dot in the top panel represents a point estimate from a different model specification. The specifications are ordered by effect size. The dots vertically aligned below indicate the combination of analytic decisions behind those estimates. Black dots represent significant effects ($p < 0.1$), while grey dots show insignificant effects.
Source: Own calculations based on KBV claims data.

In the following, we analyse the specification curve in terms of size, direction and statistical significance of the effects. We start by assessing the effect sizes. Across all six specification curves, the results are very similar with point estimates that are centred around zero. For example, for cancer (ICD C00-C97) the effect sizes range from -0.1 to 0.2 percentage points, as shown in the specification curve in Figure 2.7. Given the baseline probability of suffering from cancer in the sample of about 10 percent, these estimates reflect basically zero effects. For COPD (ICD J40-J47), effect sizes are somewhat larger, ranging from -0.6 to 0.9 percentage points on a basis of 20 percent, but still, they are very small (see Figure 2.6).

Figure 2.4: Specification curve for diabetes mellitus (E10-E15)

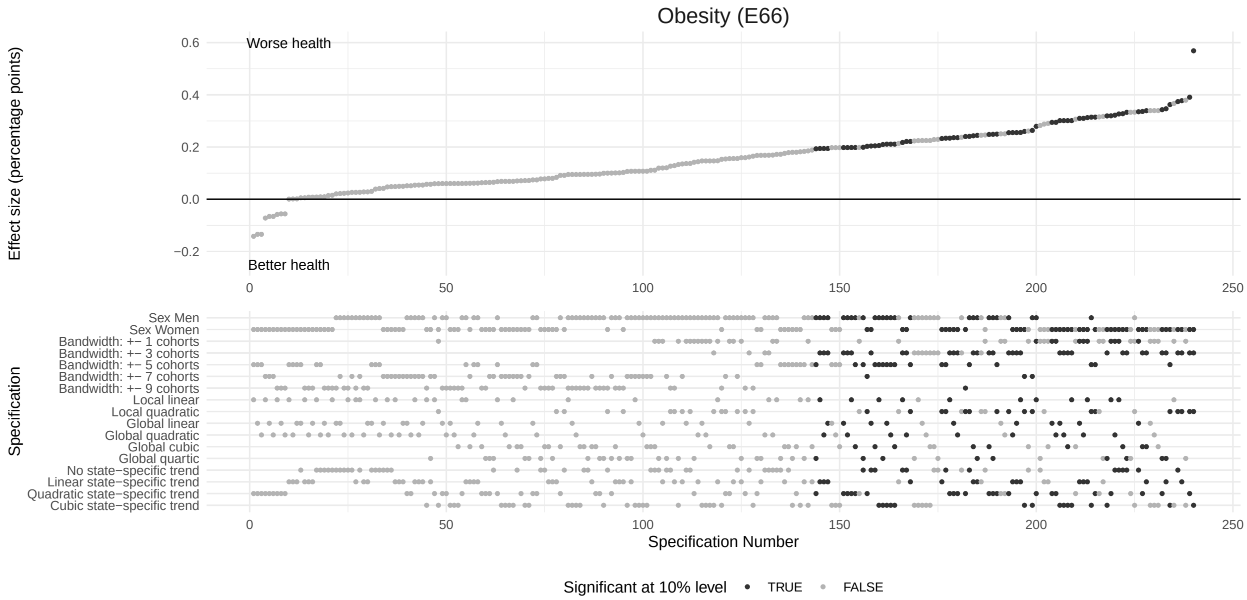


Notes: The figure shows the results of the specification curve analysis of the effect of the reforms on the probability to suffer from diabetes mellitus. Each dot in the top panel represents a point estimate from a different model specification. The specifications are ordered by effect size. The dots vertically aligned below indicate the combination of analytic decisions behind those estimates. Black dots represent significant effects ($p < 0.1$), while grey dots show insignificant effects.
Source: Own calculations based on KBV claims data.

Second, we analyse the sign of the estimated coefficients. In the presence of a morbidity-reducing effect of the compulsory schooling reforms, we would expect the estimates to have a negative sign throughout all specifications. However, we find more positive than negative estimates for each of the considered health conditions, indicating that the compulsory schooling reforms actually *increased* the probability to suffer from these diseases. One extreme case is obesity (ICD E66) with 96 percent positive coefficients that are, however, also close to zero, as shown in the specification curve in Figure 2.5.

Finally, we evaluate the specification curves in terms of statistical significance. Across all six specification curves, the estimated effects are mostly insignificant with only 2 to 10 percent of coefficients that are significant at the 10 percent level. Moreover, we find some heterogeneity by gender. In particular, we find that the significant effects for the probability to suffer from ischemic heart disease (ICD I20-I25) are almost exclusively found in the female subsample, as shown in the specification curve in Figure 2.3. Inconsistently, some of the significant effects among women are negative and some positive. For the probability to be diagnosed with cancer (ICD C00-C97), we only find significant positive coefficients, pointing to an *increase* of the probability to suffer from cancer due to the reforms. These effects are

Figure 2.5: Specification curve for obesity (E66)

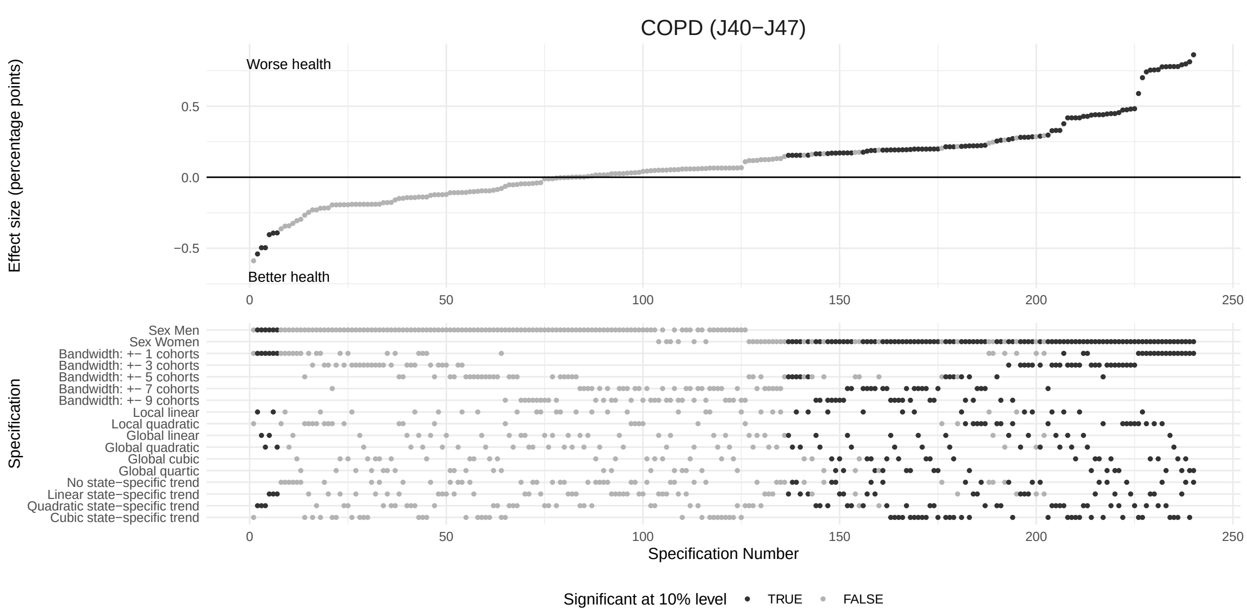


Notes: The figure shows the results of the specification curve analysis of the effect of the reforms on the probability to suffer from obesity. Each dot in the top panel represents a point estimate from a different model specification. The specifications are ordered by effect size. The dots vertically aligned below indicate the combination of analytic decisions behind those estimates. Black dots represent significant effects ($p < 0.1$), while grey dots show insignificant effects.
Source: Own calculations based on KBV claims data.

solely observed in the male subsample (see Figure 2.7). Moreover, we find some support for a significant *increase* in the probability to suffer from obesity (ICD E66) for both sexes (see Figure 2.5). For diabetes mellitus (ICD E10-E15), we find significant negative effects among women and significant positive effects among men, as shown in Figure 2.4. In terms of statistical significance, one exception is COPD (ICD J40-J47) with significant coefficients in 40 percent of the considered specifications (see Figure 2.6). However, some of the significant effects are negative and some are positive. In particular, we find some evidence that the compulsory schooling reforms significantly decrease the probability to suffer from COPD for men and significantly *increase* it for women. Previous research for Germany on educational differences in smoking might explain the heterogeneity by gender in our results for COPD. Jürges and Meyer (2020) found that prevalences in smoking, which is an important risk factor for COPD, generally increased in the first half of the 20th century, with a greater increase among women than among men, and then generally decreased for both sexes. In addition to the gender gap, Jürges and Meyer (2020) also found an educational gap in smoking, which in turn differs by gender and by cohorts. In particular, they found that there were hardly any educational differences in smoking between men with high and low levels of education until the 1950s cohorts. After 1950, the prevalences diverged, with low educated men showing higher smoking rates than highly educated men. In contrast, women with high levels of education were more likely to smoke than women with low levels of education in the first half of the 20th century, because smoking was often considered as an indicator of a sophisticated lifestyle. However, following the spread of information about the adverse health effects of smoking in the media, the educational differences among women have reversed in the second half of the 20th century.

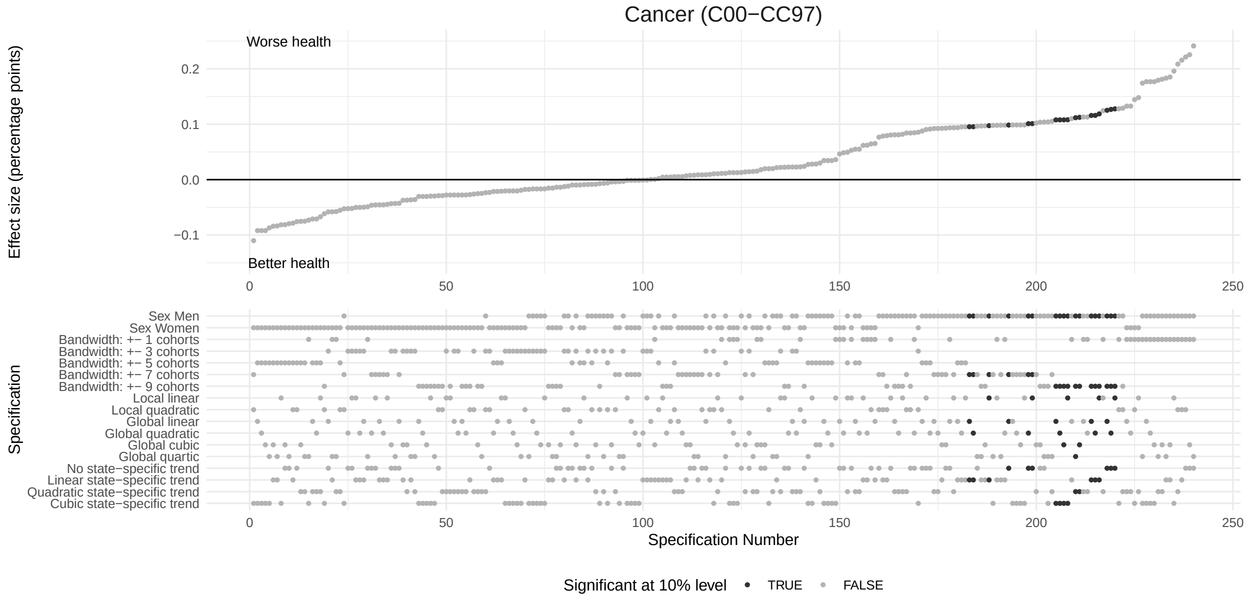
Overall, the specification curves suggest that there is only very little empirical support for a causal effect of the compulsory schooling reforms in West Germany on doctor-diagnosed conditions. The estimates point to very small effects that are close to zero, often of the "wrong" sign and mostly insignificant. The results from the specification curve analyses thus confirm the graphical impression in Figure 2.2. Hence, the evidence for an effect of the compulsory schooling reforms on health as measured by ICD-coded doctor diagnosed conditions is very weak.

Figure 2.6: Specification curve for COPD (J40-J47)



Notes: The figure shows the results of the specification curve analysis of the effect of the reforms on the probability to suffer from COPD. Each dot in the top panel represents a point estimate from a different model specification. The specifications are ordered by effect size. The dots vertically aligned below indicate the combination of analytic decisions behind those estimates. Black dots represent significant effects ($p < 0.1$), while grey dots show insignificant effects.
Source: Own calculations based on KBV claims data.

Figure 2.7: Specification curve for cancer (C00-C97)

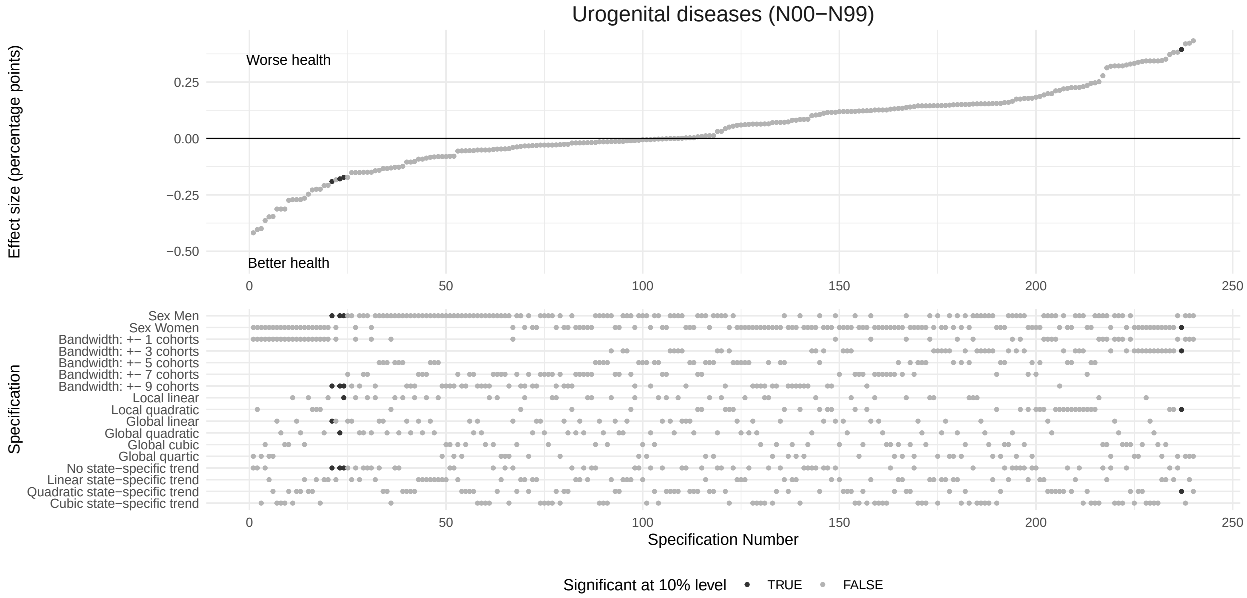


Notes: The figure shows the results of the specification curve analysis of the effect of the reforms on the probability to suffer from cancer. Each dot in the top panel represents a point estimate from a different model specification. The specifications are ordered by effect size. The dots vertically aligned below indicate the combination of analytic decisions behind those estimates. Black dots represent significant effects ($p < 0.1$), while grey dots show insignificant effects.
Source: Own calculations based on KBV claims data.

There are several possible explanations for the absence of an effect of the reforms on health in Germany. First, it might be that the reforms had an effect on schooling duration but not necessarily on schooling quality. In the context of Germany, questions that arise are what actually happened in schools, and what students actually learned in the additional ninth school year. If the teaching content of one school year was spread over two school years, then it is not surprising to find no health benefits. According to Leschinsky and Roeder (1980), the main objective of introducing the ninth grade in Germany was to expand students' career and labour market orientation (Leschinsky and Roeder 1980, p. 334). However, it could rather be that the extra school year improved basic skills, e.g. in numeracy, orthography and language, but not specific skills and knowledge that are required to increase the efficiency in health production or the allocative efficiency of health inputs. Moreover, skills that are important for the labour market might differ from those that are needed for acquiring or processing health information or for producing health more efficiently.

Second, an increase in earnings is an important channel through which education might affect health. For Germany, Pischke and von Wachter (2008) found that longer schooling does not translate into higher wages, which was confirmed by Kamhöfer and Schmitz (2016) with

Figure 2.8: Specification curve for urogenital diseases (N00-N99)



Notes: The figure shows the results of the specification curve analysis of the effect of the reforms on the probability to suffer from urogenital diseases. Each dot in the top panel represents a point estimate from a different model specification. The specifications are ordered by effect size. The dots vertically aligned below indicate the combination of analytic decisions behind those estimates. Black dots represent significant effects ($p < 0.1$), while grey dots show insignificant effects.
Source: Own calculations based on KBV claims data.

different data. Pischke and von Wachter (2008) explain the finding of zero wage returns to compulsory schooling in Germany by the fact that the additional school year did not improve skills that are relevant for the labour market because they were already learned earlier in school. In turn, zero returns to education in Germany would rule out one important channel through which education could affect health. However, in a recently published study, Cygan-Rehm (2022) casts doubts on whether the findings of zero wage returns to education in Germany hold[12].

Third, the lack of a causal effect of education on health outcomes might also stem from the fact that returns to education most likely depend on students' motivation in school. Since the compulsory schooling reforms in West Germany forced some individuals to stay one year

[12]Cygan-Rehm (2022) finds that the main result of zero wage returns to education in Germany in Pischke and von Wachter (2008) is sensitive to changes in sample restrictions and model specifications (e.g. excluding certain cohorts, excluding the lowest and highest wage earners, adding squared state-specific trends). Moreover, the author notes inconsistencies regarding the timing of the compulsory schooling reforms in West Germany compared to Pischke and von Wachter (2008), which we also do (see Table 2.A.1 in the Appendix). However, she concludes that the revision of the introduction dates does not pose a major problem for the analysis as it only affects relatively small states (Cygan-Rehm 2018).

longer in school, motivation might be limited. Accordingly, students who voluntarily attend an additional year of schooling are more likely to benefit from it.

2.5.2 Effect of Education on Self-Reported Health

In this section, we use the SOEP to examine the causal effect of years of schooling on self-reported health. Although the use of self-reported health as health indicator in applied economics is often criticized as it is subjective and potentially biased, self-reported health seems to be a strong predictor of mortality and thus more objective measures of health (Idler and Benyamini 1997, DeSalvo et al. 2006, Jürges 2008, Tamayo-Fonseca et al. 2013, Wuorela et al. 2020). The regression results for the effect of years of schooling on self-reported poor or bad health are presented in Table 2.4. Panel A shows results for the pooled sample of men and women, while Panels B and C present results separately by gender. In each Panel, we show first stage regressions results in the first row, reduced form regression results in the second row and results from fuzzy RD regressions in the third row. Although we could have estimated 240 different specifications (as above), we only show results for two of these specifications here. In particular, we report results from local linear and global linear regressions that include linear state-specific cohort trends in addition to year of birth, month of birth and federal state fixed effects and squared age (and gender in Panel A), and use a bandwidth of five cohorts around the cut-off.

First, we examine the first stage effect of the compulsory schooling reforms on years of schooling. In Panel A, the first stage estimates for the pooled sample of men and women show that the compulsory schooling reforms raised average years of schooling by about 0.56 years. The first stage F-statistic for the excluded instrument is about 18, indicating that a weak instrument problem is not a concern (Staiger and Stock 1997). Hence, the compulsory schooling reforms in West Germany have high explanatory power with respect to years of schooling and provide a strong instrument. However, it should be mentioned that the F-statistic is rather small compared to other studies (e.g. Pischke and von Wachter 2008, Kemptner et al. 2011), which can be explained by the small sample size resulting from a large number of missing values in the month of birth variable. We also perform analyses separately for males in Panel B and females in Panel C and find that the first stage estimates are somewhat larger for women than for men[13].

[13] As noted by Pischke and von Wachter (2005) and Kemptner et al. (2011), the first stage is rather mechanical. Since we assign nine years of schooling to basic track students whenever they are affected by the reform and required to stay nine years in school, the strong link between the reform indicator and the schooling variable arises largely by construction and the first stage coefficients should reflect the fraction of students in the basic school track. In our sample, 54 percent of men and women have a basic school degree

Table 2.4: Effect of years of schooling on self-reported poor or bad health

	Local linear (1)	**Global linear** (2)
Panel A: Full sample		
First stage parameter	0.561***	0.566***
	(0.133)	(0.133)
Reduced form parameter	−0.054	−0.053
	(0.039)	(0.039)
Fuzzy RD parameter	−0.095	−0.094
	(0.069)	(0.068)
F-statistic	17.729	18.171
Observations	2,457	2,457
Panel B: Men		
First stage parameter	0.451*	0.460**
	(0.234)	(0.233)
Reduced form parameter	−0.038	−0.038
	(0.055)	(0.055)
Fuzzy RD parameter	−0.084	−0.082
	(0.124)	(0.121)
F-statistic	3.712	3.886
Observations	1,115	1,115
Panel C: Women		
First stage parameter	0.564***	0.564***
	(0.162)	(0.162)
Reduced form parameter	−0.072	−0.072
	(0.054)	(0.053)
Fuzzy RD parameter	−0.129	−0.127
	(0.096)	(0.095)
F-statistic	12.137	12.195
Observations	1,342	1,342

Notes: The table shows results from fuzzy RD regressions of self-reported poor or bad health on years of schooling, based on local linear models in column (1) and global linear models in column (2), as well as the corresponding first stage and reduced form results. Panel A is based on the pooled sample of men and women, while Panel B and Panel C are based on subsamples of men and women, respectively. All regressions use a bandwidth of five cohorts before and after the pivotal cohorts and include fixed effects for year of birth, month of birth and federal state of residence as well as linear state-specific cohort trends and a quadratic age term. Panel A includes gender as additional control variable. Standard errors are clustered on cohort × state level and presented in parentheses. *p<0.1; **p<0.05; ***p<0.01
Source: Own calculations based on SOEP.

(see Table 2.3). Hence, the share of female basic track students is very close to the first stage estimate of about 0.56, although they do not match exactly.

In the second row of Panel A, we estimate the reduced form effect of the compulsory schooling reforms on self-reported poor or bad health. The reduced form estimates have a negative sign in both specifications but are not statistically different from zero. The same applies to the fuzzy RD results for the effect of years of schooling on self-reported poor or bad health in the third row of Panel A. In both specifications, the estimates are not statistically significant at conventional levels. Moreover, there are no substantial differences between men and women, as shown in Panels B and C. Therefore, the results provide insufficient evidence for an effect of years of schooling on the probability of reporting to be in poor or bad health, which is in line with our findings for doctor diagnoses.

2.5.3 Testing for Measurement Error in the Instrument

In the KBV claims data, only the federal state of residence in 2009 is available, although we would need information on the federal state of last school attendance to ensure a precise assignment of the instrument. By using the state of residence as a proxy, we implicitly assume that individuals attended school in the federal state in which they lived in 2009, which raises concerns about imprecise assignment of the instrument if cross-state mobility in Germany exists. This measurement error in the instrument would then downward bias our reduced form effect. However, using SOEP data, we are able to test whether measurement error in the instrument is a potential source of bias for our estimates. In particular, we use information on the federal state of last school attendance to construct the instrument, which is collected since 2001 and thus available at least for a subsample of our SOEP sample, and perform two tests. First, we cross-tabulate the instrument based on the federal state of residence and the instrument based on the federal state of last school attendance in order to assess if cross-state mobility in Germany exists. Second, we test whether measurement error in the instrument affects our fuzzy RD estimates of the effect of years of schooling on self-reported poor or bad health. We restrict the analysis to the subsample of individuals for which both the federal state of residence and the federal state of school attendance is available. This reduces the sample size to N=1,351.

The cross-table in Table 2.5 demonstrates that 95.5 percent of the people that are coded as treated when the federal state of residence instrument is used are also coded as treated when the instrument is based on the federal state of last school attendance. Moreover, 92.5 percent of the non-treated in case of the state of residence instrument are also not treated when the instrument is based on the state of last school attendance. Therefore, using the state of residence at the time of interview instead of the state of last school attendance leads to a misclassification of treatment status of 4.8 percent of individuals, who are coded as treated although they are actually untreated, and 7.5 percent of individuals who are coded as untreated although they are actually treated. These small differences between the two instruments suggest that cross-state mobility in Germany is quite low. Furthermore, Table 2.5 shows that the proportions of misclassified individuals are statistically different from each other (p-value = 0.039). Hence, we cannot assume symmetric (unsystematic) mobility between federal states. Instead, we can assume asymmetric (systematic) migration, although we cannot say anything about whether people move systematically with respect to the introduction of the reforms or health.

Table 2.5: Cross-table of instruments (N=1,351)

	Federal state of current residence	
Federal state of last school attendance	Not treated	Treated
Not treated	92.5%	4.8%*
Treated	7.5%*	95.5%

Notes: The cross-table shows the assignment of the instrument based on the federal state of last school attendance compared the assignment of the instrument based on the federal state of residence. The table demonstrates that cross-state mobility in Germany is quite low, leading only to small differences in the assignment of the instrument.
* Means are statistically different from each other at the 5 percent level.
Source: Own calculations based on SOEP.

Second, we test whether measurement error in the instrument affects our fuzzy RD estimates of the effect of years of schooling on self-reported poor or bad health[14]. Based on the subsample of 1,351 individuals for whom we observe both their federal state of residence and federal state of last school attendance, we re-estimate the first stage, reduced form and fuzzy RD regressions using the instrument based on the federal state of residence, and compare them to the results using the federal state of last school attendance instrument (see Table 2.A.2 in the Appendix). We find that the first stage, reduced form and fuzzy RD point estimates are almost identical and thus, the effect of measurement error in the instrument is likely to be small. In particular, we find that the reforms led to an average increase by about 0.52-0.54 years in school, depending on the specification, when we use information on the federal state of residence for assignment of the instrument (Panel A, first row compared to Panel B, first row). Panel B shows that the assignment of the instrument based on the last school attendance state generates a slightly smaller first stage estimate

[14]In case of classical (random) measurement error, the fuzzy RD estimates will be unbiased as the first stage and reduced form coefficients are proportionally attenuated. However, if measurement error is non-classical (systematic), such that it is correlated with the compulsory schooling reforms, the fuzzy RD estimates will be biased and the bias can be in both directions (Pischke and von Wachter 2008).

in the local linear specification and a slightly larger first stage estimate in the global linear specification. Moreover, we find a reduced form coefficient that is somewhat larger in the local linear model and somewhat smaller in the global linear model when we use the state of last school attendance instead of the state of current residence (Panel B, second row compared to Panel A, second row). This leads to a very small downward bias in the local linear fuzzy RD estimate that is based on information on the federal state of residence, while the global linear estimate is virtually unchanged (Panel A, third row compared to Panel B, third row).

Taken together, we are quite confident that it is not a major problem to proxy the federal state of last school attendance with the federal state of residence, which is in line with conclusions in previous studies (e.g. Pischke and von Wachter 2008, Siedler 2010, Piopiunik 2014).

2.6 Conclusion

The aim of this study was to add to the quickly growing literature on the causal effect of education on health that has produced mixed results by carrying out the largest and most comprehensive analysis for Germany to date. Using ambulatory claims data on more than 23 million statutorily insured that contain a wide range of ICD-10-coded doctor diagnoses, we exploit compulsory schooling reforms implemented in West Germany in the middle of the 20th century. Since it depends on month-year of birth whether an individual was affected by the changes in compulsory schooling, the reduced form effect of the reforms on doctor diagnoses is identified within an RD approach. Using specification curve analysis as a tool to graphically assess the robustness of results to various model specifications, we find that the compulsory schooling reforms have, at best, very small impacts on doctor diagnoses. In most of the specifications, we estimate effects that are almost zero. Moreover, in a complementary study with SOEP data, we find insufficient evidence for an effect of education on self-reported health. Using the SOEP data, we also find that using the federal state of residence as a proxy for the federal state of school attendance, which raises concerns about imprecise assignment of our instrument, is unlikely to bias our estimates. Our results of zero effects of compulsory education on health in Germany are in line with the findings in Clark and Royer (2013), which is the most closely related study to ours in terms of sample size, outcome measures and methodology. Clark and Royer (2013) estimate very small and mostly insignificant effects of two nationwide reforms in the UK on various health measures, including BMI, blood pressure and self-reported health. Moreover, our results are consistent with a recent meta-analysis on the previous literature on the causal effect of education on health by Xue et al. (2021), suggesting that the effect of education on health is close to zero after correcting for a slight positive publication bias in the previous literature. Taken together, this supports the idea that the results do not depend on the setting and its specific features, but that general education has little effect on health.

Our study has some limitations. First, as already discussed above, we construct the instrument by using the federal state of current residence to proxy the school attendance state. We show, however, that using this proxy is unlikely to be problematic for the analyses (see Section 2.5.3). Second, the probability of observing individuals in our sample in 2009 would be affected if the changes in compulsory schooling have an effect on mortality. Hence, selective mortality may change the composition of the sample and underestimate the effect of the compulsory schooling laws. To the best of our knowledge, there is no study so far that investigates effects of the German compulsory schooling changes on mortality. However, since we have concluded earlier that health and mortality effects are not context-specific and small, we are confident that effects on mortality from other countries also hold true for Germany and that our estimates are not subject to mortality driven sample selection bias. Third, the claims data only include individuals with a least one insurance claim in 2009 and thus, we cannot tell anything about health effects for individuals who did not go to the doctor in 2009. Fourth, diagnoses may be misreported by physicians due to financial incentives. In this context, it has been found that there is a large discrepancy between documented ICD-10 codes and health problems for which the patient has actually been treated, which casts doubt on the quality of coding (Erler 2007, IGES 2012). Moreover, it has been shown that there are regional differences in coding quality in Germany (Ozegowski 2013). However, this is only problematic for our analyses if coding errors vary systematically at the reform cut-off. This would imply that diagnoses are misreported to a greater extent among the cohorts that were born before the cut-off and thus affected by the compulsory schooling changes, which seems highly unlikely. Furthermore, we capture regional differences by including state fixed effects and state-specific cohort trends in the regression models. Finally, the timing of the compulsory schooling reforms in the 1940s to 1960s means that affected individuals were 50 to 70 years old in 2009. This means that we do not learn much about the effect of education on health for younger people.

Despite these limitations, our results have important policy implications. Given the large size of our data set and the wide range of health measures, we think that our study provides important insights into the causal relationship between education and health in the middle to lower parts of the educational distribution. Since we find insignificant zero effects of compulsory schooling on health, our study questions the presence of the sometimes quite large positive effects of education on health that are found in the previous literature and suggests a careful reconsideration of economic models that assume a causal link between education and health. In particular, our results cast doubt on the effectiveness of policies, which aim to improve population health through educational interventions, as suggested by the WHO and OECD. Put differently, educational interventions may not be effective in improving both health and education simultaneously and therefore, policy makers need to implement separate education and health policies, which may lead to trade-offs if one needs to be improved at the expense of the other.

Although we do not find an effect of the compulsory schooling reforms on health, it does not mean that education generally has no effect on health. The examined compulsory schooling reforms were implemented a long time ago and therefore, we cannot tell anything about health effects of more recent education reforms such as the so-called G8 reform in Germany that exposed students to increased learning intensity (Hofmann and Mühlenweg 2018). Future research should proceed examining the causal effect of education on health, health behaviours and mortality. It is important to learn about the health effects of interventions at higher levels of schooling exploiting different sources of exogenous variation as instruments and using samples that are large enough to obtain precise estimates. Since there is only little evidence on the effects of education on more objective measures of health, the particular focus should be on these type of health measures.

Appendix

Table 2.A.1: Reform introduction dates in this study compared to Pischke and von Wachter (2008) and Cygan-Rehm (2018, 2022)

	This study	**Pischke and von Wachter (2008)**	**Cygan-Rehm (2018, 2022)**
Hamburg	April 1946	1949	1946
Schleswig-Holstein	April 1947	1956	1947
Saarland	April 1958	1964	1958
Bremen	April 1959	1958	1959
Lower Saxony	April 1962	1962	1962
Hesse	April 1966	1967	1967
North Rhine-Westphalia	April 1966	1967	1967
Rhineland-Palatinate	April 1967	1967	1967
Baden-Wuerttemberg	April 1967	1967	1967
Bavaria	August 1969	1969	1969

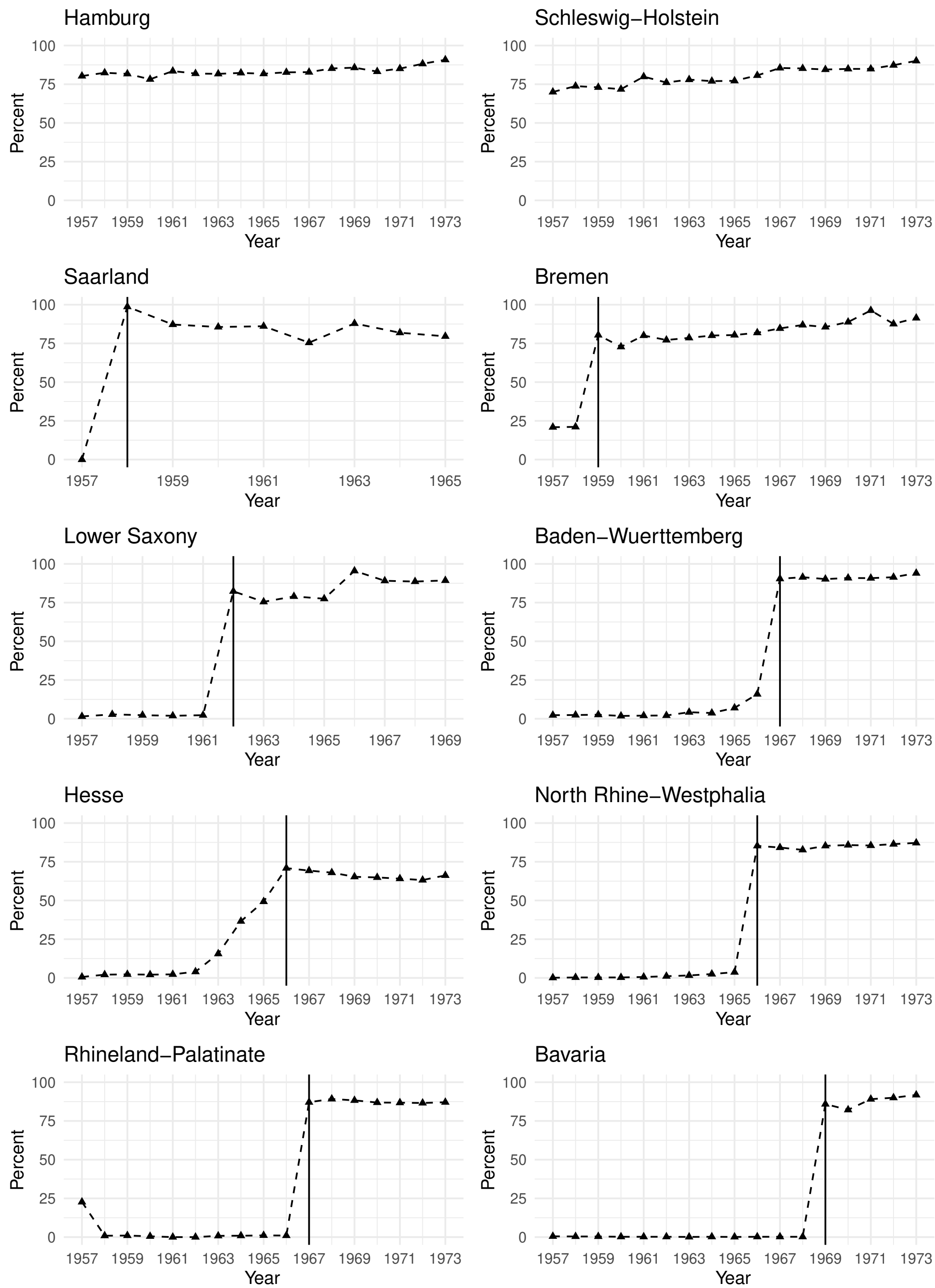
Notes: The table shows the reform implementation dates in this study compared to the dates reported in studies by Pischke and von Wachter (2008) and Cygan-Rehm (2018, 2022) exploiting the same compulsory schooling reforms in West Germany.
Source: Pischke and von Wachter (2008), Cygan-Rehm (2018, 2022).

Table 2.A.2: Effect of years of schooling on self-reported poor or bad health (different instruments)

	Local linear (1)	**Global linear** (2)
Panel A: Federal state of residence		
First stage parameter	0.542***	0.524***
	(0.170)	(0.168)
Reduced form parameter	−0.040	−0.040
	(0.044)	(0.044)
Fuzzy RD parameter	−0.075	−0.077
	(0.079)	(0.081)
Observations	1,351	1,351
F-statistic	10.181	9.702
Panel B: Federal state of last school attendance		
First stage parameter	0.524***	0.546***
	(0.177)	(0.177)
Reduced form parameter	−0.037	−0.042
	(0.047)	(0.047)
Fuzzy RD parameter	−0.071	−0.077
	(0.089)	(0.086)
Observations	1,351	1,351
F-statistic	8.780	9.555

Notes: The table shows results from fuzzy RD regressions of self-reported poor or bad health on years of schooling, based on local linear models in column (1) and global linear models in column (2), as well as the corresponding reduced form and first stage results. In Panel A the instrument is based on the federal state of residence, while Panel B uses the federal state of last school attendance to construct the instrument. All regressions are based on the pooled sample of men and women, use a bandwidth of five cohorts around the cut-off point and include fixed effects for year of birth, month of birth and federal state of residence as well as linear state-specific cohort trends, gender and a quadratic age term. Standard errors are clustered on the cohort × state level and presented in parentheses. *p<0.1; **p<0.05; ***p<0.01.
Source: Own calculations based on SOEP.

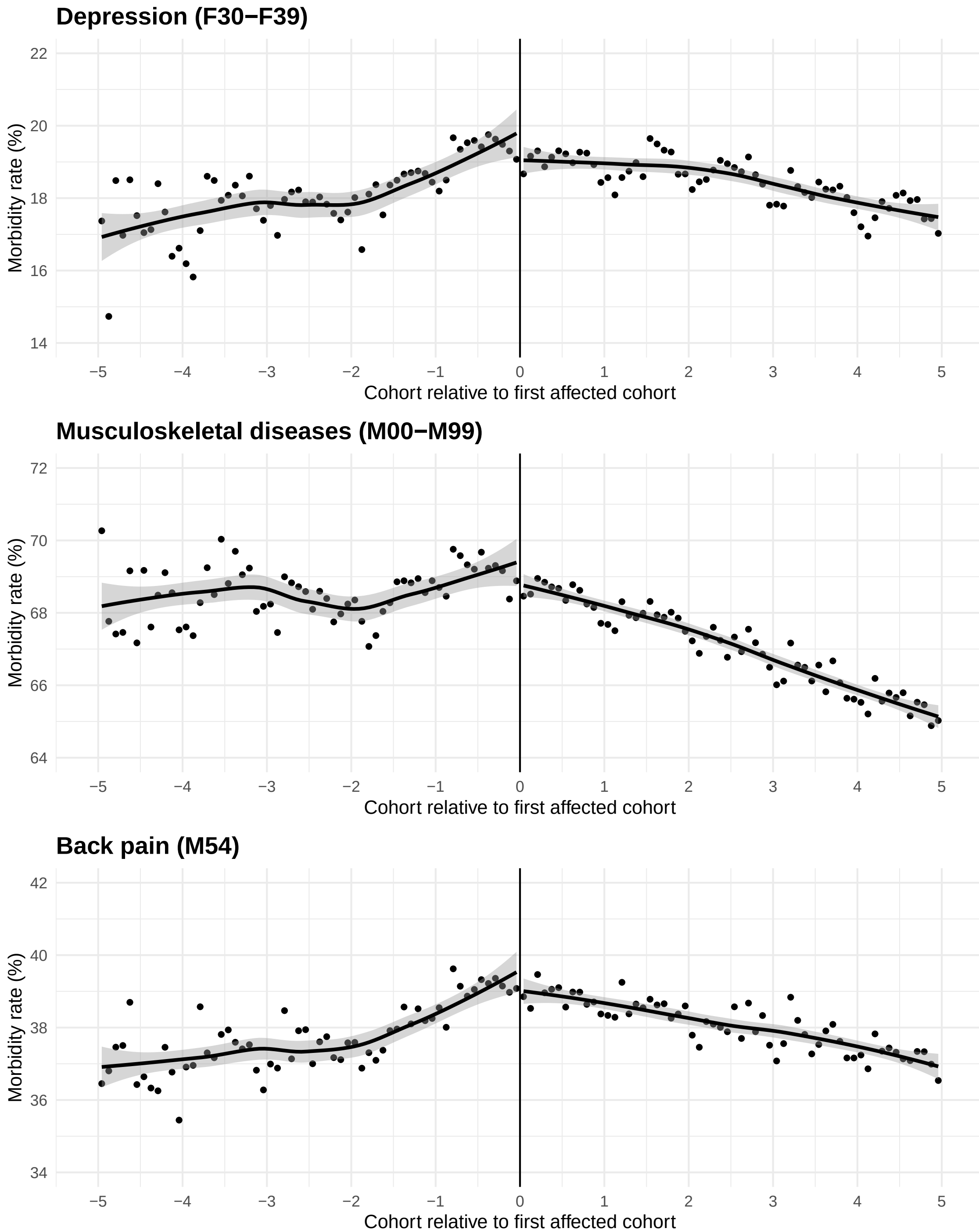
Figure 2.A.1: Basic track ninth grade students



Notes: The figure plots the number of basic track students in the ninth grade in the school year t as a fraction of the number of basic track students attending the eighth grade in the previous school year t-1 for each West German federal state. The vertical line denotes the year of implementation of the reforms. Calculations are based on absolute numbers of basic track students in the eighth and ninth grade from 1956 to 1973.

Source: Own calculations based on Federal Statistical Office (1964) and respective volumes of the following years.

Figure 2.A.2: Effect of the reforms on depression, musculoskeletal diseases and back pain



Notes: The figure plots morbidity rates of depression (F30-F39), musculoskeletal diseases (M00-M99) and back pain (M54) in percent by month-year of birth for five cohorts before and after the first birth cohort affected for the pooled sample of men and women. All data points in the figure present averages by month-year of birth. The vertical line denotes the first birth cohort affected by the law changes. The regression line is a smooth curve computed by the loess method. The grey shaded area represents the 95% confidence intervals associated with the regression line.
Source: Own calculations based on KVB claims data.

Table 2.A.3: Empirical evidence on the causal effect of education on health outcomes

Author (Year)	Country	Outcome	Results
A. Mortality			
Lleras-Muney (2005)	United States	10-year mortality rate	**–0.061**** **(0.025)**, mean: 0.11
Mazumder (2008)	United States	10-year mortality rate	−0.012 (0.016), mean: 0.21
Albouy and Lequien (2009)	France	Survival at 50 Survival at 80	0.13 (0.11) –0.24 (0.32)
Van Kippersluis et al. (2011)	Netherlands	Probability of dying before age 89	**−0.026***** **(0.004)**
Lager and Torssander (2012)	Sweden	Mortality 1986-2007 (hazard ratio)	0.98 (0.015)
Fischer et al. (2013)	Sweden	20-year mortality rate 30-year mortality rate 40-year mortality rate	**−0.014***** **(0.006)**, mean: 0.017 **−0.025***** **(0.008)**, mean: 0.026 **−0.023***** **(0.008)**, mean: 0.048
Clark and Royer (2013)	United Kingdom	Mortality (log odds of death)	RF (1947 law): 0.005 (0.005) RF (1972 law) : −0.004 (0.026)
Fletcher (2015)	United States	10-year mortality rate	−0.069 (0.078), mean: 0.18
Gathmann et al. (2015)	19 European countries	20-year mortality rate (odds ratio)	**men: 0.983**** **(0.007)** women: 0.991 (0.011)
Meghir et al. (2018)	Sweden	Mortality hazard ratio	−0.001 (0.002)
Davies et al. (2018)	United Kingdom	Mortality rate 2006–2014	**−0.005***** **(0.001)**, mean: 0.9
Grytten et al. (2020)	Norway	Probability of dying between age 16 and 64	**men: −0.017**** **(0.007)** women: −0.008 (0.007)
Malamud et al. (2021)	Romania	Mortality Rate	−0.014 (0.009), mean: 0.26
B. Self-reported health and health behaviours			
Arendt (2005)	Denmark	Health status	men: −0.430 (0.820), mean: 1.62 women: −0.223 (0.414), mean: 1.62
		Never smoked	men: −0.640 (0.307), mean: 0.30 women: – 0.639 (0.305), mean: 0.40
		BMI in healthy range	men: 0.117 (0.275), mean: 0.55 women: 0.084 (0.336), mean: 0.72

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...Table 2.A.3 continued

Mazumder (2008)	United States	Fair or poor health Health limitation	**–0.082**** **(0.034)**, mean: 0.357 **–0.074**** **(0.035)**, mean: 0.423
Oreopoulos (2006, 2008)	United Kingdom	Good health	–0.010 (0.011), mean: 0.564
Silles (2009)	United Kingdom	Good health No long-term illness No activity-limiting illness No work-preventing illness	**0.045***** **(0.009)**, mean: 0.66 **0.055***** **(0.009)**, mean: 0.69 **0.046***** **(0.008)**, mean: 0.82 **0.009*** **(0.005)**, mean: 0.92
Kemptner et al. (2011)	Germany	Long-term illness	**men: –0.041**** **(0.017)**, mean: 0.20 women: 0.010 (0.017), mean: 0.15
		Work disability	**men: –0.032**** **(0.015)**, mean: 0.17 women: 0.021 (0.016), mean: 0.13
		BMI	**men: –0.301**** **(0.121)**, mean: 26.70 women: –0.194 (0.133), mean: 25.25
		Overweight	**men: –0.030**** **(0.015)**, mean: 0.66 **women: –0.031**** **(0.015)**, mean: 0.46
		Obesity	**men: –0.030**** **(0.014)**, mean: 0.16 women: –0.004 (0.010), mean: 0.13
		Currently smoking	men: –0.005 (0.010), mean: 0.36 women: –0.001 (0.009), mean: 0.24
Clark and Royer (2013)	United Kingdom	Fair or bad health	1947 law: 0.003 (0.012), mean: 0.25 1972 law: 0.005 (0.049), mean: 0.12
		Currently smoke	1947 law: –0.022 (0.018), mean: 0.23 1972 law: –0.001 (0.038), mean: 0.30
		Currently drink	1947 law: 0.015 (0.013), mean: 0.81 1972 law: 0.010 (0.016), mean: 0.92
Brunello et al. (2013)	9 European countries	BMI	men: 0.0003 (0.178), mean: 26.49 **women: –0.414**** **(0.149)**, mean: 25.02
Jürges et al. (2013)	United Kingdom	Poor health	1947 law (men): 0.05 (0.09), mean: 0.37 1973 law (men): –0.08 (0.010), mean: 0.18 1947 law (women): –0.12 (0.08), mean: 0.35 1973 law (women): 0.11 (0.08), mean: 0.19
Mazzonna (2014)	6 European countries	Good health Depression Memory score (standardized)	**0.038*** **(0.021)**, mean: 0.63 **–0.036**** **(0.015)**, mean: 0.24 **0.118***** **(0.044)**
Crespo et al. (2014)	7 European countries	Depression Memory (standardized)	**–0.065**** **(0.027)**, mean: 0.22 **0.118**** **(0.057)**

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...Table 2.A.3 continued

Fletcher (2015)	United States	Poor health	**−0.197**** **(0.098)**, mean: 0.13
		Heart attack	**−0.088*** **(0.048)**, mean: 0.14
Silles (2015)	United Kingdom	Currently smoking	**men (NI): −0.090**** **(0.035)**, mean: 0.34 women (NI): −0.041 (0.034), mean: 0.32 men (GB): 0.012 (0.023), mean: 0.33 women (GB): −0.023 (0.017), mean: 0.31
Li and Powdthavee (2015)	Australia	Excellent health	**0.017***** **(0.005)**
		Overweight	−0.004 (0.013)
		Obesity	**0.025*** **(0.014)**, mean: 0.279
		Currently smoking	−0.006 (0.009), mean: 0.204
Brunello et al. (2016)	13 European countries	Poor health	**men: −0.051**** **(0.022)**, mean: 0.27 **women: −0.057**** **(0.025)**, mean: 0.32
Dursun et al. (2018)	Turkey	Excellent health	men: 0.209 (0.209). mean: 0.21 **women: 0.129*** **(0.073)**, mean: 0.15
		Normal weight	men: 0.129 (0.179), mean: 0.58 **women: 0.464**** **(0.182)**, mean: 0.62
		Obesity	**men: 0.296***** **(0.107)**, mean: 0.07 women: −0.028 (0.084), mean: 0.08
		Currently smoking	men: −0.058 (0.175), mean: 0.53 women: 0.092 (0.136), mean: 0.20
Baltagi et al. (2019)	Turkey	Good health	0.09 (0.22), mean: 0.82
		Obesity	0.13 (0.16), mean: 0.08
		Smoking cessation	0.70 (0.71), mean: 0.15
Dahmann and Schnitzlein (2019)	Germany	MCS score (standardized)	−0.192 (0.133)
Fonseca et al. (2020)	United States, United Kingdom, 12 continental European countries	Poor health	**−0.069***** **(0.004)**, mean: 0.34
		1 + Chronic illness	**−0.044***** **(0.007)**, mean: 0.73
		1+ ADLs	**−0.039***** **(0.007)**, mean: 0.13
		1+ IADLs	**−0.046***** **(0.008)**, mean: 0.10
		Diabetes	**−0.027***** **(0.009)**, mean: 0.14
		Heart disease	**−0.034***** **(0.007)**, mean: 0.18
		Hypertension	**−0.046***** **(0.008)**, mean: 0.44
		Arthritis	**−0.070***** **(0.004)**, mean: 0.36
		Lung disease	**−0.014**** **(0.006)**, mean: 0.07

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Avendano et al. (2020)	United Kingdom	MCS score	1.831 (1.799), mean: 48.62
Janke et al. (2020)	United Kingdom	Diabetes	**−0.036*** (0.011)**, mean: 0.04
Albarrán et al. (2020)	11 European countries	Good health	0.028 (0.030), mean: 0.55
		No chronic illness	0.038 (0.030), mean: 0.67
		Not limited in daily activities	0.012 (0.026), mean: 0.77
Malamud et al. (2021)	Romania	Health index	−0.002 (0.002), mean: 0.08
Dilmaghani (2021)	Canada	Currently smoking	men: −0.132 (0.282), mean: 0.34
			women: 0.046 (0.149), mean: 0.33
		Excellent health	men: −0.719 (0.282), mean: 0.28
			women: −0.029 (0.207), mean: 0.31
C. “More objective” health measures			
Powdthavee (2010)	United Kingdom	Hypertension	**1947 law (men): 0.070***, mean: 0.66
			1973 law (men): 0.075, mean: 0.28
			1947 law (women): −0.084*, mean: 0.66
			1973 law (women): 0.014, mean: 0.14
Clark and Royer (2013)	United Kingdom	Diastolic blood pressure	−0.389 (0.423), mean: 76.01
		Hypertension	−0.025 (0.018), mean: 0.37
		BMI	0.312 (0.162), mean: 26.83
		Obesity (BMI > 30)	0.028 (0.016), mean: 0.21
Jürges et al. (2013)	United Kingdom	log(fibrinogen)	1947 law (men): 0.03 (0.06), mean: 1.00
			1973 law (men): 0.01 (0.05), mean: 0.86
			1947 law (women): 0.04 (0.04), mean: 1.04
			1973 law (women): −0.01 (0.05), mean: 0.93
		log(CRP)	1947 law (men): −0.12 (0.23), mean: 0.63
			1973 law (men): −0.18 (0.25), mean: 0.12
			1947 law (women): −0.37 (0.24), mean: 0.66
			1973 law (women): −0.09 (0.49), mean: 0.14
Courtin et al. (2019)	France	BMI	RF: 0.024 [−0.029, 0.077]
		Waist circumference	RF: 0.009 [−0.048, 0.067]
		Waist-hip ratio	RF: −0.014 [−0.073, 0.044]
		Systolic blood pressure	RF: 0.070 [−0.016, 0.124])
		Diastolic blood pressure	RF: 0.072[−0.018, 0.126]
		Blood glucose	RF: 0.047 [−0.004, 0.098]

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...Table 2.A.3 continued

		Total cholesterol	RF: 0.031 [−0.018, 0.081]
		Cholesterol HDL	RF: 0.010 [−0.047, 0.068]
		Triglycerides	RF: **0.052*** **[0.006, 0.098]**
		Gamma GT	RF: −0.006 [−0.067, 0.054]
		Transaminase	RF: 0.005 [−0.042, 0.052]
		Creatinine	RF: 0.011 [−0.046, 0.069]
		White blood cells	RF: 0.042 [−0.014, 0.100]
		Haemoglobin	RF: −0.036 [−0.077, 0.005]
		Hematocrit	RF: 0.015 [−0.037, 0.069]
		Platelet values	RF: 0.035 [−0.020, 0.091]
Ye et al. (2022)	China	Cardiovascular system	RF: −0.053 [−0.12, 0.017], mean: 0.09
		Metabolic system	RF: −0.004 [−0.068, 0.060], mean: 0.23
		Inflammation system	RF: −0.005 [−0.065, 0.055], mean: 0.09
		Urinary system	RF: 0.026 [−0.055, 0.11], mean: 0.08
		Allostatic load	RF: 0.065 [−0.70, 0.83], mean: 2.31

Notes: The table summarizes studies that exploit compulsory schooling reforms as source of exogenous variation in education to examine the causal effect of education on mortality (Panel A), self-reported health and health behaviours (Panel B) and “more objective” measures of health (Panel C). The last column reports IV/RD estimates, standard errors in parentheses (or 95% confidence intervals in brackets) and sample means (if available). In the case of studies that analyse a large number of outcomes, we report only the most important findings. Estimates in bold are statistically significant. *p<0.1; **p<0.05; ***p<0.01

Chapter 3

Adult Children's Education and Parental Long-Term Care Dependency

3.1 Introduction

This chapter examines the causal effect of adult children's education on their parents' health and long-term care dependency. Although the literature provides consistent evidence for a positive correlation between children's education and parental health and longevity (Zimmer et al. 2002, Zimmer et al. 2007, Torssander 2013, Friedman and Mare 2014, Sabater and Graham 2016, Yang et al. 2016, Yahirun et al. 2017, De Neve and Harling 2017, Lee 2017, Lee et al. 2017, Lee 2018, Elo et al. 2018, Smith-Greenaway et al. 2018, Peng et al. 2019, Thoma et al. 2021, Sabater et al. 2020, Yahirun et al. 2020, Torres et al. 2021, Yahirun et al. 2022), is still unclear to what extent this relationship is causal. The question whether it is causal is, however, particularly relevant in light of population ageing that has rapidly progressed in recent decades and poses major challenges to European societies today. To put it in numbers, the share of the population aged 65 years and over increased from 15% in 1999 to 20% in 2019, and is expected to grow to almost 30% in 2050 (OECD 2021). Since older people are likely to have more health problems, the demand for long-term care is expected to increase during the next decades. Therefore, it is of great political relevance to identify the factors that shape health in old age.

One important determinant of health is education (Grossman 2006). Starting in the early 2000s, a large body of research has investigated causal effects of education on own health and mortality (see Xue et al. 2021 for a recent meta-analysis). However, the rapidly growing literature in this field has produced mixed results and thus, the debate to what extent the link between education and health is causal is still ongoing, as a handful of recently published studies has shown (Janke et al. 2020, Avendano et al. 2020, Fonseca et al. 2020, Dilmaghani 2021, Malamud et al. 2021). Lately, intergenerational health benefits of education have gained considerable attention in the applied economics literature. This literature is mostly based on the assumption that the intergenerational link runs in the "downward" direction from parents to their offspring (Currie and Moretti 2003, Lindeboom et al. 2009, Chou et al. 2010, McCrary and Royer 2011, Kemptner and Marcus 2013, Carneiro et al. 2013, Lundborg et al. 2014, Silles 2015, Grépin and Bharadwaj 2015, Güneş 2015, Huebener 2018, Ali and Elsayed 2018, Graeber and Schnitzlein 2019, Huebener 2020). Although it is plausible to assume that the link runs in the other direction as well, "upward" intergenerational effects from children to parents are understudied, especially in terms of causality (De Neve and Kawachi 2017). Despite the consensus that adult children's education is positively associated with parents' health and survival, the interpretation of these correlations as causal is difficult, for instance because unobserved genetic or environmental factors drive both children's education and parents' health (omitted variable problem), or because parents' poor health influences their children's educational achievement in childhood (reverse causality problem). Only recently, some studies exploited exogenous variation in education induced by compulsory schooling laws to examine causal effects of children's education on parental health and longevity. These studies find protective effects of children's education on maternal and paternal survival in Tanzania (De Neve and Fink 2018), on paternal survival in China (Cui et al. 2021), on parental cognitive function and lung function in China (Ma 2019), on parental cognitive abilities in Mexico (Ma et al. 2021) and on parental smoking cessation in China (Xie et al. 2021) but no overall effect on parental mortality in Sweden (Lundborg and Majlesi 2018).

This study aims at contributing to the existing literature on intergenerational health returns to education in two important ways. First, it provides evidence on the "upward" intergenerational causal effect of children's education on parental health outcomes for countries that have not yet been analysed in the literature. Except for Lundborg and Majlesi (2018) analysing Sweden, the focus so far has been on low- and middle-income countries. Most evidence comes from China, where the level of social security coverage is quite low and children's resources and support play an important role for older parents (Cui et al. 2021). In contrast, financial transfers typically flow from parents to children in high-income countries (Albertini et al. 2007). Therefore, the link between children's education and parental health might differ in a more developed context such as Europe. Second, the focus of this study is on long-term care dependency as a very important health outcome with far-reaching consequences for families and the society as a whole. Due to population ageing, European societies today face an increasing demand for long-term care with much of the care being provided informally by family members, while it has been shown that the provision of informal care has negative effects on caregivers' employment, health and social life (Bauer and Sousa-Poza 2015). As a result, the demand for formal care services and thus long-term care expenditure is expected to increase in the future.

To overcome the above-described problem of endogeneity in children's education in order to establish causality, I employ a fuzzy regression discontinuity (RD) approach and exploit compulsory schooling reforms implemented between 1967 and 1999 in the following seven European countries: Belgium, France, Greece, Italy, the Netherlands, Portugal and Spain. Using data from five waves of the Survey of Health, Ageing and Retirement in Europe (SHARE), parental long-term care dependency is measured by a summary score of overall disability, which was constructed by conducting a principal component analysis on variables related to parents' physical and cognitive health. Descriptive ordinary least squares (OLS) results suggest that children's education is negatively related to parents' long-term dependency, as measured by disability. In particular, one additional year of children's schooling is associated with a reduction in parents' disability score by 0.03 standard deviations - an effect size that is descriptively comparable to the decrease in the disability score resulting from about 9 months of life. However, when the potential endogeneity of children's education is taken into account, the estimates for the effect of one additional year of children's schooling on parental disability become positive and statistically insignificant, and effects smaller than -0.02 and larger than 0.03 standard deviations can be excluded at the 90 percent confidence level. Several sensitivity analyses confirm these results. Moreover, I do not find significant effects by gender of the parent or child, by number of children, or by groups of countries according to the European north-south health gradient that has been observed in the previous literature (Ahrenfeldt et al. 2019). The study concludes that the absence of an "upward" intergenerational effect of education on health in Europe might have three reasons. First, the availability and generosity of health care programs in Europe, compared to low- and middle-income countries, might weaken the importance of children and their resources for parental health. Second, intergenerational co-residence that is much less common in Europe compared to developing countries might explain the absence of an effect in Europe. Third, there might be no causal effect of education on health, neither within generations, which seems to be a growing consensus in the recent literature (Clark and Royer 2013, Albarrán et al. 2020, Avendano et al. 2020, Dilmaghani 2021, Malamud et al. 2021, Xue et al. 2021), nor across generations.

The remainder of the chapter is organized as follows. Section 3.2 reviews the existing literature and outlines potential mechanisms through which adult children's education might affect parental health and long-term care dependency. Section 3.3 describes the data used for the analyses, while Section 3.4 outlines the empirical approach, including the institutional background and the identification strategy. Section 3.5 presents results, including results from the principal component analysis in Section 3.5.1 and regression results for the effect of children's education on parental long-term care dependency in Section 3.5.2. Section 3.5.3 and Section 3.5.4 report results from various robustness checks and heterogeneity analyses, respectively. Section 3.6 summarizes findings and concludes.

3.2 Related Literature

3.2.1 Theoretical Mechanisms

In the theoretical literature, several mechanisms have been suggested to explain the link between children's education and parents' health as most important driver of the need for long-term care in old age.

First, compared to their lower-educated counterparts, better-educated children are likely to have more financial resources (Brunello et al. 2009, Devereux and Hart 2010, Grenet 2013), which allows them to financially support their parents' health in old age. For instance, they might be able to buy more or better health care services for their parents or to help them to get access to healthier living environments with less traffic and pollution. In a recent study for China, Ma (2019) has identified children's financial support as most important pathway of shaping parental health. This pathway is, however, likely to be more relevant in less-developed countries, in which financial transfers from younger to older generations are more common than in Europe (Attias-Donfut et al. 2005, Albertini et al. 2007). Moreover, to the extent that education affects earnings, children are less dependent on financial transfers from their parents and consequently, parents could invest more in their own health and well-being rather than investing in their children (Lundborg and Majlesi 2018, p. 208).

Second, better-educated children are likely to have a better health knowledge and to apply their knowledge to maintain a healthy lifestyle. In turn, better-educated children's healthy behaviours could also positively affect their parents' lifestyles, thereby improving their parents' health. In this context, some evidence suggests that health education in primary schools in the United States positively affects the engagement of children's parents in physical activity (Berniell et al. 2013), and that parents of better-educated children in China are more likely to quit smoking (Xie et al. 2021, Liu 2021). This pathway can work both directly and indirectly. On the one hand, better-educated children could directly share their health knowledge and provide health-related guidance and advice to encourage parents to adopt healthy behaviours. On the other hand, they might indirectly influence their parents to engage in healthier behaviours by simply exposing them to their own healthier lifestyles (Friedman and Mare 2014, pp. 1274-1275). Apart from that, better-educated children might help their parents to navigate the complex health care system, to communicate with health care professionals, to follow prescribed treatments and medications, and to stay informed about the latest health technology. They might also act as better agents for their parents in the health care system and might be better at managing the health needs of their parents in general. Furthermore, better-educated children are likely to be more familiar with the internet, which is an important tool for obtaining health information. In turn, they might increase internet adoption also among their parents (Korda and Itani 2013, Belo et al. 2016). Moreover, more education might open access to health professions such as working as a physician or a nurse. Since children who work in the healthcare sector are likely to have better knowledge about health care or even preferential access to specific treatments, they might be able to provide better care to their parents (Elo et al. 2018, p. 13). However, as better-educated children are more likely to be employed and to work more hours, they might have less time for their parents, which might reduce the amount of informational and instrumental support they provide. Hence, the frequency of contact between children and parents plays an important role. Recently, Xie et al. (2021) reported that parents of better-educated children are more likely to quit smoking, while the effect was found to be stronger for children and parents who live close to each other and meet frequently.

Third, children's education might affect their parents' health through a stress-related pathway. For instance, better-educated children are likely to live in more stable circumstances and to be more successful in general. In turn, this might reduce parents' worry about their children's well-being, which could have a positive effect on parental health (Hay et al. 2008, Torssander 2013, p. 640). In contrast, having children that experience problems in the transition to adulthood, such as leaving school early, might be a stressful life event for parents that could be related to poor mental and physical health (Barr et al. 2018). Moreover, having successful children might increase parents' subjectively perceived social standing in society, thereby improving health (Torssander 2013, p. 640).

3.2.2 Empirical Evidence

Correlational research on the intergenerational relationship between adult children's education and parental health outcomes dates back to the beginning of the 21st century. Starting with Zimmer et al. (2002), an emerging body of research has found consistent evidence that children's education is positively correlated with parents' physical health (Yahirun et al. 2017, Lee 2017, Lee 2018, Peng et al. 2019, Thoma et al. 2021), mental health (Sabater and Graham 2016, Lee et al. 2017, Peng et al. 2019, Yahirun et al. 2020, Torres et al. 2021, Yahirun et al. 2022) and survival (Zimmer et al. 2007, Torssander 2013, Friedman and Mare 2014, Yang et al. 2016, De Neve and Harling 2017, Elo et al. 2018, Smith-Greenaway et al. 2018, Sabater et al. 2020).

Although this strong positive association persists in developed and developing countries, it is still unclear how much of this association is due to a causal relationship running from offspring education to parental health. Establishing causality is difficult due to two major reasons. First, the observed correlation between children's education and parental health may result from reverse causality, because growing up with ill parents might influence children's educational attainment. For instance, children of unhealthy parents might receive less help with homework or might have to fulfil more household chores. Healthier parents might also have more financial resources to invest in their children's education. Second, the relationship between children's education and parental health is likely to be confounded by unobserved factors that drive both higher education of children and better health of parents such as parental cognitive ability or time preferences (Torssander 2013, p. 655).

Only recently, some quasi-experimental studies have tried to establish causality in the relationship between offspring education and parental health by exploiting exogenous variation in children's education induced by compulsory schooling reforms. Using a Swedish compulsory schooling reform in the 1950s and 1960s, which increased compulsory education from seven to nine years, Lundborg and Majlesi (2018) find no causal effect of children's education on their parents' longevity overall, but some treatment heterogeneity by the gender of the child and the parent. More specifically, increasing daughters' education was found to decrease the mortality risk of fathers from low socio-economic background. Besides Lundborg and Majlesi (2018), there are two working papers examining "downward" intergenerational causal effects of education on health in Europe. Using SHARE data, Everding (2019) exploits changes in compulsory schooling in 11 European countries to estimate the impact on parental mental health and finds that additional education decreases parents' probability of developing depression. The author finds that these improvements in parental mental health are driven by more educated sons and by fathers. Potente et al. (2020) exploit an educational reform in England and Wales, that increased the minimum school leaving age from 15 to 16 years, and finds only very limited causal effects of children's education on a wide range of outcomes related to parental health and longevity using census-linked data from the Office for National Statistics Longitudinal Study (ONS-LS).

However, most evidence to date comes from low-income settings. For instance, De Neve and Fink (2018) study the effect of children's schooling on parental mortality exploiting the 1974 Tanzania Universal Primary Education policy reform. Using Tanzanian census data, the authors find that one additional year of primary schooling reduces the probability of maternal death by 3.7 percentage points and the probability of paternal death by 0.8 percentage points, while the effects are found to be larger for male than for female offspring. In another study, Ma (2019) exploits a compulsory schooling reform in China that was implemented at different times across provinces in the 1980s and made nine years of education mandatory. Using data from the China Health and Retirement Longitudinal Study (CHARLS), they find that increasing children's education has protective effects on parents' cognitive function, lung function and longevity, while it has no effects on parental grip strength, self-reported health, and depression. Cui et al. (2021) exploit the same Chinese compulsory schooling reform in order to estimate the causal link between children's education and parental mortality using data from the China Family Panel Studies. The authors find a positive effect on paternal survival but not on maternal survival. Moreover, Ma et al. (2021) find that children's schooling improves parents' verbal learning, verbal fluency and orientation when exploiting the 1993 Constitutional Amendment reform in Mexico that increased compulsory schooling from six to nine years. The beneficial effects on those cognitive domains are found to be more pronounced for mothers than for fathers. In another recently published study, Xie et al. (2021) exploit the Chinese compulsory schooling reform in the 1980s with CHARLS data to analyse the effect of offspring's education on parents' smoking cessation. The authors find that having better-educated children increases parents' probability to quit smoking, while the effects are found to be more pronounced among parents living close to their children or having frequent contact with their children.

Three recently published studies by Ludwig et al. (2021), Liu (2021) and Wei et al. (2022) use identification strategies other than changes in compulsory schooling to estimate the effect of children's education on parental health. For instance, Ludwig et al. (2021) exploit a secondary schooling reform that was implemented in 1996 in Botswana and shifted grade 10 from senior secondary school to junior secondary school in order to increase access. Using two waves of Botswana's decennial census, their results suggest that children's education has no effect on parental survival and disability. Moreover, using CHARLS data and geographic proximity to school as instrumental variable, Liu (2021) find that having better-educated children has a positive effect on parents' self-reported health. Wei et al. (2022) exploit a educational reform in China that increased enrolment in higher education and find that parents of better-educated children have a better physical health status.

Overall, the review of the existing literature highlights that there are only few quasi-experimental studies that attempt to establish causality in the relationship between children's education and parental heath and longevity. As outlined in this section, there seems to be some heterogeneity in the results of these studies by the gender of the child and the parent. Moreover, the existing studies mainly focus on low- and middle-income countries with most evidence coming from China.

3.3 Data

This study uses data from waves 1, 2, 4, 5 and 6 of the Survey of Health, Ageing and Retirement in Europe (SHARE)[1]. SHARE is a longitudinal, multidisciplinary and cross-national micro-database containing information on health, socio-economic status and social relations of about 140,000 individuals aged 50 years or over and their partners irrespective of age from 28 European countries and Israel (see Börsch-Supan et al. 2013 for methodological

[1]Survey of Health, Ageing and Retirement in Europe (SHARE), wave 1 (2004/05, doi: 10.6103/SHARE.w1.710), wave 2 (2006/07, doi: 10.6103/SHARE.w2.710), wave 4 (2010/11, doi: 10.6103/SHARE.w4.710), wave 5 (2013, doi: 10.6103/SHARE.w5.710), wave 6 (2015, doi: 10.6103/SHARE.w6.710).

details on SHARE)[2]. SHARE is well suited for examining the relationship between children's education and older parents' care dependency as it allows me to obtain a relatively large sample with information on two generations. In particular, I focus on SHARE respondents as parents of children affected by compulsory reforms in Europe.

Measuring Parents' Long-Term Care Dependency

The main outcome is parents' long-term care dependency. It is measured by a disability score, which is constructed by conducting a principal component analysis (PCA) on a wide range of variables related to physical and cognitive health, both of which are well-recognised to decline with age (Andersen-Ranberg et al. 2005, Andersen-Ranberg et al. 2009) and thus to determine the need for long-term care in old age. In order to generate the disability score, I use information on the respondents' self-reported limitations in activities of daily living (ADL), limitations in instrumental activities of daily living (IADL) and mobility, arm and fine motor function limitations as well as grip strength, time orientation, verbal fluency, immediate word recall and delayed word recall.

ADL limitations describe problems with six basic everyday tasks required for self-care (Katz et al. 1963), including difficulties in (1) dressing, (2) walking across a room, (3) bathing or showering, (4) eating, (5) getting in and out of bed and (6) using the toilet (SHARE 2007). IADL limitations comprise seven more complex problems with competences that are necessary for living independently in a community and that require a higher level of cognitive function (Lawton and Brody 1969), including (1) using a map to get around in a unknown place, (2) preparing a hot meal, (3) shopping for groceries, (4) making telephone calls, (5) taking medications, (6) doing work around the house or garden and (7) managing money, such as paying bills and keeping track of expenses (SHARE 2007)[3]. Moreover, SHARE contains information on mobility, arm and fine motor function derived from a list of 10 items including difficulties with (1) walking 100 meters, (2) sitting for about two hours, (3) getting up from a chair after sitting for long periods, (4) climbing several flights of stairs without resting, (5) climbing one flight of stairs without resting, (6) stooping, kneeling, or crouching, (7) reaching or extending arms above shoulder level, (8) pulling or pushing large objects, (9)

[2]Wave 3, wave 7 and wave 8 are not used for the analyses. Wave 3 focuses on respondents' life histories and contains questions that are different from the other waves (SHARELIFE). Wave 7 consists of SHARELIFE modules for all respondents who did not participate in wave 3, as well as regular panel modules for all respondents who already participated in the SHARELIFE interview in wave 3. This complicated structure of the questionnaire leads to a high amount of missing values in some relevant variables, which is why I do not use wave 7. Moreover, I do not include wave 8 because the data collection was interrupted by the outbreak of the COVID-19 pandemic and might thus not be comparable to previous waves.

[3]In wave 6, the measure of IADL was extended by two additional activities (doing personal laundry, and leaving the house independently and accessing transportation services). To ensure comparability between waves, I do not use these two measures.

lifting or carrying weights over 5 kilos, and (10) picking up a small coin from a table. For the analyses, I convert the ADL limitations, IADL limitations and mobility limitations into binary variables, that is, I construct 23 binary variables that equal one if respondents have a certain limitation and zero otherwise.

Moreover, SHARE includes grip strength as an important predictor of disability, morbidity, frailty and mortality and thus indicator for overall health. It is measured by the maximum score out of two measurements on each hand recorded with a handheld dynamometer (SHARE 2007). In the analyses, grip strength is treated as continuous variable. Furthermore, respondents' cognitive abilities are measured based on tests of time orientation, verbal fluency as well as immediate and delayed word recall. Time orientation is assessed by four questions about the day of the month, month, year and day of the week. The final measure is the sum of correct answers that respondents give, ranging from zero to four. The verbal fluency test consists of counting the sum of animals that the respondent is able to name in one minute, whereas the memory tests refers to the respondent's performance of recalling as many words as possible out of 10 words immediately after the presentation of the complete list of words (immediate word recall) and with a few minutes of delay (delayed word recall). For the purpose of this study, I treat verbal fluency, immediate memory and delayed memory as continuous variables. The time orientation score is converted into four binary variables that equal one if a respondent is able to remember the day of the month, the month, the year or the day of the week correctly, and zero otherwise.

The results of the PCA based on those 31 variables related to physical and cognitive health used to construct the overall disability score are presented in Section 3.5.1. In the PCA, I retain the first principal component of the variables as disability score. The final score is standardized to a mean of zero and standard deviation of one. High values on the score indicate a high level of disability.

Measuring Children's Gender, Year of Birth and Education

Across all waves, SHARE respondents are asked to provide information on their children living inside and outside the household. Respondents could mention up to 20 children. In waves 1 and 2, SHARE collects only basic demographic information on gender, year of birth and geographic distance to the parental home for each child. Further information on children's marital status, employment status, educational attainment and number of children is only asked for up to four children. Since wave 4, detailed information is collected for all living children. For the purpose of the study, the data set is transposed so that the respondents' children are the unit of analysis. Hence, after reshaping, each row in the data represents a child-parent dyad. Figure 3.A.1 in the Appendix illustrates the structure of the data before and after reshaping. In order to follow children across waves, children have been matched on gender and year of birth.

Children's education is measured with the number of years spent in secondary education. However, SHARE respondents provide information on their children's educational degrees only. Therefore, I derived a child's years of schooling by combining information on the child's secondary school degree, year of birth and the pivotal cohort of a compulsory schooling reform[4]. Country-specific conversion tables provided by SHARE (SHARE 2011) and the International Standard Classification of Education 1997 (ISCED 1997)[5] are used to recode school-leaving degrees into years of schooling.

Control Variables

In the empirical analyses, I include several covariates that might confound the effect of children's education on parental health outcomes. In addition to child birth cohort fixed effects, country fixed effects and country-specific trends in child's birth cohort, I include the following pre-treatment parent characteristics: gender, age at birth of child, season of birth (winter, spring, summer, fall), years of schooling, height and number of children. Parents' years of schooling is used as control variable because it is likely to be correlated with both children's years of schooling and parents' need for care in old age. As in the case of children, the variable is constructed by converting parents' school-leaving degrees into years of schooling. Parents' season of birth is included in the models to control for seasonal health and ageing effects as it has been shown that individuals born in autumn are healthier and live longer, on average, compared to those born in spring (Doblhammer and Vaupel 2001, Abeliansky and Strulik 2020). As adult height has been shown to be a good predictor of educational outcomes (Magnusson et al. 2006) as well as mortality and morbidity risks (Case et al. 2005, Deaton 2007, Case and Paxson 2008), parents' height is used as control variable in the regression models. Moreover, I control for child's gender and squared age to account for gender differences and potential non-linear effects of age. To capture time trends, survey year dummy variables are included in the regression models.

[4]This calculation of years of schooling is likely to introduce measurement error because some children might start primary school one year earlier or one year later than the usual school starting age in a country. Moreover, some students repeat or skip a grade, which the schooling variable does not account for. However, this is only problematic for the analyses if educational patterns vary systematically at the reform cut-off, which seems highly unlikely.

[5]ISCED is an instrument for comparing educational attainment internationally that was developed by UNESCO in the early 1970s and updated in 1997 (OECD 1999). In 2011, this classification was updated again.

Sample Restrictions

The sample is restricted to parents aged 50 years or older[6] and children aged 18 years or older. The reason for excluding children younger than 18 years is that they would not have had the opportunity to complete school. Moreover, the sample includes only parents who were born in the country in which they live at the time of the interview or who migrated there before the age of five to ensure that both parents and children received their education in the country of interview. In the main analysis, the sample is further restricted to children born 10 years before or after the first affected cohorts of the compulsory schooling reforms.

Descriptive Statistics

The final sample comprises 69,929 observations of child-parent dyads, resulting from 22,657 children and 17,438 parents observed over a maximum period of five waves. This corresponds to 48,667 child-year observations and 37,752 parent-year observations. Table 3.1 displays mean values of all variables and standard deviations in case of continuous variables, separately for children in Panel A and parents in Panel B. The final sample consists of half daughters and half sons, while fathers are slightly under-represented. On average, children are about 42 years old and have 10 years of schooling, while parents are about 69 years old and have 8 years of schooling. About 11 percent of parents report at least one ADL limitation and about 18 percent at least one IADL limitation. Roughly 53 percent of parents have one or more mobility, arm and fine motor limitations. The mean grip strength of parents in the sample is about 31.6 kg. On average, parents are able to list 17 animals, to recall 5 out of 10 words immediately after the encoding phase and 3 out of the same 10 words after a delay time. The average time orientation score in the sample is 3.81, indicating that most parents were able to remember the correct date comprising the day of the month, the month, the year and the day of the week. As shown in columns (2) and (3), parents of children affected by the compulsory schooling reforms are less likely to report functional limitations, have a higher grip strength and higher values on the cognitive test scores, which is plausible since children affected by the compulsory schooling reforms are younger on average and thus healthier[7].

[6]This means that spouses and partners aged less than 50 years who are included in SHARE because they are partners of age-eligible respondents, are excluded since they are not representative of the underlying population.

[7]There are no substantial differences between fathers and mothers with respect to physical and cognitive measures of health, except for grip strength. The mean grip strength of fathers and mothers in the sample is about 40 kg and 25 kg, respectively (not shown). According to European Working Group on Sarcopenia in Older People (EWGSOP), the thresholds for grip strength to identify sarcopenia are 32 kg for males and 22 kg for females (Bahat et al. 2016). Therefore, about 20 percent of men and around 31 percent of women in the sample have measurements below the thresholds provided by the EWGSOP.

Table 3.1: Descriptive statistics

	Full sample (1)	Pre-reform (2)	Post-reform (3)
Panel A: Children			
Son (%)	50.61	51.34	50.13
Age	42.38 (9.53)	44.33 (10.14)	41.11 (8.89)
Years of schooling	10.41 (1.94)	9.70 (2.40)	10.87 (1.38)
(Person-year) observations	48,667	19,154	29,513
Panel B: Parents			
Father (%)	43.78	42.57	44.80
Age	68.89 (9.56)	70.64 (9.82)	67.40 (9.07)
Number of children	2.54 (1.29)	2.64 (1.34)	2.45 (1.25)
Years of schooling	8.11 (3.67)	7.71 (3.55)	8.45 (3.74)
Physical and cognitive health			
1+ ADL limitation (%)	10.88	12.75	9.29
1+ IADL limitation (%)	17.75	20.05	15.78
1+ mobility limitation (%)	53.35	56.63	50.55
Grip strength (kg)	31.58 (11.52)	30.46 (11.20)	32.53 (11.69)
Verbal fluency (0-100)	17.26 (6.70)	16.30 (6.32)	18.09 (6.90)
Immediate memory (0-10)	4.80 (1.75)	4.59 (1.71)	4.97 (1.75)
Delayed memory (0-10)	3.32 (2.03)	3.06 (1.99)	3.54 (2.04)
Time orientation (0-4)	3.81 (0.51)	3.80 (0.54)	3.82 (0.49)
(Person-year) observations	37,752	17,394	20,358

Notes: The table reports mean values and standard deviations (for continuous variables) in parentheses for the pooled sample in column (1) and by children's reform exposure status in columns (2) and (3). Panel A and Panel B report unweighed averages for children and parents, respectively.
Source: Own calculations based on SHARE, waves 1, 2, 4, 5 and 6.

3.4 Empirical Approach

3.4.1 Institutional Background

To deal with the potential endogeneity of children's education in order to identify causal effects of children's education on their parents' health and long-term care dependency, the study exploits compulsory schooling reforms in Europe as source of exogenous variation in children's education. This approach is one the most widely used and accepted quasi-experimental methods in the literature to identify causal effects of education on health and mortality (Galama et al. 2018, Hamad et al. 2018). Starting with Brunello et al. (2009), various studies have extended this identification strategy to a multi-country framework by exploiting changes in compulsory schooling across several European countries[8].

The compulsory schooling reforms used in this study were implemented at different times at the national level in the following seven European countries: Belgium, France, Greece, Italy, the Netherlands, Portugal and Spain. Table 3.2 shows the year of implementation of the reforms, the pivotal cohort of the reforms and the change in years of compulsory schooling induced by the reforms.

Table 3.2: Compulsory schooling reforms in Europe

Country	**Reform year**	**Pivotal cohort**	**Change in years of compulsory schooling**
Belgium*	1983	1969	8 to 12 (+4)
France	1967	1953	8 to 10 (+2)
Greece	1975	1963	6 to 9 (+3)
Italy	1999	1985	8 to 9 (+1)
Netherlands	1975	1959	9 to 10 (+1)
Portugal	1986	1980	6 to 9 (+3)
Spain	1970	1957	6 to 8 (+2)

Notes: The table reports, by country, the year of implementation of the compulsory schooling reform, the first birth cohort of individuals affected by the reform and the change in the number of years of compulsory education induced by the reform.
*In Belgium education is compulsory from age 6 to 18. However, students must attend full-time compulsory education only until age 15. From age 15 to 18, students may continue in part-time education (Hofmarcher 2019, p. 47).
Source: Brunello et al. (2009), Brunello et al. (2016), Aparicio Fenoll and Kuehn (2017), Brilli and Tonello (2018).

In France, a reform that increased compulsory schooling by two years (from 8 to 10 years) took place in 1967 and affected individuals born in 1953 or later. Spain implemented a change in compulsory schooling in 1970, which affected individuals born in 1957 or after, by increasing compulsory schooling from 6 to 8 years. In the Netherlands, the implementation of the reform took place in 1975 by extending compulsory schooling from 9 to 10 years for those born 1959 or later. In the same year, Greece implemented a reform that raised compulsory schooling from 6 to 9 years affecting the cohorts born in 1963 or later. In 1983, Belgium increased compulsory schooling from 8 to 12 years, first affecting the cohorts born in 1969. Portugal implemented a reform in 1986, which increased compulsory schooling from 6 to 9 years and affected individuals born in 1980 or after. Finally, Italy increased the compulsory years of schooling from 8 to nine 9 in 1999, affecting individuals born in 1985 or later. For

[8]Some examples are Brunello et al. (2013), Schneeweis et al. (2014), Mazzonna (2014), Crespo et al. (2014), Gathmann et al. (2015), Weiss (2015), Brunello et al. (2016), Fort et al. (2016), Brunello et al. (2017), Aparicio Fenoll and Kuehn (2017), Kunst et al. (2020) and Hofmarcher (2021).

a short description of each reform and the pivotal cohorts, I refer to Brunello et al. (2009), Brunello et al. (2016), Aparicio Fenoll and Kuehn (2017), Hofmarcher (2019) and Brilli and Tonello (2018).

In a recently published study, Hofmarcher (2019, 2021) provides a novel database containing information on compulsory schooling reforms that have been implemented in 32 European countries in the 20th century, affecting cohorts from 1935 to 1995. Some of those countries also participate in SHARE. However, not all of the reforms are suitable for the purpose of this study since some of them were implemented too early to obtain sufficiently large numbers of treated and untreated children, e.g. in Austria, Estonia, Bulgaria, Czech Republic, Luxembourg, Poland and Slovenia. Moreover, I do not use countries that implemented compulsory schooling reforms at the regional level such as Germany, Sweden and Switzerland because SHARE does not provide information on the regions where children completed their education or where they were living when their parents were interviewed.

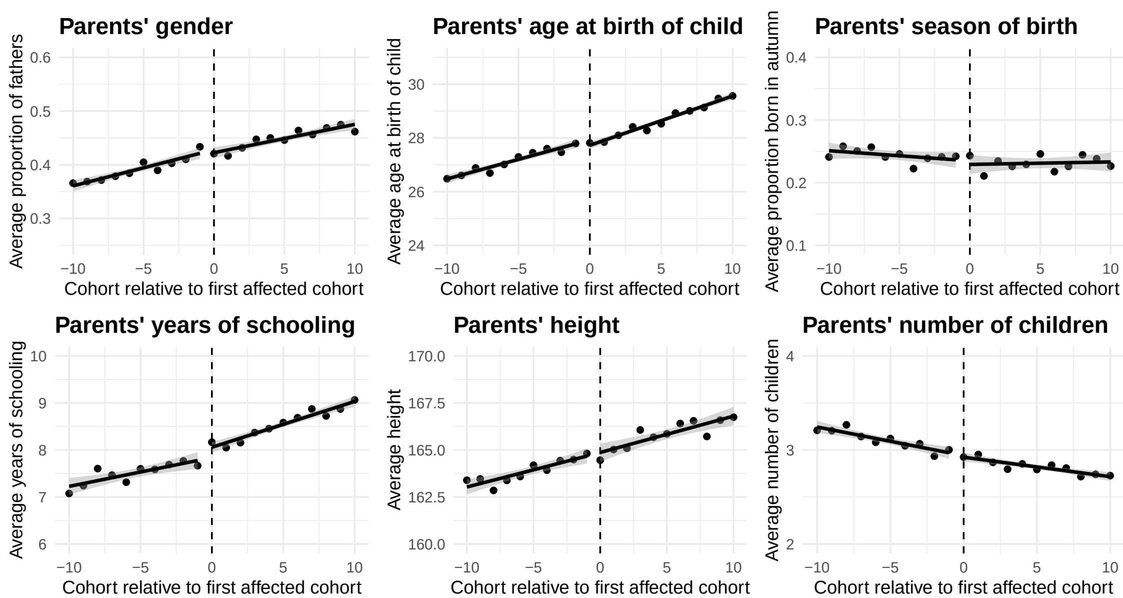
3.4.2 Identification Strategy

To identify causal effects of children's education on parental long-term care dependency, I employ an regression discontinuity (RD) design. This approach is based on the seminal work by Thistlethwaite and Campbell (1960) and is very popular in applied economics today as it requires relatively few and credible underlying identifying assumptions (Cunningham 2021, pp. 241-243). The basic idea of an RD design is to compare outcomes of observations just below and just above a fixed cut-off, i.e. the value of an observed covariate that determines whether or not they receive the treatment to identify the treatment effect at the cut-off[9]. The key identifying assumption for an RD design to be valid is that all observed and unobserved pre-treatment characteristics are continuously related to that observed covariate, the so-called assignment variable (here: child's year of birth). This implies that, just as in a randomized controlled trial, individuals to the left and right of the threshold do not differ in their observed and unobserved characteristics. In case of unobservables, this assumption cannot be tested, but it is common practice to provide evidence on its plausibility by showing that observable pre-treatment covariates do not discontinuously change at the threshold. If there is a jump in observable covariates at the cut-off, the same may be true for unobserved covariates (Lee and Lemieux 2010, p. 283-296). To assess the continuity assumption, Figure 3.1 plots mean values of the following pre-treatment covariates: parents' gender (proportion of fathers), parents' age at birth of child, parents' years of schooling, parents' season of birth (proportion of parents born in autumn), parents' height and parents' number of children.

[9]For detailed description of the RD design, I refer to Chapter 2.3.2 of this thesis.

The figure indicates that the pre-treatment covariates do not change discontinuously at the threshold. Only parents' years of schooling shows a very small jump. Since parents' years of schooling is likely to be positively correlated with children's years of schooling and negatively correlated with parents' disability, the effect of children's years of schooling on parents' disability might be downward biased (i.e. upward biased in absolute terms) if parents' years of schooling is not controlled for. To decrease potential bias due to pre-treatment differences and to increase the plausibility of the identifying assumption, I include the above-mentioned pre-treatment covariates in the regressions (Frölich and Huber 2019).

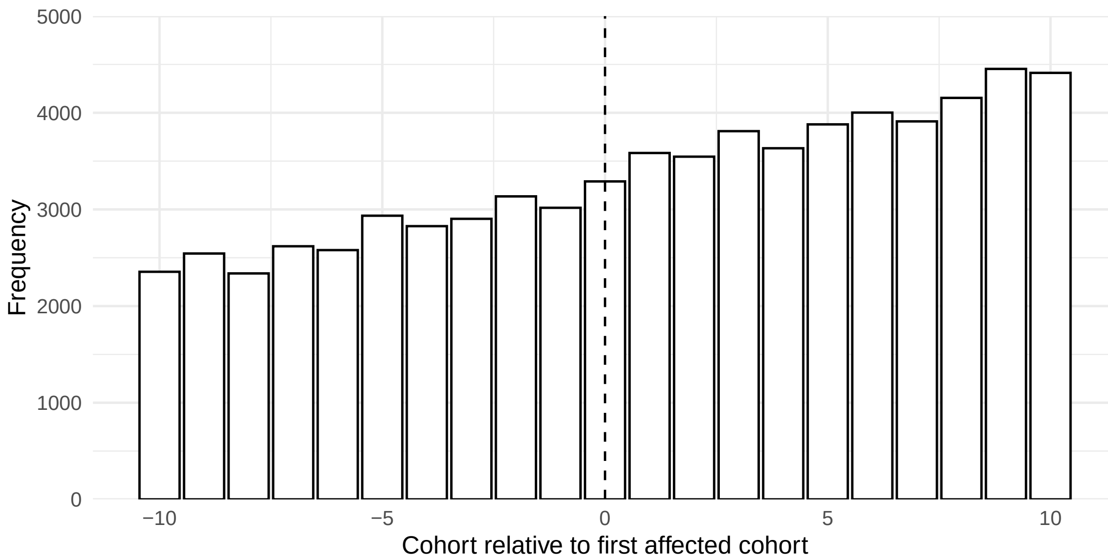
Figure 3.1: Distribution of pre-treatment covariates



Notes: The figure shows the distribution of the following pre-treatment covariates: parents' gender (proportion of fathers), parents' age at birth of child, parents' season of birth (proportion of parents born in autumn), parents' years of schooling, parents' height, and parents' number of children. Each point represents a weighted mean (weighted by the number of observations per country) to account for differences in the number of observations per country. Mean values of the pre-treatment covariates are shown for cohorts of children born 10 years before and after the pivotal cohorts of the compulsory schooling reforms. All countries are normalized by the time of the reforms, which is set at zero.
Source: Own calculations based on SHARE, waves 1, 2, 4, 5 and 6.

In addition, the continuity assumption may not be plausible if individuals are able to manipulate their treatment status to be on a particular side of the cut-off. That is, the estimate of the treatment effect will be biased if the assignment variable can be manipulated. A test to assess a potential manipulation of the assignment variable is to show its distribution. In particular, a histogram of the assignment variable for observations above and below the cut-off can be used to visually check for the presence of manipulation. If there is a sharp increase or decrease in the number of observations around the threshold, it can be assumed that the assignment variable has been manipulated. Figure 3.2 shows the distribution of the assignment variable for 10 cohorts of children below and above the cut-off in a histogram. Each point in the figure represents the number of observations in all cohorts in the different countries, which are at the same distance from the pivotal cohorts. The visual inspection does not show any unusual jump at the cut-off, suggesting that bunching around the cut-off should not be a problem in this case.

Figure 3.2: Histogram of the assignment variable



Notes: The figure shows a histogram of the assignment variable (child's year of birth) for cohorts of children born 10 years before and after the first birth cohort affected by the reforms. Each bar represents the number of observations in all cohorts in the different countries, which are at the same distance from the pivotal cohorts. There is no evidence for manipulation of the assignment variable.
Source: Own calculations based on SHARE, waves 1, 2, 4, 5 and 6.

The approach used in this study is a fuzzy RD design, in contrast to a sharp RD design, because the probability of treatment discontinuously increases at the cut-off but does not jump from 0 to 1. The reason is that some students stay in school longer in absence of the reforms because they attend higher school tracks that are not affected by the reforms. Since the treatment effect in a fuzzy RD design can be estimated as the difference in mean outcomes around the cut-off divided by the difference in mean treatment assignment around the cut-off (Cunningham (2021), p. 280), the regression model to estimate the effect of children's education on parents' long-term care dependency is specified as follows:

$$Educ_{itjc} = \alpha_1 + \alpha_2 Comp_{itjc} + f(cYob_{itjc}) + \gamma_j + \delta_c + \nu_t + (\gamma_j \times c) + \alpha_3 X_{itjc} + \alpha_4 Y^p_{itjc} + \epsilon_{itjc} \quad (3.1)$$

$$Need^p_{itjc} = \beta_1 + \beta_2 Comp_{itjc} + f(cYob_{itjc}) + \eta_j + \theta_c + \lambda_t + (\eta_j \times c) + \beta_3 X_{itjc} + \beta_4 Y^p_{itjc} + \pi_{itjc} \quad (3.2)$$

Equation (3.1) is the first stage regression model to estimate the effect of children's compulsory years of schooling induced by the compulsory schooling reforms on years of schooling. The reduced form regression model to determine the effect of children's compulsory years of schooling on parents' long-term care dependency is given by equation (3.2). The dependent variable $Educ_{itjc}$ in equation (3.1) denotes years of schooling of child i observed in year t and born in country j in year c, while the dependent variable $Need^{p}_{itjc}$ in equation (3.2) represents long-term care dependency of parent p of child i observed in year t and born in country j in year c. The instrumental variable $Comp_{itjc}$ in both equations represents child i's number of compulsory years of schooling induced by the reform in country j. It is constructed by comparing child i's year of birth with the pivotal cohort of the reform in country j and assigning the post-reform years of compulsory schooling (e.g. 9 years in Italy) if year of birth exceeds the pivotal cohort and the pre-reform years of compulsory schooling (e.g. 8 years in Italy) otherwise. I follow the recommendation of Lee and Lemieux (2010) and center the assignment variable around the cut-off. Thus, $cYob_{itjc}$ measures child's birth cohort relative to the relevant cut-off and is positive for cohorts who are affected by the reform and negative for cohorts who are not affected. γ_j and η_j, δ_c and θ_c, and ν_t and λ_t are fixed effects to account for unobservable heterogeneity across countries, cohorts and survey years, respectively. In the preferred specification, I also control for country-specific linear trends in child's birth cohort ($\gamma_j \times c$ and $\eta_j \times c$) to address different cohort trends across countries by including interactions of child's year of birth (relative to the pivotal cohort) with country dummies. X_{itjc} includes child's gender and quadratic age to account for potential differences by gender and non-linear age effects. Y^{p}_{itjc} contains additional pre-treatment characteristics of parent p, including gender, age at birth of child, season of birth, years of schooling, height and number of children. The coefficients of interest are α_2 and β_2 and hence, the causal effect corresponds to the ratio of β_2 and α_2.

The function $f(\cdot)$ in both equations captures the relationship between the assignment variable and the outcome and needs to be correctly specified since a misspecification of the functional form can generate biased estimates of the treatment effect (Lee and Lemieux 2010, p. 284). Following a recommendation by Gelman and Imbens (2019), I use a local non-parametric approach in the main analysis and estimate local linear regressions on either side of the cut-off, where $f(\cdot)$ is a linear function of the assignment variable. The regressions in the main analyses are based on a bandwidth of 10 cohorts around the cut-off. The choice of a 10-year bandwidth is largely arbitrary but ensures, in case of countries with several changes in the length of compulsory schooling, to have included only the compulsory schooling change to be studied and not a previous or subsequent one. Narrower bandwidths are used later on in robustness checks to demonstrate that the estimates are not sensitive to the chosen bandwidth. Although Imbens and Kalyanaraman (2012) have suggested an "optimal bandwidth choice" for an RD estimator, I decided to follow a sensitivity test-based approach and to pick a bunch of different bandwidths (Huntington-Klein 2021, pp. 551-552) instead of estimating an "optimal bandwidth", which is itself subject to random influences. Moreover, I present results from local regressions with a quadratic function of the assignment variable, and from parametric global regressions with first-order and second-order polynomials in birth cohort in robustness checks in Section 3.5.3.

All regressions are estimated with robust standard errors which are clustered at the parent level to account for the fact that most parents appear multiple times in the data because they have more than one child[10]. Standard errors are not clustered by the values of the assignment variable, which was standard practice for a long time in case of a discrete assignment variable (Lee and Card 2008). The reason is that Kolesár and Rothe (2018) have recently recommended against this because they found that confidence intervals based on standard errors clustered by the running variable can have worse coverage properties than those based on conventional heteroskedasticity-robust standard errors[11]. Following Lundborg and Majlesi (2018), I also run weighted regressions with weights equal to the inverse of parent's number of children to ensure that all parents in the sample receive the same weight[12]. I do not use sampling weights in the analysis as the fuzzy RD design estimates a local average treatment effects (LATE) for the subpopulation of compliers of the compulsory schooling reforms whose assignment variable is equal to the threshold (Bertanha and Imbens 2020). Therefore, as this study does not make inferences about the whole population, sample weights are not required (Solon et al. 2015).

3.5 Results

The main results are presented in three sections. First, I show results from the principal component analysis (PCA) used to construct the disability score as overall summary measure of parents' long-term care dependency. Second, I show results from ordinary least squares (OLS) regressions that do not account for the potential endogeneity of children's education, followed by results from fuzzy RD regressions of parents' disability on children's years of schooling. Third, I present results from various robustness checks and heterogeneity analyses.

[10] For example, a parent with three children appears in the sample as three child-parent pairs (see Figure 3.A.1 in the Appendix for an exemplary illustration).

[11] Kolesár and Rothe (2018) suggest an alternative way to calculate confidence intervals in RD settings with a discrete running variable, which can be implemented using the RDHonest package in R. However, since the package does not support the use of covariates, I do not use these confidence intervals.

[12] As a robustness test in Section 3.5.3, I also run unweighted regressions and the results are quantitatively similar to the main results based on weighted regressions.

3.5.1 Principal Component Analysis (PCA)

As already outlined above, the outcome variable parents' long-term care dependency is measured by an overall disability score. The score is constructed by conducting a PCA on 31 variables related to parents' physical and cognitive health. PCA is a statistical data reduction method for multivariate data analysis that can be used to convert a set of possibly correlated variables into a smaller number of linearly uncorrelated variables, the so-called principal components. Each component is a linear weighted combination of the original variables (Sarstedt and Mooi 2014, pp. 239-240). In order to construct the disability score, I follow Poterba et al. (2017) and retain the first principal component as it captures the maximum variance in the data.

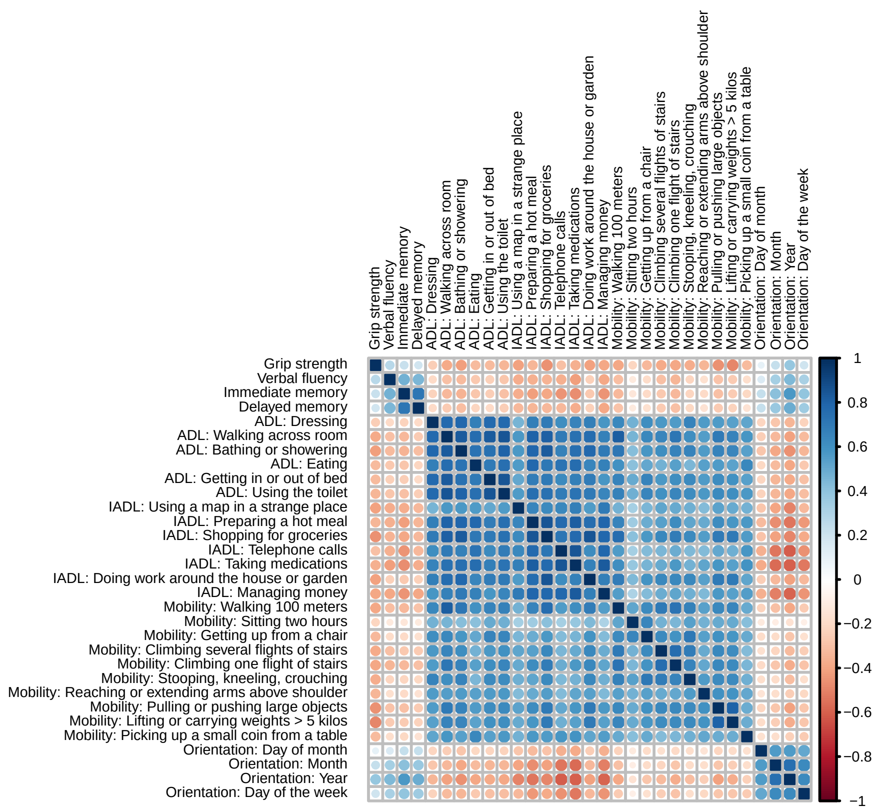
Correlation Matrix

As a first step, I compute a correlation matrix to identify the correlations between the 31 considered variables, comprising 6 binary variables for ADL limitations, 7 binary variables for IADL limitations, 10 binary variables for mobility limitations, grip strength as continuous variable, 4 binary variables for time orientation, and verbal fluency, immediate memory and delayed memory as continuous variables. To account for Pearson correlations between continuous variables, tetrachoric correlations between binary variables and biserial correlations between continuous and binary variables, the correlation matrix is obtained from the *mixedCor()* function included in the *psych* package in R (Revell 2021). The resulting correlation matrix is visualized using the function *corrplot()* included in the *corrplot* package in R. The visual inspection of the correlation matrix in Figure 3.3 reveals, as expected, both positive and negative correlations between the variables. All variables are moderately or highly correlated, with most correlations above 0.3 (in absolute value), suggesting that PCA is well suited for constructing the disability score (Sarstedt and Mooi 2014, p. 207).

Kaiser-Meyer-Olkin Measure and Bartlett's Test of Sphericity

The suitability of the data for PCA is measured by the Kaiser-Meyer-Olkin (KMO) measure of sampling adequacy and the Bartlett's test of sphericity. The results of both tests are presented in Table 3.A.1 in the Appendix. The KMO measure of sampling adequacy tests whether the correlations between variables can be explained by the other variables in the dataset (Sarstedt and Mooi 2014, pp. 207-208, Kaiser 1974). KMO values range between 0 to 1. For the calculation of the KMO measure, I use the *kmo()* function from the *psych* package in R. As shown in the upper part of Table 3.A.1, the KMO measure is 0.93 for the full set of variables and varies between 0.74 and 0.97 for the individual variables, which is above the acceptable limit of 0.6 (Sarstedt and Mooi 2014, pp. 207-208). Bartlett's test of sphericity tests the null hypothesis that the correlation matrix is an identity matrix, so that all non-diagonal elements are zero, which would indicate that the variables in the correlation matrix are uncorrelated. Put differently, Bartlett's test of sphericity measures how close the correlation matrix is to the identity matrix (Sarstedt and Mooi 2014, pp. 207-208). The test is performed using the *cortest.bartlett()* function in the *psych* package in R. As shown in the lower part of Table 3.A.1 in the Appendix, the test is highly significant (p = 0.00), indicating that the null hypothesis can be rejected and that the correlation matrix is suitable for PCA.

Figure 3.3: Visualisation of the correlation matrix

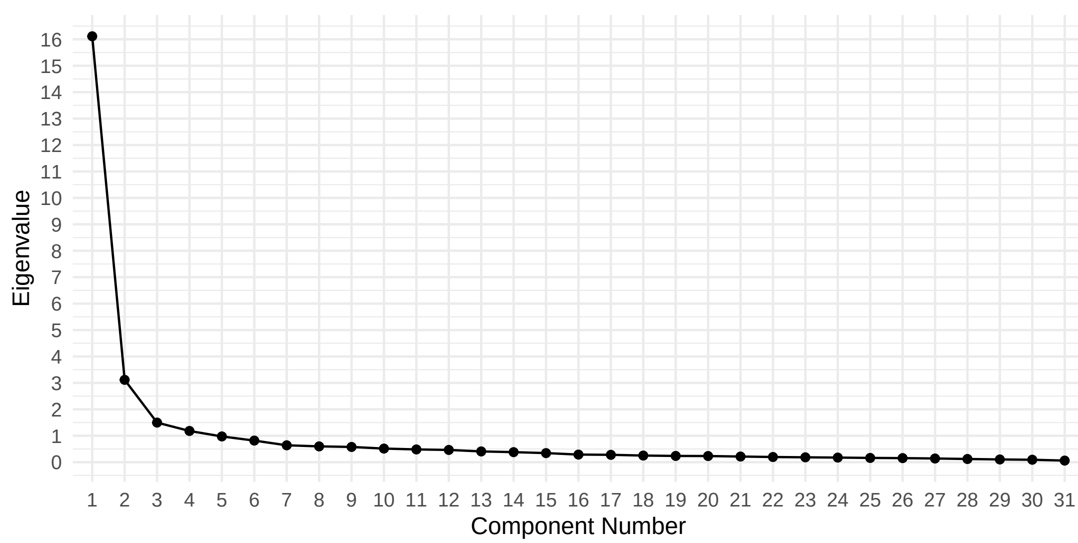


Notes: The figure is a visualization of the correlation matrix obtained from the *mixedCor()* function from the *psych* package in R that contains Pearson, tetrachoric and biserial correlations between 31 variables related to parents' health. Those variables comprise 6 binary variables for ADL limitations, 7 binary variables for IADL limitations, 10 binary variables for mobility limitations, grip strength as continuous variable, 4 binary variables for time orientation, and verbal fluency, immediate memory and delayed memory as continuous variables. The *corrplot()* function from *corrplot* package in R has been used to obtain the heatmap-like plot of the correlation matrix. The size and intensity of the color of the dots reflect the strength of the association. Blue coloured dots indicate positive correlations, while red coloured dots are negative correlations.
Source: Own calculations based on SHARE, waves 1, 2, 4, 5 and 6.

Eigenvalues, Loadings and Scores

The correlation matrix obtained from the *mixedCor()* function in the *psych* package in R is then used to conduct the PCA using the function *principal()* from the same R package[13]. In general, running a PCA returns as many principal components as there are variables. By construction, the first principal component explains the maximum percentage of the total variance in the data. The second principal component is linearly independent, i.e. orthogonal of the first component and explains the maximum percentage of the remaining variance, with subsequent principal components following in the same manner. Table 3.A.2 in the Appendix shows the eigenvalues, the percentage of explained variance by the components and the cumulative share of explained variance for all principal components. The principal components are sorted in descending order according to the eigenvalues that describe how much of the total variation a given principal component explains (Sarstedt and Mooi 2014, p. 210). To visualize the results, I also generated a so-called scree plot that shows the number of the principal component on the horizontal axis and the corresponding eigenvalue on the vertical axis (see Figure 3.4).

Figure 3.4: Scree plot of principal components



Notes: The so-called scree plot shows the principal component number on the horizontal axis and the eigenvalues on the vertical axis. The figure indicates that four principal components have eigenvalues greater than one.
Source: Own calculations based on SHARE, waves 1, 2, 4, 5 and 6.

[13]Since I am interested in summarising all of the variance in the data and in retaining only the first principal component as summary measure for disability, the PCA is conducted without rotation. The idea of rotation is to simplify the interpretation of the principal components by rotating the axes so that each observed variable or each set of observed variables loads mainly on only one principal component (Sarstedt and Mooi 2014, p. 214).

The scree plot shows that four principal components have eigenvalues greater than one, which is usually the standard criterion in the literature to select the number of principal components to retain because principal components with eigenvalues greater than one account for more variance in the data than a single variable (Sarstedt and Mooi 2014, p. 212). The purpose of the PCA is, however, to retain only the first principal component as disability score as it accounts, by definition, for the maximum variance in the data. As shown in Table 3.A.2 in the Appendix, the first principal component accounts for 52 percent of the total variance in the data, while the second principal component explains only 10 percent, which indicates that the additional value of the second principal component is quite small.

Next, I examine the loadings on the first principal component to determine which variables are strongly related to it. Loadings represent the correlations between a principal component and the variables and can take values between -1 and $+1$, where high absolute values indicate that a variable is well represented by the principal component (Sarstedt and Mooi 2014, pp. 213-214). As shown in Table 3.A.3 in the Appendix, the loadings range from 0.361 (in absolute terms) to 0.903 and have an expected sign in determining overall disability. In particular, grip strength, verbal fluency, immediate word recall, delayed word recall, and the four measures of time orientation have a negative loading on the first principal component. Each ADL limitation, IADL limitation and mobility limitation has a positive loading on the first principal component as more limitations are associated with a higher level of disability. The four variables with the largest loadings on the first principal component in absolute value are shopping for groceries (IADL), walking across the room (ADL), bathing or showering (ADL) and preparing a hot meal (IADL). Finally, I use the loadings to generate component scores, which represent weighted averages of the original variables for each observation in the data. The weights are obtained by multiplying the inverse of the correlation matrix with the loadings. In order to calculate the scores, the function *factor.scores()* from the *psych* package in R is used. The scores are then used as overall disability summary score. For the ease of interpretation, the score is standardized to a mean of zero and a standard deviation of one (z-standardization). High values on the disability score reflect a high level of disability and thus long-term care dependency.

3.5.2 Main Results

In this section, I report regression results for the effect of children's years of schooling on parental long-term care dependency, as measured by the disability score. Table 3.3 shows OLS results in Panel A, followed by first stage results in Panel B, reduced form results in Panel C and fuzzy RD results in Panel D. In column (1), only country and child birth cohort fixed effects are included. In columns (2) and (3), country-specific linear cohort trends, interview year fixed effects and child's gender and quadratic age are added. In the preferred specification in column (4), I also control for parental gender, age at birth of child, years of schooling, season of birth, height and number of children.

Table 3.3: Effect of children's years of schooling on parents' disability score

	(1)	(2)	(3)	(4)
Panel A: OLS results				
Years of schooling	−0.047***	−0.042***	−0.043***	−0.030***
	[−0.054, −0.040]	[−0.049, −0.035]	[−0.050, −0.036]	[−0.036, −0.023]
	(0.004)	(0.004)	(0.004)	(0.004)
Observations	69,929	69,929	69,929	69,929
Panel B: First stage results				
Compulsory schooling	0.513***	0.497***	0.496***	0.481***
	[0.491, 0.536]	[0.469, 0.525]	[0.468, 0.524]	[0.454, 0.509]
	(0.014)	(0.017)	(0.017)	(0.017)
F-statistic	1378.700	859.370	854.330	846.470
Observations	69,929	69,929	69,929	69,929
Panel C: Reduced form results				
Compulsory schooling	0.002	−0.003	−0.003	0.002
	[−0.009, 0.014]	[−0.017, 0.012]	[−0.017, 0.012]	[−0.012, 0.016]
	(0.007)	(0.009)	(0.009)	(0.009)
Observations	69,929	69,929	69,929	69,929
Panel D: Fuzzy RD results				
Years of schooling	0.005	−0.005	−0.005	0.005
	[−0.018, 0.028]	[−0.034, 0.024]	[−0.034, 0.024]	[−0.024, 0.034]
	(0.014)	(0.018)	(0.018)	(0.018)
Observations	69,929	69,929	69,929	69,929
Cohort fixed effects	yes	yes	yes	yes
Country fixed effects	yes	yes	yes	yes
Country-specific linear cohort trend	no	yes	yes	yes
Interview year fixed effects	no	no	yes	yes
Child characteristics	no	no	yes	yes
Parental characteristics	no	no	no	yes

Notes: The table shows estimation results for the effect of children's years of schooling on parents' disability. All regressions are based on local linear models, use a bandwidth of 10 cohorts before and after the pivotal cohorts and include cohort and country fixed effects. In column (2), I also control for linear country-specific cohort trends. Additional control variables included in column (3) are interview year fixed effects, child's gender and quadratic age. In column (4), I also control for parental age at birth of child, gender, education, season of birth, height and number of children. All regressions are weighted by the inverse of parent's number of children. Standard errors are clustered at the parent level and presented in parentheses. 90% confidence intervals are shown in brackets. *p<0.1; **p<0.05; ***p<0.01.
Source: Own calculations based on SHARE, waves 1, 2, 4, 5 and 6.

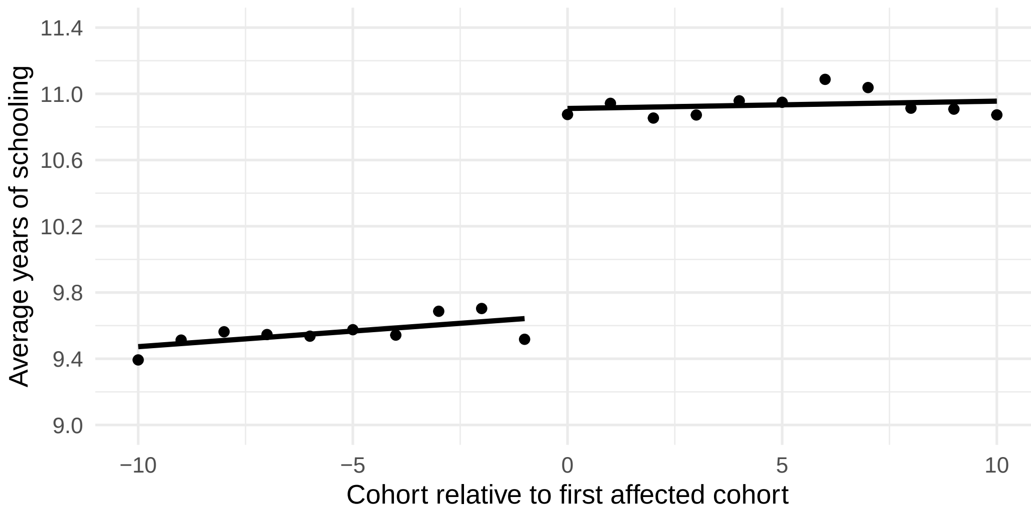
Ordinary Least Squares Results

Panel A of Table 3.3 examines the relationship between children's years of schooling and parents' disability, based on simple OLS regressions. Across all columns of Panel A, the estimates show a clear negative association between children's years of schooling and parents' disability score, that is statistically significant at the 1 percent level. For instance, as shown in column (1), the estimate suggests that each additional year of children's schooling is associated with a reduction in parents' disability score by 0.047 standard deviations. The results are qualitatively robust to the inclusion of additional covariates in columns (2) to (4). In the preferred specification in column (4) that includes the full set of covariates, I find that an one-year increase in children's years of schooling is associated with a reduction in parents' disability score by 0.030 standard deviations. In order to determine whether this effect is small or large, one could conduct a simple descriptive example by comparing the effect size of 0.030 standard deviations to the descriptive effect of 10 years of life, e.g. the reduction in the average disability score at age 70 compared to age 80. From age 80 to 70, the mean disability score in the sample decreases by 0.411 standard deviations. The reduction in parents' disability score by 0.030 standard deviations per additional year of children's schooling is thus comparable to the effect that 0.730 years of life (i.e. about 9 months) have on the disability score, which is a moderate effect. However, as already discussed above, the associations between children's education and parental disability cannot be taken as causal effects because children's education is most likely endogenous and hence, I account for the potential endogeneity in a fuzzy RD design.

First Stage Results

I start by examining the first stage effect of children's exposure to the compulsory schooling reforms on children's years of schooling graphically. Figure 3.5 plots children's average years of schooling for 10 cohorts before and after the first cohort affected by the reforms. The figure shows a general upward trend in children's years of schooling over time and a positive jump for the first birth cohorts affected by the reforms. This is confirmed by the first stage regression results of children's years of schooling on compulsory years of schooling induced by the reforms in Panel B of Table 3.3. Across all specifications, the estimates suggest that the compulsory schooling reforms significantly increased children's average years of schooling. In the preferred specification in column (4), I find that an one-year increase in compulsory schooling raises children's average years of schooling by about half a year. The coefficient is statistically different from zero at the 1 percent level. The first stage effect is similar in magnitude compared to previous studies exploiting compulsory schooling laws in European countries in a comparable multi-country setup, finding that one additional year of compulsory schooling increases average years of schooling by 0.3 to 0.6 years (Brunello et al. 2009, Borgonovi et al. 2010, Brunello et al. 2013, Stella 2013, Schneeweis et al. 2014, Mazzonna 2014, Gathmann et al. 2015, Brunello et al. 2016, Fort et al. 2016, Kunst et al. 2020). Across all columns of Panel B, the first stage F-statistic for the excluded instrument is above 800, alleviating concerns about instrument weakness (Staiger and Stock 1997).

Figure 3.5: Effect of compulsory schooling reforms on children's years of schooling



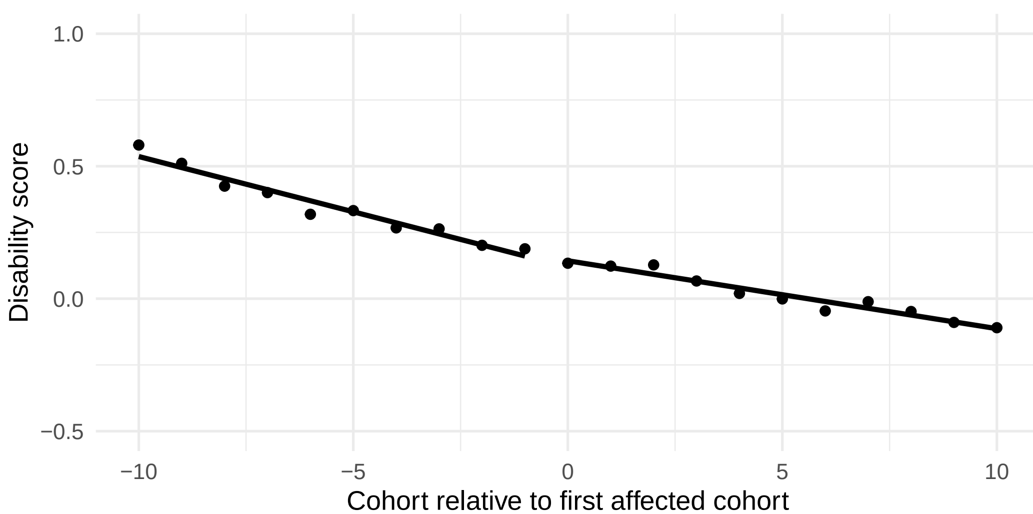
Notes: The figure displays average years of schooling for cohorts of children born within 10 years before and after the pivotal cohorts of the compulsory schooling reforms. Each point represents a weighted mean (weighted by the number of observations per country) of all cohorts in the different countries, which are at the same distance from the pivotal cohort of the compulsory schooling reforms. All countries are normalized by the time of the reform, which is set at time zero.
Source: Own calculations based on SHARE, waves 1, 2, 4, 5 and 6.

It should be noted that the jump in children's years of schooling at the cut-off displayed in Figure 3.5 differs in terms of size compared to the first stage estimates in Panel B of Table 3.3. The reason is that the figure does not take into account country differences with respect to the intensity of the reforms. Put differently, the average increase in the number of compulsory years of schooling induced by the reforms is more than one. For instance, in Belgium compulsory schooling was raised by four years, while the change in Italy was only one year (see Table 3.2). When regressing children's years of schooling on a binary indicator that is set to one for post-reforms cohorts of children and zero otherwise, and thus does not account for the actual change in the number of compulsory years of schooling, the estimate is about 1.2 and corresponds quite well to Figure 3.5 in terms of effect size.

Reduced Form Results

Figure 3.6 provides a graphical analysis of parents' disability score by children's birth cohort and is a visual representation of the reduced form relationship. The figure plots parents' disability score for 10 cohorts of children before and after the cut-off. The figure shows a general decrease in parents' disability score over time, as younger parents are likely to be healthier, but no jump at the pivotal cohorts of the reforms. Hence, the visual inspection suggests no effect of children's compulsory years of schooling on parental disability. This is corroborated by the reduced form regression results for the effect of children's compulsory years of schooling on parents' disability score in Panel C of Table 3.3. In column (1), the estimate points to an *increase* in parents' disability score by 0.002 standard deviations due to one additional year of compulsory schooling. When including country-specific linear cohort trends in column (2) and interview year fixed effects and child characteristics in column (3), the estimates become negative, but they switch the sign again to positive in the preferred specification in column (4) that includes also parental characteristics. However, across all columns of Panel C, the estimates are close to zero and insignificant at conventional levels of statistical significance. Hence, the estimates largely confirm the visual results in Figure 3.6.

Figure 3.6: Effect of compulsory schooling reforms on parents' disability score



Notes: The figure displays parents' average disability score for cohorts of children born within 10 years before and after the pivotal cohorts. Each point represents a weighted mean (weighted by the number of observations per country) of all cohorts in the different countries, which are at the same distance from the pivotal cohort of the compulsory schooling reforms. All countries are normalized by the time of the reform, which is set at time zero.
Source: Own calculations based on SHARE, waves 1, 2, 4, 5 and 6.

Fuzzy Regression Discontinuity Results

The fuzzy RD estimates for the causal effect of children's years of schooling on parental disability, obtained from local linear regressions, are presented in Panel D of Table 3.3. As already outlined above, the fuzzy RD estimator corresponds to the ratio of reduced form coefficient and first stage coefficient. Since the reduced form estimates in Panel C were already close to zero, the fuzzy RD estimates are comparable in magnitude. In the preferred specification in column (4), the estimate is *plus* 0.005 standard deviations. This effect size would be descriptively comparable to the increase in the average disability score induced by about 1 month of life, which is basically a zero effect. However, the estimate is not statistically different from zero and the 90 percent confidence interval excludes effects smaller than -0.024 standard deviations and larger than 0.034 standard deviations.

3.5.3 Robustness Checks

This section reports results from various sensitivity analyses with respect to the choice of the bandwidth and the functional form, the exclusion of the pivotal cohorts of the reforms, the inclusion of more flexible country-specific trends in child's birth cohort and the use of weights. The results are presented graphically in Figure 3.7 to Figure 3.10 by plotting point estimates and corresponding 95% confidence intervals[14]. At first glance, most confidence intervals cover zero, confirming the robustness of the main findings. The robustness checks are now presented in more detail.

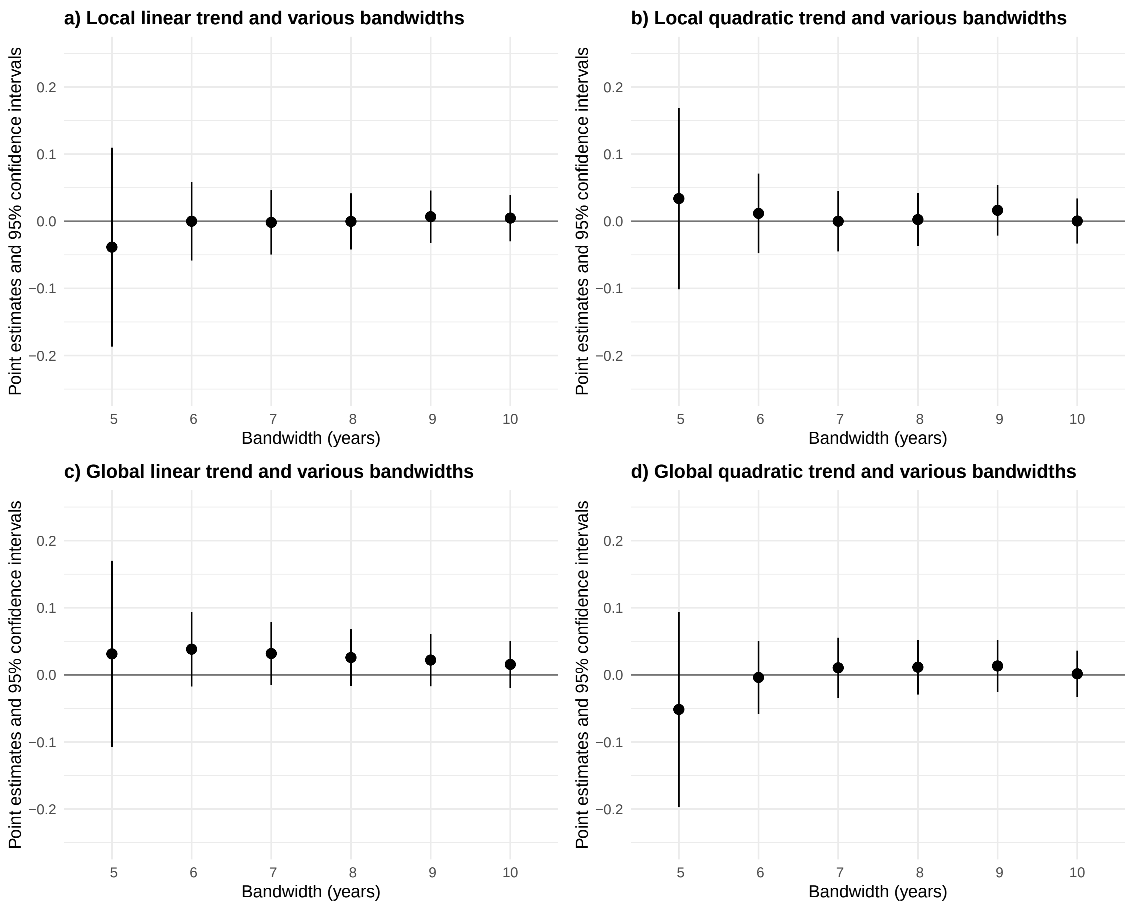
In Figure 3.7, I follow recommendations for RD designs by Lee and Lemieux (2010) and assess the robustness of results to various bandwidths and trend specifications. In particular, I estimate the fuzzy RD parameters based on local linear and local quadratic as well as global linear and global quadratic regressions on samples of varying bandwidths ranging from ± 5 to ± 10 years in one-year steps. Figure 3.7 shows, that the effect of an one-year increase in children's years of schooling on parental disability remains consistently insignificant when shrinking the bandwidth and/or changing the functional form[15]. In Figure 3.8, I analyse whether the main results are robust to the exclusion of the pivotal cohorts of the reforms. Since the data do not provide precise information on a child's month of birth, all children born in the year of the pivotal cohort of a reform are treated as being affected by the reform in the main analyses, although some of these children might actually not have been affected.

[14]Another method to visualize the robustness of results would be to perform a specification curve analysis as in Chapter 2 and Chapter 4 of this thesis.

[15]For the smallest considered bandwidth of five years before and after the discontinuity, the confidence intervals are quite large due to the small number of observations (per country and cohort). Therefore, I do not show results for smaller bandwidths.

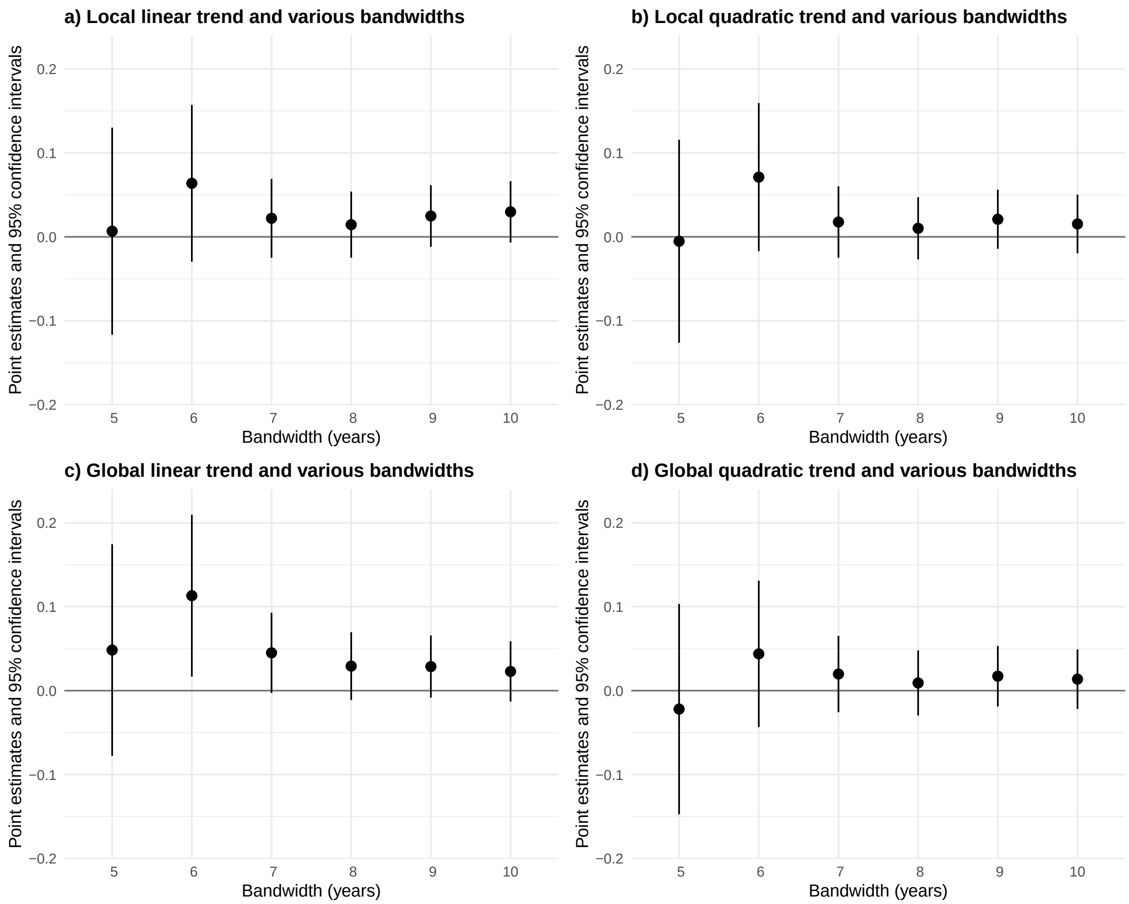
This could result in a mixture of treated and untreated children in the pivotal cohorts. With one exception in the global linear regression with a bandwidth of ±6 years, the confidence intervals shown in Figure 3.8 cover zero and hence, the results are virtually unchanged when the pivotal cohorts are excluded.

Figure 3.7: Robustness I: Various bandwidths and trend specifications



Notes: Each point in the figure represents a point estimate obtained from a) local linear, b) local quadratic, c) global linear and d) global quadratic regressions of parents' disability score on children's years of schooling based on samples of varying bandwidths, ranging from ±5 to ±10 years in one-year steps. All regressions are weighted by the inverse of parents' number of children and control for child's birth cohort and country fixed effects, linear country-specific cohort trends, interview year fixed effects, child's gender and quadratic age as well as parental age at birth of child, gender, education, season of birth, height and number of children. The vertical lines represents 95% confidence intervals that are based on robust standard errors clustered at the parent level.
Source: Own calculations based on SHARE, waves 1, 2, 4, 5 and 6.

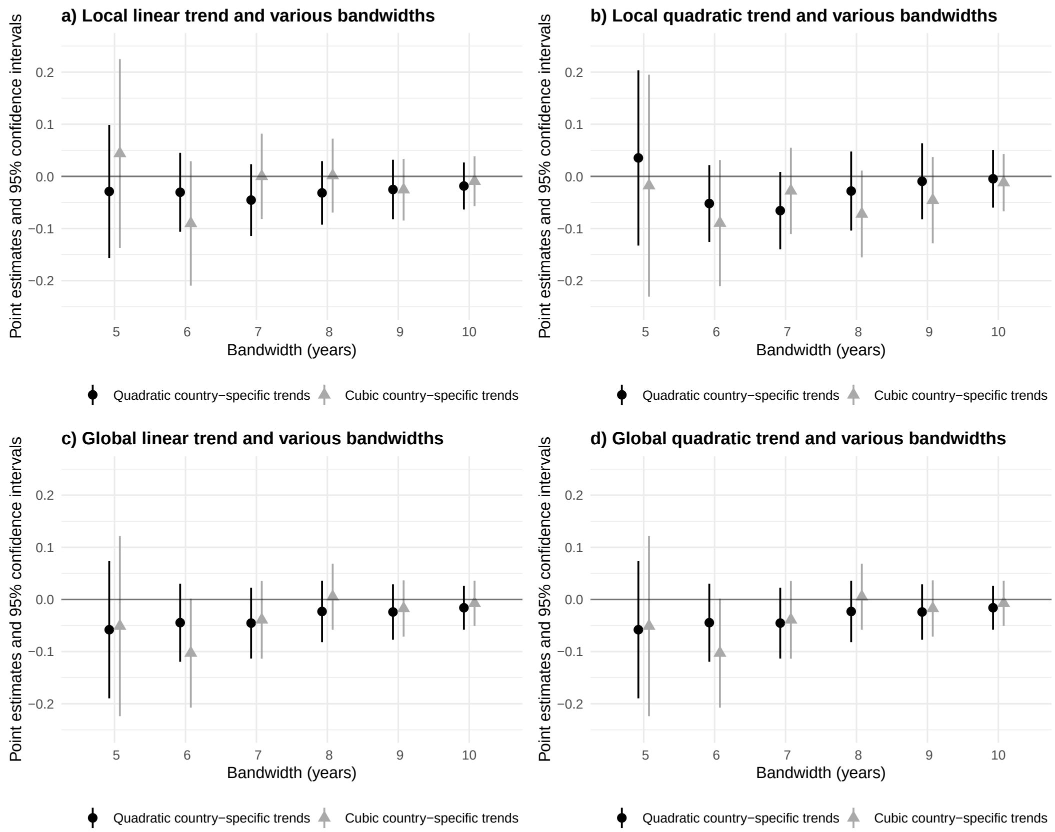
Figure 3.8: Robustness II: Pivotal cohort excluded



Notes: Each point in the figure represents a point estimate obtained from (a) local linear, (b) local quadratic, (c) global linear and (d) global quadratic regressions of parents' disability score on children's years of schooling based on samples of varying bandwidths, ranging from ±5 to ±10 years in one-year steps. In all regressions, the pivotal cohort of the reforms is excluded. Moreover, all regressions are weighted by the inverse of parents' number of children and include child's birth cohort and country fixed effects, linear country-specific cohort trends, interview year fixed effects, child's gender and quadratic age as well as parental age at birth of child, gender, education, season of birth, height and number of children. The vertical lines represents 95% confidence intervals that are based on robust standard errors clustered at parent level.
Source: Own calculations based on SHARE, waves 1, 2, 4, 5 and 6.

In Figure 3.9, I evaluate whether controlling for country-specific linear cohort trends in addition to child birth cohort fixed effects is sufficient to control for unobserved trends at the country level which might be correlated with the decision to introduce compulsory schooling reforms and with parental health outcomes. Specifically, I examine whether the main results presented in the preceding section are robust to the inclusion of quadratic and cubic in addition to linear country-specific cohort trends. In line with the main findings, Figure 3.9 shows that none of the coefficients is statistically different from zero.

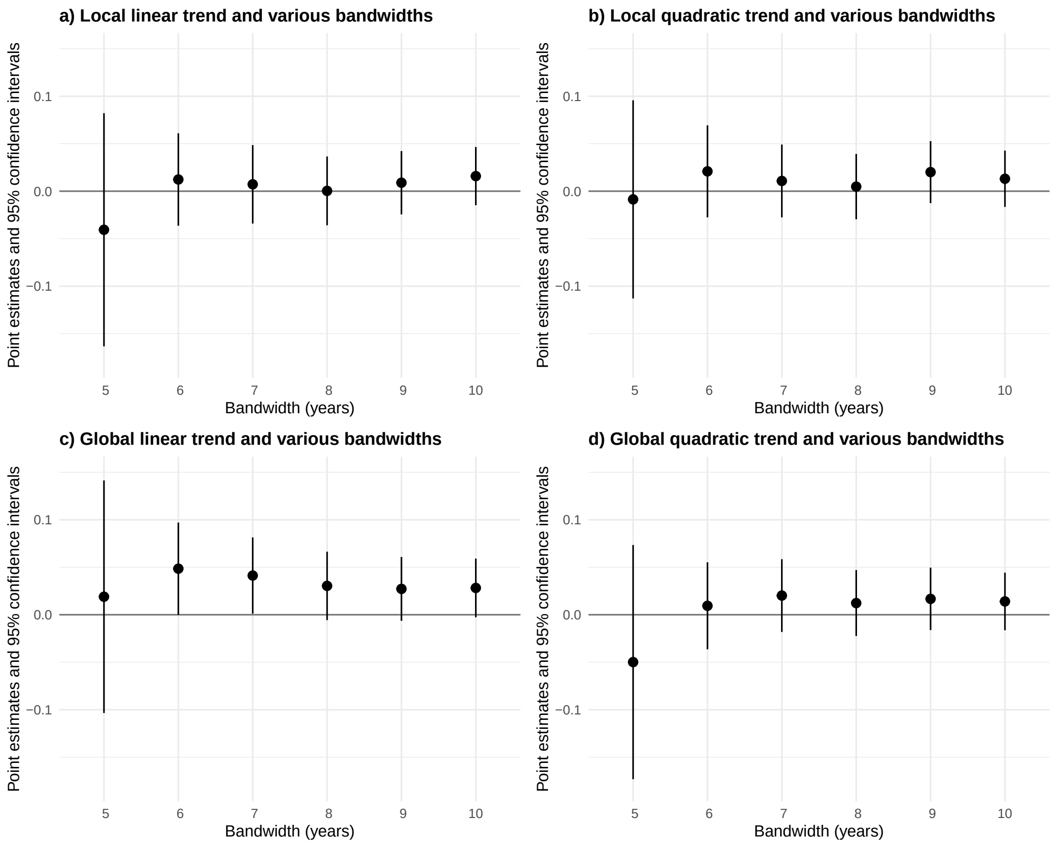
Figure 3.9: Robustness III: More flexible country-specific cohort trends



Notes: Each black circle and grey triangle in the figure represents a point estimate obtained from (a) local linear, (b) local quadratic, (c) global linear and (d) global quadratic regressions of parents' disability score on children's years of schooling based on samples of varying bandwidths, ranging from ±5 to ±10 years in one-year steps. The vertical lines represents 95% confidence intervals that are based on robust standard errors clustered at the parent level. All regressions are weighted by the inverse of parents' number of children and include child's birth cohort and country fixed effects, linear country-specific cohort trends, interview year fixed effects, child's gender and quadratic age as well as parental age at birth of child, gender, education, season of birth, height and number of children. The confidence intervals shown in black are based on regressions that control for quadratic country-specific cohort trends in addition to linear country-specific cohort trends and all other above-mentioned covariates, while confidence intervals displayed in grey are based on regressions that additionally include country-specific cubic cohort trends.
Source: Own calculations based on SHARE, waves 1, 2, 4, 5 and 6.

In Figure 3.10, I re-estimate the models by running unweighted regressions. Although it seems plausible to weight the regressions by the inverse of parents' number of children, as suggested by Lundborg and Majlesi (2018), to avoid that high-fertility parents receive a greater weight in the regressions than low-fertility parents, this would actually only be relevant if the education of an only child (i.e. a child without siblings) has a larger benefit for parents' health than the education of one of several children. Figure 3.10 demonstrates that the main results are also fairly robust to running unweighted regressions.

Figure 3.10: Robustness IV: Unweighted regressions



Notes: Each point in the figure represents a point estimate obtained from a) local linear, b) local quadratic, c) global linear and d) global quadratic regressions of parents' disability score on children's years of schooling based on samples of varying bandwidths, ranging from ±5 to ±10 years in one-year steps. All regressions are unweighted and control for child's birth cohort and country fixed effects, linear country-specific cohort trends, interview year fixed effects, child's gender and quadratic age as well as parental age at birth of child, gender, education, season of birth, height and number of children. The vertical lines represents 95% confidence intervals that are based on robust standard errors clustered at the parent level.
Source: Own calculations based on SHARE, waves 1, 2, 4, 5 and 6.

3.5.4 Heterogeneity Analyses

This section provides results from three heterogeneity analyses. First, I examine treatment effect heterogeneity by children's and parents' gender. Second, I estimate effects on groups of countries. Third, I evaluate treatment effect heterogeneity by parents' number of children.

Heterogeneity by Children's and Parents' Gender

As suggested by Lundborg and Majlesi (2018), one could expect differences in the effects by children's and parents' gender. For example, the gender of the child could matter if the returns to education are different for daughters and sons. Furthermore, the effects could differ by parents' gender, as children's financial resources could be more important for mothers than for fathers, if mothers are more financially dependent on their children due to lower labour force participation and lower pensions. Therefore, I follow Lundborg and Majlesi (2018) and analyse whether there is treatment effect heterogeneity between daughters and sons and between mothers and fathers. Table 3.A.4 in the Appendix presents estimates from local linear regressions of parental disability on children's years of schooling that account for the potential endogeneity of children's education, based on subsamples of daughters and mothers (Panel A), daughters and fathers (Panel B), sons and mothers (Panel C) and sons and fathers (Panel D). While the estimates within the subsample of daughter-mother dyads and son-mother dyads have the expected negative sign, the estimates within the subsample of daughter-father dyads and son-father dyads are positive. However, none of the coefficients is statistically significant at conventional levels and hence, there is no heterogeneity in the effects by the gender of the child or the parent.

Heterogeneity by European Regions

In another heterogeneity analysis, I evaluate whether the results vary across European countries. In Europe, socioeconomic differences in health can be observed, that is, people with a higher socioeconomic status are more likely to have less health problems. In addition to the socioeconomic health gradient prevailing across Europe, previous research has also suggested a north–south health gradient. In particular, it has been found that older people in Southern Europe are more likely to suffer from physical health problems, including functional limitations, and to have lower cognitive scores than their Northern European counterparts (Huisman et al. 2003, Mackenbach et al. 2005, Andersen-Ranberg et al. 2009, Ahrenfeldt et al. 2019). To shed light on potential heterogeneity by European countries, I estimate causal effects on groups of countries according to the north-south health gradient (see Table 3.A.5 in the Appendix). Unfortunately, the sample size is not large enough to carry out a more disaggregated analysis by countries instead of groups of countries to explore country heterogeneity. Since the sample does not include Northern or Eastern European countries, the sample is divided into two groups of European countries: Western Europe in Panel A, including the Netherlands, Belgium and France and Southern Europe in Panel B, comprising Greece, Italy, Portugal and Spain. The results in Table 3.A.5 in the Appendix suggest that there is no heterogeneity in the effects by groups of European countries.

Heterogeneity by Number of Children

Finally, I investigate potential treatment effect heterogeneity by parents' number of children as the effect of children's education on parental disability might be stronger for only children, i.e. children without siblings, as opposed to children with siblings. In the sample, about 77 percent of parents have more than one child[16]. To this end, I divide the sample into parents with only one child (i.e. only children) and parents with more than one child (i.e. children with siblings). Table 3.A.6 in the Appendix shows results from local linear regressions of parental disability on children's years of schooling, that address the potential endogeneity of education, based on subsamples of only children in Panel A and children with siblings in Panel B. The results indicate that the effect of children's years of schooling on parents' disability does not depend on parents' number of children because all 90 % confidence intervals cover zero.

3.6 Conclusion

The aim of this study was to add to the growing literature on intergenerational causal effects of education on health by extending the limited causal evidence on "upward" intergenerational effects from children to parents. Using data from five waves of the Survey of Health, Ageing and Retirement in Europe (SHARE), the study analyses the causal effect of adult children's education on their parents' long-term care dependency as very important health outcome with major consequences for families and societies. In order to measure long-term care dependency, a principal component analysis on a wide range of variables related to parents' physical and cognitive health is performed, and the first principal component is extracted as overall disability summary score. In a fuzzy regression discontinuity (RD) approach, compulsory schooling reforms implemented in seven European countries in the 20th century are exploited to take the potential endogeneity of children's education into account.

Descriptive OLS estimates indicate that children's years of schooling is negatively related to parental long-term care dependency, as measured by disability. One additional year of children's schooling is associated with a reduction in parents' disability score by 0.03 standard deviations, which is descriptively comparable to the decrease in the disability score

[16]The sample also includes parents who have children affected by the reforms as well as children not affected by the reforms. As pointed out by Lundborg and Majlesi (2018), the effect of an increase in children's years of schooling might be weaker for parents who have both affected and unaffected children. However, about 76 percent of parents in the sample have children who do not differ in terms of their treatment status, i.e. where all or none of the children are affected by the reforms. Moreover, weighting the regressions by parents' inverse number of children ensures that parents with both affected and unaffected children receive the same weight as other parents in the sample.

induced by about 9 months of life. However, when the potential endogeneity of education is addressed, the results suggest no causal effect of children's education on their parents' disability. Although the first stage estimates indicate that an additional year of compulsory schooling increases children's actual years of schooling by about half a year, on average, the fuzzy RD estimates are positive but not statistically different from zero, and effects smaller than -0.02 and larger than 0.03 standard deviations can be excluded at the 90 percent confidence level. The findings are fairly robust to various specifications and persist in heterogeneity analyses by gender of children and parents, by parents' number of children and by groups of countries according to the European north-south health gradient. This suggests that the negative relationship between children's years of schooling and parents' disability is likely to be confounded by unobservable factors that are correlated with both higher education of children and lower levels of parental disability such as parents' cognitive ability. Moreover, there might be reverse causality in that children of ill parents obtain less education in childhood. Therefore, the results of this study have important policy implications in that they cast doubt on the effectiveness of policies aimed at affecting education and health simultaneously, e.g. by improving population health through educational interventions, as regularly proposed by international organizations such as the WHO and OECD (WHO 2015, OECD 2010), at least when evaluated from an intergenerational perspective. In other words, educational interventions in one generation may not be effective in improving health of other generations and thus overall population health. The results imply that policymakers need to implement separate policies in each generation to improve population health, which can lead to trade-offs in deciding which generation to invest in.

The results of this study are largely consistent with those of Lundborg and Majlesi (2018), which is, to the best of my knowledge, the only peer-reviewed study in the existing literature examining causal effects of children's education on parental health in Europe. In line with my findings for Western and Southern Europe, the authors find no causal effect of children's education on parental survival overall in the Northern European country of Sweden. However, they find some heterogeneity in their results, suggesting that daughters' education positively affects fathers' longevity. Moreover, in a working paper, Potente et al. (2020) find that children's education has only very limited effects on a wide range of outcomes related to parental mortality as well as objective and subjective health in England and Wales, which is largely in line with my findings for Continental Europe. Taken together, this implies that a child's socioeconomic status might be less important for the health of parents in higher income settings.

The absence of a causal upward intergenerational effect of education on health in Europe might have three reasons. First, it could be that there is no causal effect of education on health, not within generations, which seems to be a growing consensus in the recent literature (Clark and Royer 2013, Albarrán et al. 2020, Avendano et al. 2020, Dilmaghani 2021, Malamud et al. 2021, Xue et al. 2021), nor across generations. Second, the availability and generosity of health care and long-term care programs in European countries compared to less-developed countries might mitigate the importance of children and their financial resources for parental health and longevity. In more developed settings such as Europe, financial help typically flows from parents to offspring instead of vice versa. In contrast, parents often rely on support provided by their children in less-developed settings because public support is largely absent. Third, intergenerational co-residence, which is less common in developed countries such as those in Europe, might be beneficial for parents' health as it promotes different forms of support, including financial, instrumental and informational support. Parents co-residing with their children might also feel less socially isolated or depressed, thereby improving their well-being and health in the long run. Public support and intergenerational co-residence may be two reasons why De Neve and Fink (2018) find evidence for a protective effect of additional schooling on parental longevity in low-income Tanzania, where little public welfare provision is available and intergenerational households are very common. Moreover, Ma (2019) and Cui et al. (2021) provide evidence for China, suggesting that children's education has a causal effect on parental health and longevity. Although China is facing a decline in intergenerational co-residence and has introduced several social security reforms in recent years to improve public support, there is still unequal access to health care and formal long-term care for older Chinese individuals. Therefore, upward intergenerational support from children to parents is still more important in China than in Europe, which might explain the findings for China.

The present study has some limitations that should be discussed. First, information on children is based on reports from their parents, which might increase non-response on some child characteristics and inconsistency across waves. Moreover, recall bias in old age due to cognitive decline may have affected the accuracy of the information provided. Second, SHARE includes only parents who survived at least to the age of 50 years, raising concerns about potential survivor bias. However, as only a very small proportion of people in Europe dies before the age of 50 (mostly due to accidents or cancer), I am quite confident that survivor bias is not a severe problem in this case (European Union 2022, World Bank Group 2022). Third, although the findings in this study suggest no causal effect of children's education on parental long-term care dependency, it does not mean that this is true for the overall population. As pointed out by Bertanha and Imbens (2020), the estimated effects in a fuzzy RD design are not average treatment effects across the entire population but local average treatment effects at the threshold and only for the group of compliers of the reforms in Europe who can be found in the middle to lower parts of the educational distribution. Therefore, external validity is limited and the estimated effects may not be representative for the overall population. In turn, there could be causal effects of children's education on parental long-term care dependency at other stages of the educational distribution such as college education. Therefore, future research should proceed examining causal upward intergenerational effects of education on health, exploiting exogenous variation in education at higher stages of the educational distribution. Another direction for future research is to explore treatment effect heterogeneity using machine learning techniques. Given that the overall effect of children's education on parental long-term care dependency in this study is zero, one can assume some treatment effect heterogeneity that could not be investigated in this study. One of the most popular causal machine learning techniques to explore treatment effect heterogeneity are causal forests. Compared to traditional subgroup analyses to examine treatment effect heterogeneity that only consider a small number of pre-defined subgroups, causal forests allow to explore treatment effect heterogeneity in a more flexible and data-driven manner, without making a priori restrictions and assumptions about the relevant covariates and associated cut-offs to choose (Athey and Imbens 2017, Wagner and Athey 2018, Athey et al. 2019).

From a policy perspective, "upward" intergenerational spillover effects remain an attractive area for future research. Provided that intergenerational spillover effects exist, improving the educational attainment of the child generation could be an effective policy instrument affecting not only the health outcomes of that generation but also those of the parental generation, thereby reducing potential concerns regarding intergenerational fairness of policies. Knowledge about the heterogeneity of treatment effects obtained though machine learning techniques, i.e. about how the impact of educational interventions varies across different subgroups of children and/or parents, would allow policy makers to better target those policies to maximize their impact and cost-effectiveness.

Appendix

Figure 3.A.1: Structure of the SHARE data before and after reshaping

	Parent ID/Name	Year	Child 1	Child 2	Child 3
1	Erna	2004	Anna	Benjamin	Clara
2	Dieter	2004	Anna	Benjamin	Clara
3	Erna	2006	Anna	Benjamin	Clara
4	Dieter	2006	Anna	Benjamin	Clara

	Child ID/Name	Year	Parent ID/Name
1	Anna	2004	Erna
2	Anna	2004	Dieter
3	Anna	2006	Erna
4	Anna	2006	Dieter
5	Benjamin	2004	Erna
6	Benjamin	2004	Dieter
7	Benjamin	2006	Erna
8	Benjamin	2006	Dieter
9	Clara	2004	Erna
10	Clara	2004	Dieter
11	Clara	2006	Erna
12	Clara	2006	Dieter

Notes: The figure gives an impression on how the SHARE data are structured. It shows an example of SHARE respondent Erna and her husband Dieter and their three children Anna, Benjamin and Clara, observed over two years (2004 and 2006). The upper table shows the original data in "wide" format with Erna and Dieter being the observational units. Both Erna and Dieter are observed in the years 2004 and 2006. Each of the four data rows contains, amongst others, information on their three children's gender, year of birth and education. The table below shows the structure of the data after reshaping the data to "long" format. In the table, the children Anna, Benjamin and Clara are the units of observation. In each year, each of them is observed once with mother Erna and once with father Dieter. Consequently, in each of the two years, mother Erna and father Dieter appear three times in the data. In the empirical analyses, regressions are thus weighted by the inverse of the parent's number of children to ensure that all parents in the sample receive the same weight.
Source: Own illustration.

Table 3.A.1: Kaiser-Meyer-Olkin measure of sampling adequacy and Bartlett's test of sphericity

Panel A: KMO measure of sampling adequacy (MSA)	
Overall measure of sampling adequacy	0.93
Measure of sampling adequacy for each item	
ADL: Dressing	0.96
ADL: Walking across room	0.94
ADL: Bathing or showering	0.96
ADL: Eating	0.95
ADL: Getting in or out of bed	0.94
ADL: Using the toilet	0.93
IADL: Using a map in a strange place	0.97
IADL: Preparing a hot meal	0.95
IADL: Shopping for groceries	0.94
IADL: Telephone calls	0.93
IADL: Taking medications	0.92
IADL: Doing work around the house or garden	0.96
IADL: Managing money	0.94
Mobility: Walking 100 meters	0.96
Mobility: Sitting two hours	0.93
Mobility: Getting up from a chair	0.94
Mobility: Climbing several flights of stairs	0.94
Mobility: Climbing one flight of stairs	0.95
Mobility: Stooping, kneeling, crouching	0.94
Mobility: Reaching or extending arms above shoulder	0.97
Mobility: Pulling or pushing large objects	0.94
Mobility: Lifting or carrying weights over 5 kilos	0.93
Mobility: Picking up a small coin from a table	0.97
Grip strength	0.94
Verbal fluency	0.91
Immediate memory	0.76
Delayed memory	0.74
Orientation: Day of month	0.90
Orientation: Month	0.81
Orientation: Year	0.86
Orientation: Day of the week	0.87
Panel B: Bartlett's test of sphericity	
Approx. chi square	2720.39
p value of chi square	0.00
Degrees of freedom	465

Notes: The table reports the Kaiser-Meyer-Olkin (KMO) measure of sampling adequacy for the set of variables (Overall MSA) and for the individual variables (MSA for each item) in Panel A as well as the results of the Bartlett's test of sphericity in Panel B. Both measures confirm the suitability of the data for PCA.

Source: Own calculations based on SHARE, waves 1, 2, 4, 5 and 6.

Table 3.A.2: Eigenvalues and explained variance by the principal components

Components	Eigenvalues	Variance	Cumulative variance
PC1	16.114	0.520	0.520
PC2	3.116	0.101	0.620
PC3	1.498	0.048	0.669
PC4	1.182	0.038	0.707
PC5	0.975	0.031	0.738
PC6	0.818	0.026	0.765
PC7	0.639	0.021	0.785
PC8	0.598	0.019	0.805
PC9	0.577	0.019	0.823
PC10	0.515	0.017	0.840
PC11	0.483	0.016	0.855
PC12	0.464	0.015	0.870
PC13	0.407	0.013	0.883
PC14	0.380	0.012	0.896
PC15	0.344	0.011	0.907
PC16	0.288	0.009	0.916
PC17	0.279	0.009	0.925
PC18	0.251	0.008	0.933
PC19	0.237	0.008	0.941
PC20	0.233	0.008	0.948
PC21	0.214	0.007	0.955
PC22	0.196	0.006	0.962
PC23	0.185	0.006	0.968
PC24	0.175	0.006	0.973
PC25	0.161	0.005	0.978
PC26	0.154	0.005	0.983
PC27	0.140	0.005	0.988
PC28	0.121	0.004	0.992
PC29	0.104	0.003	0.995
PC30	0.092	0.003	0.998
PC31	0.061	0.002	1.000

Notes: The table shows the eigenvalues, the percentage of variance explained by each principal component (PC) as well as the cumulative explained variance. Eigenvalues and variances are calculated using the *principal()* function from the *psych* package in R. The table shows that four principal components have eigenvalues greater than one.
Source: Own calculations based on SHARE, waves 1, 2, 4, 5 and 6.

Table 3.A.3: Loadings on the first principal component

Variables	**Loadings**
ADL: Dressing	0.794
ADL: Walking across room	0.895
ADL: Bathing or showering	0.882
ADL: Eating	0.800
ADL: Getting in or out of bed	0.826
ADL: Using the toilet	0.862
IADL: Using a map in a strange place	0.720
IADL: Preparing a hot meal	0.875
IADL: Shopping for groceries	0.903
IADL: Telephone calls	0.805
IADL: Taking medications	0.865
IADL: Doing work around the house or garden	0.844
IADL: Managing money	0.818
Mobility: Walking 100 meters	0.816
Mobility: Sitting two hours	0.553
Mobility: Getting up from a chair	0.710
Mobility: Climbing several flights of stairs	0.736
Mobility: Climbing one flight of stairs	0.774
Mobility: Stooping, kneeling, crouching	0.735
Mobility: Reaching or extending arms above shoulder	0.664
Mobility: Pulling or pushing large objects	0.788
Mobility: Lifting or carrying weights over 5 kilos	0.765
Mobility: Picking up a small coin from a table	0.664
Grip strength	-0.486
Verbal fluency	-0.415
Immediate memory	-0.460
Delayed memory	-0.397
Orientation: Day of month	-0.361
Orientation: Month	-0.493
Orientation: Year	-0.596
Orientation: Day of the week	-0.460

Notes: The table shows the loadings on the first principal component that were calculated using the function *principal()* from the *psych* package in R.
Source: Own calculations based on SHARE, waves 1, 2, 4, 5 and 6.

Table 3.A.4: Heterogeneity by children's gender and parents' gender

	(1)	(2)	(3)	(4)
Panel A: Daughter-mother dyads				
Years of schooling	0.001	−0.020	−0.020	−0.006
	[−0.045, 0.047]	[−0.074, 0.033]	[−0.073, 0.033]	[−0.060, 0.048]
	(0.028)	(0.033)	(0.032)	(0.033)
Observations	19,489	19,489	19,489	19,489
Panel B: Daughter-father dyads				
Years of schooling	0.008	0.013	0.013	0.030
	[−0.035, 0.052]	[−0.041, 0.066]	[−0.041, 0.067]	[−0.024, 0.084]
	(0.027)	(0.033)	(0.033)	(0.033)
Observations	14,961	14,961	14,961	14,961
Panel C: Son-mother dyads				
Years of schooling	−0.001	−0.013	−0.012	−0.006
	[−0.041, 0.038]	[−0.065, 0.040]	[−0.064, 0.041]	[−0.058, 0.045]
	(0.024)	(0.032)	(0.032)	(0.031)
Observations	20,209	20,209	20,209	20,209
Panel D: Son-father dyads				
Years of schooling	0.011	0.021	0.020	0.031
	[−0.030, 0.052]	[−0.039, 0.082]	[−0.041, 0.081]	[−0.029, 0.091]
	(0.025)	(0.037)	(0.037)	(0.037)
Observations	15,270	15,270	15,270	15,270
Cohort fixed effects	yes	yes	yes	yes
Country fixed effects	yes	yes	yes	yes
Country-specific linear cohort trend	no	yes	yes	yes
Interview year fixed effects	no	no	yes	yes
Child characteristics	no	no	yes	yes
Parental characteristics	no	no	no	yes

Notes: The table shows results from regressions of parents' disability score on children's years of schooling accounting for the potential endogeneity of education in a fuzzy RD design. Panel A includes all daughter-mother dyads, Panel B all daughter-father dyads, Panel C all son-mother dyads and Panel D all son-father dyads. All regressions are based on local linear models, use a bandwidth of 10 cohorts before and after the pivotal cohort of each reform and include child's year of birth and country fixed effects. In column (2), I also control for linear country-specific trends in child's birth cohort. Additional control variables included in column (3) are interview year fixed effects and child's quadratic age. In column (4), I also control for parental age at birth of child, education, season of birth, height and number of children. All regressions are weighted by the inverse of the parent's number of children. Standard errors are clustered at the parent level and presented in parentheses. 90% confidence intervals are shown in brackets. *p<0.1; **p<0.05; ***p<0.01.
Source: Own calculations based on SHARE, waves 1, 2, 4, 5 and 6.

Table 3.A.5: Heterogeneity by European regions

	(1)	(2)	(3)	(4)
Panel A: Western Europe				
Years of schooling	−0.004	0.004	0.007	0.011
	[−0.044, 0.037]	[−0.035, 0.042]	[−0.032, 0.045]	[−0.027, 0.049]
	(0.025)	(0.023)	(0.023)	(0.023)
Observations	38,402	38,402	38,402	38,402
Panel B: Southern Europe				
Years of schooling	−0.001	0.004	0.006	0.009
	[−0.065, 0.063]	[−0.078, 0.086]	[−0.077, 0.089]	[−0.070, 0.088]
	(0.039)	(0.050)	(0.050)	(0.048)
Observations	31,527	31,527	31,527	31,527
Cohort fixed effects	yes	yes	yes	yes
Country fixed effects	yes	yes	yes	yes
Country-specific linear cohort trend	no	yes	yes	yes
Interview year fixed effects	no	no	yes	yes
Child characteristics	no	no	yes	yes
Parental characteristics	no	no	no	yes

Notes: The table shows results from regressions of parents' disability score on children's years of schooling accounting for the potential endogeneity of education in a fuzzy RD design. The sample is divided into two groups of European countries: Western Europe (Netherlands, Belgium and France) in Panel A and Southern Europe (Greece, Italy, Portugal and Spain) in Panel B. All regressions are based local linear models, use a bandwidth of 10 cohorts before and after the pivotal cohort of each reform and include child's year of birth and country fixed effects. In column (2), I also control for linear country-specific trends in child's birth cohort. Additional control variables included in column (3) are interview year fixed effects, child's gender and child's quadratic age. In column (4), I also control for parental age at birth of child, gender, education, season of birth, height and number of children. All regressions are weighted by the inverse of the parent's number of children. Standard errors are clustered at the parent level and presented in parentheses. 90% confidence intervals are shown in brackets. *p<0.1; **p<0.05; ***p<0.01.
Source: Own calculations based on SHARE, waves 1, 2, 4, 5 and 6.

Table 3.A.6: Heterogeneity by the number of children

	(1)	(2)	(3)	(4)
Panel A: Only one child				
Years of schooling	−0.024	−0.054	−0.056	−0.040
	[−0.074, 0.026]	[−0.119, 0.012]	[−0.122, 0.009]	[−0.106, 0.026]
	(0.030)	(0.040)	(0.040)	(0.040)
Observations	15,780	15,780	15,780	15,780
Panel B: More than one child				
Years of schooling	0.002	−0.005	−0.003	0.019
	[−0.022, 0.025]	[−0.034, 0.025]	[−0.033, 0.026]	[−0.010, 0.048]
	(0.013)	(0.017)	(0.016)	(0.017)
Observations	54,149	54,149	54,149	54,149
Cohort fixed effects	yes	yes	yes	yes
Country fixed effects	yes	yes	yes	yes
Country-specific linear cohort tend	no	yes	yes	yes
Interview year fixed effects	no	no	yes	yes
Child characteristics	no	no	yes	yes
Parental characteristics	no	no	no	yes

Notes: The table shows results from regressions of parents' disability score on children's years of schooling accounting for the potential endogeneity of education in a fuzzy RD design. Panel A includes parents with only one child, while Panel B contains parents with more than one child. All regressions are based local linear models, use a bandwidth of 10 cohorts before and after the pivotal cohort of each reform and include child's year of birth and country fixed effects. In column (2), I also control for linear country-specific trends in child's birth cohort. Additional control variables included in column (3) are interview year fixed effects, child's gender and child's quadratic age. In column (4), I also control for parental age at birth of child, gender, education, season of birth, height and number of children. All regressions are weighted by the inverse of the parent's number of children. Standard errors are clustered at the parent level and presented in parentheses. 90% confidence intervals are shown in brackets. *p<0.1; **p<0.05; ***p<0.01.

Source: Own calculations based on SHARE, waves 1, 2, 4, 5 and 6.

Chapter 4

Adult Children's Education and Informal Care Provision

4.1 Introduction

This chapter analyses education as an important determinant of informal provision by examining whether adult children's education causally affects the provision of informal care to older parents. This question is particularly relevant given recent trends in population ageing due to declining fertility rates and longer life expectancy. The share of the population aged 65 years and over increased from 7 percent in 1960 to 17 percent in 2019, and is expected to grow to almost 27 percent in 2050 across OECD countries (OECD 2021). As a result, demand for long-term care has considerably increased in those countries and is expected to increase further. In general, care to older people can be provided informally by unpaid family members, friends, neighbours or other acquaintances or formally by paid helpers in the home of the ageing person or in nursing homes. In Europe, informal care constitutes the largest share of long-term care. To put it in numbers, about 80 percent of long-term care is provided by informal caregivers (Eurocarers 2021). In addition to spouses and partners, adult children play an important role in providing informal care to older persons. Among the adult children caregivers, daughters have traditionally been engaged in caregiving more frequently and at a higher intensity than sons. Since female education and labour force participation have increased markedly over the past few decades, the availability of informal caregivers is likely to decrease (Norton 2016, pp. 952-961). In turn, this might raise the demand for formal care in both home and institutional care and thus long-term care expenditure.

Theoretically, there are at least two plausible mechanisms that would predict a negative effect of adult children's education on the provision of informal care to older parents. First, education might increase opportunity costs as better-educated children are more likely to have a paid job and to work more hours (Van Houtven et al. 2013). Moreover, compared to their less-educated counterparts, better-educated children might move to other cities or countries due to better job opportunities (Machin et al. 2012, Weiss 2015, Malamud and Wozniak 2012), thereby raising the geographic distance to parents. In turn, this might decrease the probability to engage in informal caregiving due to forgone labour market opportunities and lost income. Second, better-educated children tend to have higher wages (Brunello et al. 2009, Grenet 2013). On the one hand, earning higher wages would allow them to finance formal care services and, on the other hand, it might decrease their dependency on inter-vivos transfers of money or bequests that are often provided by parents in exchange for support in old age (Norton et al. 2014), possibly reducing the likelihood of informal care provision to older parents. However, to the extent that education improves health, better-educated children might be healthier themselves and thus more capable of providing informal care because poor health generally limits the provision of care (Bauer and Sousa-Poza 2015). This would predict a positive effect of children's education on informal caregiving.

Previous studies report associations between children's education and care provided to parents (McGarry and Schoeni 1995, Laditka and Laditka 2001, Bonsang 2007, Silverstein et al. 2006, Bonsang 2009). However, the interpretation of this correlation as causal is difficult because it may be the result of unobserved variables, e.g. (time management) skills or empathy, which are correlated with both education and informal care provision. For instance, individuals with better time management skills tend to obtain more education, and at the same time, they might use their available time more efficiently to care for family members. Moreover, empathy has been found to play an important role in the educational process and for educational outcomes (Feshbach and Feshbach 2009) and simultaneously this attribute may increase the likelihood of helping family members. The omission of time-management skills or empathy from the econometric model is likely to result in an overestimation of the true effect[1]. Jiang and Kaushal (2020) offer the first attempt to establish causality in the relationship between children's education and caregiving by controlling for several potential confounders that might be correlated with both children's education and care provision. In sibling-fixed effects models that account for unobserved family-level characteristics that are shared among siblings, the authors compare the monetary, knowledge and instrumental support that siblings with different levels of education provide to their ageing parents. However, the authors cannot claim to have eliminated all potential confounders and thus, their findings cannot be interpreted as causal. This study adds to the existing literature by providing, to the best of my knowledge, the first causal evidence on the impact of children's education on the provision of informal care to older parents using quasi-experimental methods. In doing so, the study aims at contributing to the existing literature on intergenerational returns to education (De Neve and Kawachi 2017) and to the broader literature on informal care provision and its determinants (Broese van Groenou and De Boer 2016).

Using data from four waves of the Survey of Health, Ageing and Retirement in Europe (SHARE), I exploit compulsory schooling reforms implemented in seven European countries between 1967 and 1999 as source of exogenous variation in education in a fuzzy regression discontinuity (RD) approach. Ordinary least squares results suggest that daughters' and sons' education is negatively related to the likelihood of informal care provision to parents. The estimates indicate that one additional year of schooling is associated with a 0.3 percentage point lower probability to provide care among daughters, relative to a mean of

[1] If the overall effect of children's education on the likelihood of informal care provision is negative, the true effect will be underestimated in absolute terms.

about 12 percent, and a 0.1 percentage point lower probability to provide care among sons, relative to a mean of roughly 8 percent. When the potential endogeneity of education is taken into account in the fuzzy RD approach, the estimates reveal substantial heterogeneity by the gender of the child, providing strong evidence for a significant negative causal effect of education on the probability to provide informal care among daughters but not among sons. In particular, one additional year of daughters' schooling is found to decrease the probability of providing care to parents by about 2 percentage points, which is a substantially large effect given that 12 percent of daughters in the sample provide care to their ageing parents. The findings are robust to different model specifications, including various bandwidths, functional forms, sample selection choices and estimation methods. Further analyses provide some suggestive evidence that the effect is driven by daughters from Southern Europe providing care to parents with a high degree of long-term care dependency. Furthermore, I investigate potential mechanisms behind the results and find that better-educated daughters' reduced probability to provide care might be explained by increased opportunity costs of care due to increased female labour force participation. The geographic distance to parents and the composition of the own family in terms of being married and having own children do not seem to play an important role in explaining the results. The findings imply a future reduction in the availability of informal caregivers, which might increase formal care utilization and thus long-term care expenditure if formal and informal services are substitutes. This urges policy makers to address the challenges faced by the formal care sector resulting from the demographic change (i.e. the shortage of formal care workers), or to increase the incentives for informal care provision.

The remainder of the chapter is structured as follows. Section 4.2 reviews the existing literature on informal care provision and outlines mechanisms though which adult children's education might affect informal care provision to older parents. Section 4.3 describes the data and estimation sample used in this study. Section 4.4 outlines the empirical approach, including the institutional background and the identification strategy. Section 4.5 presents the main results as well as results from various robustness checks, heterogeneity analyses and analyses of mechanisms. Section 4.6 summarizes the main findings and concludes.

4.2 Literature Review

This section reviews the related literature, focussing on informal care patterns across European countries, the United States and China, and on the theoretical and empirical literature regarding the link between adult children's education and informal care provision to older parents.

4.2.1 Informal Care Provision Across Europe, the United States and China

Although informal care plays an important role in providing long-term care to older persons everywhere, there is a large cross-country variation. This section reviews the existing literature on informal care patterns, focussing on studies from Europe, the United States and China.

A large number of studies from Europe show that the provision of informal care follows a clear north–south gradient, both at the extensive margin and the intensive margin (Bolin et al. 2008a, Bolin et al. 2008b, Brandt et al. 2009, Hank and Jürges 2010, Brandt 2013, Verbakel 2018, Barczyk and Kredler 2019). At the extensive margin, the prevalence of informal care provision is found to be greatest in Northern European countries. Moving from the North to the South of Europe, the share of informal caregivers decreases. Central Western European countries take up an intermediate position (Brandt 2013). However, over the last ten years, Europe has experienced a decline in the proportion of informal caregivers - a trend that has been stronger in central Western European and Southern European countries than in Northern European countries (Barczyk and Kredler 2019, p. 361). Regarding the amount of informal care at the intensive margin the picture is reversed: the frequency of informal care is lowest in Northern Europe and highest in Southern Europe, while central Western European countries take up an intermediate role again (Brandt 2013). Using data from the Survey of Health, Ageing, and Retirement in Europe (SHARE), Barczyk and Kredler (2019) found that 22 percent of all care hours are provided informally in Northern European countries, while informal care accounts for 43 percent of all provided care hours in central Western European countries and 81 percent in Southern European countries.

The European north-south gradient in the provision of informal care can be explained by cultural contexts along with the availability of public formal care services. In Southern Europe, family ties are typically strong in terms of cultural patterns of family loyalties, allegiances and authority but also in terms of intergenerational co-residence between adult children and older parents, which is quite common in Southern Europe. Southern European countries are thus commonly referred to as "strong-family-ties countries" (Reher 1998). Moreover, the availability of universal public care services, including formal in-kind services at home or in a nursing home or cash benefits, is comparatively low. Barczyk and Kredler (2019) show that public spending on long-term care is less than half of the OECD average in Southern European countries such as Spain or Italy. As a result, the provision of care is perceived as responsibility of the family and concentrated on a few family members, typically adult children or spouses providing considerable amounts of time-consuming care (Brandt

2013). The prevalence of informal care provided by friends, neighbours or other acquaintances is comparatively low in Southern Europe (Kohli et al. 2009, p. 332). In contrast, in the "weak family-ties countries" of Northern Europe, care to parents is provided by several individuals since the caregiving responsibility is usually shared among siblings (crowding in). The provision of informal care is perceived as a voluntary and sporadic task and is less time-intensive (crowding out) because informal care is often complemented by formal care services in both home and institutional care (Brandt 2013). In Northern European countries, long-term care expenditure is quite high. For instance, Sweden spends almost twice as much on long-term care as the OECD average (Barczyk and Kredler 2019, p. 349).

In the United States, informal care is also the main source of care for older people. With respect to the frequency of informal care provision, the United States lie between central Western European and Southern European countries. Using data from the Health and Retirement Study (HRS), Barczyk and Kredler (2019) report that 54 percent of long-term care in the United States is provided by informal carers. Moreover, as opposed to Europe, the United States experienced a slight increase in informal care provision over the last ten years. The large amount of informal care in the United States is likely to be the result of the low public spending on long-term care. With only one-third of the OECD average, public spending on long-term care in the United States is found to be even lower than in Southern Europe. Moreover, while the access to public long-term care services is universal in most countries, it is means-tested and granted only if income is sufficiently low and assets are substantially exhausted in the United States, which makes informal care more attractive (Barczyk and Kredler 2019).

In China, informal care provided by family members in the home of older people is still the predominant source of long-term care. In particular, adult children have traditionally been obliged to provide informal care to their parents due to the Confucian norm of filial piety, which is deeply rooted in Chinese culture. In contrast to Western societies, co-residence of adult children and older parents has been very common in China. However, demographic shifts due to rapid population ageing and socioeconomic changes resulting from the Chinese one-child policy and increasing female labour force participation threaten the sustainability of this traditional family-based support system. To address the increasing long-term care needs of the population, the Chinese government has therefore issued several policies in recent years to establish a long-term care system that offers a comprehensive range of formal care services in both rural and urban areas. Hence, public spending on long-term care for Chinese older adults is increasing in recent years. However, the development of the formal care sector in China is still in early stages due to a lack of professional caregivers and care facilities and hence, the utilisation of formal care is still low in China (Cui et al. 2021, p. 4, Li and Otani 2018).

4.2.2 Education and Informal Care Provision

Theoretical Mechanisms

From a theoretical perspective, children's education could affect the likelihood to provide care to older parents at least through three potential channels: (1) increased opportunity costs due to labour force participation, geographic distance and family composition, (2) higher wages and (3) better health.

The first pathway relates to the opportunity costs that an informal caregiver may perceive, resulting from having a paid job, from living at a great geographic distance to the parental home, and from having competing responsibilities to take care of older parents and the own family. First, better-educated children are likely to be in the labour force more often and to work more hours, on average. As a consequence, providing time-intensive care to parents raises opportunity costs due to foregone labour-market opportunities and lost income through reduced work hours (Carmichael and Charles 2003, Heitmueller and Inglis 2007, Bittman et al. 2007, Van Houtven et al. 2013). Furthermore, providing informal care might increase the use of sick leaves because it can negatively affect the caregiver's health through the increased emotional stress and physical strain involved (Coe and Van Houtven 2009, Schmitz and Westphal 2015, Do et al. 2015). Higher opportunity costs may, in turn, reduce the probability to engage in informal caregiving, although better-educated children may have more flexible jobs or may allocate the available time more efficiently as education raises their non-market productivity (Grossman 2006). The distinction between daughters and sons is important here because daughters have traditionally been more frequently and intensively involved in caregiving than sons. This has usually been explained by different employment patterns between men and women since men have been in the labour force more often and have worked more hours than women, on average (Haberkern et al. 2015, p. 299, Norton 2016, pp. 960-961). As female labour force participation has increased over time, the effect of education on opportunity costs as important mediator may be larger for daughters than for sons.

It has also been suggested in the literature that better-educated children are likely to move to other cities or regions within their country (Machin et al. 2012, Weiss 2015, Haapanen and Böckerman 2017) or to other countries (Malamud and Wozniak 2012). The reasons for moving are better job opportunities because the job market for skilled workers operates on a national or even international basis, while the job market for unskilled workers is more localized so that jobs requiring higher levels of education are often more geographically dispersed. Moreover, better-educated children could be more selective about the jobs they take, resulting in a job search over a wider geographic area (Ermisch and Mulder 2019, p. 591). In turn, an increased geographic distance to the parental home is likely to raise opportunity costs, thereby decreasing the probability that adult children provide care to their ageing parents.

Moreover, education might affect the composition of the own family in terms of being married and having children because individuals who invest more in education might have fewer children and might marry later in life (Cygan-Rehm and Maeder 2013, Liang and Yu 2022). One the one hand, middle-aged married persons with children belong to the so-called "sandwich generation", which means that they have competing caregiving responsibilities for their own children and their ageing parents (Miller 1981). As women increasingly have their children at a later age, this increases the likelihood that there are young children in the household by the time ageing parents need care. In addition, being married is associated with having more relatives, e.g. parents-in-laws, who are potentially in need of care. Due to the time constraints that arise from the competing caregiving responsibilities, married persons with children might be less likely to provide informal care to parents. On the other hand, spouses can support the caregiver by assuming household chores or child care, which could reduce the double burden of the "sandwich generation". In addition, from a financial perspective, spouses typically contribute to the household income, which allows to purchase care from the formal sector.

Another potential pathway is related to financial resources as better-educated children tend to earn higher wages (Brunello et al. 2009, Devereux and Hart 2010, Grenet 2013). To the extent that education affects wages, better-educated children might be better able to pay for formal care services, including institutionalized care or home care, which could lead to a reduction in the provision of informal care (Carmichael et al. 2010, De Koker 2009). In addition, higher earnings might reduce children's dependency on money transfers from parents. Previous studies have shown that children who provide informal care to their parents in old age receive larger inter-vivos cash transfers or bequests than their siblings who do not so (Norton and Van Houtven 2006, Norton et al. 2014, Groneck 2017). Put differently, parents often make financial gifts or future bequests conditional on children's supply of informal care. In the literature, this is referred to as "exchange motive" (Cox 1987, Cox and Rank 1992) or "strategic bequest motive" in the case of bequests (Bernheim et al. 1986). In turn, the promise of larger transfers of money or larger bequests might motivate children to provide informal care to parents. However, better-educated children are likely to be less dependent on financial transfers by parents, thereby reducing the likelihood of providing informal care to parents.

A third pathway relates to the physical and mental health capacities of adult children. Previous research shows that poor physical or mental health generally limits the provision of informal care (Bauer and Sousa-Poza 2015, p. 134). To the extent that education raises the efficiency in health production or improves the choice of health inputs (Grossman 1972), better-education children might be healthier themselves. As a result, they might be more capable of providing informal care to parents than their less-educated counterparts.

To summarize, the direction of the overall effect of adult children's education on informal care provision to older parents is a priori unclear because the above-mentioned channels work in both directions.

Empirical Evidence

To the best of my knowledge, the literature to date lacks causal evidence on whether adult children's education affects the provision of informal care to older parents. There is only some correlational evidence on significant associations between education and informal caregiving. This literature is, however, very scarce and has produced mixed results. Some studies do not analyse the association between adult children's education and care provided to their parents, but examine correlations between caregivers' education and care provision in general. One part of these studies finds positive correlations between educational attainment and the probability of being a caregiver (Verbakel 2018, Baji et al. 2019, De Klerk et al. 2021), while the other part reports negative correlations (McGee et al. 2008, Tokunaga and Hashimoto 2017, Angst et al. 2019). Other studies explicitly analyse the intergenerational association between adult children's education and care provided to older parents. For example, McGarry and Schoeni (1995) analyse intergenerational transfers within families using data from the Health and Retirement Study (HRS) and find that children with higher levels of education are more likely to provide help with regard to basic personal needs like dressing, eating and bathing. A positive correlation between offspring's education and the probability to provide time assistance to parents has also been found by Bonsang (2007) when analysing the determinants of financial and time transfers from adult children to their older parents using data from the Survey of Health, Ageing and Retirement in Europe (SHARE). In contrast, Couch et al. (1999) find no support for a significant correlation between children's education and time transfers to parents using data from the 1988 wave of the Panel Study of Income Dynamics (PSID). This is consistent with findings from Indonesia (Frankenberg et al. 2002). In another study, Laditka and Laditka (2001) use the 1993 wave of the PSID to examine help provided to older parents in the United States. The authors find that adult children's education is positively correlated with the likelihood to provide care but negatively associated with helping hours. Silverstein et al. (2006) analyse the determinants of adult children's support to their ageing parents, including help with personal care, instrumental household help, financial help and emotional support. Using data from the Longitudinal Study of Generations (LSOG), their findings suggest that having more education is positively associated with the provision of support to fathers but not to mothers. Finally, Bonsang (2009) uses SHARE data to examine the effect of informal care provision by adult children on the use of formal home care among older people in Europe, taking into account the potential endogeneity in the relationship between formal and informal care by using child characteristics as instruments. Their results suggest that informal care provision is higher among children with a low level of education.

In the aforementioned studies, education is not the main explanatory variable but only used as a control variable in the regression models and thus, these studies are not able to address the endogeneity of education. Jiang and Kaushal (2020) offer the first attempt to establish causality in the relationship between children's education and the care provided to parents. The authors examine whether education affects the financial support, knowledge support (i.e. help with financial management, future planning and health care costs) and instrumental support (i.e. help with household tasks and personal care) provided to older parents in the last years of their lives. In sibling fixed-effects models that account for unobserved family-level characteristics, the authors compare the care that siblings with different levels of education provide to their parents. As opposed to the above-mentioned studies, Jiang and Kaushal (2020) use education as main explanatory variable in the regressions models. Moreover, they include several control variables that might be correlated with both education and caregiving such as parents' self-reported health, functional limitations, formal and informal care service utilisation in the year prior to death and circumstances of death. Using 10 waves of data from the HRS from 1994 to 2012, the authors find that having at least a college degree has a significantly positive association with knowledge support and monetary support. The relationship between eduction and instrumental support is found to be non-linear in that having some college education is positively associated with instrumental support, while having a college degree is not significantly related to instrumental support. Moreover, the authors find no differences in the probability of providing financial support, knowledge support or instrumental support between daughters and sons. However, Jiang and Kaushal (2020) cannot claim to have eliminated all potential confounders and thus the results cannot be interpreted as causal.

4.3 Data

In order to investigate the causal effect of adult children's education on informal care provision to older parents, this chapter draws on the same data as in Chapter 3. More specifically, the study uses data from waves 1, 2, 5 and 6 of the Survey of Health, Ageing and Retirement in Europe (SHARE)[2]. Wave 4 had to be excluded from the analyses because information on informal care utilization cannot be assigned to a particular child in this wave. In the study, I focus on SHARE respondents as parents of adult children affected by the compulsory schooling reforms in Europe and reshape the data set so that the adult children are the unit of analysis. Since adult children are not the respondents in SHARE, they do not have a unique identifier and thus I match them across waves based on gender and year of birth. Information on children is provided by the SHARE respondents (i.e. the parents) who can mention up to 20 children living inside or outside their household, including natural, fostered and adopted children and stepchildren and those of a spouse or partner. This information includes only some basic demographic data on gender, year of birth, educational attainment, marital status, occupational status, geographic proximity to parents and the number of children (i.e. the respondents' number of grandchildren)[3].

Measuring Parents' Care Utilisation and Children's Provision of Informal Care

Parents' long-term care utilisation is captured by a categorical variable with the categories "no care", "informal care only", "formal care only", and "formal and informal care". The variable is constructed based on questions whether respondents received either informal care from persons living inside or outside the household, formal home care or nursing home care during the last 12 months prior to the interview. The different types of care to measure parents' long-term care utilisation are summarised in Table 4.A.1 in the Appendix. Informal care includes the help that respondents received in the year before the interview from persons inside and outside the household. Informal care from outside the household comprises personal care, practical household help or help with paperwork provided by family, friends, neighbours or relatives residing outside the respondents' household[4]. Informal care from inside the household includes personal care provided by someone within the respondents'

[2] For methodological details on SHARE, see Börsch-Supan et al. (2013).

[3] In waves 1 and 2, basic information on children's gender, year of birth and geographic proximity is collected for all living children, while the remaining information is only asked for up to four children. Since wave 4, detailed information is collected for all living children.

[4] Up to wave 5, SHARE asks about help that all members of the household received together from persons residing outside the household. Put differently, it is not explicitly mentioned who received care within the household. In this case, the care received is attributed to both partners.

household regularly in the year before the interview. Respondents could mention up to three informal caregivers from inside and outside their household, including their children. Formal home care is defined as receiving either professional or paid nursing or personal care, professional or paid home help for domestic tasks, or meals-on-wheels. Nursing home care comprises permanent or temporary admissions to nursing home during the last 12 months prior to the interview.

The outcome variable in this study is a dummy variable that equals one if a child has provided informal care to his or her parent in the past year and zero otherwise. The variable is constructed using information on informal care that respondents received during the 12 months prior to the interview, combined with information and the caregiver identity. In SHARE, information on the intensity of care (i.e. hours of care) is available but very limited as it is only asked in waves 1 and 2. Therefore, due to small sample size issues, the focus in the analyses is on the decision to be or not to be a caregiver, i.e. the extensive margin of informal care provision, without making statements on the amount of care provided by children (intensive margin).

Measuring Children's Education

Children's education is measured in years of completed schooling. Since SHARE contains information on a child's school degree only, I combine information on the number of years usually taken to obtain a certain school degree with information on a child's year of birth and the pivotal cohort of the compulsory schooling reform in the respective country of origin to measure years of schooling. Country-specific conversion tables provided by SHARE (2011) and the International Standard Classification of Education 1997 (OECD 1999) were used to recode school-leaving degrees into years of schooling. Table 4.A.2 in the Appendix summarizes the country-specific school-leaving degrees as in the SHARE questionnaire, the number of school years to obtain a certain school degree and the number of school years that were assigned to individuals who were affected and not affected by the reforms.

Control Variables

Three different sets of control variables that might confound the effect of education on caregiving are included in the analyses. In particular, I distinguish between child characteristics, parental characteristics and additional controls. Child characteristics include gender and year of birth dummy variables to account for gender differences and cohort-level differences in caregiving patterns. Moreover, I control for the number of siblings since the existence of siblings is likely to be related to caregiving decisions (Tolkacheva et al. 2010) and educational attainment (Black et al. 2005, Black et al. 2010). Child's squared age is also included to capture potential non-linear age effects. Parental characteristics include gender, age, level of education according to ISCED 1997 (primary or lower secondary education, upper secondary education, tertiary education) and the number of limitations in activities of daily living (ADL) and instrumental activities of daily living (IADL)[5]. ADL and IADL limitations are included in the regression model as a set of dummy variables because they are likely to be associated with a higher caregiving intensity and thus expected to affect the likelihood of informal care provision by adult children due to higher opportunity costs that high-intensity caregivers face[6]. Furthermore, I include country dummies and country-specific linear cohort trends (i.e. interactions of country dummies with a linear trend in year of birth) to account for time-invariant differences in caregiving patterns between countries and linear trends in caregiving patterns and schooling at the country level. To capture time trends, survey year dummy variables are included in the analyses.

Mechanisms

The selection of variables to analyse potential mechanisms that might explain the link between child's education and informal care provision is restricted by the data as child information is rather limited. For instance, the data lack information on wages or health. However, SHARE contains geographic distance to parents, employment status, marital status and parenthood status, which allows me to investigate whether the opportunity cost mechanism plays a role in explaining the results. Regarding geographic distance, the questionnaire asks whether a child lives "in the same household", "in the same building", "less than 1 kilometre away", "between 1 and 5 kilometres away", 'between 5 and 25 kilometres away", "between 25 and 100 kilometres away","between 100 and 500 kilometres away", "more than 500 kilometres away" and "more than 500 kilometres away in another country". In the analyses, I use a dummy variable equal one if a child lives more than 100 kilometres away from the parental home, and zero otherwise. To measure employment status, I use the original 9-category

[5]ADL limitations describe problems with six common everyday tasks required for self-care, including dressing, walking across a room, bathing or showering, eating, getting in and out of bed and using the toilet (Katz et al. 1963). IADL limitations comprise seven more complex problems with competences that are necessary for living independently in a community such as using a map, preparing meals, shopping for groceries, making telephone calls, taking medication, doing work around the house or garden and managing money (Lawton and Brody 1969).

[6]Parents' limitations are measured after the treatment, that is after children's exposure to the compulsory schooling reforms. However, including the variable as a control variable will not introduce a bias in the treatment effect because it is not caused by the treatment. In this context, the study in Chapter 3 showed that children's exposure to the compulsory schooling reforms does not affect parents' disability, measured by a wide range of physical and cognitive health variables including ADL and IADL limitations. Hence, parents' limitations are not "bad controls" but rather "neutral controls" that can increase the precision of the treatment effect (Cinelli et al. 2022, pp. 7-8).

variable on a child's occupational status included in SHARE to generate a dummy variable on whether or not an adult child is full-time employed[7]. In SHARE, the marital status of an adult child is classified into the categories "married and living together with spouse", "registered partnership", "married and living separated from spouse", "never married", "divorced" and "widowed". For the analyses, I generate a dummy variable which takes the value of one if the child is married or in a registered partnership, and zero otherwise. Adult children's parenthood status is measured by a continuous variable for the number of children, including natural, fostered, adopted and stepchildren and those of a spouse or partner.

Sample Restrictions

The sample used in this chapter is very similar to the one used in Chapter 3 of this thesis. Figure 4.A.1 in the Appendix gives an overview on how the two samples differ. In line with previous studies on informal care utilisation in Europe, e.g. Bonsang (2009), Gannon and Davin (2010) and Geerts and Van den Bosch (2012), the sample in this chapter is restricted to parents aged 65 or older as few persons below that age are in need of care[8]. Moreover, following McGarry and Schoeni (1995), Bonsang (2007) and Schmid et al. (2012), I do not consider children co-residing with their parents. Put differently, child-parent dyads are excluded from the sample if the child lives in the parental household. Although co-resident adult children actually play an important role in meeting the care needs of older parents, there are two reasons why they are excluded from the analyses[9]. First, comparisons of co-residing and non-co-residing children are subject to bias due to the potential endogeneity of living arrangements between parents and children as co-residence and human capital investment decisions are likely to be made jointly. For example, better-educated adult children may be less likely to co-reside with their parents because education is likely to affect labour mobility (Machin et al. 2012). In addition, adult children's decision to co-reside

[7]The original variable has the nine categories "full-time employed", "part-time employed", "self-employed or working for own family business", "unemployed", "in vocational training/retraining/education", "parental leave", "in retirement or early retirement", "permanently sick or disabled" and "looking after home or family".

[8]Using a minimum age of 65 years for sample selection does not ensure to have excluded all parents who are actually not in need for care or who do not receive care. Moreover, the sample includes not only parents with so-called "met needs" but also parents with "unmet needs" for care, i.e. parents who have a need for care but receive neither formal nor informal care. Therefore, I perform three robustness checks in Section 4.5.2. First, only parents with at least one ADL or IADL limitation are included. Second, the sample is restricted to parents who report to have received either formal care, including home care and nursing home care, or informal care or a combination of both. Third, parents with an "unmet need" for care are excluded so that the sample includes only parents who report to have at least one ADL or IADL limitation and to have received formal care, informal care or a combination. In Section 4.5.2, I show that these robustness checks yield comparable estimates.

[9]However, in a sensitivity analysis in Section 4.5.2, I assess whether the results are robust to the inclusion of co-residing children and find that the results are very similar.

with their parents might depend on parents' long-term care dependency since children might decide to move to their parents in case of parents' need of care. Second, co-resident children might not be directly comparable to children living outside the parental household because they are likely to benefit from shared accommodation and food or other in-kind or financial transfers, which could affect their decision to provide care. In addition, the probability of providing care in case of parents' need of care is likely to be higher for co-residing children as any child that lives in the parental household is probably involved in caregiving in some way. Hence, the decision to provide care to parents might not be as free as for children who do not live in the parental household[10]. In line with Chapter 3 of this thesis, I only include children aged 18 or older because children below that age would not have had the opportunity to complete school, and exclude parents who were not born in the country of interview or who immigrated there after the age of five to ensure that children are likely to have received their education in the country of interview[11]. Moreover, I restrict the sample to children born 10 years before or after the first affected cohorts of the compulsory schooling reforms.

Descriptive Statistics

The final sample comprises 37,846 observations of child-parents dyads. Table 4.1 shows descriptive statistics for the variables used in the empirical analyses. The table reports mean values of all variables and standard deviations of continuous variables in parentheses for the full sample in column (1), and by children's reform exposure status in columns (2) and (3). The sample consists of half daughters (i.e. daughter-parent dyads) and half sons (i.e. son-parent dyads). On average, children in the sample are about 47 years old, have 10 years of schooling and two siblings. Roughly 64 percent of adult children are full-time employed, about 16 percent live at least 100 kilometres away from their parents' home and about 78 percent are married or live in a registered partnership. On average, adult children have two children themselves. Moreover, about 8 percent of adult children provide informal care to their parents. Moreover, the sample consists of slightly more fathers (i.e. child-father dyads) than mothers (i.e. child-mother dyads). The average age of parents in the sample is about 75 years. On average, about 69 percent of parents have primary or lower secondary education degrees (ISCED 0-2), 16 percent have upper secondary education degrees (ISCED 3-4) and 15 percent have tertiary education degrees (ISCED 5-6). At least one ADL or

[10]In the sample, co-residing children are about 40 percent more likely to provide care to older parents: 8.2 percent of non-co-resident children provide care, compared to 12.2 percent of co-residing children.

[11]Although it is not so important for the analyses for parents to have received their education in the country of interview, because I use ISCED categories to capture parental education, a maximum immigration age of five years is used to make the samples in Chapter 3 and Chapter 4 comparable to each other. As shown in Figure 4.A.1 in the Appendix, the two samples differ only with respect to parents' minimum age and the exclusion of co-residing children (and the inevitable exclusion of wave 4 in Chapter 4).

IADL limitation is reported by about 31 percent of parents. Roughly 18 percent of parents report to receive only informal care by family members, friends or neighbours, while about 10 percent of parents receive formal care at home or in a nursing home. A combination of informal and formal care use is reported by about 10 percent of parents in the sample. The remaining 63 percent of parents in the sample do not receive any type of care. As shown in column (3) of Table 4.1, parents of children affected by the compulsory schooling reforms in Europe are less likely to receive any type of care and to have at least one ADL or IADL limitation, which is plausible since they are younger, on average. Moreover, adult children affected by the changes in compulsory schooling are less likely to care for their parents in need.

Table 4.1: Summary statistics

	Full sample (1)	**Pre-reform** (2)	**Post-reform** (3)
Children			
Son (%)	49.94	50.08	49.84
Age	46.77 (7.12)	48.60 (7.92)	45.51 (6.20)
Years of schooling	10.19 (2.01)	9.29 (2.28)	10.81 (1.51)
Number of siblings	2.11 (1.53)	2.14 (1.57)	2.10 (1.50)
Full-time employed (%)	63.86	59.84	66.64
Geographic distance to parents ≥ 100 km (%)	16.05	15.47	16.46
Married or partnered (%)	78.36	77.83	78.72
Informal care provision (%)	8.09	9.65	7.02
Parents			
Father (%)	43.53	41.97	44.61
Age	75.34 (6.77)	76.67 (7.21)	74.43 (6.28)
Education (%)			
Primary or lower secondary education (ISCED 0-2)	69.07	70.09	68.36
Upper secondary education (ISCED 3-4)	16.31	16.06	16.49
Tertiary education (ISCED 5-6)	14.62	13.85	15.15
1+ ADL or IADL limitation (%)	31.45	35.29	28.81
Care utilisation (%)			
Informal care only	17.77	18.61	17.19
Formal care only	9.51	11.05	8.45
Formal and informal care	9.94	12.65	8.06
No care	62.78	57.69	66.30
Observations	37,846	15,470	22,376

Notes: The table reports mean values and standard deviations (for continuous variables) in parentheses for the full sample (column (1)) and by children's reform exposure status (columns (2) and (3)).
Source: Own calculations based on SHARE, waves 1, 2, 5 and 6.

Table 4.A.3 in the Appendix shows that the sample includes not only parents with so-called "met needs" but also parents with "unmet needs" for long-term care. "Met needs" is defined as having a need for long-term care (here: having at least one ADL or IADL limitation), and receiving either formal care, informal care or a combination of both. "Unmet needs" means having a need for long-term care but receiving neither formal nor informal care. As shown in Table 4.A.3, about 21 percent of parents in the sample have a "met need", while about 11 percent have an "unmet need". Moreover, the sample includes parents reporting neither ADL or IADL limitations nor any type of long-term care utilisation (52 percent), and parents reporting no ADL or IADL limitation but some long-term care utilisation (17 percent).

4.4 Empirical Strategy

4.4.1 Institutional Background

The empirical strategy to identify the causal effect of adult children's education on the provision of informal care to their older parents is the same as in Chapter 3 of this thesis. It builds on an established literature, which uses compulsory schooling reforms as a source of exogenous variation in education to estimate returns to education (Galama et al. 2018, Hamad et al. 2018). In particular, the study follows Brunello et al. (2009) who were the first to apply this strategy also to a multi-country setup, drawing on compulsory schooling reforms in several European countries. This multi-country strategy was also adopted in various subsequent studies, e.g. Brunello et al. (2013), Schneeweis et al. (2014), Mazzonna (2014), Crespo et al. (2014), Gathmann et al. (2015), Weiss (2015), Brunello et al. (2016), Fort et al. (2016), Brunello et al. (2017), Aparicio Fenoll and Kuehn (2017), Kunst et al. (2020) and Hofmarcher (2021).

In particular, the study exploits exogenous variation in years of schooling induced by compulsory schooling reforms that were implemented between 1967 and 1999 in the seven European countries Belgium, France, Greece, Italy, Netherlands, Portugal and Spain. Table 3.2 in Chapter 3 of this thesis lists the compulsory schooling reforms used for the analyses, including the year of the reform, the first birth cohort affected by the reform and the change in years of compulsory schooling. The reforms vary in terms of the length of extended compulsory schooling. Compulsory schooling was increased by one year in Italy and the Netherlands, by two years in France and Spain, by three years in Greece and Portugal and by four years in Belgium.

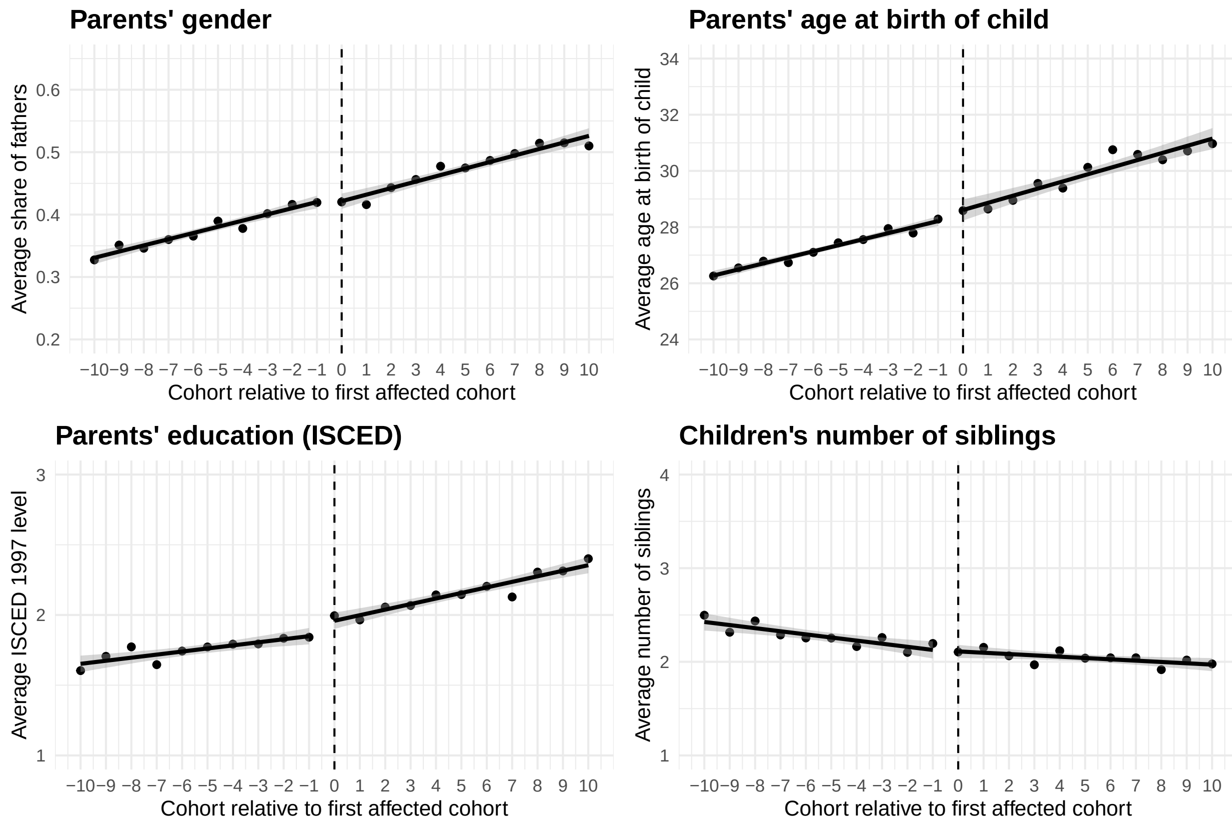
4.4.2 Identification Strategy

To address the problem that children's education is likely to be endogenous with respect to the provision of informal care, this study uses a quasi-experimental fuzzy regression discontinuity (RD) design. An RD design is based on the idea that the probability of receiving the treatment (i.e. additional years of compulsory schooling) is a discontinuous function of a continuous assignment variable (i.e. child's year of birth). This allows to estimate the causal effect of the treatment by comparing the outcome (i.e. provision of informal care by adult children) for birth cohorts just below and just above the cut-off as these individuals are expected to be comparable in their pre-treatment characteristics. The key identifying assumption of a valid RD design is the continuity of observable or unobservable pre-treatment characteristics at the discontinuity threshold. This assumption cannot be tested, but it is common practice to provide evidence on its plausibility by showing that no observable pre-treatment covariates discontinuously change around the cut-off (Lee and Lemieux 2010, pp. 283-296). Figure 4.1 plots the distribution of four pre-treatment characteristics, i.e. parents' gender, parents' age at birth of child, parents' education and children' number of siblings. The figure shows that the pre-treatment covariates do not jump discontinuously at the threshold[12]. Moreover, I check for a potential manipulation of the assignment variable by plotting a histogram of child's year of birth and find no jump in the number of observations at the cut-off point and thus no evidence for manipulation of the assignment variable (see Figure 4.A.3 in the Appendix).

A fuzzy RD design is used because, unlike in a sharp RD design, the treatment variable is not a deterministic but a probabilistic function of the assignment variable. This means, the probability of assignment to treatment does not jump from zero to one at the cut-off because the treatment is not purely assigned on basis of child's year of birth. Consequently, there are both treated and untreated observations on both sides of the cut-off. The reason is that already before the introduction of the reforms individuals went to school longer. This applies in particular to students on higher school tracks who are not affected by the reforms. In a fuzzy RD design, the discontinuity in the probability of treatment is used as instrument for the treatment status, and therefore the treatment effect can be estimated as in an instrumental variable approach by dividing the reduced form coefficient by the first stage coefficient (Angrist and Pischke 2009, pp. 259-267, Cunningham 2021, pp. 279-282).

[12]In Figure 4.A.2 in the Appendix, I also plot the distribution of the four pre-treatment covariates separately by country. While the figures for Italy and Portugal are quite noisy due to the relatively small number of observations per country and cohort, especially to the right of the cut-off, the results are generally consistent with the aggregated analysis illustrated in Figure 4.1.

Figure 4.1: Distribution of pre-treatment covariates



Notes: The figure shows the distribution of four pre-treatment covariates, i.e. parents' gender (proportion of fathers), parents' age at birth of child, parents' education (ISCED 1997 level) and children' number of siblings, for cohorts of children born 10 years before and after the pivotal cohorts of the reforms. Each point represents a weighted mean (weighted by the number of observations per country). All countries are normalized by the time of the reforms, which is set at zero.
Source: Own calculations based on SHARE, waves 1, 2, 5 and 6.

Hence, I estimate the following two equations for child i, observed in year t and born in country j in year c:

$$Education_{itjc} = \alpha_0 + \alpha_1 Reform_{itjc} + f(Yob_{itjc}) + \gamma_j + \delta_c + \psi_{jc} + \tau_t + \alpha_2 X_{itjc} + \epsilon_{itjc} \quad (4.1)$$

$$Care_{itjc} = \beta_0 + \beta_1 Reform_{itjc} + f(Yob_{itjc}) + \eta_j + \theta_c + \omega_{jc} + \rho_t + \beta_2 X_{itjc} + \pi_{itjc} \quad (4.2)$$

Equation (4.1) is the first stage model to estimate the effect of adult children's compulsory years of schooling on years of schooling. The reduced form model to estimate the effect of children's compulsory years of schooling on the likelihood of providing informal care to older parents is given by equation (4.2). $Education_{itjc}$ in equation (4.1) is the number of actual years of schooling, while $Care_{itjc}$ in equation (4.2) is a dummy variable equal to one if a child provides care to his or her parent. $Reform_{itjc}$ in both equations denotes the number of compulsory schooling years induced by the compulsory schooling reforms. Yob_{itjc} measures child's birth cohort relative to the first birth cohort affected by a reform. γ_c and η_c and δ_j and θ_j are birth cohort fixed effects and country fixed effects to control for differences between birth cohorts and countries. Survey year fixed effects are given by τ_t and ρ_t. ψ_{jc} and ω_{jc} are country-specific linear trends in birth cohort. X_{itjc} includes several covariates, among them children's number of siblings and squared age as well as parents' age at birth of child, gender, level of education and number of ADL and IDL limitations. ϵ_{itjc} and π_{itjc} are error terms that capture all unobserved factors which may affect education or informal care provision. Both equations are estimated separately for daughters and sons. The coefficients of interest are α_1 and β_1. The function $f(\cdot)$ captures the relationship between the assignment variable and the outcome. In the main specification, I follow suggestions by Gelman and Imbens (2019) and employ a non-parametric method that models the relationship by local linear regressions, that is, linear regressions separately for children below and above the threshold. As part of the robustness checks, I also estimate local quadratic regressions and parametric global regressions with first-order and second-order polynomials in Yob_{itjc}. In the main analysis, I choose a bandwidth of 10 cohorts before and after the first affected cohorts, which is rather arbitrary but ensures to have a large enough window and sample size to precisely estimate the models and to exclude other reforms that might have affected the length of schooling. Robustness checks with smaller bandwidths are performed in Section 4.5.2. Following Jiang and Kaushal (2020), standard errors are clustered at the parent level to account for multiple children of the same parent.

4.5 Results

This section presents regression results for the effect of adult children's years of schooling on the probability to provide care to older parents. In Section 4.5.1, results from OLS, first stage, reduced form and fuzzy RD models are reported, followed by results from various robustness checks in Section 4.5.2. Finally, I investigate potential treatment effect heterogeneity in Section 4.5.3 and underlying mechanisms in Section 4.5.4.

4.5.1 Main Results

Table 4.2 reports results for the effect of children's years of schooling on the probability to provide care to parents. First, I discuss the OLS results presented in Panel A, which are based on simple linear regressions of the probability to provide care on years of schooling without accounting for the potential endogeneity of education. As shown in column (1), one additional year of daughters' schooling is significantly associated with a 0.3 percentage points lower probability of providing informal care to parents. Compared to the pre-reform mean of informal care provision among daughters of 11.5 percent, this corresponds to a decrease by about 2.6 percent.

Table 4.2: Effect of children's years of schooling on informal care provision

Dependent Variable: Provision of informal care to parents (1=Yes, 0=No)			
	Daughters (1)	Sons (2)	Difference (3)
Panel A: OLS results			
Years of schooling	−0.003*	−0.001	0.002
	(0.002)	(0.001)	(0.002)
Control mean	0.115	0.078	
Observations	18,945	18,901	37,846
Panel B: First stage results			
Compulsory schooling	0.544***	0.424***	−0.119***
	(0.026)	(0.025)	(0.037)
First stage F-statistic	431.89	284.04	
Observations	18,945	18,901	37,846
Panel C: Reduced form results			
Years of schooling	−0.012***	−0.001	0.011*
	(0.004)	(0.004)	(0.006)
Control mean	0.115	0.078	
Observations	18,945	18,901	37,846
Panel D: Fuzzy RD results			
Years of schooling	−0.022***	−0.003	0.019
	(0.008)	(0.009)	(0.012)
Control mean	0.115	0.078	
Observations	18,945	18,901	37,846

Notes: The table shows results for the effect of children's years of schooling on the probability to provide care to parents, separately for daughters in column (1) and for sons in column (2). Column (3) reports differences between sons and daughters. All regressions are based on local linear models, use a bandwidth of ±10 cohorts and include cohort fixed effects, country fixed effects, linear country-specific cohort trends, interview year fixed effects, children's quadratic age, children's number of siblings, parents' age at birth of child, parents' gender, parents' education and parents' number of ADL and IADL limitations. Standard errors are clustered at the parent level and presented in parentheses. *p<0.1; **p<0.05; ***p<0.01.
Source: Own calculations based on SHARE, waves 1, 2, 5 and 6.

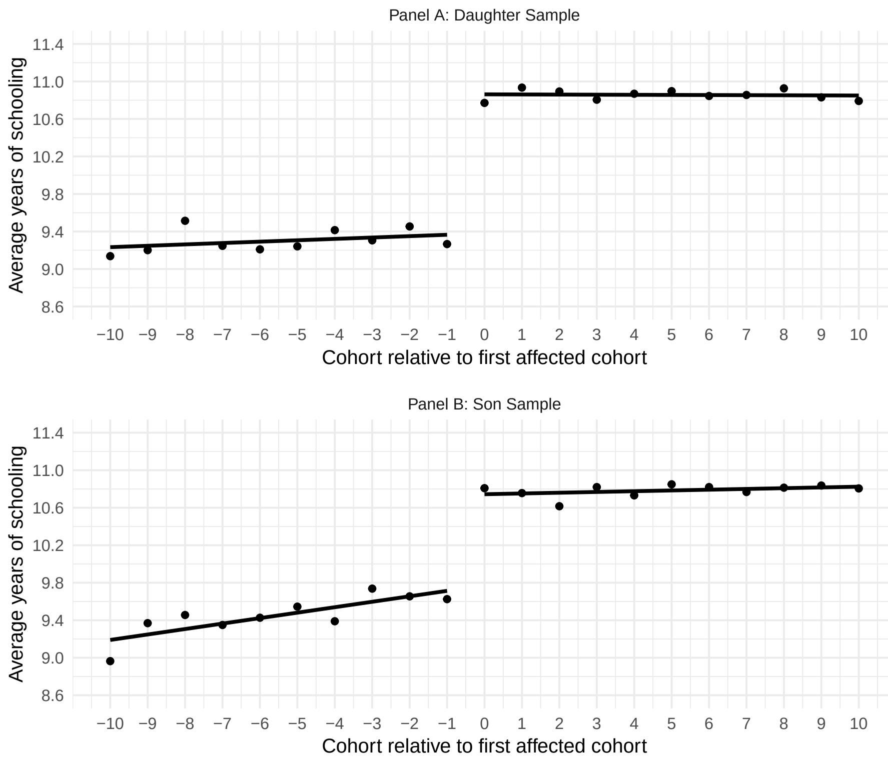
Sons' years of schooling is associated with a -0.1 percentage points lower probability of providing informal care to parents, as shown in column (2), which equals a decrease by about 1.3 percent when evaluated at the control group mean of 7.8 percent in the subsample of sons. The relationship is significant at the 10 percent level for daughters[13] in column (1) but not statistically different from zero for sons in column (2). However, relying on OLS results only is problematic as children's decision to provide informal care might be driven by unobserved factors that are also related to children's educational attainment. Moreover, measurement error in the education variable might attenuate the estimates towards zero. Therefore, fuzzy RD regressions are estimated to correct for potential endogeneity and measurement error in children's education.

Figure 4.2 provides a graphical presentation of the first stage relationship between children's exposure to the compulsory schooling reforms and years of schooling. The figure plots average years of schooling for cohorts of children born 10 years before and after the first birth cohort affected by the reforms, separately for daughters in Panel A and for sons in Panel B. The figures show a general increase in average years of schooling for younger birth cohorts but also a clear jump at the cut-off. Moreover, the figures reveal some heterogeneity by gender as the jump in sons' average years of schooling is slightly smaller than the increase in daughters' average years of schooling at the time of the reforms. This is corroborated by the first stage regression results presented in Panel B of Table 4.2. The results suggest that an additional year of compulsory schooling induced by the reforms raises daughters' years of schooling by about 0.54 years (column (1)) and sons' years of schooling by about 0.42 years (column (2))[14]. The first stage coefficients are statistically significant at the 1 percent level in both subsamples. This is a sizeable effect that is, however, similar in magnitude compared to previous studies exploiting compulsory schooling laws in European countries in a comparable multi-country setup (e.g. Brunello et al. 2009, Borgonovi et al. 2010, Brunello et al. 2013, Stella 2013, Schneeweis et al. 2014, Mazzonna 2014, Gathmann et al. 2015, Brunello et al. 2016, Fort et al. 2016, Kunst et al. 2020). The first stage F-statistics are much larger than the conventional rule-of-thumb of 10 provided by Staiger and Stock (1997), indicating that the instrument is strong.

[13]In a simple specification that only includes cohort fixed effects, country fixed effects and country-specific linear cohort trends, the relationship is significant at the 1 percent level (not shown).

[14]The jump in daughters' and sons' years of schooling at the cut-off shown in Figure 4.2 is larger than the first stage estimates in Panel B of Table 4.2 because the figure does not take into account country differences with respect to the intensity of the compulsory schooling reforms. For instance, compulsory schooling has been increased by four years in Belgium but only by one year in Italy (see Table 3.2). In other words, the average increase in the number of compulsory years of schooling induced by the considered reforms is more than one.

Figure 4.2: Effect of the reforms on years of schooling (first stage)

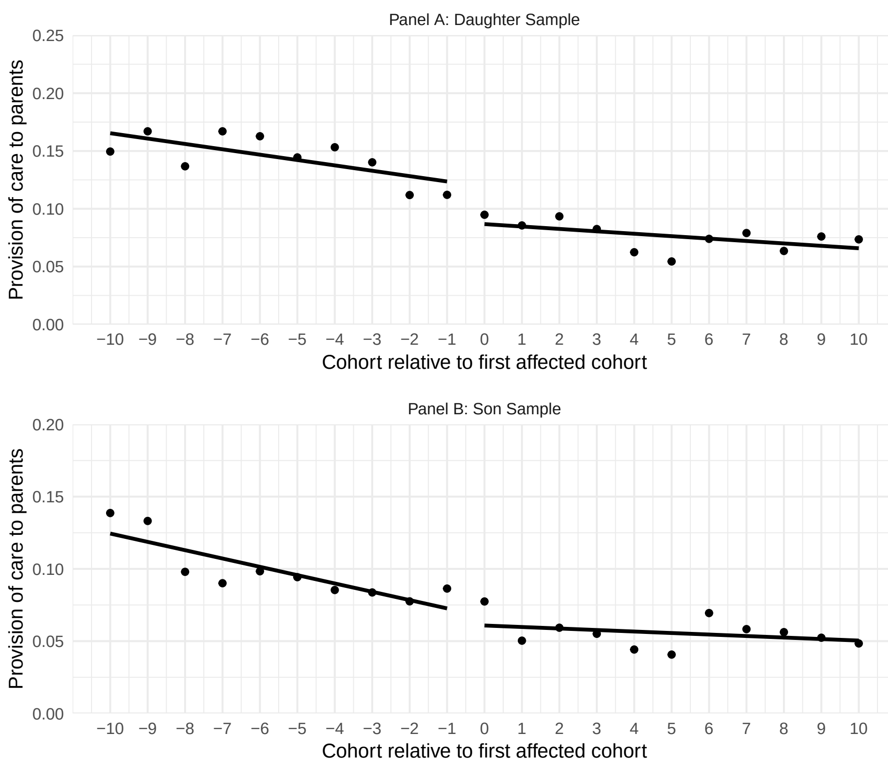


Notes: The figure plots average years of schooling for cohorts of daughters in Panel A and cohorts of sons in Panel B born 10 years before and after the pivotal cohorts. Each point represents a weighted mean (weighted by the number of observations per country) of all cohorts in the different countries, which are at the same distance from the pivotal cohorts. All countries are normalized by the time of the reforms, which is set at zero.
Source: Own calculations based on SHARE, waves 1, 2, 5 and 6.

Figure 4.3 is a graphical representation of the reduced form relationship between exposure to the compulsory schooling reforms and informal care provision to older parents. In particular, the figure plots the probability of providing informal care to parents for cohorts of children born 10 years before and after the pivotal cohorts, separately for daughters in Panel A and sons in Panel B. The figures in Panel A and Panel B show a slight downward trend in the probability to provide care for both sexes and a somewhat higher probability of informal care provision among daughters in Panel A than among sons in Panel B. Moreover, there is a small jump at the cut-off in both figures, which is somewhat clearer for daughters than for sons. Estimation results from reduced form regressions of the probability to provide informal care to older parents on adult children's compulsory years of schooling are presented in Panel C of Table 4.2. As shown in column (1), the reduced form estimate is negative and significantly different from zero, indicating that one additional year of compulsory schooling significantly reduces daughters' probability to provide care by 1.2 percentage points (10.4

percent), on average. However, the reduced form results reveal substantial heterogeneity by the gender of the child because there is no support for a significant effect of sons' compulsory years of schooling on the probability to provide care to parents in column (2). The estimate has a negative sign but is close to zero and statistically insignificant at conventional levels. In column (3), I test whether the coefficients for daughters and sons are significantly different from each other. The test involves estimating a local linear model that includes the full set of covariates and interaction terms between a gender dummy and all covariates. A significant gender interaction effect would demonstrate that the coefficients for daughters and sons are significantly different from each other. As shown in column (3), the reduced form estimates for daughters and sons are significantly different from each other at the 10 percent level. Hence, the results provide evidence for an effect of reform exposure on informal care provision for daughters but not for sons.

Figure 4.3: Effect of the reforms on informal care provision (reduced form)



Notes: The figure plots the probability to provide informal care to parents for cohorts of daughters in Panel A and cohorts of sons in Panel B born 10 years before and after the pivotal cohorts. Each point represents a weighted mean (weighted by the number of observations per country) of all cohorts in the different countries, which are at the same distance from the pivotal cohorts. All countries are normalized by the time of the reforms, which is set at zero.
Source: Own calculations based on SHARE, waves 1, 2, 5 and 6.

Panel D of Table 4.2 reports the fuzzy RD estimates, which represent the estimates for the causal effect of children's years of schooling on the probability to provide care to older parents. As expected from the negative coefficient in the reduced form model and the positive coefficient in the first stage model, the coefficient in column (1) has a negative sign, suggesting that one additional year of schooling reduces the probability that a daughter provides care to her mother or father by 2.2 percentage points, on average. The coefficient is statistically significant at the 1 percent level. Given the mean of 11.5 percent in the pre-reform sample of daughters, this is a quite large effect. In contrast, I find no significant effect of sons' years of schooling on the probability to provide care to parents in column (2). However, the estimates for daughters and sons are not significantly different from each other at conventional levels when tested in an interacted model in column (3).

The fuzzy RD estimate for daughters is about seven times larger than the corresponding OLS estimate in absolute value. There are at least three possible explanations for these findings. First, as already discussed above, there might be unobserved confounders that are positively correlated with both education and the likelihood to engage in informal caregiving, such as time management skills or empathy. That is, more education is likely to increase time-management skills or empathy and simultaneously, these factors may increase the likelihood of providing care to family members. Second, there might be classical measurement error in children's years of schooling that attenuates the OLS estimate towards zero, while the fuzzy RD estimate is unbiased because first stage and reduced form coefficients are proportionally attenuated. Third, the affected populations are different. While OLS estimates the effect across the entire population, fuzzy RD identifies a local average treatment effect at the threshold and only in the sub-population of compliers of the compulsory schooling reforms (Imbens and Angrist 1994, Bertanha and Imbens 2020). The compliers of the compulsory reforms in Europe can typically be found in the middle to lower parts of the educational distribution. The returns to education might be higher in this group of individuals, which would explain why the fuzzy RD estimate is larger than the OLS estimate in absolute value.

4.5.2 Robustness Checks

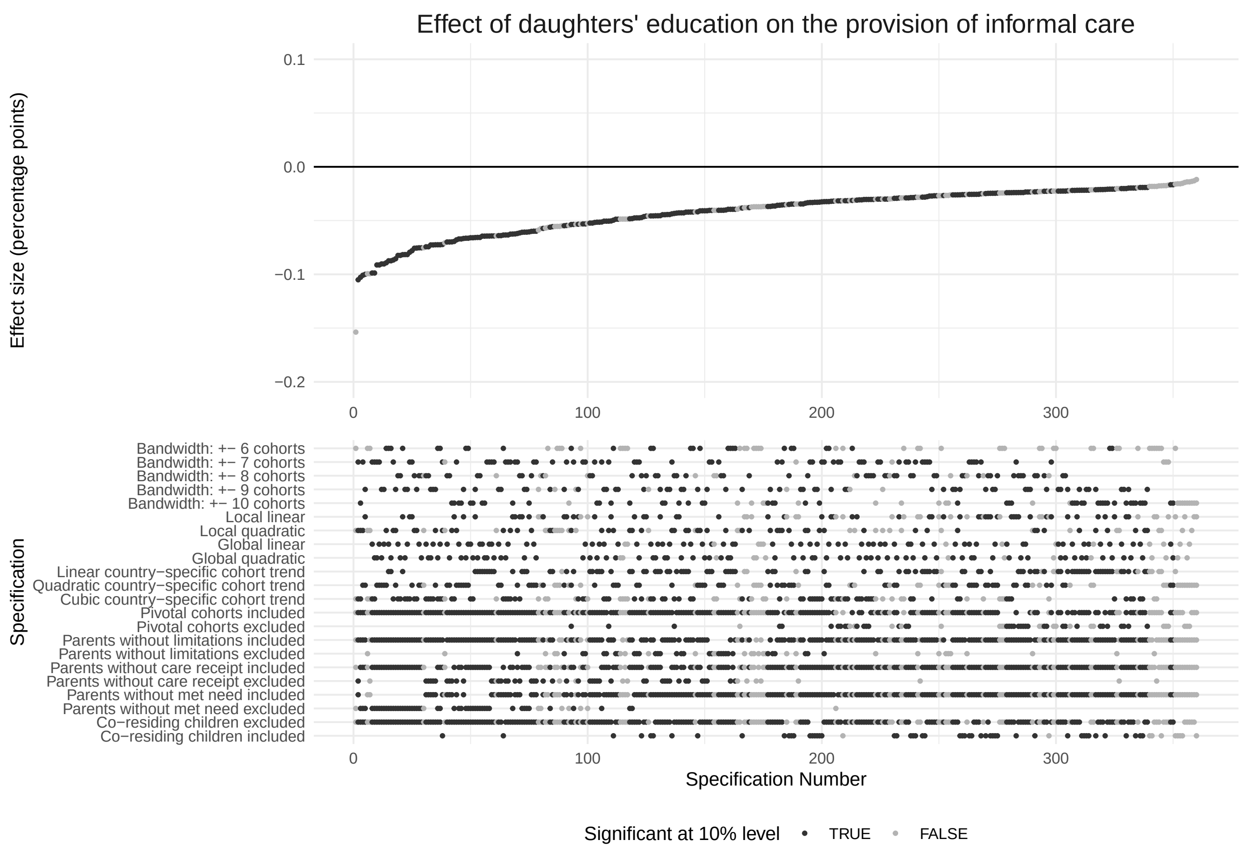
In this section, I explore the sensitivity of findings in a so-called specification curve analysis. This approach was first suggested by Simonsohn et al. (2020) as a tool to prevent selective reporting of results. The idea of a specification curve analysis is to define a set of plausible and non-redundant model specifications, to estimate all specifications and to visualize the results[15].

[15]This technique is also used in Chapter 2 of this thesis. For a detailed description of the specification curve method, I refer to Chapter 2.3.3 of this thesis and to the paper by Simonsohn et al. (2020).

In particular, I assess the robustness of findings with respect to the bandwidth choice (± 10 years to ± 6 years), the selection of the functional form (local linear, local quadratic, global linear, global quadratic), the inclusion of different specifications of country-specific cohorts trends (linear, quadratic, cubic) and sample selection choices regarding the exclusion of certain observations of child-parent dyads. Due to missing information on a child's month of birth in the data, all children born in the year of the pivotal cohort of a reform are considered as being affected in the main analysis. However, some of these children might actually not have been affected, resulting in a mixture of treated and untreated children in the first birth cohort affected by a reform, and hence, I drop the pivotal cohorts in some specifications. Moreover, I examine whether the results remain unchanged if the sample is restricted to parents who report to have at least one ADL or IADL limitation or to have received either informal care or formal care or a combination of both. Furthermore, I assess the robustness of results to including only parents with a so-called "met need" for care, that is, parents who report to have at least one ADL or IADL limitation and to have received formal or informal care. Finally, as opposed to the main analysis, I do not exclude co-residing children in some specifications. The combination of those modelling choices results in 360 different specifications, which are estimated and visualized separately by gender in Figure 4.4 and Figure 4.5. A summary of the modelling choices is shown in Table 4.A.4 in the Appendix. Each dot in the upper panel of Figure 4.4 and Figure 4.5 represents a point estimate in percentage points obtained from a different model specification. The 360 specifications are ordered by effect size in ascending order. The dots in the lower panel of the figures indicate the combination of modelling choices behind those estimates. Black dots represent effects that are statistically significant at the 10 percent level, while grey dots show statistically insignificant effects.

Figure 4.4 shows the specification curve for daughters' probability to provide informal care to older parents. It emerges that the coefficients are negative throughout all specifications. Moreover, the majority of specifications (75 percent) are statistically significant at the 10 percent level. There is no clearly discernible pattern regarding the characteristics of the specifications that yield insignificant estimates. However, the proportion of insignificant estimates is larger in local specifications compared to global polynomial specifications and in specifications with quadratic and cubic country-specific cohort trends compared to linear trends. Among the different bandwidths, the estimates are most often statistically insignificant in specifications with the smallest considered bandwidth of ± 6 years, which is a result of small sample sizes leading to imprecise estimates. Moreover, the share of insignificant estimates is larger in specifications that do not exclude the pivotal cohorts of the reforms. Effect sizes vary, but in most specifications they are close to the main analysis, at least in relative terms. For example, in the local linear regression that uses a bandwidth of ± 10 years, includes linear country-specific cohort trends and excludes parents without any limitations, I find that one more year of daughters' schooling reduces the probability to care for parents by 4 percentage points. Relative to the mean of about 22 percent in this sample, this is equivalent to a reduction by about 19 percent and thus in line with the main findings. Hence, I conclude that the finding of a reduction in better-educated daughters' probability to provide care to parents is robust to a wide range of model specifications.

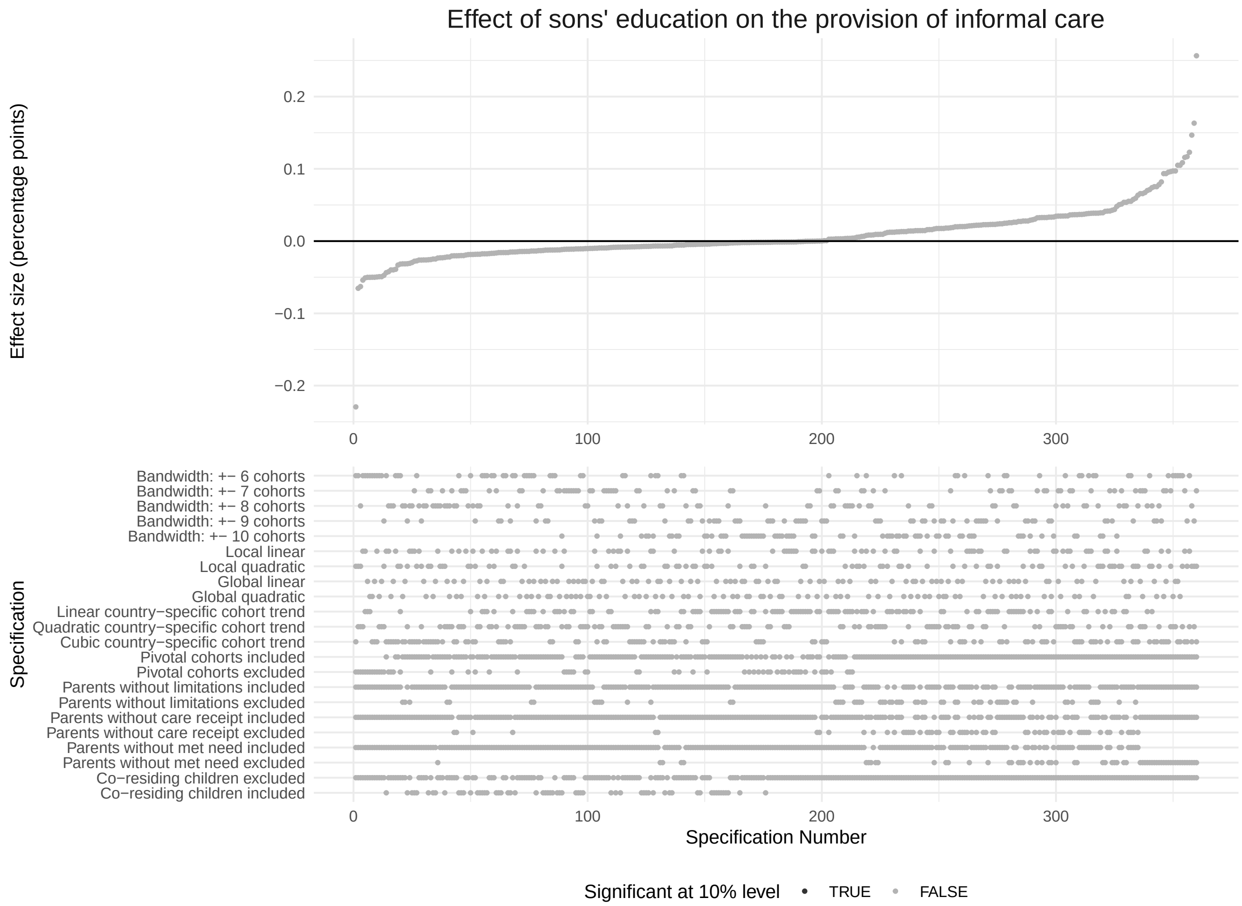
Figure 4.4: Specification curve for the effect of daughters' education on care provision



Notes: The figure shows the specification curve analysis for the effect of daughters' years of schooling on the probability to provide care to parents. Each dot in the top panel represents a point estimate (in percentage points) obtained from a different model specification. The specifications are ordered by effect size. The dots vertically aligned below indicate the combination of model decisions behind those estimates. Black dots represent statistically significant effects ($p < 0.1$), while grey dots show insignificant effects. All regressions control for cohort fixed effects, country fixed effects, linear country-specific cohort trends, interview year fixed effects, daughters' quadratic age and number of siblings as well as parents' age at birth of child, gender, education and number of ADL and IADL limitations. Standard errors are clustered at the parent level.
Source: Own calculations based on SHARE, waves 1, 2, 5 and 6.

The specification curve for sons' probability to provide informal care to parents in Figure 4.5 shows that about half of the coefficients are positive and negative. However, it is striking that no coefficient is significantly different from zero at the 10 percent level. The results for sons are thus very robust to alternative specifications.

Figure 4.5: Specification curve for the effect of sons' education on care provision



Notes: The figure shows the specification curve analysis for the effect of sons' years of schooling on the probability to provide care to parents. Each dot in the top panel represents a point estimate (in percentage points) obtained from a different model specification. The specifications are ordered by effect size. The dots vertically aligned below indicate the combination of analytic decisions behind those estimates. Black dots represent statistically significant effects ($p < 0.1$), while grey dots show insignificant effects. All regressions control for cohort fixed effects, country fixed effects, linear country-specific cohort trends, interview year fixed effects, sons' quadratic age and number of siblings as well as parents' age at birth of child, gender, education and number of ADL and IADL limitations. Standard errors are clustered at the parent level.
Source: Own calculations based on SHARE, waves 1, 2, 5 and 6.

Another robustness check addresses potential issues related to the chosen estimation approach. In particular, I report results from two-stage residual inclusion (2SRI) models in Table 4.3 because the outcome variable in this study is binary and somewhat rare at approximately 10 percent. 2SRI models have been found to be appropriate to account for endogeneity in non-linear models with rare outcomes (Terza et al. 2008, Basu et al. 2018). The 2SRI approach is implemented by including the residuals from the first stage regression as additional regressor in the second stage regression instead of replacing the endogenous variable with predicted values from the first stage regression.

Table 4.3: Two-Stage Residual Inclusion (2SRI) results

Dependent Variable: Provision of informal care to parents (1=Yes, 0=No)		
	(1) Daughters	(2) Sons
Panel A: Local linear		
Years of schooling	-0.018^{***}	0.001
	(0.007)	(0.007)
Observations	18,945	18,901
Control mean	0.115	0.078
Panel B: Local quadratic		
Years of schooling	-0.017^{**}	0.001
	(0.007)	(0.007)
Observations	18,945	18,901
Control mean	0.115	0.078
Panel C: Global linear		
Years of schooling	-0.017^{**}	0.0004
	(0.007)	(0.007)
Observations	18,945	18,901
Control mean	0.115	0.078
Panel D: Global quadratic		
Years of schooling	-0.018^{***}	0.001
	(0.007)	(0.007)
Observations	18,945	18,901
Control mean	0.115	0.078

Notes: The table shows results from 2SRI regressions of children's probability to provide care on years of schooling with a logit in the second stage, separately for daughters in column (1) and for sons in column (2). Panel A and Panel B report results from local linear and local quadratic models, while Panel C and Panel D show results from models with first and second order polynomials. Logit coefficients are reported as marginal effects. All regressions are based on a bandwidth of ±10 years and include cohort fixed effects, country fixed effects, linear country-specific trends in child's birth cohort, interview year fixed effects, children's quadratic age, children's number of siblings, parents' age, parents' gender, parents' education and parents' number of ADL and IADL limitations. Standard errors are clustered at the parent level and presented in parentheses. $^{*}p<0.1$; $^{**}p<0.05$; $^{***}p<0.01$.
Source: Own calculations based on SHARE, waves 1, 2, 5 and 6.

As part of this robustness check, I estimate 2SRI models with a logit for the probability to provide care in the second stage and report estimates as average marginal effect in Table 4.3[16]. Panel A and Panel B report results from local linear and local quadratic models, while Panel C and Panel D show results from models with first and second order polynomials. The estimate for daughters in Panel A, column (1) points to a significant reduction in the likelihood that a daughter will provide informal care to parents by 1.8 percentage points due to one additional year of schooling (15.6 percent). Hence, the estimate is somewhat smaller in absolute terms but qualitatively in line with the main findings. The estimate is robust to different functional forms, as shown in Panels B, C and D. The estimates for sons in column (2) are insignificant at conventional levels of statistical significance, irrespective of the functional form.

4.5.3 Heterogeneity Analyses

This section sheds light on potential heterogeneity in the main results. In particular, I investigate heterogeneity by parents' level of care dependency and by groups of European countries according to the European north-south gradient in informal care provision.

Heterogeneity by Parents' Level of Dependency

A first heterogeneity analysis evaluates potential treatment effect heterogeneity by parents' level of care dependency because the effect of adult children's education on informal care provision to parents might depend on the intensity of care provided. For the purpose of the analysis, parents' level of care dependency is measured by the reported number of ADL and IADL limitations. Table 4.4 shows results for the effect of children's years of schooling on the probability to provide care to parents with zero limitations in column (1), to parents with one limitation in column (2) and to parents with two or more limitations in column (3). Panel A provides some suggestive evidence that the effect of daughters' years of schooling on the probability to provide care to parents is driven by parents with a high level of care dependency. In column (3), I find that each additional year of schooling significantly reduces the probability to care for parents with two or more ADL or IADL limitations by 6.5 percentage points relative to a mean of 27.1 percent. In contrast, the estimates for the effect of one additional year of schooling on the likelihood to care for parents with no limitations in column (1) and for parents with one limitation in column (2) are not statistically different from zero. The estimates in Panel B demonstrate that the effect of sons' years of schooling on informal care provided to parents is statistically insignificant, irrespective of parents' level of care

[16]2SRI models with a probit in the second stage yield very similar results.

dependency. The findings for daughters might be explained by the intensity of informal care provided. Daughters may suffer more from care provided to parents with a high degree of dependency since it is likely to be more intensive and more physically demanding in this case. As a result, it might be more difficult for daughters to reconcile work and time-consuming caregiving, which is associated with a higher opportunity cost of care and might reduce daughters' probability to engage in informal caregiving to parents with a high level of dependency.

Table 4.4: Heterogeneity by parents' number of ADL and IADL limitations

Dependent Variable: Provision of informal care to parents (1=Yes, 0=No)			
	No limitation (1)	1 limitation (2)	2+ limitations (3)
Panel A: Daughters			
Years of schooling	−0.011	−0.008	−0.065**
	(0.008)	(0.020)	(0.025)
Control mean	0.058	0.143	0.271
Observations	13,058	2,416	3,471
Panel B: Sons			
Years of schooling	−0.008	0.039	−0.022
	(0.009)	(0.026)	(0.034)
Control mean	0.044	0.089	0.172
Observations	12,885	2,466	3,550

Notes: The table shows results for the effect of children's years of schooling on the probability to provide care to parents, accounting for the potential endogeneity of education in a fuzzy RD design. The results are presented separately for daughters in Panel A and for sons in Panel B. The sample is stratified by parents' number of ADL and IADL limitations. The sample is divided into parents who report zero limitations (column (1)), one limitation (column (2)) and at least two limitations (column (3)). All regressions are based on local linear models, use a bandwidth of ±10 cohorts and include cohort fixed effects, country fixed effects, linear country-specific cohort trends, interview year fixed effects, children's quadratic age, children's number of siblings, parents' age at birth of child, parents' gender and parents' education. Standard errors are clustered at the parent level and presented in parentheses. *p<0.1; **p<0.05; ***p<0.01.
Source: Own calculations based on SHARE, waves 1, 2, 5 and 6.

Heterogeneity by European Countries

To shed light on potential heterogeneity in the results by European countries, Table 4.5 provides results for the effect of years of schooling on the probability to provide care to parents for groups of European countries according to the north-south gradient observed in the previous literature (e.g. Brandt et al. 2009, Brandt 2013, Verbakel 2018). For this purpose, the sample is grouped into Western Europe (the Netherlands, Belgium, France) and Southern Europe (Greece, Italy, Portugal, Spain)[17]. Grouping European countries into two broad regions is a pragmatic response to small sample sizes, but it nevertheless captures essential differences in family patterns and welfare regimes across Europe.

Table 4.5: Heterogeneity by European regions

Dependent Variable: Provision of informal care to parents (1=Yes, 0=No)			
	Western Europe (1)	Southern Europe (2)	Difference (3)
Panel A: Daughters			
Years of schooling	−0.010	−0.031*	−0.021
	(0.010)	(0.018)	(0.020)
Control mean	0.099	0.141	
Observations	11,367	7,578	18,945
Panel B: Sons			
Years of schooling	−0.012	−0.034	−0.022
	(0.010)	(0.029)	(0.040)
Control mean	0.075	0.083	
Observations	11,312	7,589	18,901

Notes: The table shows results for the effect of children's years of schooling on the probability to provide care to parents, accounting for the potential endogeneity of education in a fuzzy RD design. The results are presented separately for daughters in Panel A and for sons in Panel B. The sample is stratified by groups of European countries. Western Europe includes the Netherlands, Belgium and France (column (1)), while Southern Europe includes Greece, Italy, Portugal and Spain (column (2)). Column (3) reports differences between Southern Europe and Western Europe. All regressions are based on local linear models, use a bandwidth of ±10 cohorts and include cohort fixed effects, country fixed effects, linear country-specific cohort trends, interview year fixed effects, children's quadratic age, children's number of siblings, parents' age at birth of child, parents' gender, parents' education and parents' number of ADL and IADL limitations. Standard errors are clustered at the parent level and presented in parentheses. $^{*}p<0.1$; $^{**}p<0.05$; $^{***}p<0.01$.
Source: Own calculations based on SHARE, waves 1, 2, 5 and 6.

The results in Table 4.5 reveal a heterogeneous picture. In Southern Europe, one additional year of daughters' schooling significantly reduces the probability to provide care to older parents by 3.1 percentage points (i.e. 22.0 percent), as shown in column (2) of

[17]Another heterogeneity analysis would be to estimate effects of education on caregiving for groups of countries according to the availability of formal care services. In the previous literature, the percentage of employees in the social service sector has frequently been used to measure the availability of social services in a country (e.g. Igel et al. 2009, Brandt et al. 2009, Brandt 2013, Brandt and Deindl 2013). Since the percentage of employees in the social service sector seems also to follow a north-south gradient with a large share in the European North and a comparatively low proportion in Southern Europe, the grouping of countries would be the same.

Panel A, whereas the effect is statistically insignificant in Western Europe (see column (1)). Hence, Southern European countries seem to drive the results. However, this evidence for daughters is only suggestive as the coefficients for Western Europe and Southern Europe are not significantly different from each other at conventional levels when tested in an interacted model in column (3). In line with the overall effect, I find no significant effects of years of schooling on informal care provision for sons living in Western Europe or Southern Europe in Panel B.

Although the evidence for daughters is only suggestive, differences in public welfare systems and cultural norms across Europe may explain the finding of an effect of education on informal care provision in Southern Europe but not in Western Europe. In Southern European countries that are considered as "strong family ties countries" and characterized by a low availability and generosity of public formal care, informal care is almost exclusively provided by family members and especially by adult daughters who provide care at a high frequency and intensity. Support to older parents is perceived as an obligatory and inevitable task in those countries. In contrast, in the so-called "weak family ties countries" of Northern Europe, public formal care services are more generous and thus only complemented by informal care services. This leads to a situation where more people, i.e. adult children, spouses, other relatives, friends or neighbours, are involved in caregiving but at a lower intensity because informal care is perceived as a voluntary task. Therefore, the opportunity costs of care are likely to be higher for daughters living in Southern Europe.

4.5.4 Mechanisms

The results in this study provide consistent evidence that adult daughters' education negatively affects the likelihood of providing care to older parents. This section investigates potential mechanisms behind those results. As already discussed above, there are at least three plausible mechanisms: (1) higher wages, associated with an increased ability to pay for formal care and a reduced dependency on inter-vivos transfers of money and bequests, (2) better health, and (3) increased opportunity costs due to higher labour force participation, greater geographic distance or the composition of the own family. Unfortunately, SHARE does not contain information on children's wages or health status and therefore I cannot address all potential mechanisms. However, SHARE provides information on children's employment status, marital status, parenthood status and geographic distance to parents, which allows me to shed light on whether the opportunity cost mechanism drives the results. Table 4.6 shows results for the effect of daughters' years of schooling on being full-time employed (Panel A), on being married or in a registered partnership (Panel B), on the number of children (Panel C) and on living more than 100 kilometres away from the parental home (Panel D). Looking at the correlations based on OLS regressions in column (1) reveals that daughters' years of schooling are positively associated with being full-time employed and living far away from parents' home (i.e. 100 km or more), and negatively correlated the number of children and with being married or partnered. In order to address the potential endogeneity of education that might bias the OLS results presented in column (1), fuzzy RD estimates of the effect of daughters' years of schooling on daughters' outcomes are presented in column (2).

Table 4.6: Potential mechanisms

Dependent variable	OLS (1)	Fuzzy RD (2)
Panel A: Full-time employed (1=Yes, 0=No)		
Years of schooling	0.034***	0.024*
	(0.003)	(0.014)
Control mean	0.474	0.474
Observations	18,945	18,945
Panel B: Married or in registered partnership (1=Yes, 0=No)		
Years of schooling	−0.007***	0.009
	(0.002)	(0.012)
Control mean	0.786	0.786
Observations	18,945	18,945
Panel C: Number of children		
Years of schooling	−0.048***	0.029
	(0.007)	(0.036)
Control mean	1.794	1.794
Observations	18,945	18,945
Panel D: Living $\geq$ 100 km away from parents (1=Yes, 0=No)		
Years of schooling	0.010***	−0.017
	(0.002)	(0.011)
Control mean	0.147	0.147
Observations	18,945	18,945

Notes: The table shows OLS and fuzzy RD results for the effect of daughters' years of schooling on daughters' outcomes. All regressions are based on local linear models, use a bandwidth of ±10 cohorts and include cohort fixed effects, country fixed effects, country-specific linear cohort trends, interview year fixed effects, daughters' quadratic age, daughters' number of siblings, parents' age at birth of child, parents' gender, parents' education and parents' number of ADL and IADL limitations. Standard errors are clustered at the parent level and are presented in parentheses. *p<0.1; **p<0.05; ***p<0.01.
Source: Own calculations based on SHARE, waves 1, 2, 5 and 6.

In Panel A, I analyse whether more schooling affects daughters' labour force participation. As explained earlier, education is likely to affect employment, thereby reducing the probability to provide care through increased opportunity costs. In line with this theoretical consideration, the estimate in column (2) suggests that one additional year of daughters' schooling significantly increases the probability of being full-time employed by 2.4 percentage points, on average, which corresponds to an increase by about 5.1 percent in relative terms. Therefore, column (2) provides some suggestive evidence for a causal effect of education on the extensive margin of female labour market participation. These findings on the importance of education on (female) labour force participation are consistent with those by Fischer et al. (2020) and Hofmarcher (2021). Fischer et al. (2020) analyse the effect of two parallel educational reforms that extended the annual term length and years of compulsory schooling by comparable amount in Swedish primary schools. They find an increase in female employment by 4 percentage points due to the term extension. Moreover, Hofmarcher (2021) report a significant increase in being full-time employed by about 4.8 percentage points due to an additional year of schooling for both males and females when investigating potential mechanisms for a causal effect of education on poverty in Europe.

In Panels B and C, I examine the effect of education on two variables related to family composition, i.e. the probability of being married or partnered, and the number of children. As outlined earlier, education might affect fertility because individuals who invest more in education might postpone childbearing until they are well established on their career path and might have fewer children, on average. Moreover, better-educated women might marry later in life. In turn, fertility and marital status are likely to affect the likelihood of providing care to older parents because belonging to the so-called "sandwich generation" that has competing caregiving responsibilities for own children and older parents is associated with increased opportunity costs. However, as shown in column (2) of Panel B, there is no support for a significant effect of daughters' years of schooling on the likelihood of being married or in a registered partnership. This finding is generally in line with previous findings in the literature, e.g. by Anderberg and Zhu (2014) who do not find evidence for a causal effect of education on marital outcomes for women in the UK. Moreover, the results in column (2) of Panel C suggest no significant effect of education on fertility, as measured by the average number of children. This is also consistent with previous findings in the literature, e.g. by Monstad et al. (2008) and Fort et al. (2016), reporting no effects of education on childlessness and the number of children in Norway and Continental Europe, respectively[18].

[18]In contrast, Cygan-Rehm and Maeder (2013) find a negative effect on the number of children and a positive effect on childlessness for Germany.

Panel D examines whether education affects the geographic distance to parents, measured by the probability of living more than 100 kilometres away from the parental home. In general, compared to their less-educated counterparts, better-educated children are more likely to move to other regions or countries due to better job opportunities (Machin et al. 2012, Weiss 2015, Haapanen and Böckerman 2017). In turn, greater geographic distance might increase opportunity costs, thereby decreasing the probability that adult children provide care to their ageing parents. However, as shown in column (2) of Panel D, the estimate for the effect of one additional year of daughters' years of schooling on the probability of living more than 100 kilometres away from the parental home is not statistically different from zero at conventional levels of statistical significance[19]. This finding contradicts prior evidence on potential channels explaining effects of children's education on parental longevity in Sweden by Lundborg and Majlesi (2018), finding that increased schooling among females increases distance to parents. However, the difference in findings might be explained by the European countries studied because previous evidence suggests a European north–south difference in labour mobility (Machin et al. 2012). Moreover, mobility is mainly found to be affected by post-secondary education (Malamud and Wozniak 2012, McHenry 2013, Böckerman and Haapanen 2013, Haapanen and Böckerman 2017, Aparicio Fenoll and Kuehn 2017) and therefore, it is not surprising to find no causal effect of one more year of (compulsory) schooling in the middle to lower parts of the education distribution on mobility in Europe. For example, Malamud and Wozniak (2012) study the effect of education on migration using the draft avoidance behaviour during the Vietnam War as an instrument for college education and find evidence for a positive effect on migration. Böckerman and Haapanen (2013) and Haapanen and Böckerman (2017) use a reform in the higher education system in Finland in the 1990s that gradually transformed former vocational colleges into polytechnics offering a Bachelor's degree, thereby expanding higher education to regions that did not have universities before, and report also positive effects of education on migration. In contrast, McHenry (2013) and Aparicio Fenoll and Kuehn (2017) find that additional schooling *reduces* mobility when exploiting compulsory schooling laws implemented in the United States in the 20th century or in several European countries between 1950 and 1990, respectively. McHenry (2013) argues that additional education at lower levels may increase the strength of local job network ties and may thus provide employment stability in the local area, resulting in increased opportunity costs of migration and thus reduced geographic mobility.

[19] The previous literature often classifies care provided to parents living at least 100 kilometres away as so-called "distance caregiving" (Wagner et al. 2020). The reason for considering a threshold of 100 kilometres for the analysis is that I expect only large geographic distances to the parental household to be a potential barrier to caregiving. Using alternative thresholds of the geographic distance between parents and children, however, does not change the results.

Taken together, the results in this section provide some suggestive evidence in favour of the opportunity cost mechanism, suggesting that the causal link running from daughters' education to informal care provision to older parents might be explained by increased female labour force participation. Geographic mobility and family patterns in terms of being married or having own children do not seem to be important mechanisms driving the results. Better health and higher wages are other plausible mechanisms that can, however, not be addressed with the data to hand.

4.6 Conclusion

The aim of the study was to analyse the causal effect of adult children's education on the provision of informal care to older parents. In a fuzzy regression discontinuity approach, the study exploits exogenous variation in education induced by compulsory schooling reforms passed between 1967 and 1999 in seven European countries. Using data from four waves of the Survey on Health, Ageing and Retirement in Europe (SHARE), the results suggest that children's years of schooling are negativity related to informal care provision to parents. However, taking the potential endogeneity of education into account, the estimates become smaller in magnitude and reveal substantial heterogeneity by the gender of the adult child. While the results suggest no significant effects for sons, one additional year of daughters' schooling is found to significantly decrease the probability to care for parents by about 2 percentage points, which corresponds to a substantial reduction by about 19 percent. Several sensitivity analyses confirm the robustness of the results. This suggests that the simple correlation between education and informal caregiving overstates the effect because daughters self-select into longer schooling due to unobserved factors (e.g. time-management skills or empathy), which simultaneously increase the probability to engage in informal caregiving. Additional analyses provide some suggestive evidence that the effect is driven by daughters from Southern Europe providing care to parents with a high level of care dependency. Moreover, the results suggest that opportunity costs in terms of increased female labour force participation are a potential mechanism explaining these results, while geographic distance to the parents' home and family composition do not seem to play an important role.

The present study has some limitations that should be discussed. First, the focus is on the extensive margin of informal care provision, i.e. the decision to be a caregiver or not, although it would be also of interest to examine the intensive margin. Indeed, SHARE provides information on the hours of provided care and thus on the intensive margin of informal care provision, but this information is only available in waves 1 and 2, and therefore the study focuses on the extensive margin to avoid small sample size issues. However, I estimate causal effects by parents' level of care dependency and use this as a proxy for intensity since the care provided to parents with a high degree of care dependency is likely to be more intensive and thus more physically demanding and time-consuming. The question whether parents' care dependency is a good proxy for care intensity depends on the availability of substitutes or complements within the family such as siblings who take over or share caregiving responsibilities. This is, however, partially captured by controlling for the number of siblings in the regression models. Second, the study focuses on informal care provided by adult children and does neither address informal care from other sources such as spouses, other relatives, neighbours or friends, nor formal home or nursing home care. In particular, it would be interesting to analyse the interactions that take place among siblings when deciding whether or not to become an informal caregiver. In this context, it has been shown that the presence of siblings and the birth order of siblings is relevant for the geographic distance between adult children and their parents. Konrad et al. (2002) argue that first-born children who are typically the first to be in the position to leave the parental home, tend to avoid providing care to their parents due to a strategic decision to live at a greater distance to their parents. Third, information on children is provided by the SHARE respondents, i.e. the parents. This might be problematic as it is likely to increase non-response on some child characteristics such as education and inconsistency across waves, especially for older parents with cognitive impairments. Apart from that, information on informal care received could be under- or over-reported by parents, which may be the result of the framing of the survey questions and the interpretation of these questions by different respondents. In addition to the fact that surveys often use different terms like "care", "support", "help" or "looking after", it is important to know how respondents define the term "care". While this is often straightforward for formal care as it is typically well defined, it is more difficult in an informal care setting to distinguish between caregiving tasks and non-caregiving tasks that are part of the routine within families, such as preparing a hot meal. As a result, there may be a discrepancy in informal care reports between providers and recipients (Rutherford and Bu 2017, Urwin et al. 2021). Fourth, SHARE respondents can report a maximum number of three potential caregivers only. As a consequence, some children might not be identified as an informal caregiver if a respondent has more than three informal caregivers. Fifth, I am not able to investigate other mechanisms beyond increased opportunity costs because information on children is limited to some basic demographic information in SHARE. Information on wages and health status is absent in the data, but would be necessary to examine whether better-educated daughters are less likely to provide care because they are healthier themselves or because they have higher wages that enable them to finance formal care and simultaneously reduce their dependency on parental cash transfers. Sixth, the sample size is not large enough to carry out a more disaggregated analysis by European countries instead of groups of countries to explore country heterogeneity. However, this would be particularly interesting because European countries differ considerably with respect to the supply of informal care and public formal care services. Finally, the estimated effects do not represent average treatment effects across the entire population but local average treatment effects at the threshold and only for the subpopulation of compliers (Imbens and Angrist 1994, Bertanha and Imbens 2020). The compliers of the reforms are those individuals whose educational attainment changed because they have been affected by the compulsory schooling reforms, i.e. individuals in the middle to lower parts of the education distribution. As their returns to education might be higher than those of the average person, the estimates are not necessarily representative for the overall population.

Despite these limitations, the findings have important implications for the organization and financing of long-term care. Since the largest share of long-term care is provided informally with daughters being the most common family caregivers, the findings imply a future reduction in the availability of informal caregivers. In turn, this might increase the demand for formal care and thus long-term care expenditure. However, the demographic change also affects the supply of formal care as a general decline in the working-age population due to the upcoming retirement of the "baby boom" generation leads to a shortage of skilled workers in the care sector. This is intensified by tough working conditions, low wages and low job satisfaction in the formal care sector (Spasova et al. 2018, p. 9). This urges policy makers to address these challenges, e.g. by increasing the attractiveness of the care profession or by recruiting immigrants as formal care providers. Alternatively, policy makers could increase the incentives for informal care provision by improving the compatibility of work and informal care, e.g. by providing financial compensation or flexible working hours for informal caregivers. To evaluate the cost-effectiveness of long-term care policies, the substitutability of informal care and formal care is important. Bonsang (2009) finds that the effect of informal care by adult children on formal care use among older people in Europe depends on the type of formal home care. He shows that informal care is an effective substitute for (low-skilled) paid domestic help, although this substitution effect seems to disappear as the disability level of the ageing person increases. Moreover, Bonsang (2009) finds that informal care is a weak complement for (high-skilled) nursing care, regardless of the level of disability. This suggests that informal care is no effective substitute for formal care when the needs of the older person are high. In this case, policies aimed at addressing the challenges of the formal care sector have to be accompanied by policies increasing the incentives for informal care provision.

Furthermore, this study reveals several opportunities for future research. First, future research should proceed examining the causal effects of education on informal care provision exploiting different sources of exogenous variation in education that target individuals at higher parts of the education distribution. Second, it would be interesting to study causal effects of adult children's education on the provision of informal care to parents at the intensive margin, i.e. caregiving hours. Third, future research should address strategic considerations in location choices and interactions between siblings when investigating causal effects of adult children's education on informal care provided to older parents and underlying mechanisms. Finally, the prevalence and intensity of informal care provision to ageing parents might have changed during the COVID-19 pandemic due to limited formal care services available, social-distancing and increased unemployment (Rodrigues et al. 2021, Bergmann and Wagner 2021). Moreover, the pandemic might have affected existing gender differences in informal caregiving (Raiber and Verbakel 2021). At the same time, the pandemic has affected educational systems across Europe due to the closure of schools and universities as a preventive measure to the spread of COVID-19. Hence, exploring the effects of COVID-19 on education and informal caregiving patterns provides future research avenues.

Appendix

Table 4.A.1: Types of care to measure parents' long-term care utilisation

Type of care	Description
Informal care	a) Informal care from any family member outside the household or any friend or neighbour, including: - personal care, e.g. dressing, bathing or showering, eating, getting in or out of bed, and using the toilet - practical household help, e.g. with home repairs, gardening, transportation, shopping, and household chores - help with paperwork, such as filling out forms, and settling financial or legal matters b) Informal care from within the household, including - help with personal care such as washing, getting out of bed, or dressing
Formal home care	Formal care received in the own home, including: - professional or paid nursing or personal care - professional or paid home-help for domestic tasks that could not be performed because of health problem - meals-on-wheels
Formal nursing home care	- Temporary admissions to nursing homes - Permanent admissions to nursing homes

Notes: The table reports the different types of care that are used to measure parents' long-term care utilisation.
Source: Own illustration.

Table 4.A.2: Country-specific school degrees and years of schooling

Country	Reforms	Name of school degree (as in the questionnaire)	School years	Pre-reform	Post-reform
A. Belgium[1]	**1983 (8 → 12)**	**Wave 1 and wave 2 (Flemish/French)**			
		Lager onderwijs/Enseignement primaire	*6*	*8*	*10.5*
		Lager secundair onderwijs - Kunst/Enseignement secondaire inférieur général	*8*	*8*	*10.5*
		Lager secundair onderwijs - Algemeen/Enseignement secondaire inférieur artistique	*8*	*8*	*10.5*
		Lager secundair onderwijs - Technisch/Enseignement secondaire inférieur technique	*8*	*8*	*10.5*
		Lager secundair onderwijs - Beroeps/Enseignement secondaire inférieur professionnel	*12*	*8*	*10.5*
		Hoger secundair onderwijs - Kunst/Enseignement secondaire supérieur général	*12*	*8*	*10.5*
		Hoger secundair onderwijs - Algemeen/Enseignement secondaire supérieur artistique	*12*	*8*	*10.5*
		Hoger secundair onderwijs - Technisch/Enseignement secondaire supérieur technique	*12*	*8*	*10.5*
		Hoger secundair onderwijs - Beroeps/Enseignement secondaire supérieur professionnel	*12*	*8*	*10.5*
		Wave 5 and wave 6 (Flemish/French)			
		Getuigschrift Basisonderwijs/Ecole primaire uniquement, certificat d'études de base (CEB) ou primaire	*6*	*8*	*10.5*
		Getuigschrift van de eerste graad algemeen secundair onderwijs; Diploma van het lager algemeen secundair onderwijs/Diplômé de l'enseignement secondaire inférieur général ou du premier cycle de l'enseignement secondaire	*8*	*8*	*10.5*
		Getuigschrift van de eerste graad secundair kunstonderwijs; Diploma van het lager secundair kunstonderwijs/Diplômé de l'enseignement secondaire inférieur artistique	*8*	*8*	*10.5*

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…Table 4.A.2 continued

		Getuigschrift van de eerste graad secundair technisch onderwijs; Diploma van het lager secundair technisch onderwijs/Diplômé de l'enseignement secondaire inférieur technique	*8*	*8*	*10.5*
		Getuigschrift van de eerste graad secundair beroepsonderwijs; Diploma van het lager secundair beroepsonderwijs/Diplômé de l'enseignement secondaire inférieur professionnel	*12*	*8*	*10.5*
		Diploma van het secundair onderwijs (ASO); Diploma van het hoger algemeen secundair onderwijs/Certificat d'enseignement secondaire supérieur général ou technique de transition	*12*	*8*	*10.5*
		Diploma van het secundair onderwijs (KSO); Diploma van het hoger secundair kunstonderwijs/Certificat de qualification de l'enseignement artistique	*12*	*8*	*10.5*
		Diploma van het secundair onderwijs (TSO); Diploma van het hoger secundair technisch onderwijs	*12*	*8*	*10.5*
		Studiegetuigschrift van secundair onderwijs (na 6e jaar BSO); Diploma van het hoger secundair beroepsonderwijs/Certificat de qualification de l'enseignement technique	13	13	13
		Studiegetuigschrift van het 3e leerjaar van de 3e graad TSO, KSO of BSO; 4e graad BSO; Diploma 'Ondernemersopleiding'/ Certificat de qualification de l'enseignement professionnel	13	13	13
		Diploma van het secundair onderwijs (na 7e jaar BSO)/7ème année de l'enseignement secondaire professionnel et technique de qualification; Diplômé de formation des chefs d'entreprise	13	13	13
		Secundair onderwijs voorbereidend jaar op het hoger onderwijs/7ème année de l'enseignement secondaire professionnel permettant d'obtenir le certificat d'enseignement secondaire supérieur	13	13	13
		Année práparatoire à l'enseignement supérieur	13	13	13
B. France	**1967 (8 $\rightarrow$ 10)**	**Wave 1 and wave 2**			
		Certificat d'études primaires (CEP)	*5*	*8*	*10*
		Brevet des collèges, BEPC, brevet élémentaire	*9*	*8*	*10*
		CAP, BEP, ou diplôme de ce niveau	11	11	11
		Baccalauréat technologique ou professionnel	12	12	12
		Baccalauréat général	12	12	12

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...Table 4.A.2 continued

		Wave 5 and wave 6			
		Certificat d'études primaires (CEP) ou scolarité interrompue après la fin du primaire et avant la fin du collège	*5*	*8*	*10*
		BEPC, brevet élémentaire, brevet des collèges, DNB ou scolarité jusqu'à la fin du collège ou au-delà, sans diplôme	*9*	*8*	*10*
		CAP, BEP ou diplôme de niveau équivalent (Diplôme d'aide-soignante, auxiliaire de puériculture, aide médico-pédagogique, aide à domicile)	11	11	11
		Baccalauréat technologique (séries F, G, H, SMS, STI, STL, STT, STG, ST2S, STAV) ou de technicien, BEA, BEC, BEI, BES, BEH, BSEC	12	12	12
		Baccalauréat professionnel (ou brevet professionnel ou de technicien ou de maîtrise), diplôme de moniteur-éducateur	12	12	12
		Baccalauréat général (ou brevet supérieur, diplôme des professions sociales et de santé de niveau Bac)	12	12	12
		Capacité en droit, Diplôme d'accès aux études universitaires (DAEU), Examen spécial d'entrée à l'université (ESEU)	12	12	12
C. Greece	**1975 (6 $\rightarrow$ 9)**	**Wave 1 and wave 2**			
		Dimotikó	*6*	*6*	*9*
		Gymnásio (3táxio)	*9*	*6*	*9*
		Genikó í Epangelmatikó Lýkeio (TEL, TEE, Polykladikó) í 6táxio Gymnásio	12	12	12
		IEK	13	12	12
		Wave 6			
		Apolytírio Dimotikoú	*6*	*6*	*9*
		Apolytírio Gymnasíou (3táxio)	*9*	*6*	*9*
		Apolytírio Genikoú Lykeíou	12	12	12
		Ptychío Epangelmatikís Ekpaídefsis	12	12	12
		Apolytírio Epangelmatikoú Lykeíou	12	12	12
		Pistopoiitikó Epangelmatikís Katártisis Epipédou 1	12	12	12
		Díploma Epangelmatikís Katártisis Epipédou metadefterováthmias epangelmatikís katártisis	13	13	13

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...Table 4.A.2 continued

D. Italy	1962 (5 → 8)	**Wave 1, wave 2, wave 5 and wave 6**			
	1999 (8 → 9)	*Esame di seconda elementare*	*2*	*8*	*9*
	2006 (9 → 10)	*Licenza elementare*	*5*	*8*	*9*
		Scuola media o avviamento professionale	*8*	*8*	*9*
		Diploma ginnasiale	10	10	10
		Diploma di scuola professionale, scuola magistrale o istituto d'arte (3 anni)	11	11	11
		Diploma di scuola magistrale o liceo artistico (4 anni)	12	12	12
		Maturità liceale (classico, scientifico, linguistico, artistico)	13	13	13
		Maturità tecnica, professionale o istituto d'arte (5 anni)	13	13	13
E. Netherlands[2]	**1975 (9 → 10)**	**Wave 1 and wave 2**			
	1985 (10 → 12)	*Basisonderwijs*	*6*	*9*	*10*
	2007 (12 → 13)	*VGLO of LAVO*	*10*	*9*	*10*
		Voortgezet (speciaal) onderwijs (b.v. MLK, VSO, LOM, MAVO of MULO)	*10*	*9*	*10*
		HAVO, VWO, Atheneum, Gymnasium, HBS, MMS, Lyceum	12	12	12
		Lager beroepsonderwijs (b.v. LTS, LEAO, Lagere Landen Tuinbouwschool)	*10*	*9*	*10*
		Middelbaar beroepsonderwijs (b.v. MTS, MEAO, Middelbare Landen Tuinbouwschool)	14	14	14
		Hoger beroepsonderwijs (b.v. HTS, HEAO, opleidingen MO-akten)	15	15	15
		Hoger beroepsonderwijs 2e fase (b.v. accountant NIVRA, opleidingen)	16	16	16
		Wetenschappelijk onderwijs (universiteit)	18	18	18
		Leerlingwezen	*10*	*9*	*10*
		Wave 5			
		Alleen basisschool (inclusief speciaal onderwijs)	*8*	*9*	*10*
		LBO, VBO, LEAO, LTS ambachtsschool, huishoudschool, LHNO, LAVO/VGLO, VMBO (niveaus 1-3; basisberoepsgericht, kaderberoepsgericht, gemengd)	*10*	*9*	*10*
		MULO, ULO, MAVO, VMBO (niveau 4; theoretische leerweg); HAVO jaar 3	*10*	*9*	*10*
		HAVO, middelbare school voor meisjes (MMS/MSVM)	12	12	12
		VWO, HBS, atheneum, gymnasium	13	13	13

Continued on next page...

...Table 4.A.2 continued

F. Portugal	1964 (4 → 6)	**Wave 6**			
	1986 (6 → 9)	*Ensino Básico - 1° Ciclo*	*4*	*6*	*9*
	2009 (9 → 12)	*Ensino Básico - 2° Ciclo*	*6*	*6*	*9*
		Ensino Básico - 3° Ciclo	*9*	*6*	*9*
		Ensino secundário científico-humanístico (12°ano, 7° ano dos liceus, propedêutico, serviço cívico)	12	12	12
		Ensino secundário tecnológico	12	12	12
		Ensino secundário artístico especializado	12	12	12
		Ensino secundário profissional	12	12	12
G. Spain	**1970 (6 → 8)**	**Wave 1 and wave 2**			
	1990 (8 → 10)	*Enseñanza primaria, o primera etapa de la EGB, o equivalente*	*6*	*6*	*8*
		Bachillerato elemental, EGB, Graduado escolar, o equivalente	*10*	*6*	*8*
		Bachillerato superior, BUP, o equivalente	12	12	12
		Pre-universitario o COU	13	13	13
		Estudios técnicos no superiores, FP, o equivalente	12	12	12
		Wave 5 and wave 6			
		Certificado de estudios primarios, hasta 5° de EGB, o educación primaria (LOGSE) (tambi én Grado Elemental en Música y Danza)	*6*	*6*	*8*
		Bachillerato elemental, Graduado escolar, EGB o ESO (tambi én Grado Medio en Música y Danza)	*10*	*6*	*8*
		Bachillerato superior, BUP, o Bachillerato (LOGSE)	12	12	12
		Pre-universitario (PREU) o COU	13	13	13

Notes: The table reports country-specific compulsory schooling reforms and the changes in the number of compulsory years of schooling induced by the reforms (taken from Hofmarcher 2019, 2021) in column (2). Reforms in bold are those considered in this study (see Table 3.2). Country-specific school degrees (as in the SHARE questionnaire) and the number of years usually taken to obtain a certain degree are shown in columns (3) and (4) (see SHARE (2011) for waves 1 and 2, and ISCED 1997 (OECD 1999) for waves 5 and 6). Columns (5) and (6) report years of schooling by reform exposure status. School degrees and years of schooling indicated in italics show the school types/degrees affected by the reforms.

[1] In Belgium education is compulsory from age 6 to 18. However, students must attend full-time compulsory education only until age 15. From age 15 to 18, students may continue in part-time education (Hofmarcher 2019, 2021). Therefore, I assigned $1 + \frac{3}{2} = 2.5$ additional years of schooling for affected cohorts.

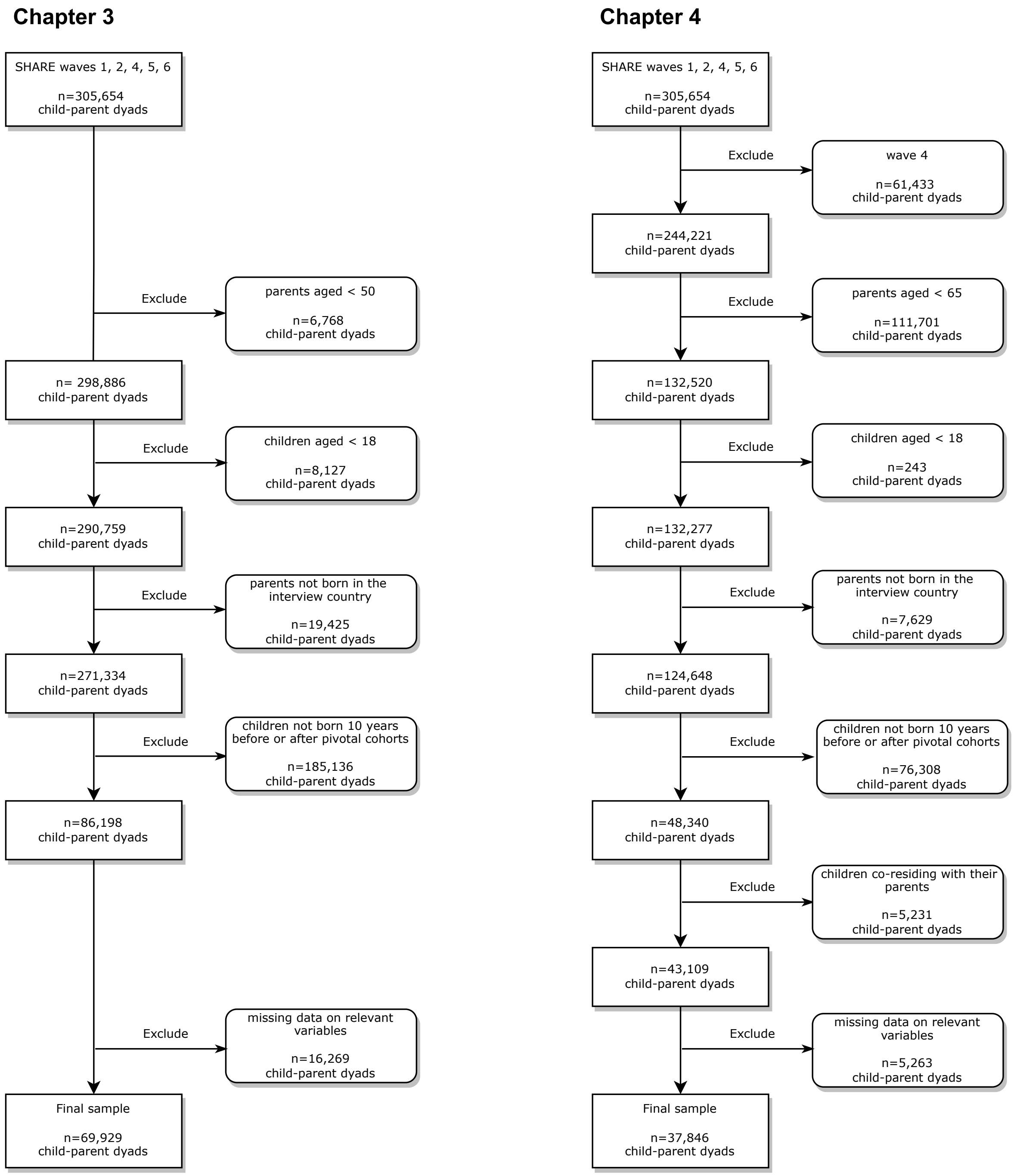
[2] In the Dutch questionnaire of waves 1 and 2, school degrees and higher educational degrees were jointly asked.

Table 4.A.3: Cross-table of parents' long-term care dependency and utilisation

	Parents' long-term care utilisation		
Parents' long-term care dependency	Yes	No	$\sum$
Yes	7,837 (20.71%)	4,066 (10.74%)	11,903 (31.45%)
No	6,249 (16.51%)	19,694 (52.03%)	25,943 (68.55%)
$\sum$	14,086 (37.22%)	23,760 (62.78%)	37,846 (100%)

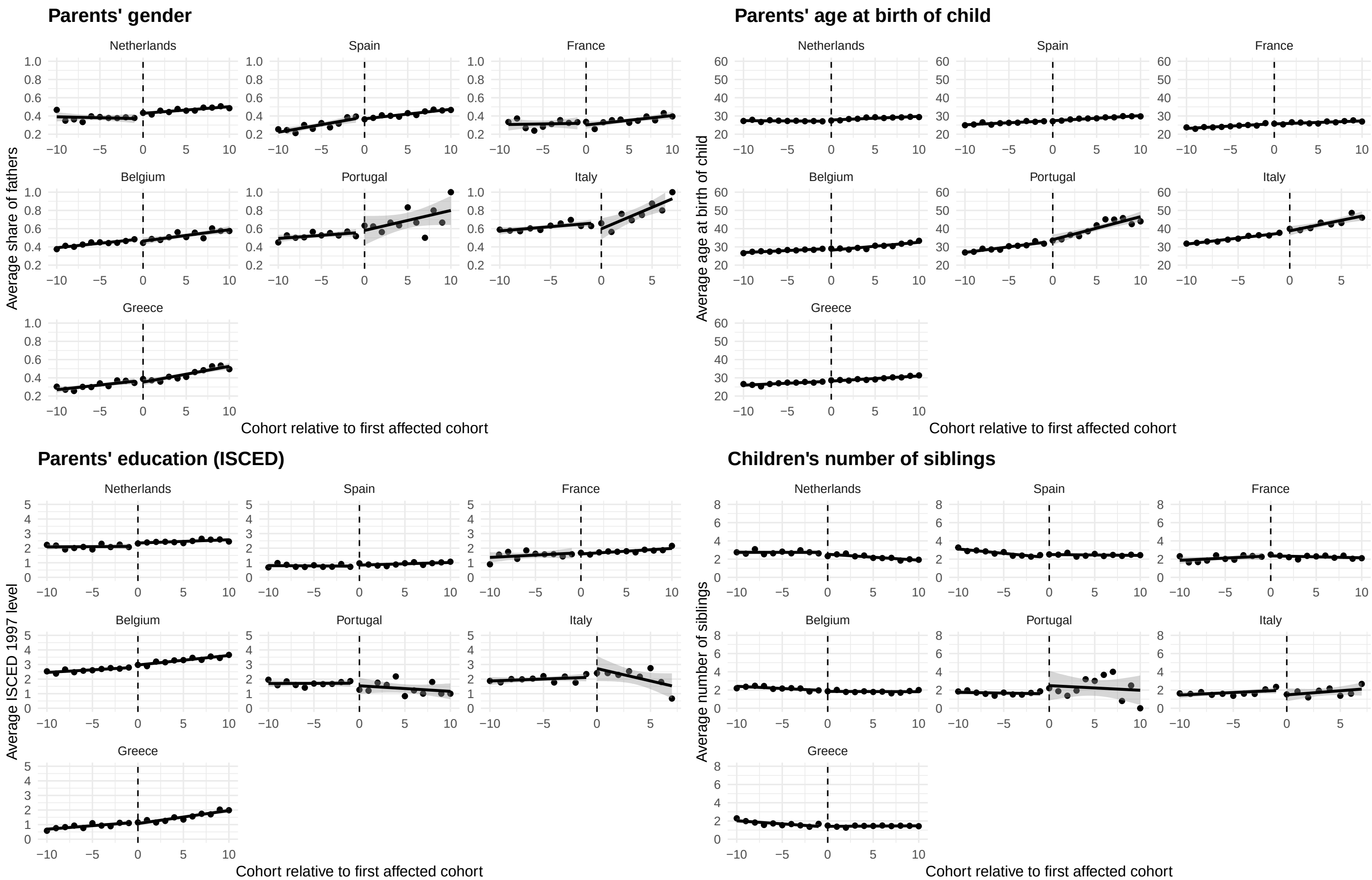
Notes: The table cross-tabulates parents' long-term care dependency and long-term care utilisation. "Long-term care dependency" means that a parent reports to have at least one ADL or IADL limitation. "Long-term care utilisation" means that a parent has received either formal care, informal care or a combination of both. The table shows that the sample includes not only parents with so-called "met needs" (21%) but also parents with "unmet needs" for long-term care (11%). Moreover, the sample consists of parents reporting neither ADL or IADL limitations nor long-term care utilisation (52%), and parents reporting no ADL or IADL limitation but some long-term care utilisation (17%).
Source: Own calculations based on SHARE, waves 1, 2, 5 and 6.

Figure 4.A.1: Flow-chart of sample selection in this chapter compared to Chapter 3



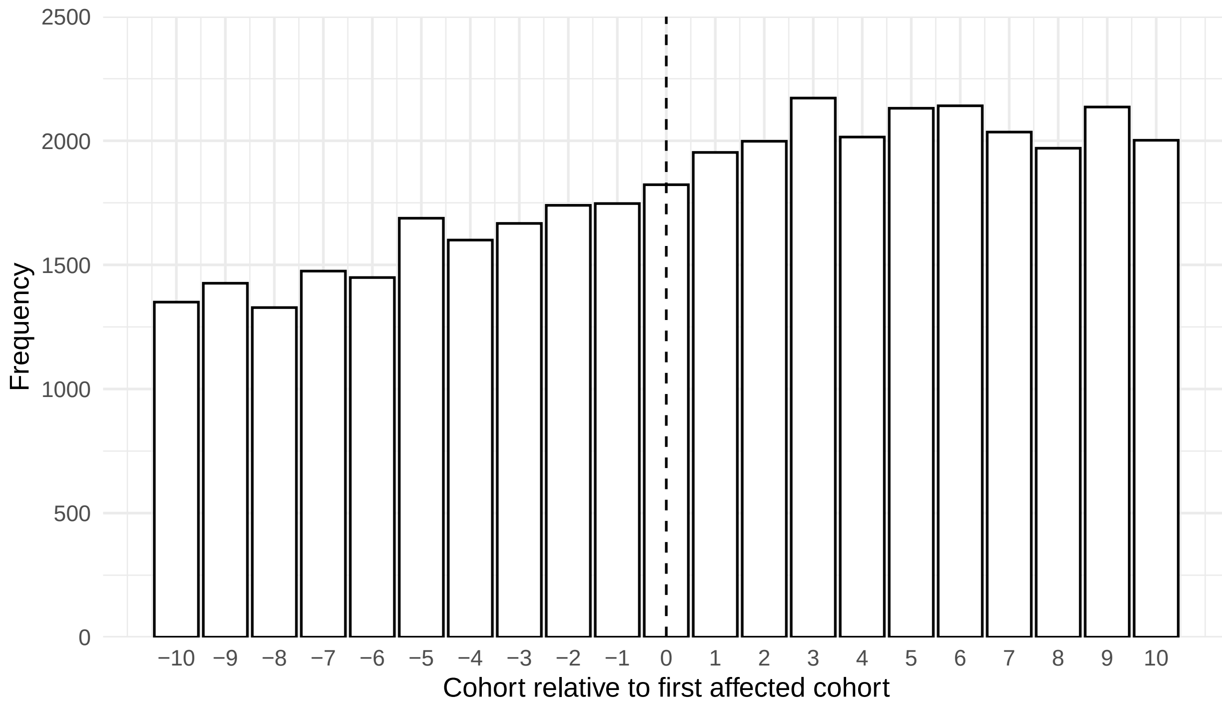
Notes: The figure shows how the sample in this chapter differs from the sample used in Chapter 3. In Chapter 3, the initial SHARE sample of 305,654 child-parent dyads is restricted to parents aged 50 years or older, who were born in the country of interview or who immigrated there before the age of 5, and to children aged 18 years or older, who were born 10 years before or after the pivotal cohorts of the compulsory schooling reforms. After excluding missing values on relevant variables, the final sample in Chapter 3 includes 69,929 child-parent dyads. The sample selection in Chapter 4 is very similar, but there are three exceptions. First, wave 4 cannot be included. Second, the sample is restricted to parents aged 65 years or older. Third, co-residing children are excluded. The final sample in Chapter 4 consists of 37,846 child-parent dyads.
Source: Own illustration.

Figure 4.A.2: Distribution of pre-treatment covariates, by country



Notes: The figure shows, separately by country, the distribution of pre-treatment covariates (parents' gender, parents' age at birth of child, parents' education, children' number of siblings) for cohorts of children born 10 years before and after the pivotal cohorts of the compulsory schooling reforms. While the figures for Italy and Portugal are quite noisy due to the relatively small number of observations per country and cohort, especially to the right of the cut-off, the figures are generally consistent with the aggregated analysis illustrated in Figure 4.1.
Source: Own calculations based on SHARE, waves 1, 2, 5 and 6.

Figure 4.A.3: Histogram of the assignment variable



Notes: The figure shows a histogram of the assignment variable (child's year of birth) for cohorts of children born 10 years before and after the pivotal cohorts of the compulsory schooling reforms. Each bar represents the number of observations in all cohorts in the different countries, which are at the same distance from the pivotal cohorts.
Source: Own calculations based on SHARE, waves 1, 2, 5 and 6.

Table 4.A.4: Modelling choices for specification curve analysis

First set of specifications	- Pivotal cohorts included - Parents without limitations included - Parents without care receipt included - Parents without "met need" included - Co-residing children excluded This specification is estimated for 5 bandwidths $\times$ 4 functional forms $\times$ 3 country-specific cohort trends = 60 specifications
Second set of specifications	- Pivotal cohorts **excluded** - Parents without limitations included - Parents without care receipt included - Parents without "met need" included - Co-residing children excluded This specification is estimated for 5 bandwidths $\times$ 4 functional forms $\times$ 3 country-specific cohort trends = 60 specifications
Third set of specifications	- Pivotal cohorts included - Parents without limitations **excluded** - Parents without care receipt included - Parents without "met need" included - Co-residing children excluded This specification is estimated for 5 bandwidths $\times$ 4 functional forms $\times$ 3 country-specific cohort trends = 60 specifications
Fourth set of specifications	- Pivotal cohorts included - Parents without limitations included - Parents without care receipt **excluded** - Parents without "met need" included - Co-residing children excluded This specification is estimated for 5 bandwidths $\times$ 4 functional forms $\times$ 3 country-specific cohort trends = 60 specifications
Fifth set of specifications	- Pivotal cohorts included - Parents without limitations included - Parents without care receipt included - Parents without "met need" **excluded** - Co-residing children excluded This specification is estimated for 5 bandwidths $\times$ 4 functional forms $\times$ 3 country-specific cohort trends = 60 specifications
Sixth set of specifications	- Pivotal cohorts included - Parents without limitations included - Parents without care receipt included - Parents without "met need" included - Co-residing children **included** This specification is estimated for 5 bandwidths $\times$ 4 functional forms $\times$ 3 country-specific cohort trends = 60 specifications

Notes: The table summarizes the modelling choices for the specification curve analysis. The specification curve includes in total $6 \times 60 = 360$ different specifications.
Source: Own illustration.

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