Does Compulsory School Leaving Age Affect the Intergenerational Transmission of Education? Evidence from Four European Countries

This version: September 19, 2019

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Abstract

Reforms in compulsory school leaving age are candidates for policies affecting the intergenerational transmission of advantage. I propose that the societal contexts in which reforms occur may moderate the effects of these reforms on intergenerational mobility as well as that there may be gender differences in these effects. In order to test these hypotheses, I estimate cross-country variation and gender differences in the effects of increases in compulsory school leaving age on intergenerational educational mobility using data on four European countries. I employ data from the European Social Survey (ESS) and the Survey of Health, Ageing and Retirement in Europe (SHARE) on Austria, Denmark, France, and the Netherlands. Results suggest that increasing compulsory school leaving age did increase educational mobility for men and women in France but left the intergenerational transmission of education unaffected for men and women in Austria, Denmark, and the Netherlands. These findings show that the effects of reforms in compulsory school leaving age on intergenerational mobility can be moderated by the societal contexts in which these reforms occur. Results are in line with a hypothesis according to which the effects of compulsory school leaving age reforms are more pronounced in contexts of low educational attainment.

Keywords

Even in advanced, industrialized Western societies, men and women from different socioeconomic backgrounds do not have the same chances to achieve a high level of education (Breen and Jonsson 2005; Torche 2015). Notwithstanding the existence of educational inequalities in contemporary societies, there has been an increase in intergenerational educational mobility across generations in many advanced, industrialized Western societies (Breen et al. 2009). These findings lead to the question what caused these changes in educational mobility? Answering this question is important for designing policies that can reduce educational inequalities—a policy aim shared by many parties across the political spectrum.

Many sociologist have noted that declines in educational inequalities have coincided with the process of educational expansion in many Western societies (Ballarino et al. 2009; Bloome, Dyer, and Zhou 2018; Breen and Jonsson 2007; Breen 2010; Hout 1988; Pfeffer and Hertel 2015; Pölyliö, Erola, and Kilpi-Jakonen 2018). Descriptive research describing the simultaneous evolution of educational expansion and changes in inequality in educational opportunity, whilst being suggestive, can, however, not identify the causal relations between these two processes, as there are unobserved variables that can confound the relationship between educational expansion and educational mobility. To identify the causal effects of educational expansion on educational mobility, research designs are needed that can identify the effect of an increase in educational attainment on the association between social origin and educational attainment controlling for the influence of unobserved, confounding variables.

Educational reforms implemented in specific countries provide natural experiments that can be employed for such a purpose (Oreopoulos, Page, and Stevens 2006; Raudenbush and Eschmann 2015). These reforms were implemented from one cohort to another allowing researchers to estimate the effect of each reform by comparing the first cohort affected by the reform to the cohort immediately preceding the reform. There are four previous studies that

investigated the effects of increases in compulsory school leaving age on intergenerational educational mobility in England and Wales (Sturgis and Buscha 2015), Germany (Betthäuser 2017), and the United States (Rauscher 2014, 2016). These studies led to diverging results finding a decrease in the association between father's occupation and school attendance in the United States (Rauscher 2014, 2016), an increase in educational mobility for some groups but stability for others in Germany (Betthäuser 2017), and largely no change in intergenerational educational mobility in England and Wales (Sturgis and Buscha 2015).

A possible explanation for these diverging findings is that these differences could be caused by the institutional set-ups of societies moderating the effects of reforms in compulsory school leaving age on educational mobility. Differences in findings could, however, also be due to differences in methodological approaches and operationalizations of variables used in the empirical analyses in different studies. To discriminate between these two possibilities, I estimate the causal effects of reforms in compulsory school leaving age on intergenerational mobility using the same analytical strategy in four European countries. I analyze the effects of reforms that occurred in Austria, Denmark, France, and the Netherlands after the Second World War. I apply the same methodological approach and operationalization of variables to pooled data from the European Social Survey (ESS) and the Survey of Health, Ageing and Retirement in Europe (SHARE). These data are particularly suited for comparative analyses, as they collect the same information for representative samples of respondents from different European countries. I selected all countries in which reforms in compulsory school leaving age occurred that affected a large enough number of respondents included in the data to conduct a reliable analysis and in which the reforms were implemented at the national level.[1] In addition, I limited the case selection to reforms that changed compulsory school leaving age but that did not provide other radical changes to the educational system. This case selection criterion is necessary, as I am interested in isolating the effects of reforms in comprehensive school leaving

age, and otherwise these effects could be confounded by the consequences of other changes in the respective educational systems, e.g. reforms of the allocation to tracks or of national curricula. Because of this latter sample selection criterion, the number of cases I look at is much lower than the number of cases in other studies that looked at the consequences of reforms in compulsory school leaving age (e.g., Braga et al. 2014; Fort et al. 2016; Schneeweis et al. 2014).[2]

An important advantage of the comparative approach I advance in this manuscript is that it allows me to increase the generalizability of findings. Generalizability is a common problem that occurs if one natural experiment is studied in isolation and is the main limitation that plagues this type of research (Torche 2015). The findings of this manuscript provide a more robust case to draw general conclusions from policy reforms than the analysis of a single case. For this reason, the methodological contribution of this manuscript goes beyond the question whether changes in compulsory school leaving age affect the intergenerational transmission of advantage. It also proposes that analyzing several instead of one natural experiment increases the external validity of analyses of the effects of political reforms on processes of sociological interest.

I also investigate gender differences in the effects of reforms in compulsory school leaving age on intergenerational educational mobility. Given that educational expansion and the reforms I study affected women more than men (see the results), there are good reasons to expect that these reforms decreased the intergenerational transmission of advantage more for women than for men. Analyzing gender differences is another important dimension of potential heterogeneity in the effects of political reforms on intergenerational mobility, which previous research, with the notable exception of Rauscher (2014), has not investigated.

Do Reforms in Compulsory School Leaving Age Affect Intergenerational Educational Mobility?

Education is an important predictor of life chances in contemporary societies (Heckman 2000). Educational reforms are therefore an important political topic in public debates in many countries. From a historical perspective, however, the most consequential educational reforms in Western societies have already taken place in the last century, in particular in the time after the Second World War. Given that these reforms changed the educational systems in most Western societies more than any educational reforms carried out today, I expect these educational reforms to have had stronger effects on intergenerational mobility than contemporary educational reforms, if educational reforms have indeed the power to alter the intergenerational transmission of advantage. Reforms in compulsory school leaving age were arguably the most important drivers of educational expansion in Western societies in the 20th century.[3]

The analysis of the effects of increases in compulsory school leaving age on intergenerational mobility can therefore be understood as providing upper bound estimates of the consequences of educational reforms for the transmission of advantage across generations. In this manuscript, I focus on educational reforms that increased the compulsory school leaving age in four European countries. These reforms were all implemented after the Second World War.

Why may reforms in compulsory school leaving age affect the intergenerational transmission of advantage? Sturgis and Buscha (2015) speculated that a later compulsory school leaving age leads to an increase in educational mobility because children from socioeconomically advantaged families would have stayed in the educational system beyond the compulsory school leaving age even in the absence of any reform. Betthäuser (2017) developed this argument more extensively. His theory is in the tradition of rational choice

approaches to educational inequalities according to which different social origin groups have different chances to succeed in education (Boudon 1973; Breen and Goldthorpe 1997; Erikson and Jonsson 1996). Changes in compulsory school leaving age affect the educational decision-making of these groups, as they reduce the costs of completing a higher educational degree. This is the case because increasing compulsory school leaving age, whilst leaving the age needed to complete a certain degree unaffected, shortens the time between compulsory school leaving age and the completion of this degree.

In addition, changing compulsory school leaving age may increase the success probabilities of children from socioeconomically disadvantaged families, as students who stay longer in the educational system can become better aware of their own abilities (Erikson and Jonsson 1996). At the same time, the influence of parents on educational decision making may decrease with increasing age of children. Increasing compulsory school leaving age may therefore lead to students making more often their own educational choices.

Besides rational choice theories, there are other theories about the intergenerational transmission of educational advantage that predict effects of reforms in compulsory school leaving age on educational inequalities. The cultural capital hypothesis focuses on the cultural differences between children from socioeconomically advantaged and children from socioeconomically disadvantaged families to explain educational inequalities (Bourdieu and Passeron 1964, 1970; Willis 1979). One may expect that the cultural resistance to education in socioeconomically disadvantaged families is reduced by additional exposure to schooling, in particular when this additional exposure includes other children from socioeconomically disadvantaged families. This theoretical perspective leads us therefore also to expect increases in educational mobility due to changes in compulsory school leaving age.

A third reason why increases in compulsory school leaving age may reduce educational inequalities comes from research on inequalities in educational performance. This research has

shown that inequalities in academic performance emerge in early childhood and are reduced by education (Heckman 2000; Raudenbush and Eschmann 2015). The arguably best causal evidence for this hypothesis comes from research showing that educational inequalities grow more in summer when no schooling takes place than during the school year (e.g., Downey, von Hippel, and Broh 2004; von Hippel, Workman, and Downey 2018). This perspective supports the notion that extending compulsory schooling by one or more years reduces socioeconomic gaps in academic performance.

A fourth pathway through which compulsory school leaving age can affect educational mobility is that compulsory schooling can be perceived as a particularly crucial form of tracking. From this perspective, extending compulsory schooling by one year is similar to postponing tracking by one year. As tracking is widely believed to affect educational inequalities (Braga, Checchi, and Meschi 2013; Brunello and Checchi 2007; Hanushek and Wößmann 2006; Horn 2009; Pekkarinen, Uusitalo, and Kerr 2009; Pfeffer 2008, 2015; van de Werfhorst 2018; van de Werfhorst and Mijs 2010), increasing compulsory school leaving age may reduce educational inequalities, as it mechanically changes the age at tracking.

Are there any reasons why reforms in compulsory school leaving age may not affect educational inequalities? This depends on which social group is most affected by these reforms. The four arguments developed above assume that all social groups are affected to the same degree by these reforms or that socioeconomically disadvantaged families are affected more than socioeconomically advantaged families. However, this does not necessarily have to be the case. What if the reforms increased mostly the educational attainment of low performing children from socioeconomically advantaged families? This is precisely what Rauscher (2016) argued. She referred to Collins' (1971) conflict theory, which stated that socioeconomically advantaged families obtained even more schooling if children from disadvantaged families increased their educational attainment. Such an argument is also in line with maximally

maintained inequality theory (Raftery and Hout 1993), which argues that a reduction in educational inequalities takes place only when one educational level becomes universal. In addition, equally maintained inequality theory (Lucas 2001) argues that reductions in educational inequalities at one educational level lead to increases in educational inequalities at higher levels of education, or to increased qualitative differences at the same level of education. With respect to compulsory school leaving age, this theory predicts therefore that reforms in compulsory school leaving age increase educational mobility at a lower but decrease mobility at a higher level of education. Therefore, reforms in compulsory school leaving age may not affect inequalities in final educational attainment. This set of theories leads to the hypothesis that increasing compulsory school leaving age does not affect or does even reduce intergenerational educational mobility.

Findings from previous research testing these hypotheses are mixed. Sturgis and Buscha (2015) found that the 1972 compulsory school leaving age reform in England and Wales, which raised the compulsory school leaving age from 14 to 15, reduced socioeconomic inequalities in education for one out of six levels of education they distinguished. They showed that increases in education at this level were strongest for children with parents in the middle of the distribution of occupational status. Children from the most disadvantaged families increased their education due to the educational reform less than children from these intermediate status families. For the other five levels of education, however, socioeconomic inequalities in education were not affected by the reform in compulsory school leaving age. Therefore, this study provides evidence that intergenerational educational mobility was largely not affected by the increase in compulsory school leaving age in England and Wales in 1972.

Betthäuser (2017) found that intermediate social classes improved their educational attainment compared to the most advantaged group (the service class in the EGP class schema [Erikson and Goldthorpe 1992], which he employed) due to the increase in compulsory school

leaving age in Germany. The most disadvantaged social group in his study, the children of unskilled workers, could, however, not decrease their disadvantage in educational attainment compared to the service class through the reform. The findings of this study provide therefore some evidence for an increase in educational mobility but demonstrate also that the second most advantaged group, which is also the second least disadvantaged group, profited most from the reform in compulsory school leaving age in Germany.

Rauscher (2014, 2016) found an increase in educational mobility attributable to the introduction of compulsory schooling laws in the United States in the 19th century. She could not observe educational attainment directly but she found that the association between father's occupational status and school attendance declined due to the educational reform. Contrary to the findings by Sturgis and Buscha (2015) as well as Betthäuser (2017), there were no nonlinearities in the increase in educational mobility in the United States. Of course, it is difficult to compare Rauscher's (2014, 2016) studies to the others, as she studied reforms that introduced a compulsory school leaving age whilst the other studies and the analysis reported in the present manuscript estimate the effects of reforms that changed the compulsory school leaving age in countries in which compulsory schooling laws were already in place.[4]

Variation in the Effects of Reforms in Compulsory School Leaving Age on Educational Mobility across Countries

There are two main hypotheses about the way in which the effects of reforms in compulsory school leaving age reforms may vary across countries. First, the reforms may be moderated by institutional characteristics, in particular by the selectivity of the education system. Second, the effects of reforms may be a function of the general level of educational attainment in a society prior to the reform. In the following, I motivate these two hypotheses and discuss the predictions derived from these hypotheses about how the effects of the reforms in compulsory

school leaving age vary across the countries included in my analysis (Austria, Denmark, France, and the Netherlands).

The first hypothesis is motivated by the idea that advanced, industrialized Western societies vary in their welfare (Esping-Andersen 1990, 1999) and education regimes (Busemeyer and Nikolai 2010). From a rational choice perspective, institutional differences across countries can lead to cross-country differences in the costs associated with education. In some countries, education is free and universal whilst other educational systems are more selective (Busemeyer and Nikolai 2010). From this perspective, changes in compulsory school leaving age may increase educational mobility more in more than in less selective educational systems, as in the former the reduction in costs are larger. According to this perspective, we expect therefore the strongest increases in educational mobility due to the reforms in compulsory school leaving age in Austria, followed by the Netherlands, followed by Denmark and France.

Second, the effects of reforms in compulsory school leaving age on educational mobility may vary as a function of educational expansion. These reforms may increase educational mobility more in societies in which the level of educational attainment is low at the time the reforms are implemented. With increasing level of educational attainment, the gains of reforms in compulsory school leaving age in terms of educational mobility may be reduced. As can be seen from the descriptive statistics reported in Table 1, educational attainment in the pre-reform cohort was, on average, lowest in France, followed by Austria. Educational attainment was considerably higher in Denmark and the Netherlands. From the perspective of this second hypothesis about cross-country variation in the effects of educational reforms, we can therefore expect stronger effects of the reforms on educational mobility in France and in Austria than in Denmark and in the Netherlands.

Results of previous research are more in line with the second than with the first explanation of cross-country variation. There was only an unequivocal increase in educational mobility due to the introduction of compulsory schooling laws in the United States (Rauscher 2014, 2017). The increases in compulsory schooling leaving ages in societies with existing compulsory schooling laws had less effects on educational mobility (Betthäuser 2017; Sturgis and Buscha 2015). Because of the limited number of cases and the different analytical strategies used by previous studies, it is, however, difficult to verify or refuse any of the two hypotheses about cross-country variation. To rule out the influence of methodological differences in study designs and to enlarge the number of cases, I estimate cross-country differences in the effects of reforms in compulsory school leaving age on educational mobility using the same research design and comparable data on four European countries.

Gender Differences in the Effects of Compulsory School Leaving Age on Educational Mobility

Educational expansion in Western societies was accompanied by another social transformation of great consequence: the increase in female educational attainment (Buchmann and DiPrete 2006; Buchmann, DiPrete, and McDaniel 2008). Educational expansion in general and the educational reforms I study in particular often affected women more than men.

Why may changes in compulsory school leaving age affect differently men and women? From theories that assume that educational expansion increases intergenerational educational mobility, we may expect socioeconomic differences in educational attainment to decrease more for women than for men because these educational reforms often affected women more than men.

Contrary to these expectations, Rauscher (2014) argued that socioeconomic differences in school attainment should be reduced more by compulsory schooling laws for boys than for

girls. She argued that men had the higher earnings potential and therefore they should be affected more by the reduction in the costs of education and in the opportunity costs brought about by the introduction of compulsory schooling laws. Her empirical analysis found, in line with her theoretical expectations, that the reduction in the association between parental occupational status and school attainment caused by the reform was concentrated among boys in the North in the United States.

Other studies than Rauscher (2014) did not investigate gender differences in the variation in the effects of reforms in compulsory school leaving age on intergenerational educational mobility. Rauscher (2016) investigated only how compulsory school attendance laws affected the association between fathers' and sons' occupational statuses. Betthäuser (2017) included both men and women in his analyses without looking at gender differences. Sturgis and Buscha (2015) analyzed gender differences in occupational mobility finding that the 1972 reform in England and Wales did neither affect occupational mobility for men nor for women. They did, however, not analyze gender differences in intergenerational educational mobility.

Data and Methods

Data

The empirical analyses presented in this manuscript uses data on four European countries from the European Social Survey (ESS) and the Survey of Health, Ageing and Retirement in Europe (SHARE). The ESS samples the adult population in several European countries since 2001 (European Social Survey 2018). I use data from waves 1 to 8 (European Social Survey Cumulative File, ESS 1–8 2018). The SHARE samples the population aged 50 and older and their partners, who can be younger, in several European countries. This paper uses data from the seven SHARE Waves that are currently available (Börsch-Supan et al. 2013).[5] I pool observations from both data sets to increase the sample sizes underlying the analyses for each

country, allowing me to obtain more precise estimates.[6] In order to ensure that the men and women in my study were affected by the reforms, I limit the samples in all countries to respondents who were born in the country in which they participated in the interview.

Variables

Years of Education. The outcome variable in all models is respondent's final educational attainment, measured through years of education. Using this variable has the advantage that it allows me to take into account the whole distribution of education and not only specific educational transitions. Both data sets provide self-reported years of education and ISCED classifications of final educational attainment. I use the ISCED classifications, recoded into the years of education needed to obtain a certain degree, to correct the information from the self-reported years of education (see Schneeweis et al. 2014 for a similar procedure). For respondents without valid ISCED information, I use the self-reported years of education.

Social Origin. I measure social origin using parental education defined as the highest level of education of either parent with valid information. This practice follows the dominance approach (Erikson 1984). I distinguish between a low level of education (no formal degree and ISCED 1 and 2), a medium level of education (ISCED 3 and 4), and a high level of education (ISCED 5 and 6). In all regression models, a medium level of parental education is the reference category. I use information on parental education as a measure of social origin because this information is available in both data sets.

Reform. I identify exposure to the reform based on respondent's year of birth. I drop respondents who were in the first birth year affected by the reform because often only a part of a birth year was affected by a reform (Sturgis and Buscha [2015] used the same approach). I define a dummy variable that is coded as one for all respondents who were born in the second birth year that was affected by the reform and the seven birth years after. I define the dummy

variable as zero for all respondents who were born in the eight birth years immediately preceding the first birth year affected by the reform. All other respondents are also dropped from the specific country samples. I provide a robustness check using a smaller bandwidth of four years in both directions from the reform (again dropping the respondents from the first birth year affected by the reform) in Tables S1 to S4 in Appendix B in the *Online Supplement*.

The reform variable is defined using different birth years in different countries. This is necessary as the reforms in compulsory school leaving age were introduced in different years in different countries. For that reason, any variation between countries can also be understood as cross-time variation. Table 1 gives an overview of the reforms used in the analysis and the precise birth years included in the before- and after-reform cohorts in the different countries. More details on the reforms and the rational for defining the cohorts in the ways described in Table 1 are provided in Appendix A in the *Online Supplement*.

[INCLUDE TABLE 1 ABOUT HERE]

Gender. I report separate results for men and women along with results on the combined samples. In the combined samples, I always control for gender through a dummy variable that is coded as one for male respondents.

Country. I report separate results for the four countries included in the analysis: Austria, Denmark, France, and the Netherlands. The descriptive statistics of the variables used in the analysis are reported separately for each country in Table 2.

[INCLUDE TABLE 2 ABOUT HERE]

In addition to reporting descriptive statistics on the full sample, Table 2 reports descriptive statistics on the before- and the after-reform cohorts. These descriptive statistics allow us to judge whether these cohorts differ on observed variables other than educational attainment. As can be seen from this table, this is not the case. This supports the notion that the central assumption of the analytical strategy used in this manuscript holds, according to which the before- and the after-reform cohorts differ only in their level of educational attainment.

Analytical Strategy

The causal effect of increasing compulsory school leaving age on intergenerational educational mobility is identified in the empirical analyses reported below comparing the first cohort affected by the reform to the immediately preceding cohort. The difference in the association between parental and respondents' education between both cohorts is the estimate of the causal effect of the reform on educational mobility.

In each country, I estimate the following OLS regression models with respondent's education Edu_C, parental education Edu_P, and the reform dummy Ref:

$$Edu_C = \alpha + \beta_1\ Edu_P + \beta_2\ Ref + \beta_3\ Edu_P\ X\ Ref + \beta_4\ Male + \varepsilon \tag{1}$$

The interaction term Edu_P X Ref is the estimate of the causal effect of the specific educational reform on intergenerational mobility. I focus on this term in the interpretation of the results of the analyses.

The causal identification of the effects of the reforms rely on the assumption that the cohort immediately proceeding and the cohort immediately preceding the reform do not differ in any other aspect than being exposed to the reform. To increase the plausibility of this assumption, it is important to limit the analysis to a short bandwidth around the discontinuity

created by the reform but there is a trade-off between a shorter bandwidth to increase identification and a larger bandwidth to increase the precision of estimates through more observations (Schneeweis et al. 2014; Sturgis and Buscha 2015). I therefore use two different bandwidths, which lead to virtually identical estimates.[7]

The following analyses also have to assume that the Stable Unit Treatment Value Assumption (SUTVA) is not violated. SUTVA states that the values of one respondent are not affected by the values of the other respondents (Gangl 2010; Morgan and Winship 2015). This assumption is more plausible with respect to educational than with respect to occupational mobility. The educational attainment of those not affected by the reform is unlikely to have changed due to the reform. In terms of occupational attainment, the general increase of education in the population could have, however, led to lower occupational attainment for those who were not affected by the reform, as the higher educated, later cohorts had advantages over them in the labor market. For that reason, SUTVA may not hold when studying occupational outcomes (Betthäuser 2017; Rauscher 2016; Sturgis and Buscha 2015). This is, however, less of a problem for the study of educational attainment as an outcome.

Results

I report the results separately for each country. I do not pool the data across countries, as I want to test whether the reforms affected educational mobility within each country. I then compare these results across countries. This research strategy is very different from the one employed by studies that pool data on different countries and therefore ignore heterogeneity in the effects of policies across countries (e.g., Braga et al.'s [2013] analysis of the effects of different educational policies and van de Werfhorst's [2018] analysis of the effects of tracking).

Table 3 reports the results for Austria. There are strong positive associations between parental education and children's education in Austria. At the population level, children from

low educated parents have, on average, around 1.06 years of education less than children from medium educated parents (the reference group) in Austria (Model 1). Children with highly educated parents have, on average, 1.61 more years of education than children with medium educated parents. These associations are similar between men and women.

[INCLUDE TABLE 3 ABOUT HERE]

The reform in compulsory school leaving age in Austria had a small positive effect on education. However, the reform affected educational attainment, at the population level, only for women but not for men. The reform did not change the intergenerational transmission of advantage for women, as the interactions between the reform and parental education are small and statistically insignificant (Model 4). This finding is, however, not true for men (Models 4 and 6). For men, the estimates indicate an *increase* in educational mobility due to the reform comparing men with low to men with medium educated parents. This result is statistically significant. At the same time, the reform did, however, *decrease* educational mobility between men with medium and men with highly educated parents. Therefore, the reform in compulsory school leaving age in Austria led to no general increase or decrease in educational mobility, neither for men nor for women.

Table 4 reports the results for Denmark. On average, children from low educated families have 0.94 less years of education than children from medium educated families (Model 1). Children with highly educated parents have, on average, 1.33 years more of education than children with medium educated parents. The associations between parental and own education are weaker for women than for men in Denmark, although gender differences are statistically not significant.

[INCLUDE TABLE 4 ABOUT HERE]

The effects of the reform in compulsory school leaving age on educational attainment, at the population level, are small in Denmark and, as in Austria, concentrated among women. Model 2 suggests that the reform did not change the intergenerational transmission of advantage as the interaction between parental education and the reform in compulsory school leaving age is close to zero and statistically not significant. This finding is confirmed in the separate analyses for men and women. If both gender are analyzed separately, no statistically significant change in the intergenerational transmission of education due to the reform is found (Models 4 and 6). Due to the large confidence intervals, small changes cannot be ruled but it is quite clear that the reform in compulsory schooling age did not have strong effects on the intergenerational transmission of educational advantage in Denmark.

Table 5 reports the results for France. In France, children with low educated parents have, on average, 1.88 less years of education than children with medium educated parents (Model 1). On average, highly educated parents have children with 2.11 years of education more than medium educated parents. There are virtually no differences in these associations between men (Model 5) and women (Model 3).

[INCLUDE TABLE 5 ABOUT HERE]

The reform in compulsory school leaving age in France had an effect on educational attainment, which was, as in the case of Austria and Denmark, concentrated, at the population level, among women. Contrary to these two cases, the reform did, however, affect educational mobility in France. In the sample combining men and women, the estimates of changes in the associations between parental and children's education caused by the reform are large and

statistically significant (Model 2). This is also true if the sample is restricted to women (Model 4) or men (Model 6) in the children's generation. For both men and women, the reform in compulsory school leaving age reduced the difference in educational attainment between children with low and children with medium educated parents. These changes are substantial. For instance, women from low educated families had, on average, 2.32 years of education less than women from medium educated families before the reform. This difference was reduced by the reform to 2.32 – 0.89 = 1.43 years of education, i.e. a reduction by 38 percent. For men, the difference changed due to the reform from 2.40 years of education to 2.40 – 0.88 = 1.52 years of education. The increase in educational mobility was therefore of a very similar size for men and women.

At the same time, the differences between children with medium and children with highly educated parents did not change. Admittedly, the estimate for women is large and negative. It is, however, statistically not significant and therefore does not allow us to generalize a reduction in the intergenerational persistence between female children with medium and highly educated parents outside of the analyzed sample. In any case, the reform in compulsory school leaving age in France increased overall educational mobility.

Finally, Table 6 reports the results for the Netherlands.[8] On average, children with low educated parents have 1.28 years of education less than children with medium educated parents in the Netherlands. 1.10 years of education more, on average, have children with medium educated parents than children with highly educated parents. There are virtually no gender differences in the associations between parental and children's education in the Netherlands.

[INCLUDE TABLE 6 ABOUT HERE]

The reform in compulsory school leaving age in the Netherlands had small positive effects on educational attainment. The increase in educational attainment due to the reform was, as in Austria, Denmark, and France, concentrated among women. For both men and women in the Netherlands, increasing compulsory school leaving age did, however, not affect educational mobility. All six interaction terms are close to zero and statistically insignificant (Models 2, 4, and 6). They also point in different directions with some interaction terms indicating an increase, some a decrease, and some no change in educational mobility. Therefore, the reform in compulsory schooling leaving age in the Netherlands had no clear, robust effect on educational mobility.

Robustness Checks

In the analyses presented above, the causal effects of increasing compulsory school leaving age on educational mobility in the different countries are identified by comparing the first cohort affected by the reform to the immediate preceding cohort, which was not yet affected by the reform. One concern with this before-after-comparison is that simultaneously occurring time trends could confound the identification of the causal effects of the reforms in compulsory school leaving age on educational mobility.

To address this concern, I estimated the models reported above with a smaller bandwidth around the reform. In Tables S1 to S4 in the *Online Supplement*, I report models which use four instead of eight years to define the before- and the after-cohorts, again excluding the first cohorts affected by the reform in each country. These models support all findings of the main specification. In France, educational mobility changed for both men and women between the group with low and the group with medium educated parents due to the reforms in compulsory school leaving age carried out in these countries. The interaction between the reform and low parental education is not statistically significant for women but the estimate is similar to the

one obtained in the main specification. In Austria, changes in educational mobility are statistically insignificant and small in size, providing further support to the conclusion that educational mobility in Austria was not affected by the reform in comprehensive schooling age. In Denmark and in the Netherlands, there is no evidence for changes in the intergenerational transmission of educational advantage due to the reforms in compulsory school leaving age that occurred in these societies.

Discussion and Conclusion

Does compulsory school leaving age affect intergenerational mobility? Answers to this question vary across countries and studies. Educational mobility increased due to reforms in compulsory school leaving age in France and the United States (Rauscher 2014, 2016). Increasing compulsory school leaving age increased educational mobility for some but not for other groups in Germany (Betthäuser 2017). At the same time, educational mobility was not affected by the reforms in compulsory school leaving age carried out in Austria, Denmark, England and Wales (Sturgis and Buscha 2015), and the Netherlands.

How can these diverging results across countries and studies be interpreted? Differences in findings of the effects of reforms in compulsory school leaving age on intergenerational mobility between different studies could be due to methodological differences. This is precisely why I employed the same analytical approach and data to compare the effects of reforms in compulsory school leaving age on educational mobility across different countries. My findings of cross-country variation therefore suggest that methodological differences do not drive differences in results across different studies. As a result, differences in results across countries are due to real cross-country variation in the effects of reforms in compulsory school leaving age on educational mobility.

The variation of the effects of reforms on educational mobility across countries is in line with the idea that the level of educational attainment in the population before the reform moderates the effects of the reforms on educational mobility. In contexts of low education (France, the United States), the increases in educational mobility were largest. In contexts with higher educational attainment Denmark, England and Wales, and the Netherlands), changing the compulsory school leaving age did not increase educational mobility. Austria is a bit of an exception, as even though educational attainment prior to the reform was only slightly lower in Austria than in France, there was no clear change in educational mobility due to the reform of compulsory school leaving age in Austria. In any case, the results are clearly at odds with a view according to which the effects of compulsory school leaving age reforms vary with the selectivity of an educational system.

These findings show that there are limits for educational policies to affect educational mobility. At the same time, findings for these countries are in line with conflict theory (Collins 1971), maximally maintained inequality theory (Raftery and Hout 1993), and equally maintained inequality theory (Lucas 2001), which all claim that socioeconomically advantaged families respond to educational reforms and find ways to transmit their advantage to their children. They did so successfully when reforms in compulsory school leaving age occurred in Austria, Denmark, England and Wales, and the Netherlands.

Nevertheless, my results are not in line with views according to which institutions and policies have no effects on the transmission of advantage across generations (Clark 2014; Erikson and Goldthorpe 1992; Shavit and Blossfeld 1993; Sorokin 1959). Under some circumstances, educational reforms affect education mobility, as was also demonstrated by previous research. For instance, van de Werfhorst (2018) found that tracking reduced the association between parental occupation and children's skills in mathematics in Europe (but not the association between parental education conditional on parental occupation and

children's skills in mathematics). Moreover, Meghir and Palme (2005) found that an extensive reform in Sweden increased educational mobility.

At the same time, my results challenge notions that specific institutions have context-independent effects on intergenerational mobility. Comparative research often proposes, besides compulsory school leaving age, tracking age as affecting intergenerational educational mobility (Braga, Checchi, and Meschi 2013; Brunello and Checchi 2007; Hanushek and Wößmann 2006; Horn 2009; Pekkarinen et al. 2009; Pfeffer 2008, 2015; van de Werfhorst 2018; van de Werfhorst and Mijs 2010). This research has so far not investigated cross-country variation in the effects of reforms in tracking age on education. For instance, van de Werfhorst (2018) pooled data on a number of European countries, ignoring thereby potential heterogeneity in the effects of political reforms on educational mobility across countries. As has been the case for compulsory school leaving age, the effects of reforms in age at tracking may, however, vary across countries and that is an important topic for future research.

The results of my study have implications for the methodologies used to estimate the causal effects of educational reforms on intergenerational mobility. Some previous studies employed difference-in-differences research designs exploiting variation across European countries (Braga et al. [2013] with a set of educational policies and van de Werfhorst [2018] with respect to tracking) or across regions within countries (Betthäuser [2017] and Rauscher [2014, 2016] with respect to reforms in compulsory school leaving age; Meghir and Palme [2005] and Pekkarinen et al. [2009] with respect to tracking). These research designs are build on the assumption that there is no heterogeneity in the effects of the reforms on educational mobility within their units of analysis (countries or regions). The results of my analysis suggest that this assumption may be violated. Results from difference-in-differences research designs should therefore be interpreted with caution.

A shortcoming of previous research is that the authors employed only one measure of social origin, in all cases some measure of parental occupation (Betthäuser 2017; Rauscher 2014, 2016; Sturgis and Buscha 2015). Contrary to these studies, I employed a measure of parental education. Different measures of social origin are highly collinear but not completely (Bukodi and Goldthorpe 2013; Erola, Jalonen, and Lehti 2016; Mood 2017). It would therefore be welcome to see whether results are robust to employing different measures of social origin. With respect to the effects of tracking on educational inequalities, van de Werfhorst (2018) showed that this is not the case. He found that reforms in tracking age in Europe affected the association between parental occupation and children's mathematics skills but did not affect the association between parental education and children's mathematics skills (conditioning on parental education and parental occupation simultaneously). Unfortunately, my data does not allow me either to conduct an extensive robustness check in this respect, as only one indicator of social origin (parental education) is available for a large enough number of respondents in my data.[8] Future research may improve on my and the other studies in this respect.

Contrary to my theoretical expectations, there were no gender differences in the effects of reforms in compulsory school leaving age on educational mobility. My findings suggest that the gender differences found by Rauscher (2014) are not generalizable to other contexts than the United States. The theoretical implication of these findings is that, even though women were affected more by the reforms, the effects of the reform did not differ between women and men from different social groups. In France, both men and women experienced more educational mobility due to the reform in compulsory school leaving age. In Austria, Denmark, and the Netherlands, neither men and women experienced a change in educational mobility due to increasing compulsory school leaving age.

One may perceive as a limitation of my study that it focuses on educational reforms that occurred, at least in advanced, industrialized Western societies, a long time ago. These reforms

may not be very representative for the type of reforms carried out in these societies today. Nevertheless, it is instructive to study these reforms for at least three reasons. First, these reforms did have strong effects on educational attainment. Arguably, these effects were stronger than the effects of any educational reforms carried out today. From this perspective, the effects of the reforms I study in this manuscript may be perceived as providing upper bound estimates of the effects of educational reforms on intergenerational mobility.

Second, the discussion on educational expansion is ongoing in Western societies. It has shifted, however, from the compulsory to the university level. Analyses of the effects of university reforms on intergenerational mobility are therefore an important extension of the analyses of reforms in compulsory school leaving age.

Third, some countries are still carrying out reforms in compulsory school leaving age. This includes less developed countries but also, for instance, in Switzerland the political discussion is about changing the compulsory school leaving age from 18 to 19. These reforms may improve the overall level of educational attainment in these societies, as well as increase educational mobility. My results, however, suggest that these consequences are likely to vary across countries.

Finally, this manuscript makes a methodological contribution to studies on the effects of policies on outcomes of a sociological interest. In order to draw general conclusions from these studies, a cross-national approach, as advanced in this manuscript, is needed. Studies of the effects of policies on intergenerational mobility usually focus on single cases. These single case studies have, however, the problem that their generalizability is unclear (Torche 2015). Applying a cross-national, comparative approach is an important methodological advancement of this line of research. It promises to increase the external validity of estimates of the causal effects of policies on intergenerational mobility.

Notes

1. Previous research estimated the effects of these reforms, among others, on earnings (Brunello, Fort, and Weber 2009), the body mass index (Brunello, Fabbri, and Fort 2013), cognitive skills (Schneeweis, Skirbekk, and Winter-Ebmer 2014), and female fertility (Fort, Schneeweis, and Winter-Ebmer 2016).

2. For that reason, I excluded a number of reforms from the case selection. For instance, Meghir and Palme (2005) analyzed an educational reform in Sweden that increased compulsory school leaving age but that also affected tracking and changed the national curriculum (Garrouste 2010). It is therefore not possible to attribute any effect of this reform to the change in compulsory school leaving age, which is the focus of this manuscript. In addition, I excluded a reform in compulsory school leaving age in Denmark in 1958 from the analysis. This reform also changed tracking and restructured the middle school (Garrouste 2010). The only reform in Denmark I use is the one from 1971. I also included other reforms used in other studies, e.g. the reform in Italy in 1963 that made junior high school mandatory (Fort et al. 2016).

3. Economists have used reforms in compulsory school leaving age to estimate the labor market returns to education (Aakvik, Salvanes, and Vaage 2010; Acemoglu and Angrist 2000; Brunello et al. 2009; Devereux and Hart 2010; Grenet 2013; Harmon and Walker 1995; Oreopoulos 2006, 2007; Pischke and von Wachter 2008). In addition, Brinch and Galloway (2012) demonstrated an increase in IQ due to a reform in compulsory school leaving age in Norway.

4. Three of these four studies did not only analyze educational but also occupational mobility (Betthäuser 2017; Rauscher 2016; Sturgis and Buscha 2015). Given the potential violation of SUTVA in estimating the effects of reforms in compulsory school leaving age on occupational mobility, I limit my analysis to the study of educational mobility. I focus

therefore also on educational mobility in discussing the findings from previous research. I only would like to add that the three studies I discuss also diverge in their findings with respect to intergenerational occupational mobility. Rauscher (2016) found that occupational mobility declined in the short term but did not change in the long term in the United States. Sturgis and Buscha (2015) found no effect of the reform in compulsory school leaving age in England and Wales on occupational mobility. Betthäuser (2017) found similar effects of the reform in Germany on occupational and on educational mobility.

5. The data sets can be found with the following DOIs: 10.6103/SHARE.w1.700, 10.6103/SHARE.w2.700, 10.6103/SHARE.w3.700, 10.6103/SHARE.w4.700, 10.6103/SHARE.w5.700, 10.6103/SHARE.w6.700, and 10.6103/SHARE.w7.700 (Börsch-Supan 2019a–g).

6. Results on the separate analyses of the data sets (available upon request) are in line with those on the pooled data but the estimates are far less precise, which is why I decided to report only the more precise and reliable estimates obtained using the pooled data.

7. Rauscher (2016) estimated the effects of compulsory schooling laws using a regression discontinuity design (RDD). This approach implies conditioning additionally on age at time of the law. Betthäuser (2017) and Rauscher (2014) proceeded similarly. In other words, these three studies controlled for a linear time trend. Given that the effects of the reforms in compulsory schooling age are, as the effect of age at time of the law, identified based on year of birth, controlling for age introduces overcontrol bias because part of the effects of the reforms are taken up by the linear age controls. It is also theoretically unclear to me why we would want to control for a linear time trend. Essentially this picks up educational expansion but educational expansion is driven (among other factors) by the reforms in compulsory schooling age. For these reasons, I do not control for a linear time trend in estimating the effects of the reforms in compulsory school leaving age on intergenerational

educational mobility. My analysis is in that respect in line with the one by Sturgis and Buscha (2015).

8. Some of the respondents in the late birth years in the before-cohort in the Netherlands were also affected by another educational reform in the Netherlands that affected the transition from primary to secondary education (Dronkers 1993). Whilst this educational reform may lead to some differences within the before-compulsory-schooling-age-cohort in my analysis, it should not affect the conclusions drawn from the comparison of the before-compulsory-school-leaving-age-reform cohort to the after-compulsory-school-leaving-age-reform-cohort.

9. For some respondents, information on father's and mother's occupation during childhood is available. However, in both surveys, this information was not collected in all waves and using this information would result in small sample sizes and selective samples. Other data are therefore needed to conduct an analysis comparing the effects of reforms in compulsory school leaving age on the association between parental education and child education *and* the association between parental occupation and child education.

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TABLES

Table 1. Overview of the Compulsory School Leaving Reforms and the Definition of Cohorts

Country (Year of Reform, Legal Decision)	First Birth Year Affected by the Reform (Actual Implementation)	Increase in Compulsory School Leaving Age	Before-Reform Cohort	After-Reform Cohort
Austria (1962)	1951	Age 14 to 15	1943–1950	1952–1959
Denmark (1971)	1957	Age 14 to 16	1949–1956	1958–1965
France (1967)	1953	Age 14 to 16	1945–1952	1954–1961
Netherlands (1971)	1957	Age 14 to 16	1949–1956	1958–1965

Table 2. Descriptive Statistics by Country

	Austria	Denmark	France	Netherlands
Panel A: All				
Years of education	11.69	13.68	11.50	12.43
	(2.73)	(2.61)	(3.54)	(2.51)
Low parental education	0.36	0.35	0.70	0.72
	(0.48)	(0.48)	(0.46)	(0.45)
Medium parental education	0.46	0.43	0.20	0.15
	(0.50)	(0.50)	(0.40)	(0.35)
High parental education	0.17	0.21	0.10	0.13
	(0.38)	(0.41)	(0.30)	(0.34)
Reform	0.50	0.48	0.48	0.41
	(0.50)	(0.50)	(0.50)	(0.49)
Male	0.46	0.48	0.46	0.46
	(0.50)	(0.50)	(0.50)	(0.50)
N	2,756	3,657	4,179	3,652
Panel B: Before-Reform Cohort				
Years of education	11.58	13.51	11.27	12.29
	(2.74)	(2.68)	(3.80)	(2.59)
Low parental education	0.36	0.40	0.74	0.74
	(0.48)	(0.49)	(0.44)	(0.44)
Medium parental education	0.45	0.42	0.17	0.14
	(0.50)	(0.49)	(0.37)	(0.35)
High parental education	0.19	0.18	0.09	0.13
	(0.39)	(0.38)	(0.29)	(0.33)
Male	0.45	0.49	0.47	0.47
	(0.50)	(0.50)	(0.50)	(0.50)
N	1,371	1,897	2,153	2,152
Panel C: After-Reform Cohort				
Years of education	11.80	13.86	11.74	12.64
	(2.72)	(2.51)	(3.23)	(2.38)
Low parental education	0.36	0.30	0.65	0.70
	(0.48)	(0.46)	(0.48)	(0.46)

Medium parental education	0.48	0.44	0.24	0.15
	(0.50)	(0.50)	(0.43)	(0.36)
High parental education	0.16	0.26	0.11	0.14
	(0.37)	(0.44)	(0.31)	(0.35)
Male	0.46	0.46	0.46	0.44
	(0.50)	(0.50)	(0.50)	(0.50)
N	1,385	1,760	2,026	1,500

Notes: The table reports the means and in brackets the standard deviations.

Source: SHARE, Waves 1−7 and ESS, Waves 1−8.

Table 3. OLS Regression Models Predicting Years of Education, Austria

	All		Women		Men	
	(1)	(2)	(3)	(4)	(5)	(6)
Low parental education	−1.06*	−1.29*	−1.03*	−1.09*	−1.11*	−1.53*
	[−1.27, −0.85]	[−1.59, −0.99]	[−1.32, −0.73]	[−1.51, −0.67]	[−1.41, −0.81]	[−1.96, −1.11]
High parental education	1.61*	1.34*	1.57*	1.39*	1.64*	1.26*
	[1.34, 1.88]	[0.97, 1.71]	[1.19, 1.95]	[0.87, 1.92]	[1.27, 2.02]	[0.74, 1.78]
Reform	0.27*	0.01	0.53*	0.42*	−0.05	−0.48*
	[0.08, 0.46]	[−0.27, 0.29]	[0.27, 0.80]	[0.03, 0.82]	[−0.31, 0.22]	[−0.87, −0.08]
Male	0.62*	0.62*				
	[0.43, 0.81]	[0.43, 0.81]				
Low parental education X Reform		0.45*		0.12		0.82*
		[0.02, 0.87]		[−0.47, 0.72]		[0.23, 1.42]
High parental education X Reform		0.55*		0.37		0.77*
		[0.02, 1.09]		[−0.39, 1.12]		[0.02, 1.52]
N	2,756	2,756	1,501	1,501	1,255	1,255

Notes: 95% confidence intervals in brackets.

Source: SHARE, Waves 1−7 and ESS, Waves 1−8.

* $p < 0.05$ (two-tailed tests)

Table 4. OLS Regression Models Predicting Years of Education, Denmark

	All		Women		Men	
	(1)	(2)	(3)	(4)	(5)	(6)
Low parental education	−0.94*	−1.01*	−0.90*	−0.88*	−0.97*	−1.14*
	[−1.12, −0.76]	[−1.25, −0.77]	[−1.15, −0.65]	[−1.22, −0.53]	[−1.23, −0.71]	[−1.48, −0.80]
High parental education	1.33*	1.45*	1.14*	1.35*	1.55*	1.58*
	[1.12, 1.54]	[1.14, 1.76]	[0.85, 1.43]	[0.91, 1.79]	[1.24, 1.85]	[1.14, 2.03]
Reform	0.13	0.12	0.28*	0.37*	−0.04	−0.17
	[−0.03, 0.29]	[−0.12, 0.36]	[0.06, 0.51]	[0.04, 0.71]	[−0.27, 0.19]	[−0.52, 0.18]
Male	−0.48*	−0.48*				
	[−0.64, −0.32]	[−0.64, −0.32]				
Low parental education X Reform		0.17		−0.03		0.40
		[−0.20, 0.53]		[−0.54, 0.48]		[−0.12, 0.92]
High parental education X Reform		−0.21		−0.37		−0.03
		[−0.64, 0.21]		[−0.96, 0.22]		[−0.65, 0.58]
N	3,657	3,657	1,918	1,918	1,739	1,739

Notes: 95% confidence intervals in brackets.

Source: SHARE, Waves 1−7 and ESS, Waves 1−8.

* $p < 0.05$ (two-tailed tests)

Table 5. OLS Regression Models Predicting Years of Education, France

	All		Women		Men	
	(1)	(2)	(3)	(4)	(5)	(6)
Low parental education	−1.88*	−2.35*	−1.85*	−2.32*	−1.93*	−2.40*
	[−2.13, −1.63]	[−2.72, −1.97]	[−2.20, −1.50]	[−2.83, −1.80]	[−2.29, −1.56]	[−2.95, −1.86]
High parental education	2.11*	2.20*	2.21*	2.42*	2.00*	1.93*
	[1.72, 2.50]	[1.63, 2.77]	[1.67, 2.76]	[1.63, 3.20]	[1.46, 2.55]	[1.11, 2.76]
Reform	0.27*	−0.33	0.47*	−0.11	0.03	−0.59
	[0.06, 0.47]	[−0.77, 0.12]	[0.19, 0.75]	[−0.73, 0.51]	[−0.25, 0.32]	[−1.23, 0.05]
Male	0.31*	0.32*				
	[0.11, 0.51]	[0.12, 0.52]				
Low parental education X Reform		0.88*		0.89*		0.88*
		[0.37, 1.39]		[0.18, 1.59]		[0.15, 1.61]
High parental education X Reform		−0.23		−0.48		0.08
		[−1.00, 0.54]		[−1.57, 0.61]		[−1.01, 1.18]
N	4,179	4,179	2,242	2,242	1,937	1,937

Notes: 95% confidence intervals in brackets.

Source: SHARE, Waves 1−7 and ESS, Waves 1−8.

* $p < 0.05$ (two-tailed tests)

Table 6. OLS Regression Models Predicting Years of Education, Netherlands

	All		Women		Men	
	(1)	(2)	(3)	(4)	(5)	(6)
Low parental education	−1.28*	−1.28*	−1.31*	−1.43*	−1.24*	−1.11*
	[−1.50, −1.06]	[−1.57, −0.99]	[−1.60, −1.02]	[−1.82, −1.05]	[−1.57, −0.90]	[−1.55, −0.67]
High parental education	1.10*	1.34*	1.12*	1.36*	1.06*	1.33*
	[0.81, 1.39]	[0.96, 1.73]	[0.74, 1.50]	[0.84, 1.88]	[0.62, 1.51]	[0.75, 1.91]
Reform	0.30*	0.36	0.49*	0.36	0.06	0.36
	[0.14, 0.45]	[−0.04, 0.77]	[0.29, 0.70]	[−0.17, 0.89]	[−0.17, 0.30]	[−0.26, 0.98]
Male	0.48*	0.48*				
	[0.33, 0.64]	[0.33, 0.64]				
Low parental education X Reform		0.01		0.28		−0.30
		[−0.43, 0.46]		[−0.30, 0.86]		[−0.98, 0.38]
High parental education X Reform		−0.54		−0.48		−0.64
		[−1.12, 0.04]		[−1.25, 0.28]		[−1.53, 0.26]
N	3,652	3,652	1,983	1,983	1,669	1,669

Notes: 95% confidence intervals in brackets.

Source: SHARE, Waves 1−7 and ESS, Waves 1−8.

* p < 0.05 (two-tailed tests)

ONLINE SUPPLEMENT TO

Does Compulsory School Leaving Age Affect the Intergenerational Transmission of Education? Evidence from Five European Countries

This version: September 19, 2019

APPENDIX A. DETAILS ON THE EDUCATIONAL REFORMS

Austria. Austria passed a law to increase the compulsory school leaving age by one year in 1962 (Fort et al. 2016; Schneeweis et al. 2014). The increase in compulsory school leaving age affected children who were 14 years old in September 1966, when the law came into effect. The first cohort partially affected by the reform were children born in 1951. Therefore, my analyses compares children born 1952 and later with those born 1950 and earlier.

Denmark. Denmark introduced a policy to increase the compulsory school leaving age by two years in 1971 (Fort et al. 2016; Garrouste 2010). Children who were eleven in 1957 were the first to be affected by the reform. Those children were born in 1957. Therefore, my analysis compares children born in 1958 and later to those born in 1956 and earlier.

France. The French reform increased compulsory school leaving age by two years from age 14 to 16 (Fort et al. 2016; Schneeweis et al. 2014). The law was decided upon in 1958 but the reform was only implemented in 1967. The children, who were first affected by this reform, were born in 1953. This cohort is dropped from the analysis, which compares children born 1954 and later to those born 1952 and earlier.

Netherlands. Dutch reforms, which became laws in 1971 and 1975, increased the compulsory school leaving age by one year each (Fort et al. 2016). The total increase was therefore two years from age 14 to 16. The first cohort affected by the first reform was born in 1957 and the first cohort affected by the second reform in 1959. I drop the cohort born in 1957 from the analysis and compare those born in 1958 and later to those born 1956 and earlier.

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APPENDIX B. SUPPLEMENTARY ANALYSES

Table S1. OLS Regression Models Predicting Years of Education, Austria, Robustness Check Using Cohorts of Four Years Before and After Discontinuity

	All		Women		Men	
	(1)	(2)	(3)	(4)	(5)	(6)
Low parental education	−1.01*	−1.08*	−1.03*	−1.02*	−0.98*	−1.15*
	[−1.29, −0.72]	[−1.47, −0.69]	[−1.43, −0.62]	[−1.58, −0.46]	[−1.37, −0.59]	[−1.69, −0.61]
High parental education	1.73*	1.47*	1.62*	1.44*	1.86*	1.52*
	[1.37, 2.09]	[0.98, 1.96]	[1.11, 2.13]	[0.74, 2.13]	[1.36, 2.37]	[0.83, 2.21]
Reform	0.19	0.04	0.28	0.22	0.08	−0.18
	[−0.07, 0.44]	[−0.33, 0.42]	[−0.09, 0.64]	[−0.31, 0.75]	[−0.28, 0.43]	[−0.71, 0.34]
Male	0.65*	0.65*				
	[0.39, 0.91]	[0.39, 0.91]				
Reform X Low parental education		0.14		−0.02		0.35
		[−0.42, 0.71]		[−0.84, 0.79]		[−0.43, 1.14]
Reform X High parental education		0.55		0.40		0.74
		[−0.17, 1.28]		[−0.62, 1.43]		[−0.27, 1.76]
N	1,491	1,491	814	814	677	677

Notes: 95% confidence intervals in brackets.

Source: SHARE, Waves 1–7 and ESS, Waves 1–8.

* p < 0.05 (two-tailed tests)

Table S2. OLS Regression Models Predicting Years of Education, Denmark, Robustness Check Using Cohorts of Four Years Before and After Discontinuity

	All		Women		Men	
	(1)	(2)	(3)	(4)	(5)	(6)
Low parental education	−0.98*	−1.03*	−0.85*	−0.72*	−1.11*	−1.32*
	[−1.22, −0.73]	[−1.36, −0.69]	[−1.19, −0.50]	[−1.20, −0.24]	[−1.46, −0.76]	[−1.78, −0.85]
High parental education	1.22*	1.41*	1.03*	1.26*	1.43*	1.62*
	[0.93, 1.51]	[1.00, 1.82]	[0.63, 1.43]	[0.70, 1.82]	[1.01, 1.85]	[1.02, 2.22]
Reform	0.14	0.18	0.08	0.27	0.19	0.09
	[−0.08, 0.36]	[−0.15, 0.51]	[−0.22, 0.38]	[−0.19, 0.74]	[−0.12, 0.50]	[−0.38, 0.56]
Male	−0.51*	−0.51*				
	[−0.73, −0.30]	[−0.72, −0.29]				
Reform X Low parental education		0.11		−0.26		0.48
		[−0.38, 0.61]		[−0.95, 0.43]		[−0.22, 1.19]
Reform X High parental education		−0.38		−0.45		−0.34
		[−0.96, 0.20]		[−1.25, 0.34]		[−1.18, 0.49]
N	1,926	1,926	990	990	936	936

Notes: 95% confidence intervals in brackets.

Source: SHARE, Waves 1–7 and ESS, Waves 1–8.

* $p < 0.05$ (two-tailed tests)

Table S3. OLS Regression Models Predicting Years of Education, France, Robustness Check Using Cohorts of Four Years Before and After Discontinuity

	All		Women		Men	
	(1)	(2)	(3)	(4)	(5)	(6)
Low parental education	−1.79*	−2.18*	−1.74*	−2.29*	−1.85*	−2.08*
	[−2.13, −1.45]	[−2.68, −1.68]	[−2.23, −1.26]	[−2.98, −1.59]	[−2.33, −1.38]	[−2.78, −1.38]
High parental education	2.31*	2.48*	2.36*	2.73*	2.26*	2.18*
	[1.76, 2.85]	[1.69, 3.27]	[1.58, 3.15]	[1.63, 3.83]	[1.51, 3.01]	[1.03, 3.32]
Reform	0.08	−0.42	0.26	−0.41	−0.11	−0.42
	[−0.19, 0.35]	[−1.02, 0.18]	[−0.13, 0.65]	[−1.26, 0.44]	[−0.50, 0.27]	[−1.27, 0.42]
Male	0.22	0.24				
	[−0.05, 0.50]	[−0.03, 0.51]				
Reform X Low parental education		0.75*		1.05*		0.42
		[0.07, 1.44]		[0.09, 2.02]		[−0.54, 1.39]
Reform X High parental education		−0.35		−0.85		0.16
		[−1.44, 0.74]		[−2.42, 0.72]		[−1.36, 1.67]
N	2,240	2,240	1,168	1,168	1,072	1,072

Notes: 95% confidence intervals in brackets.

Source: SHARE, Waves 1–7 and ESS, Waves 1–8.

* $p < 0.05$ (two-tailed tests)

Table S4. OLS Regression Models Predicting Years of Education, Netherlands, Robustness Check Using Cohorts of Four Years Before and After Discontinuity

	All		Women		Men	
	(1)	(2)	(3)	(4)	(5)	(6)
Low parental education	−1.38*	−1.39*	−1.38*	−1.48*	−1.38*	−1.29*
	[−1.67, −1.09]	[−1.78, −1.00]	[−1.78, −0.99]	[−2.02, −0.94]	[−1.80, −0.96]	[−1.86, −0.71]
High parental education	0.98*	1.05*	1.02*	1.07*	0.92*	1.03*
	[0.60, 1.37]	[0.54, 1.57]	[0.50, 1.55]	[0.35, 1.78]	[0.36, 1.49]	[0.28, 1.78]
Reform	0.06	0.06	0.22	0.09	−0.13	0.04
	[−0.14, 0.26]	[−0.46, 0.59]	[−0.05, 0.50]	[−0.63, 0.81]	[−0.43, 0.17]	[−0.72, 0.80]
Male	0.42*	0.42*				
	[0.22, 0.62]	[0.21, 0.62]				
Reform X Low parental education		0.02		0.21		−0.20
		[−0.55, 0.60]		[−0.58, 1.00]		[−1.04, 0.64]
Reform X High parental education		−0.15		−0.09		−0.25
		[−0.92, 0.61]		[−1.14, 0.95]		[−1.38, 0.89]
N	1,993	1,993	1,080	1,080	913	913

Notes: 95% confidence intervals in brackets.

Source: SHARE, Waves 1–7 and ESS, Waves 1–8.

* p < 0.05 (two-tailed tests)