Intergenerational Transmission of Human Capital: Is It A One-Way Street?*

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Abstract

Studies on the intergenerational transmission of human capital usually assume a one-way spillover from parents to children. However, children may also affect their parents' human capital. Using exogenous variation in education, arising from a Swedish compulsory schooling reform in the 1950s and 1960s, we address this question by studying the causal effect of children's schooling on their parents' longevity. We first replicate previous findings of a positive and significant cross-sectional relationship between children's education and their parents' longevity. Our causal estimates tell a different story; on average, children's schooling has no significant effect on parents' survival. However, the average estimate hides substantial heterogeneity by the gender of the child and the parent; female schooling is found to affect longevity of fathers and especially those from low socio-economic background. Taken together, our results point to the importance of children's schooling for parental health and to the importance of considering heterogeneous impacts.

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1 Introduction

Economists have become increasingly interested in the non-pecuniary benefits of schooling. Besides increasing wages, schooling has been shown to improve health, reduce crime, and increase trust and social interactions (Oreopoulos and Salvanes 2011; Lochner 2011; Grossman 2006). Moreover, recent evidence suggests that some of these effects transcend across generations, thus creating positive externalities. Such spillovers from one generation to the other should be taken into account when valuing the societal returns to schooling (Björklund and Salvanes 2010).[1]

Despite the widespread interest in intergenerational spillovers, previous studies have been based on the assumption that externalities only work in one direction; from parents to children. However, there are reasons to believe that the externalities can work the other way around as well. Well-educated children might for instance have more resources to invest in their elderly parents' health. Parents' morale may also increase if their children are more successful and have a better life as a result of getting more education. In addition, well-educated children have better knowledge of health and technology to share with their parents and can help them with informal care, medication adherence and act as their agents in the health and long-term care system (Friedman and Mare 2014).

In this paper, we provide the first set of evidence on the *causal* effect of children's schooling on their parents' health. We do so by exploiting the Swedish compulsory schooling reform, that was rolled out over the country during the 1950s and 1960s. An important feature of the reform was that the timing of the roll-out varied across municipalities. This gives us variation in reform exposure both within and between

[1]The range of topics that has been explored includes, but is not limited to, the effect of parental education on children's educational outcomes (Black et al. 2005; Magnuson 2007; Page 2009) cognitive and non-cognitive abilities (Lundborg et al. 2014) and health (Currie and Moretti 2003; McCrary and Royer 2006; Lundborg et al. 2014). In most cases, studies that have looked into the causal effects of parental education have found positive and significant effects of increases in both or one of the parents' educational attainments on children's outcomes. In addition to schooling, other studies have examined the effect of parental health on children's outcomes (Black et al. 2014; Persson and Rossin-Slater 2016), and transmission of IQ and cognitive and non-cognitive skills (Black et al. 2009; Anger and Heineck 2009; Gronqvist et al. 2009; Björklund et al. 2010).

cohorts and provides us with plausibly exogenous variation in schooling.[2] We use data from the Swedish Cause of Death Register and proxy parents' health by their age at death.

Our paper relates to a recent literature studying the influence of children on parents. Using data from the U.S., Friedman and Mare (2014) show that parents whose children go to college live longer on average, even after controlling for financial resources and level of education of the parents.[3] Zimmer et al. (2007) use data from Taiwan and show that offspring's schooling is associated with older parents' mortality and the severity of parents' health in old age. Torssander (2013) links parents born between 1932 and 1941 to their children in the Swedish Multi-generation Register and shows a similar relationship for parental mortality and children's education. Controlling for parents' education, social class, and income, she finds a positive association between children's education and parents' mortality risk. Even after comparing siblings in the parental generation, to control for family background characteristics, the results hold.[4]

Although these papers try to control for a number of variables that could be correlated with both parents' longevity and children's schooling, none of them are able to identify the causal effect of children's schooling. Since schooling is an endogenous variable, one should be worried that it correlates with unobserved factors that are shared between children and parents, such as intrinsic abilities and underlying health. In addition, the relationship could work the other way around, where

[2]The crucial assumption of our identification strategy is that conditional on birth cohort fixed effects, municipality fixed effects, and municipality-specific linear trends, exposure to the reform is as good as random. We provide evidence for this later in the paper.

[3]The authors employ a Cox proportional hazard for their analyses on parents' age at death. For the analyses on specific causes of death, the authors use competing risk models. Only individuals who survived to the age of 50 are included in their analyses. The authors aim to identify the causal effect of education by controlling for a set of potential (observed) confounders.

[4]More generally, our paper relates to the literature on the determinants of mortality and the role of economic conditions and education. Previous papers have shown the importance of early life conditions for adult mortality (e.g. Case et al. 2003 and van den Berg et al. 2006). A related literature has estimated the effects of education, income, and wealth on mortality with mixed evidence. Some papers have found positive effects of income (Adda et al., 2009), education (e.g. Lleras-Muney 2005; Lager and Torssander 2012; Garcia-Gomez et al. 2013; Lundborg et al. 2017), whereas others have found no effects of education (e.g. Clark and Royer 2013) or wealth (Cesarini et al. 2016).

healthier parents invest more in their children's human capital. These identification threats become all the more important since a positive association between children's education and parents' longevity is not the only conceivable relation. There are also reasons to think that more education for children could negatively affect parents' during the old age, the most important of which being that individuals with more education are more likely to move to other municipalities or even other countries and are more likely not to live close to their parents, as a result (Machin, Pelkonen, and Salvanes 2012).

We are aware of only one study that estimates the causal effect of children's human capital on that of their parents. Kuziemko (2014) models how children's acquisition of a specific type of human capital generates incentives for adults in the household to either learn from them or lean on them. She tests the model using variation in compliance with an English-immersion mandate in California schools and shows that improved language skills among immigrants leads to lower language skills among their parents. She interprets this as evidence of crowding out, where parents lean on their children instead of learning on their own.

In this paper, we first replicate the previous findings of a positive and significant cross-sectional relationship between children's education and their parents' longevity; our OLS estimates suggest that both daughters' and sons' schooling are strongly associated with parents' longevity and that the relationship is equally strong for mothers and fathers. Our instrumental variables estimates tell a different story. On average, we obtain small and insignificant IV estimates when we pool all children and parents and we can in most cases reject the null that the OLS and IV estimates are the same. Our conclusion does not change when we study separate causes of death.

The estimated mean impacts hide substantial heterogeneity, however. When we study the effect of female schooling separately, we find positive and significant effects on the survival of parents. These effects are especially pronounced for fathers who typically are among the frailest. Since females are often primary care givers, our estimates suggest that when the group that are most involved in care-giving obtain more schooling, fathers gain in terms of longevity.

To interpret our findings we study the effect of schooling on a number of intermediate outcomes of the children and their parents. We find that the positive effects of female schooling on fathers' survival arise despite the lack of an effect of female schooling on earnings and on parental economic outcomes, such as financial wealth, income, and retirement age. The effect of female schooling on parental survival might instead reflect increased health knowledge leading to higher quality informal care and better knowledge on how to navigate the health care and long-term care systems. We also find that females who obtain more schooling marry more highly educated males so that the total human capital of the household increases. We obtain no such corresponding effect for males.

Acknowledging that our instrumental variables estimates only reflect variation at the lower part of the education distribution, we provide OLS estimates showing that the positive relationship between children's schooling and parental longevity is obtained both at the lower and upper part of the education distribution.

Our findings suggest that the positive cross-sectional relationship between children's years of schooling and elderly parents' health to a large extent reflects the influence of unobserved factors that affect both children and parents. Our results also illustrate the importance of moving beyond analyzing the mean impact when examining the effect of children's schooling, however, as female schooling was found to be of greater importance than male schooling for parental survival. Although we are aware that the institutional context for elderly could be different in Sweden compared to some other Western countries, it is important to note that the parents we study in this paper were born in the first part of the 20th century and belong to cohorts where a high fraction live with the minimum level of pension income.[5] Many of them were financially vulnerable and it is reasonable to believe that they could have potentially benefited from having better-educated children.

[5]We provide more details about the lives of this generation of parents in Section IV.

The paper unfolds as follows. Section 2 discusses potential channels through which children's schooling might affect parental mortality. Section 3 discusses the compulsory schooling reform, while Section 4 describes the relevant institutional context. Section 5 describes our data. Section 6 outlines our empirical strategy and Section 7 discuss the validity of our instrument. Section 8 present our main results and Section 9 show results on potential mechanisms. Section 10 provides some additional robustness checks and Section 11 concludes.

2 Schooling and Parental Mortality

In this section, we discuss a number of mechanisms through which children's schooling might affect parental longevity.

A positive effect of children's schooling may arise for several reasons. First, to the extent that better educated children earn more when they enter the labor market, their parents could spend more money on themself instead of on their children. Moreover, better educated children would have greater means to support an elderly and ill parent.[6]

Second, it is straightforward to extend the Demand-for-Health framework to the case where children can invest time and material resources in the production of both their own and their parents' health.[7] Well-educated children may have better health knowledge to share with their parents and may be more efficient informal care-provides and act as better agents for their parents in the health and long-term care system (Friedman and Mare 2014). In addition, parents' mental health might improve as a result of having well-educated children. Children also face several incentives to invest in their parents; healthy parents can provide otherwise expensive services, such as child-care and home services, and may be wealthier due to having better health.

However, there are also disincentives for well-educated children to invest in their

[6] Previous papers on the Swedish schooling reform have established that there are income returns for increased schooling, at least among males (e.g. Black et al. (2015).

[7] See Grossman (2006) for an extended discussion on the role of education in the production of health.

parents' health. Higher earnings mean a higher opportunity cost of providing time-intensive health services to parents. Well-educated children are also more likely to live farther away from their parents due to better job opportunities (Machin, Pelkonen, and Salvanes 2012). The medical literature provides suggestive evidence on the importance of having children living in close proximity for elderly parents' health. Silverstein and Bengtson (1991) hypothesize that close intergenerational relations could reduce pathogenic stress among elderly parents and, through that, enhance their ability to survive. Using data from the U.S.C. Longitudinal Study of Generations, they find that greater intergenerational contact increases survival time among parents who experienced a loss in their social network, particularly among those who were widowed recently. They conclude that the mortal health risks associated with the stress of being widowed can be partially offset by affectionate relations with adult children. In another study, using the same dataset, Silverstein and Bengtson (1994) find that instrumental and expressive forms of social support moderate declines in well-being of elderly parents associated with poor health and widowhood.[8]

Parents may also adjust their own health investments in response to the human capital of their children. Having children with higher education could, for instance, mean that some parents rely on them for health knowledge and investments. In addition, incentives to take care of one's health may be reduced if parents know that their children will take care of them in the event of illness. Evidence of parent-children "crowding out" effects were observed in Kuziemko (2014), mentioned above, where improved language skills among immigrant children led to lower language skills among their parents.

As the discussion above suggests, children's education may affect parental mortality both positively and negatively and it is a priori not clear what the net effect will be. In this study, we provide the first causal estimates on the net effect of

[8] Also, using face-to-face interviews with elderly people in Spain, Zunzunegui et al. (2001) show that controlling for age, gender, education, and functional status, low emotional support and reception of aid in daily activities from children were significantly associated with poor self-rated-health of elderly parents.

children's schooling on parental longevity. In addition, we provide evidence that allows us to distinguish between some of the mechanisms discussed above.

3 The Compulsory Schooling Reform

In 1948, a parliamentary committee proposed a comprehensive primary schooling reform in Sweden. The key feature of the proposal was to extend the number of mandatory years of schooling from seven to nine years.[9] In order to facilitate an evaluation of the reform, it was decided that the schooling reform was to be rolled out gradually across municipalities during the 1950s and 1960s before implementing it nation-wide. Starting in 1949, 14 municipalities, selected to be representative of the country's population in demographic and geographic terms, introduced the reform (Marklund 1981). More municipalities were then added year by year and in 1962 the parliament decided that all municipalities had to implement the reform by 1969 at the latest. The reform was usually implemented in all school districts within a municipality, with the exceptions of the three largest cities (Stockholm, Gothenburg, and Malmö), where the reform was implemented in different school districts in different years.

In addition to extending the number of mandatory school years, a second feature of the reform was to change the way students were tracked in school. Before the reform, students were tracked in grade 4 or 6. The reform delayed tracking until the 9th grade, meaning that students with different capabilities were kept together for a longer period.

A third feature of the reform was a change to the national curriculum. The most important change was that English became a compulsory subject in reform schools and was taught from the fifth grade. The same requirement was also introduced in non-reform schools in the autumn of 1953 (Hjalmarsson et al. 2015). Except for adding English as a compulsory subject, the reform did not lead to any other changes in the total number of hours taught or to the distribution of hours designated to

[9] In a few larger cities, mandatory schooling was eight years before the reform.

different subjects. A potentially important consequence of the reform was that the demand for teachers increased. The supply of teachers did not keep pace with the demand in the early years of the reform, which meant that some schools had to hire teachers that were not formally qualified. In the later years of the reform period, several teacher colleges were opened and the shortage began to ease in the mid-60s (Marklund 1981). In order to compensate municipalities for the additional financial burden of hiring teachers and expanding school facilities, the government earmarked resources to the municipalities. These resources were ear marked for the new grades 7-9 but the system was abolished in 1958.[10]

In section 7.4 we discuss how the various features of the reform affect our use of reform exposure as an instrument for schooling.

4 Institutional Context

In this section, we provide a brief description of the Swedish institutional context, as the extent to which children affect their parents' health when old likely depends on the institutional context.

Sweden can be characterized as a relatively generous welfare state, where elderly people are guaranteed a pension income and are guaranteed health care and long-term care by the state. Despite this, and despite the fact that children since 1979 have had no legal obligation to support and take care of their elderly parents, Swedish children provide quite extensive care to their parents.[11] For parents above

[10]There is a substantial literature that uses changes in the compulsory schooling reform in Sweden. Meghir and Palme (2005) show that the reform increased educational attainment and led to higher labor incomes. Holmlund et al. (2011) use the reform as an instrument for parental schooling to study the causal effect of parent's educational attainment on child's educational attainment, and Lundborg et al. (2014) use a similar strategy to examine the effect of maternal education on the health and skills of sons. Meghir, Palme, and Schnabel (2012) use the Swedish reform to examine the effect of education on both the individuals affected and for their children. Finally, Black et al. (2015) use the exogenous variation in education due to compulsory schooling laws to show that there is a positive effect of educational attainment on risk-taking in financial market for men.

[11]By Swedish law, every elderly person has the right to get support and care from the welfare system. In order to receive care from the public sector, a person applies to his or her municipality of residence, after which the needs of the person are examined by a social worker. The help provided ranges from services provided at home, such as meals-on-wheels and cleaning, to housing at long-term care institutions. The cost of long-term care is means-tested and elderly with little or no income receive care free of charge.

75 living in their own home in 1994, children were found to provide 60 percent of the hours of care they receive annually (Johansson, 2007). This fraction increased to 70 percent by the year 2000. More than 50 percent of children aged 50 and above provide informal care to their parents and among those, females on average provide 4.3 weekly hours and males 1.6 hours (Bolin, Lindgren, and Lundborg, 2008).

The large involvement of children reflects changes across time in the extent to which the public sector provides long-term care to elderly in Sweden. The strong public sector expansion in the long-term care system during the 60s and 70s was followed by a sharp contraction from the 80s and onwards. Whereas the fraction of individuals aged 80 or above that received elderly care was 62 percent in 1980, this share declined to 37 percent in 2006 (Szebehely and Ulmanen, 2008). The decline cannot be solely explained by younger cohorts being more healthy. The probability of getting an application for public elderly care approved has declined substantially over time. It has also become more common that the social services look at the availability of informal care-givers when deciding on the extent of public elderly care provided (Szebehely, 2005).

When it comes to old-age financial support, all Swedes are covered by the public pension system and the retirement age is flexible, where individuals can start claiming retirement benefits as early as age 61. Sweden has a mix of public and private pension schemes, and individuals are allocated to different pension systems depending on the public or private sector affiliation and year of birth of the individual. In general, the longer one works, the higher the pension one receives. Until 1999, the public pension system almost entirely consisted of a national pension plan financed on a pay-as-you-go basis. According to the Swedish Pensions Agency, about 90% of employees receive some pension benefits from their employer as a condition of employment. On average, around 4.5% of the employee's salary is put into employer provided schemes (Thörnqvist and Vardardottir, 2014).[12] For those who have had little or no income from work there is also a guaranteed pension, where the size

[12] In 1999, an individual account system known as the Premium Pension System (PPS) was introduced, where 2.5 percent of labor earnings are invested in public or private funds.

of it is based on how long the person has lived in Sweden. In 2000, the maximum guaranteed pension, which applies to those who have lived in Sweden for at least 40 years, was 2394 SEK per month ($254) before taxes for those who were married, and 2928 SEK per month ($311) for a single person. A tax rate of 30 percent was applied. Since the after-tax guarantee pension may result in a very low income, there are various benefits, such as housing subsidies, that a person can apply for only if he/she receives the guaranteed pension.

The cohorts of parents we study in this paper are among the ones with the lowest pension incomes. In 2008, nearly half of the women aged 65-70 received guaranteed pension *only* (Olsson, 2011). This share is even higher in the age groups 80-85 and 90 and above, where the shares are 80 and 90 percent, respectively. Among males in the same age groups, the shares receiving only guaranteed pension are much lower, reflecting the stronger labor market attachment of males in these cohorts. While only 10 percent of males aged 65-70 received guaranteed pension, 25 and 50 percent in the age groups 80-85 and 90 and above did, respectively, (Olsson, 2011). Since our parent generation is mostly found in the age groups above 80, it means that a substantial fraction of the parents studied can be said to have quite low income. In the data section below, we provide some descriptive statistics on the income of the parents in our data.

5 Data

Our empirical analyses are based on a comprehensive dataset covering all Swedish citizens born during the reform period. This dataset was created by merging a number of registers, including information on educational attainment, municipality of residence, basic demographic information, and causes of death. To create our reform sample, we start with the register of the total population (RTB), including all Swedes born between 1930 and 1980. Using the Multigenerational Register, that links individuals born 1932 and onwards to their parents, we link the parents and children in our dataset.

In order to assign reform exposure to individuals, we use data on which municipalities and parishes individuals grew up in, taken from the 1960 and 1965 censuses. For cohorts born between 1943-1949, we use information from the 1960 census and for those born between 1950-1955 we use information from the 1965 census.[13]

In order to determine which individuals were exposed to the reform, we make use of a reform algorithm, constructed by Helena Holmlund. Together with birth year and municipality of residence when growing up, the algorithm assigns a binary reform exposure variable to each individual in these cohorts, distributed across 1,020 municipalities. The algorithm is able to assign reform exposure to 90 percent of individuals born between 1943-1955 who have non-missing information on municipality of residence. Most of the missing cases, about 50 percent, are in the three largest cities of Stockholm, Gothenburg, and Malmö, where the reform was introduced at different times in different school districts.[14] For non-missing cases, the reform algorithm is able to assign starting dates of the reform in different school districts using parish information.[15]

For a number of reasons, some measurement errors in the reform exposure variable can be expected. First, the reform exposure algorithm assumes that the students were in the right grade according to their age. This is not always the case. Svensson (2008) shows that 88 percent of all children born in 1949 were in the right

[13]The cohorts born between 1943 and 1955 covers the main part of the cohorts affected by the reform. For cohorts born prior to 1943, we have less precise information on the municipality in which the individual grew up.

[14]As explained in footnote 9, some schools in the larger cities also introduced an 8-year school already before the reform. Unfortunately, there is no information on which schools or in which of the large cities the 8-year school had been introduced and the reform algorithm therefore assigns 7 years to all municipalities before the reform. While time invariant differences like having a 7 or 8 year school before the reform is picked up by the municipality fixed effects we have also ran regression where we exclude the 3 largest cities. This does not change our main results (results available on request).

[15]Our empirical design would be compromised if certain types of parents moved to other municipalities in response to reform implementation. This type of endogenous mobility was investigated by Meghir and Palme (2005) and by Holmlund (2008) who both found little reason for concern. Only between 3 and 4 percent moved from a municipality that had not yet implemented the reform to a one that had and an equal share moved in the opposite direction. In addition, the mobility was not found to be systematic on important traits such as parent's education. Therefore, we do not think that endogenous mobility is an important concern.

grade in 1961, reflecting both that some students repeated a class and that some students started school earlier. Second, it is not always possible to assign a sharp starting date of the reform. These measurement problems only concern the cohorts born right around the assumed starting date of the reform and do not affect the consistency of the instrumental variables estimator we use.

Information on schooling for our reform sample comes from the Education Register in 1990 and we impute years of schooling from the highest educational attainment.[16] For parents, we use information on schooling from the 1960 census. For data on parents' income, we use the income and taxation register (IoT). We use the same register in order to study parents' retirement age, defined as the first year in which they received any pension income.[17] We measure parental wealth from the Wealth register that existed in Sweden between 1999 and 2007. In order to measure parental longevity, we use data from the Causes of Death Register. The register started in 1964 and covers all deaths among individuals who were permanently residing in Sweden, irrespective of whether the death took place in or outside Sweden. The register includes information on the date and cause of death, as well as information on where the death took place, until the year 2013.

Our final sample contains about 2.5 million observations on schooling, reform exposure, municipality of residence when growing up, and parental mortality for the reform cohorts and their parents. This sample includes cases were both the mother and father are observed for an individual. Table 1 provides summary statistics for children and Table 2 for parents.

The parent generation in our data includes fathers born between 1899 and 1940 and mothers born between 1899 and 1941. The average birth year for fathers and

[16]We follow Holmlund et al. (2011) and impute years of schooling in the following way: 7 for (old) primary school, 9 for (new) compulsory schooling, 9.5 for (old) post-primary school (realskola), 11 for short high school, 12 for long high school, 14 for short university, 15.5 for long university, and 19 for a PhD university education. Since the education register does not distinguish between junior-secondary school (realskola) of different lengths (9 or 10 years), it is coded as 9.5 years. For similar reasons, long university is coded as 15.5 years of schooling.

[17]The income register starts in 1968 but we only observe pension income from 1974 and onwards. In our analysis on pension age, we therefore restrict our analysis to parents who are observed to retire after 1974.

mothers is 1917 and 1920, respectively. We observe a large fraction of parents dying in our sample; 72 percent of mothers and 86 percent of fathers. As illustrated in Figure 1, which illustrates the survival rates to ages 80, 85, and 90 for the parent generation, we see a steady increase in survival for all ages considered and for both males and females.

We have information on fathers in 95 percent of the cases and mothers in 98 percent of the cases. The reason for the missing information is mainly that some parents did not survive until 1947, when personal identifiers were introduced in Sweden. If the parent is deceased prior to 1947, he or she will not be included in the multigeneration register. In addition, we lose 2 percent of fathers and 0.05 percent of mothers by imposing the restriction that the parent should be born 1899 or later. The latter restriction is made since we cannot be sure at which age a parent born in, for example, 1890 deceased, in cases where a death age is missing in the data. We know that such a parent must have survived until 1947, since he or she is included in the data, and we know that the parent cannot have survived until 2013, which is the last year of the causes of death register in our data. Since the causes of death register starts in 1964, we thus know that the parent deceased at some point between 1947 and 1964 but we cannot tell at which age. For observed parents born from 1899 and onwards, however, we can use data from the 1965 census and conclude that they must have deceased before the age of 65 if they lack a death date and if they are not included in the census. Since survival to age 65 is one of the outcomes we study we are thus able to include parents born 1899 and onwards in the sample. In the robustness section, we show that excluding cases with missing parents and those with parents born before 1899 has no consequences for our findings.

Table 2 also illustrates that the parents in our sample had rather low incomes in the year 2000, where a large fraction had reached retirement age.[18] Among mothers, the average before tax (pension) income was about 8,200 SEK monthly,

[18]The fact that a large fraction of the parents had pension incomes in 2000 also explains why average earnings were greater in 1968, when none of them had retired yet.

corresponding to USD 1,430 using 2014 prices. The corresponding figure for fathers was around USD 2,323. Incomes of their children, amounted to USD 3,875 and USD 2,846 for males and females, respectively.

In our data, we observe on average 3.3 children for each parent. We observe 1,308,455 children distributed across 801,262 mothers and 1,239,511 children distributed across 755,901 fathers.

6 IV methodology

We base our empirical analyses on the following two equations:

$$(1) \quad S_{icm} = \pi_0 + \pi_1 R_{cm} + \theta_m + \delta_c + trend_m + \varepsilon_{icm},$$

$$(2) \quad Y_{icm} = \gamma_0 + \gamma_1 S_{cm} + \theta_m + \delta_c + trend_m + \varepsilon_{icm},$$

In Equation (1), S_{icm} denotes years of schooling of individual i, belonging to cohort c, and growing up in municipality m. Reform exposure is measured by a dummy variable, R_{cm}, taking the value of one if the individual was exposed to the reform. θ_m and δ_c are municipality and cohort fixed effects, respectively, and $trend_m$ denotes municipality-specific linear trends.[19] Equation (1) is our first-stage regression. In Equation (2) Y_{icm} indicates survival of person i's parent to various ages. In our main analyses, we focus on survival in 5-year intervals between ages 65-90. The parameter of prime interest is γ_1, capturing the causal effect of child's schooling on parents' survival. We cluster our standard errors at the municipality level.

In order to interpret γ_1 as the casual effect of schooling on parental survival, two assumptions need to be fulfilled. First, reform exposure must act as a sufficiently strong instrument for schooling. Second, reform exposure should affect parental survival only through its effect on years of schooling. In the subsequent sections, we will investigate whether these assumptions are fulfilled.

[19]The municipality fixed effects and municipality-specific linear trends refer to the municipality in which the individual grew up in.

Since parents with multiple children born during the sample window appear multiple times in the data we use weights to adjust for this in the analyses. By weighting our estimates by the number of children to each parent, we give equal weight to each parent in our regressions. For a parent with three children, for instance, who therefore appear three times in the data, each observation of that parent is weighted by one third, so that the sum of the weights equal one. Without such an adjustment, we would give greater weight to high-fertility parents in our regressions.[20]

7 Is Reform Exposure A Valid Instrument?

Before reporting our main results, this section provides evidence on the validity of using reform exposure as an instrument for schooling.

7.1 Does reform exposure affect schooling?

We start by checking the predictive power of our instrument. Table 3 shows the effect of reform exposure on years of schooling among males and females, using a number of different specifications.

Panel A shows the effects of reform exposure among males. In the first column, we only include birth cohort fixed effects. In this specification, reform exposure has a strong, positive, and significant effect on years of schooling. Males exposed to the reform have 0.67 more years of schooling compared to other males. Since reform exposure was not random, this specification might overstate the reform effect, as municipalities with higher average levels of schooling were more likely to implement the reform in the early years. In Column 2 we add municipality fixed effects, thus accounting for differences in time-invariant observed and unobserved factors across municipalities. As expected, the effect of reform exposure is now reduced in

[20]By assigning multiple children to each parent, our sample also includes parents who have children that are both exposed and unexposed to the reform. However, as it turns out, 83 percent of the parents have children where all or none are exposed to the reform. The effect of an increase in one child's schooling might be weaker for parents with both exposed and unexposed children but by weighting the estimates by the number of children such parents (from potentially larger families) will receive the same weight as other parents in the analyses.

magnitude and in this specification, reform exposure increases years of schooling by 0.24 years on average. The F-statistic, shown at the bottom of the table, reveals that reform exposure is a sufficiently strong instrument with a F-value well above the common rule of thumb.

While the DiD specification in Column 2 accounts for time-constant heterogeneity across municipalities, it does not address the potential influence of time-varying unobserved heterogeneity. In Column 3, we therefore add linear trends that are allowed to vary across municipalities. This increases the effect of reform exposure to 0.30 and the F-statistic to 180. This is our preferred difference-in-differences specification, where the underlying assumption is that conditional on birth cohort fixed effects, municipality fixed effects, and municipality-specific trends, exposure to the reform is as good as random. We can also check the sensitivity of our first-stage results to the addition of county-by-year fixed effects.[21] As shown in Column 4, adding county-by-year fixed effects to the first-stage regression hardly affects the effect of reform exposure on schooling. Finally, an alternative way of addressing possible time-varying changes across cohorts is to add controls for parental schooling to the regressions. In Column 5 we show that this is of little consequence; the estimates are virtually unchanged in comparison with the specification in Column 2.

In panel B, we run the corresponding first-stage regressions for females. The impact of the reform is somewhat weaker among females. This is expected, since more females than males were already proceeding beyond 7 years of schooling before the reform was implemented. Again, the estimates are robust to the various specifications and the F-statistics show that reform exposure is a strong instrument.

7.2 Parallell trends?

While the implementation of the reform was not random, a key assumption

[21] As recently shown by Stephens and Yang (2014), IV estimates using U.S. compulsory schooling laws often change sign and significance with the addition of region by year controls and are thus not robust across reasonable specifications. Sweden is divided into 20 regional county councils, whose main responsibilities are to provide health care and public transportation.

of any difference-in-differences specification is that treated and non-treated units follow similar pre-reform trends in important characteristics. To shed light on this assumption, Figure 2 plots pre and post-reform trends in parental survival to ages 75-90 for children born between 1934 to 1955 in reform municipalities. We divide the sample into early, mid, and late adopting municipalities since they constitute each other controls in the DiD-model. Early adopters are defined as municipalities whose first affected cohorts were born between 1943-1946. Mid and late adopters are those whose first affected cohorts were born 1947-50 and 1951-1955, respectively.

Figure 2 illustrate two things. First, children living in early, mid, and late adopting municipalities of the reform looked rather similar in terms of parental survival before the reform was initiated (pre-1943 cohorts). Differences are in general small although we see that parents to children in late adopting municipalities are less likely to survive to age 90. Second, trends in parental survival are similar for all three groups even before the reform was implemented. This suggests that the parallell assumption is fulfilled in our context.[22] We provide further evidence in Section 10.1 where we provide an event study analysis on the effect of reform exposure on parental mortality.

7.3 Other unobservables?

Were there other unobservable factors that were associated with reform exposure? In 7.1, we showed that adding parental schooling to the first stage regression does not affect the point estimates once we take out birth cohort and municipality fixed effects. As an alternative test, we can also try to predict reform exposure by parental schooling, using our preferred specification. We first run regressions on the effect of parental schooling on the child's reform exposure without controlling for municipality fixed effects. As shown in Panel A of Table 4, both mothers and fathers' schooling are positively and significantly associated with the reform exposure, illustrating again that the schooling reform was not randomly implemented

[22] We provide graphs for survival until ages 65 and 70 in Figure A1 in the appendix. The patterns are similar.

across municipalities. These results confirm that, in a given year, the reform was more likely to be implemented in municipalities where the parent generation held a higher level of schooling on average. However, when we add municipality fixed effects and municipality-specific linear trends, as shown in Panel B, the significant correlations between parental schooling and children's reform exposure are wiped out and the point estimates get tiny. This is reassuring since it is in line with our assumption that conditional on municipality and birth year controls, reform exposure is as good as random. It also means that with this empirical design, any significant correlations between children's reform exposure and parental survival do not run through parental schooling.[23]

7.4 Does the exclusion restriction hold?

Even if reform exposure can be viewed as random in our DiD-setup above, we also need to assume that children's reform exposure only affects parental mortality through its effect of children's schooling. In this section, we disuss potential threats to the exclusion restriction.

As described in Section II, the reform also postponed tracking to the 9th grade and one might be concerned that this change in tracking of the children had an independent effect on parental longevity. The change in tracking was clearly less dramatic than it sounds, however, since students in the new school system were allowed to choose between different types of courses and between harder and easier courses in key subjects such as mathematics and foreign languages. In a thorough description of the schooling reform, Marklund (1987, p. 180) notes that "the reform school between 1955 and 1960 conformed to a streaming system that in terms of routes was not too much different from the old parallel school with one common

[23]Another concern would be the existence of other concurrent reforms that affected the longevity of parents, such as health care reforms. Since health care is organised at the county level, however, we can fully account for any such concurrent reforms by running regressions that include county-by-year fixed effects. As shown in the section 8.2 our main results do not change when we include county-by-year fixed effects. Yet another concern would be that reform participation is associated with mothers' and fathers' age at birth of the child. We have checked this by looking for an effect of reform exposure on parents' year of birth. The estimates were small and insignificant, however (results available on request).

school route and one junior secondary school route". The tracking in 9th grade was abandoned in 1969 when only ability grouping in mathematics and English was kept. This means that all cohorts in our study, except for the 1954 and 1955 cohorts, were subject to tracking in the old system or to the system of parallel classes that allowed for tracking in the reform period.[24][25]

The two other main features of the reform, the introduction of English as a compulsory subject and ear marked resources to grades 7-9, are unlikely to be major threats to the exclusion restriction. English was introduced in all schools in 1953 meaning that both treated and untreated cohorts born 1946 and onwards was exposed to English in 5th grade. The system of ear marked resouces was abolished in 1958 meaning that it did not affect students from cohorts born 1945 and onwards since they entered 7th grade in 1958 or later. As we show in the robustness section, our results are robust to excluding cohorts born 1943-1944.

Finally, the shortage of teachers that existed during the early phase of the reform period may have initially reduced the quality of teachers. As the results are robust to excluding the early cohorts this initial shortage is unlikely to violate the exlusion restriction.

Summing up, our reading of the Swedish schooling reform makes us conclude that the most important feature of the reform was the extension of mandatory years of schooling and that the other features do not constitute important threats to the exclusion restriction. This conclusion is shared by Hjalmarsson et al. (2015) but we note that some previous papers on the reform have focused on the reduced form estimates (see for instance Meghir et al. 2012). Focusing on years of schooling facilitates the comparison of our estimates to other estimates on the returns to schooling.

[24] In the robustness section, we show that the results are robust to excluding the 1954-1955 cohorts.

[25] The literature on tracking in developed countries often find small and insignificant effects; see the overview by Betts (2011). In a paper by Kerr, Pekkarinen and Uusitalo (2012), who study the impact of reform in the Finnish school system that delayed tracking from age 11 to 16, but that did not change the mandatory years of schooling, they find no average impact on cognitive test scores.

8 Results

We start our empirical analysis by replicating previous findings of a positive relationship between children's schooling and parental survival. We then turn to our instrumental variables estimates, using reform exposure as an instrument for schooling.

8.1 OLS estimates

In Table 5 we show OLS estimates of the relationship between children's schooling and parental survival where we control for parental education and income (measured in 1970), in addition to birth year fixed effects, municipality fixed effects, and municipality-specific trends.[26] In Panel A, we include both sons and daughters and both parents (if both are observed). The different columns show linear probability estimates of the relationship where the outcomes is parental survival in 5-year intervals between ages between 65 and 90.

The estimates in Panel A imply that one additional year of schooling for children is associated with a 1 percentage point increase in the probability of parents surviving until age 75. The point estimates are significant for all the age thresholds considered. In Panels B and C we see that the estimates are similar when analyzing sons and daughters separately. Our results thus confirm those obtained in the recent papers by Friedman and Mare (2014) and Torssander (2013).[27]

In order to put our OLS estimtates into context we can illustrate what the estimates would imply in terms of changes in parental survival over time. Average (weighted by family size) schooling increased by 0.95 years between the earliest (1943) and latest (1955) children cohort in our data. If we take our OLS estimate on the relationship between schooling and parental survival to age 80 from panel

[26] In these regressions we also control for reform exposure, since we want to net out the variation in schooling coming from reform exposure in these regressions.

[27] Since Friedman and Mare (2014) studied the effect of sending a child to college, we have also replicated this finding. We find large and significant estimates at all survival ages studied. Our estimates suggest that a college degree is associated with a 2.9 percentage points increase in the probability of the parent surviving to age 65. The corresponding number for survival until age 80 is 6.7 percentage points.

A as an example, an increase in schooling by 0.95 implies an increase in parental survival by 1.23 percentage points. Since parental survival to age 80 increased by 6.3 percentage points between children cohorts 1943 to 1955, we can "attribute" 20 percent of the increase in parental survival to the increase in schooling. For survival to ages 75, 85, and 90 we can "attribute" 22, 20, and 39 percent, respectively, to the increase in schooling. Hence, if the OLS estimates would represent causal estimates, children's schooling would play an important role for the increase in parents' longevity over time.

8.2 Instrumental variables estimates

In Table 6, we turn to our instrumental variables estimates of the effect of children's schooling on parental longevity. In all subsequent tables, our preferred specification includes birth cohort fixed effects, municipality fixed effects, and municipality-specific linear trends.

In Panel A of Table 6, we show the effect of children's schooling on parental survival until ages 65-90. The contrast with the OLS estimates is large; the effects of schooling on parental survival until ages 65-90 are small and insignificant. Moreover, it is not just a matter of precision; the point estimate at age 85, for instance, is only a quarter of the corresponding OLS estimate. When we make pairwise comparisons between the IV and OLS coefficients we can reject the null that they are the same in 4 out of 6 cases at the 10 percent level.[28]

The instrumental variables estimates obtained above are robust to including county-by-year fixed effects, as shown in Table A1 in the appendix. Taken together, these results thus suggest that the positive relationship between children's schooling and parental longevity obtained in the OLS analyses reflect the influence of unobserved characteristics or reverse causality, rather than a causal impact of

[28] For the comparions we follow Clogg et al. (2005) and calculate Z-values on the differences through the formula $\frac{\beta IV - \beta OLS}{\sqrt{(se_{IV}^2 + se_{OLS}^2)}}$.

children's schooling.

8.3 Heterogeneity

Next, we ask if the mean impact of children's schooling estimated above masks important heterogeneity. First we consider the gender of the child and the parent. We then proceed by studying heterogeneity by the education and income of the parents and by family size.

Differences in the effects by the gender of the child could be expected if, for instance, the returns to schooling differ across genders and if females more often act as care-providers to their elderly parents. Panels B and C of Table 6 reveal that while the effects among male children are small, negative, and insignificant the effects among females are mostly positive, larger in magnitude, and significant at the 10 percent level when studying survival to ages 75 and 80. The latter two estimates are similar in magnitude to the corresponding OLS estimates and we cannot reject the null that they are the same.

We next examine heterogeneity by the gender of the parent. In the parent generation, the labor market participation rate of women was much lower than that of men and women's pension were therefore on average substantially lower than that of men (as shown in Table 2). The mothers in our sample may, therefore, constitute a more financially vulnerable group than the fathers; meaning that children's resources may matter more to their welfare and survival. On the other hand, elderly fathers are often in worse shape healthwise.

Table 7 shows the effect of children's schooling on fathers' survival. In Panel A, the point estimates are positive and significant at the 1 or 10 percent level when considering parental survival to ages 75 and 80, whereas the point estimates are also positive but insignificant when we consider other ages. As shown in panels B and C revels the effects at ages 75 and 80 are driven primarily by the effect of female education. For survival to age 75 the female estimate is double in size compared to the estimate for male education and at age 85 the estimate is almost 10 times larger.

In Table 8 we show the effect of children's schooling on mothers' survival. The estimates are small and insignificant at almost all ages and this result holds when studying male and female children are separately, as shown in Panels B and C.

In summary, our heterogeneity analysis shows that children's schooling has an effect on fathers' survival but no effect on mothers' survival. Moreover, it is mainly the schooling of females that affects father survival. The pattern is consistent with the common finding that females are the most important care providers and that elderly fathers are more frail than elderly mothers and therefore in greater need of care for a given age.

We next study if low-educated and low-income parents gain more than other parents from having well-educated children. Panels A and B of Table 9 show estimates on the effect of schooling on parents who belong to the bottom quartile of the income distribution. We measure parents' income in 1968, which is the earliest year for which we have data on income. The effects are small and insignificant for both low-income fathers and low-income mothers.

In Panels C and and D, we show results for those with low-educated parents. Note that a higher education was uncommon among the parent cohorts and 73 percent of the parents in our sample only had primary school education. When we restrict the sample to parents with the lowest level of schooling; the estimates for fathers' survival to ages 75 and 85 are positive, large, and signficant at the 1 or 5 percent level, whereas the effects at other ages of survival are also mostly positive but imprecisely estimated. For mothers, we obtain no significant estimates.

We can also investigate if the positive effects on low-educated fathers' survival are driven by female schooling, in line with the patterns shown in Table 7. This is indeed the case as shown in Table A2 in the appendix where female schooling positively and significantly affects survival to all ages between 70-85.

Another potentially important source of heterogeneity is family size. A larger family means that more children can share the burden of caring for their parents which means that additional schooling of a particular child might be less important. We can check this by running separate regressions for single-child families. As shown in Appendix Table A3 there is a tendency towards greater point estimates when we pool all children and parents but none of the estimates reach statistical significance. When we study the impact of female schooling (panel C) we obtain a statistically significant estimate for parent survival to age 80.

Summing up, these results point to the importance of moving beyond the mean impact when studying the effect of parental schooling on parental longevity. We find positive effects of children's schooling on fathers' longevity where the effects are driven by female schooling and are especially strong for female children to low-educated fathers.

9 Mechanisms

In order to investigate potential mechanisms behind our results we next perform two sets of analyses. First, we estimate the effect of schooling on some potentially important child outcomes including distance to parents, likelihood of entering high school and university, earnings, labor supply, spousal schooling, and marital status. Second, we perform a set of analyses on parental outcomes including parental wealth, income, labor supply, and retirement age.

9.1 Distance to parents

We start by analysing whether higher education increases the geographical distance to parents, due to greater job opportunities associated with education, for instance. Distance to children has been found to be an important source of parents' welfare. If children who obtain more schooling are more likely to move away and locate at further distances from their parents, compared to low-educated children, this could negatively affect both the physical and mental health of parents and such effects could balance out any positive effect. Physical health could also be affected if children are important informal-care givers and if formal care does not fully substitute for informal care.[29] Mental health could be affected if longer distance to the children means less physical contact and thereby a reduced incentive for parents to invest in their health.

We can test the distance hypothesis by running regressions on the effect of schooling on the likelihood that an adult child resides in the same municipality as his or her parents. To this end, we make use of data from the register of the total population (RTB) that records the municipality of residence each year for the entire population. As our main outcome we focus on whether or not the child was living in the same municipality as his or her parents at age 30. At this age, most children have completed their studies and might have moved in order to get a job. In addition, most parents are still alive and we can keep the sample rather intact.

The results in Panel A of Table 10 show that increased schooling among females indeed increases the distance to one's parents, as measured through an increased probability of not living in the same municipality as their parents. The effect is smaller and insignificant among males.

We can also investigate if the effect of female schooling on distance reflect a higher probability of moving in order to attend higher studies, i.e. beyond 9 years of schooling. If moving also means an increase in the likelihood of attending college or university this may explain why we still see some positive effects of female schooling on parental survival. We check this by running regressions on the direct effect of reform exposure on (1) attending high school and (2) attending university. As shown in Panel B of Table 10, reform exposure increases the likelihood of attending high school for females but does not affect university attendence.

For males, we obtain positive and significant effects on both entering high school and university. If anything, males thus seem more likely to attend higher studies than females when exposed to the reform.

[29] On the other hand, positive effects of increased distance might arise if elderly parents rely more on formal care-giving and if such care-giving is of greater quality than informal care-giving.

9.2 Returns to schooling

We next examine the effect of children's schooling on earnings, labor supply, spousal schooling, and marital status.[30] If more schooling increase the children's earnings it means that parents can spend more money on themselves and less on their children which could potentially lead to increased survival of the parents. In addition, positive returns to schooling means that the child has greater means to support an elderly and ill parent. Schooling might also affect the likelihood of getting married and the quality of the spouse in terms of education which, in turn, might have an effect on parental survival.

The results in panel A of Table 11 show that additional schooling only has a small and insignificant effect on female earnings, however. In addition, female schooling does not increase labor market participation and we can thus rule out that the distance effect found above reflects that females who obtain more schooling move to better paid jobs in other municipalities. The effect might instead reflect that higher educated females move to other types of jobs that does not necessarily pay more. We have also checked if there are positive returns to schooling for females to low-educated fathers, where we previously estimated a positive effect of schooling on fathers' survival, but the effects are small and insignificant in that case as well.

Females who obtain more schooling do marry higher educated males, however, and the results in Table 11 suggests that one additonal year of schooling increases spousal years of schooling by 0.3 years. This suggests that the total human capital of the household increases when females obtain more schooling although the likelihood of getting married in itself does not change. The effect on spousal schooling may also partly explain the distance effect where more educated females are moving away from their parents; such an effect could arise if higher educated spouses are more likely to live and work in other places, and if the work of the spouse is given

[30]For these analyses, we construct a measure of total income between 1980 and 2000. We then use the log of this measure as an outcome. Labor market participation is defined as having positive earnings and we take the average of this measure over the period 1980-2000. Only children surviving to the year 2000 are included in the analyses.

priority in the household.

For males, one additional year of schooling increases earnings by 5.4 percent but labor market participation remains unaffected (Panel B). Moreover, males who obtain more schooling are not more likely to get married and do not marry females with higher education.

Summing up, the results suggest that males obtain greater monetary returns to education and are more likely to enter higher education when exposed to the reform. In addition, males are more likely to live close to their parents. These pattterns do not translate into a positive effect of male education on parental longevity and one explanation for the absence of an effect is that there are offsetting effects at work. An increase in income increases the opportunity cost of providing care for one's parents and this effect may offset any positive effect of male children staying in the municipality of their parents and having a higher income. Another explanation is simply that males are less likely to be involved in the care of their elderly parents.

For females, the fact that higher education increases the distance to their parents does not translate into a negative effect on parents' age at mortality. One reason might be that increased schooling translates into better health knowledge and that this effect overrides that of greater distance. Better health knowledge may for instance translate into higher-quality informal care and better knowledge in how to navigate the health care and long-term care systems. Since females are typically primary care-givers the effect of increased health knowledge might be larger for females than for males. Moreover, females who obtain more schooling marry higher educated males so that the household level of human capital increases. It is also possible that increased distance leads to a substitution from informal to formal care-giving to the parents and if the latter type is of higher quality, parental health may improve. In addition, the lack of monetary returns to schooling among females means that there is no increase in the opportunity cost of caring for their parents.

9.3 Parental wealth and labor market outcomes

We next consider the effect of children's schooling on parental wealth and labor market outcomes. We start by examining parental financial wealth since the burden of financially supporting children may be reduced for parents with well-educated children. A similar effect could occur if well-educated children earn more and transfer wealth to their parents or provide financial advice to their parents.[31] We find no support for any of these hypotheses however since the estimates shown in the first column of Table 12 are all small and insignificant.[32]

In columns 2 and 3 we examine if children's schooling affect their parents income or labor supply.[33] We find no effects on parental income for either parent but for fathers we obtain a small and negative effect on labor supply that is significant at the 10 percent level. For mothers, the corresponding estimate is positive and significant and implies that one additional year of child schooling increases maternal labor supply by 1.5 percentage points. In Table A4 in the appendix we investigate whether these effects differ by the gender of the child. We see that the labor supply effect is mainly driven by the effect among female children but other than that we see no significant effects for any gender.

In column 4 we also investigate if children's schooling affect their parents retirement age, defined as the age at which they first started to receive any retirement income. We obtain no evidence that parents' retirement age is affected and the same is true when we investige the effects by the gender of the child.

Taken together, these results suggest that the absence of an average impact of children's schooling on parental survival might reflect that children does not seem to affect economic outcomes of parents.[34] Moreover, the positive effect of female

[31] It is not clear a priori how transfers from children would affect parental labor supply and earnings. If wealth is transferred parents may response by working less. If the transfers make the parents healthier, however, they may respond by working more.

[32] Financial wealth includes all financial holdings at banks and credit institutes. We have also studied total wealth but since that measure includes persons with negative holdings we chose to focus on the former. When studying a measure that ranks people according to their total wealth, we obtain similar results as for financial wealth, i.e. insignificant estimates.

[33] We calculate the average over the years between ages 50-60 for which we observed non-missing values. Labor supply is measured as having positive earnings.

[34] Another explanation might be that education simply does not change the amount of time

schooling on fathers' survival cannot be explained by increased economic resources of the parent and, instead, the effect may reflect better health knowledge among females who obtain more schooling (and their spouses) as discussed above.

9.4 Causes of Death

Next, we investigate if the effect of children's schooling differs across various causes of death. As noted by Friedman and Mare (2014), if well-educated children positively affect their parent's health behavior, we might expect stronger effects for lifestyle-related causes of death. Such effects could be hidden when one focuses on all-causes mortality.

In Table A5 in the appendix we show results for some of the major causes of death, where several of them are believed to have a strong lifestyle component. The outcome in these regressions is whether or not the parent died before a certain age and for a specific cause of death. For the sake of exposition, the results are only shown for the specification where we pool all parents and all children.

In Panel A, we show results for cancers, where the estimates are negative but small and insignificant across all survival ages. Since some cancer-related deaths are believed to be more lifestyle-related than others, we have also checked results separately for lung cancer and liver cirrhosis. The former cause of death was found to be affected by children's schooling in Friedman and Mare (2014). Our estimates for lung cancer and liver cirrhosis are small and insignificant, however.

Panels B-E show results for heart disease, respiratory conditions, mental and behavioral disorder, and accidents and external causes. The overall picture is that children's schooling does not affect any of these causes of death. One exception is respiratory conditions, where we obtain a *positive* effect of schooling on the probability of a parent dying because of such condition before the age of 65. This effect has the opposite sign from what one would expect and probably occurs by chance when running a large number of regressions. Overall, we obtain no evidence that

children spend with children much. While we lack data on time use of children a recent Swedish paper shows descriptive evidence that differences in informal care-giving are quite small between different educational groups in Sweden (Berglund et al. 2015).

there are any differences in the average impact of children's schooling across causes of parental death.

10 Additional Robustness Checks

In the following section we provide a number of additional robustness checks. First, we provide an event study analysis in order to examine the parallell trend assumption in more detail. Second, we test to what extent our results are sensitive to adding additional control variables. If our main difference-in-differences specification is correct the magnitude of the estimates should not change much when we add additional (pre-determined) control variables. Third, we test if our results change when we narrow the window of parent cohorts. The parents in our sample our born across a 42-year window (1899-1941) where life expectancy has increased substantially and where many other things have changed as well. Fourth, we test the sensitivity of our results to dropping certain cohorts of children who were subject to some specific features of the reform, such as poor teacher quality. Fifth, we test whether our results are sensitive to dropping the small number of cases where we are unable to link children to parents.

10.1 Event study analysis

In order to construct event study graphs, we use the same difference-in-differences specification as in our main analysis but replace the binary reform exposure indicator with a set of dummies indicating the timing of reform exposure. The estimates of these dummies both illustrate the validity of the parallell trend assumption and the dynamics of the reform exposure effect. If the assumption is valid, we expect the estimates of the pre-treatment dummies to be all at zero.

We plot the point estimates of the dummies indicating cohorts from 5 years before to 5 years (or more) after the reform in Figure 3.[35] The outcome in the graph is parental survival to age 75. Here, t-1 represent the last cohort in a municipality that was not exposed to the schooling reform whereas 0 represents the first cohort

[35] Cohorts born at t-6 or earlier constitute the reference category.

that was exposed. The pattern in the figure shows no evidence of any pre-trends as all pre-reform estimates are close to zero. After treatment sets in, the coefficients mostly remain at zero which illustrates the absence of an average impact. For completeness, Figure A2 in the appendix show that the parallell trend assumption also holds when considering parental survival to ages 65 and 70.

10.2 Additional control variables

To investigate the sensitivity of our results to adding additional control variables we re-run our main specification (for all parents and all children) and add the following controls: parent's birth year, parental years of schooling, mother's age at birth, and the child's birth order. In our difference-in-difference setting these variables should not be affected by the children's own schooling or reform exposure. The results in Table A6 confirm this and the estimates are similar to those in our main analysis in Table 6.

10.3 Shrinking the window

In Table A7 we test the sensitivity of our results to shrinking the window of parents' birth year. In Panel A we restrict the sample to parents born 1910 onwards and in Panels B and C we put the restriction at 1920 and 1930, respectively. The results are similar to our main estimates in Table 6.[36]

10.4 Dropping cohorts

As discussed in Section 7.4, the reform came with a system of ear marked resouces for pupils in grades 7-9 that affected cohorts born before 1945. Moreover, the earliest cohorts in our data might have suffered from lower teacher quality as there was an initial shortage of teacher in early adoping municipalities. In panel A of Table A8, we show that are results are robust to excluding cohorts born 1943 and 1944.

As we also explained in 7.4 all cohorts in our study, except for the 1954 and

[36]In Panel C, regressions on survival to ages 85 and 90 are lacking since parent cohorts born 1930 onwards are too young to have reached these ages.

1955 cohorts, were subject to tracking in the old system or to the systerm of parallel classes that allowed for tracking in the reform period. As shown in Panel B of Table A8, our estimates do not change much when we drop the 1954 and 1955 cohorts.

10.5 Missing parents

For 2 percent of the children, we lack information on mothers and for 5 percent we lack information on fathers. The main reason for this is that a small fraction of parents do not survive until 1947 when personal identifiers were introduced in Sweden. If education has an effect on the probability of a parent surviving to 1947, our estimates may be biased. For instance, if education would have a positive effect on this probability, we would miss out some of the positive effect of children's education on parental survival. To investigate this issue, we run a regression on the effect of reform exposure on the probability of having missing parental information, using our main IV specification. For fathers, the point estimate is small (-0.004) and insignificant. For mothers, the point estimate is of similar magnitude and significant at the 10 percent level. These results suggest that missing parents cannot drive our main results.[37]

10.6 External Validity

Our IV estimates represent local average treatment effects (LATE), measuring the impact of schooling among the group of compliers. In our context, this group represents children who because of the reform stayed at least 9 years in compulsory school but who would have otherwise stayed 7 years in school. Our IV estimates are therefore identified mainly from variation in schooling at the lower end of the

[37] We can also completely rule out that missing parents are affecting the results by restricting our analysis to later cohorts of children, where almost no parents are missing from the data. When we do so, by running our models on children born 1948 and later, we find similar effects as we find for our main sample that includes the cohorts born 1943 and later.

schooling distribution, which has consequences for the interpretation and external validity of our results. It is not obvious that variation in schooling at the lower end of the schooling distribution has the same consequences for parental mortality as variation in schooling at other parts of the schooling distribution has. We should therefore be concerned about the external validity of our IV estimates.

We can partly address this concern by examining whether the relationship between schooling and parental mortality differs across the schooling distribution when we run simple OLS regression. In Table A9, we show OLS estimates on the relationship between schooling and parental mortality for those with less than 10 years of schooling, where we condition on parental schooling and parental income, in addition to municipality of residence and birth cohort. The OLS estimates are in most cases significant and positive, although somewhat less precisely estimated, and we conclude that there exists a statistical relationship between schooling and parental mortality also at the lower end of the schooling distribution.

The results suggest that the OLS estimates are upward biased even at the lower end of the schooling spectrum. It might seem surprising that unobservables would play such an important role when moving from 7 to 9 years of schooling but the difference between 7 or more years of schooling reflects the tracking of students, based on past performance, at grade 4 or 6 in the "old" system. The more able students were selected into five-year or the three- to four-year long junior-secondary school called realskolan. Students who went 7 or 9 years thus clearly in terms of ability and we would in fact expect ability bias to be important when comparing these two groups.

11 Concluding remarks

The literature on the intergenerational transmission of human capital has usually assumed that the link runs from parents to children. For certain types of human capital, such as health, it is possible, however, that the link runs in the other direction as well. In line with this reasoning, a number of recent papers have found a positive relationship between children's schooling and parental longevity. It has remained unclear, however, if such estimates reflect a causal effect of children's schooling or just simply reflect the influence of unobserved factors shared by parents and children.

This paper aims to fill this gap by providing causal estimates of the effect of children's schooling on parental mortality. For this purpose, we exploit the Swedish compulsory schooling reform, which provides us with exogenous variation in children's schooling. While we can replicate previous findings of a positive relationship between children's schooling and parental longevity, our IV estimates on the mean impact of children's schooling are substantially smaller in magnitude and are statistically insignificant. This suggests that there are important unobserved factors that relate both to parental longevity and children's schooling that confound simple OLS regressions.

The estimated mean impacts hide substantial heterogeneity, however. We find that increases in female schooling has positive and significant effects on the survival of parents and especially on the survival of fathers. When we restrict the sample to low-educated fathers, who are typically among the frailest, the estimates increase in magnitude. Since females are often primary care givers, our estimates suggest that when the group that are most involved in care-giving obtain more schooling, especially fathers gain in terms of longevity.

In our analyses we provide evidence on potential mechanisms behind our findings. We show that the positive effects of female schooling arise despite the lack of an effect of schooling on earnings and on parental economic outcomes, such as financial wealth, income, and pension age. For male children we find positive income returns to schooling but this does not translate into improved survival of parents, perhaps because an increase in income also means an increased opportunity cost of caring and because males are less likely to be primary care-givers to their elderly parents in the first place.

The effect of female schooling on parental survival might be explained by increased health knowledge, leading to higher quality informal care and better contacts with the health and long-term care system, rather than an increase in economic resources. The fact that we only observe the effect for females is not entirely surprising. First, daughters are often found to be primary care-givers to their elderly parents and an increase in their schooling might therefore mean more for parent longevity (Johansson 2007; Bolin et al. 2008). Second, the absence of income returns to schooling among females means that there is no increase in the opportunity cost of caring for their elderly parents. Third, females who obtain more schooling marry more highly educated males so that the total human capital of the household increases. No such corresponding effect was obtain for males who obtained more schooling.

The results have some important policy implications. If female schooling has a positive effect on fathers' longevity this should be taken into account when assesing the total returns to schooling in society. Since the effects are most pronounced among low-educated fathers, increases in female schooling many also serve to reduce education-related inequality in longevity.

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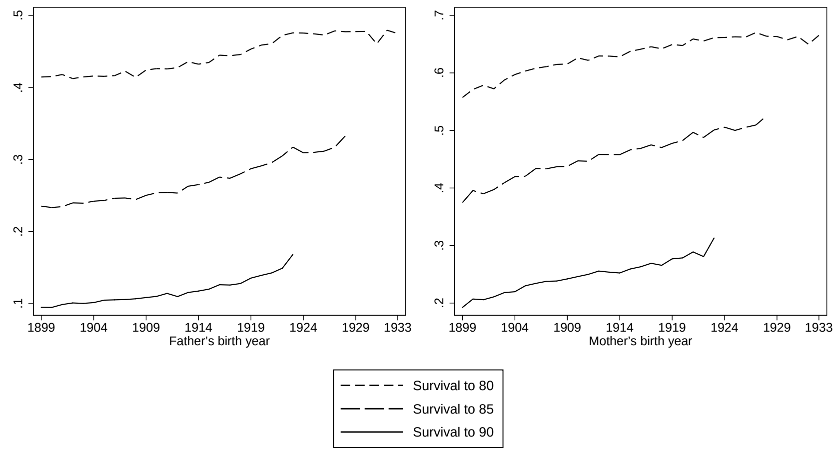
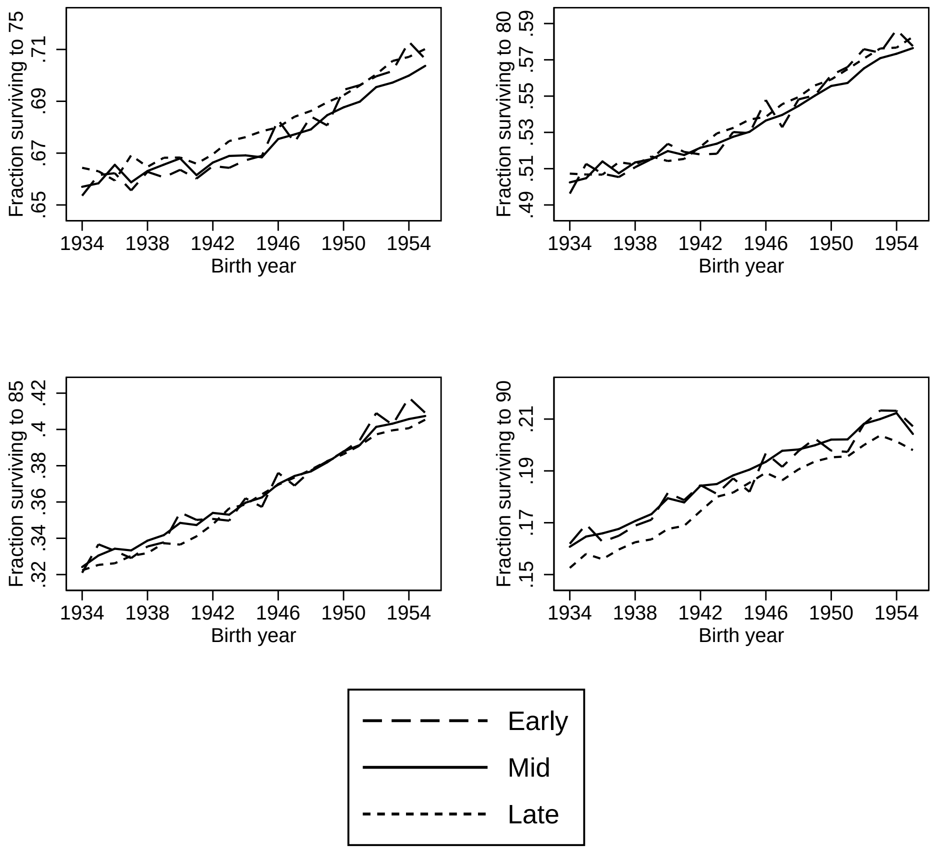
Figure 1: Survival by parent cohort



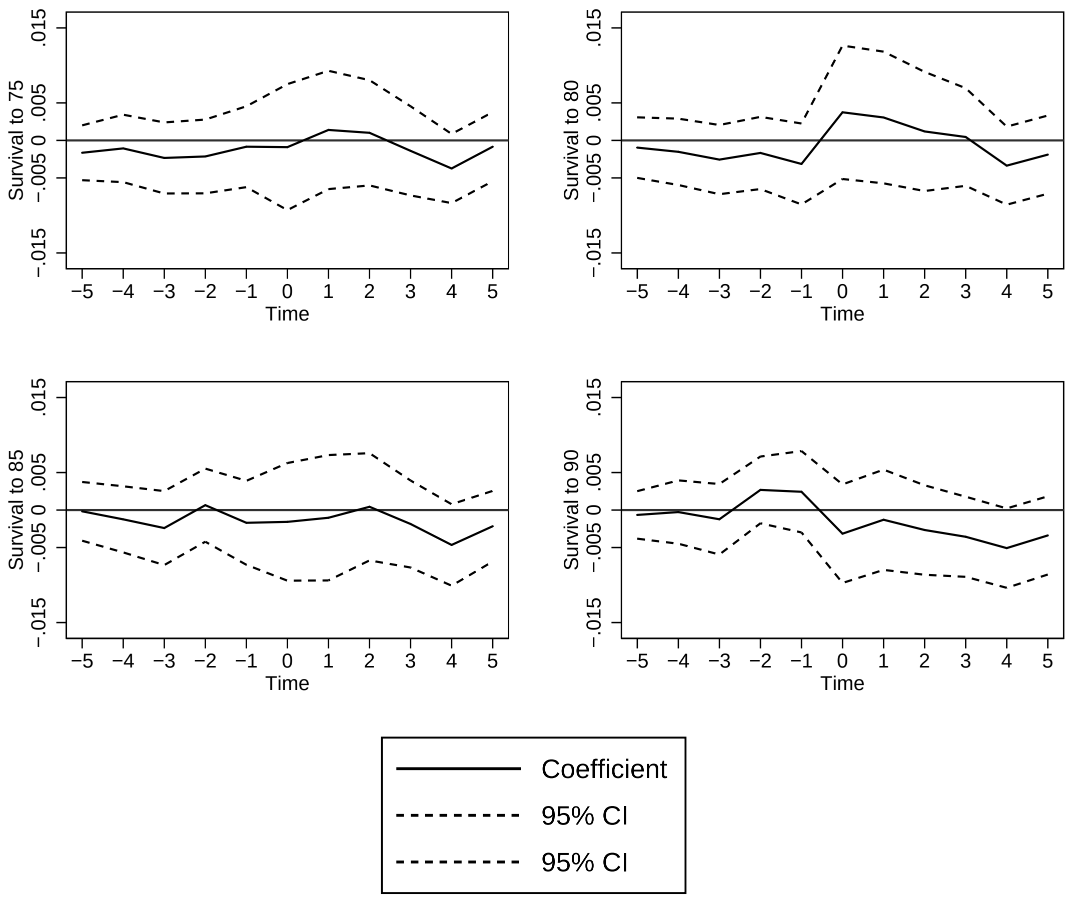
Notes: The graph plots the likelihood of surviving to different ages for the the parent cohorts born 1899-1933 in our data.

Figure 2: Trends in parental survival to ages 75-90 for early, mid, and late adopting municipalities.



Notes: The graph plots mean parental survival by cohorts of children born 1934-1955 and by early, mid, and late adopting municipalities of the schooling reform.

Figure 3: Event study graph. Effect of reform exposure on parents' probability of surviving to ages 75-90. All parents and all children



Notes: The graph plots estimates from a difference-in-differences regression on the effect of the timing of reform exposure on parental survival to ages 75-90. The regression includes dummies for cohorts born before and after the first cohort that was exposed to the reform in a municipality. See text for details.

Table 1: Descriptives

	Mean	Sd	Mean	Sd
	Reform=0		Reform=1	
Years of schooling	11.295	3.01	11.97	2.50
Birth year	1946.80	2.94	1951.79	2.77
Female	0.49	0.5	0.49	0.5
Income year 2000	229,290	222,899	237,293	200,431
N	1,491,964		1,056,002	

Notes: This table shows descriptive statistics by reform exposure status. Standard deviations within parantheses. Means are weighted by the number of children in the family.

Table 2: Descriptives

	Mean	Sd	Mean	Sd
	Fathers		Mothers	
Birth year	1916.90	8.10	1919.95	7.99
Survival 65	0.821	0.383	0.895	0.307
Survival 70	0.728	0.445	0.843	0.364
Survival 75	0.604	0.489	0.765	0.424
Survival 80	0.452	0.498	0.65	0.477
Survival 85	0.282	0.450	0.483	0.5
Survival 90	0.126	0.331	0.271	0.444
Years of schooling	8.694	2.200	8.060	1.854
Income in 1968	201,380	165,497	73,165	56,469
Income in 2000	159,913	106,676	98,687	56,898
N	1,239,511		1,308,455	

Notes: This table shows decriptive statistics for mothers and fathers. Standard deviations within parantheses. Income is measured in year 2000 prices. Means are weighted by the number of children in the family.

Table 3: First-stage regressions

Independent variable:	(1) Schooling	(2) Schooling	(3) Schooling	(4) Schooling	(5) Schooling
			Panel A: First-stage regression, males		
Reform exposure	0.665	0.243	0.302	0.286	0.232
	(0.026)***	(0.046)***	(0.023)***	(0.024)***	(0.046)***
N	1,302,318	1,302,318	1,302,318	1,302,318	1,302,318
F-stat.	635.43	27.99	179.78	146.06	25.91
			Panel B: First-stage regression, females		
Reform exposure	0.478	0.149	0.199	0.170	0.136
	(0.026)***	(0.035)***	(0.020)***	(0.019)***	(0.035)***
N	1,245,648	1,245,648	1,245,648	1,245,648	1,245,648
F-stat	335.56	18.54	96.58	83.44	14.97
Birth FE	YES	YES	YES	YES	YES
Municip. FE	NO	YES	YES	NO	YES
Municip. trends	NO	NO	YES	NO	NO
County-by-year FE	NO	NO	NO	YES	NO
Parental schooling	NO	NO	NO	NO	YES

Notes: This table shows first-stage regressions. Column (1) shows the effect of reform exposure on years of schooling from specifications including only birth cohort fixed effects. The specification in column (2) includes birth cohort and municipality fixed effects. In addition, columns (3)-(5) include: (3) municipality-specific linear trends, (4) county-by-year fixed effects, and (5) controls for mothers' schooling and an indicator of missing information on mothers' schooling. Panel A shows the effect among males and Panel B among females. Standard errors clustered at the municipality level; * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 4: Predicting reform participation

Independent variable:	All children Reform exposure	Males Reform exposure	Females Reform exposure
		Panel A: limited controls	
Parental schooling	0.012	0.012	0.012
	(0.000)***	(0.000)***	(0.000)***
N	2,547,966	1,302,318	1,245,648
		Panel B: extended controls	
Parental schooling	-0.000	-0.000	-0.000
	(0.000)	(0.000)	(0.000)
N	2,547,966	1,302,318	1,245,648
Birth FE	YES	YES	YES
Municip. FE	NO	YES	YES
Municip. trends	NO	YES	YES

Notes: This table shows regressions on reform participation as a function of parental schooling. Panel A shows results while only controlling for birth cohort fixed effects. Panel B in addition controls for municipality fixed effects and municipality-specific trends. Standard errors clustered at the municipality level; * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 5: OLS relationship between children's schooling and parental survival.

Independ. variable:	(1) 65	(2) 70	(3) 75	(4) 80	(5) 85	(6) 90
	Panel A: Males, females, and all parents					
Schooling	0.005	0.008	0.011	0.013	0.013	0.010
	(0.0002)***	(0.0003)***	(0.0004)***	(0.0004)***	(0.0005)***	(0.0004)***
N	2,547,966	2,547,966	2,547,721	2,508,277	2,293,541	1,841,102
	Panel B: Males and all parents					
Schooling	0.005	0.008	0.010	0.013	0.013	0.009
	(0.0003)***	(0.0004)***	(0.0005)***	(0.0005)***	(0.0005)***	(0.0004)***
N	1,302,318	1,302,318	1,302,202	1,282,198	1,172,391	941,229
	Panel C: Females and all parents					
Schooling	0.005	0.008	0.011	0.013	0.015	0.012
	(0.0002)***	(0.0002)***	(0.0003)***	(0.0003)***	(0.0004)***	(0.0004)***
N	1,245,648	1,245,648	1,245,519	1,226,079	1,121,150	899,873

Notes: Panel A shows OLS estimates of the relationship between children's schooling and parents' survival until ages 65-90. Panel B shows OLS estimates on the relationship between sons' schooling and parents' survival. Panel C shows OLS estimates on the relationship between daughters' schooling and parents' survival.al. Specifications include municipality fixed effects, birth cohort fixed effects, municipality-specific trends, and controls for parental education and income (in 1970). Standard errors clustered at the municipality level;* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 6: Effect of schooling on parental survival: Results from instrumental variables regressions.

Independent variable:	(1) 65	(2) 70	(3) 75	(4) 80	(5) 85	(6) 90
			Panel A: Males, females, and all parents			
Schooling	-0.002	-0.001	0.007	0.005	-0.001	-0.002
	(0.004)	(0.005)	(0.005)	(0.006)	(0.008)	(0.007)
N	2,547,966	2,547,966	2,547,721	2,508,277	2,293,541	1,841,102
F-statistic	109.80	109.80	110.23	128.35	141.97	93.03
			Panel B: Males and all parents			
Schooling	-0.006	-0.007	-0.002	-0.005	-0.009	-0.001
	(0.004)	(0.005)	(0.006)	(0.006)	(0.008)	(0.007)
N	1,302,318	1,302,318	1,302,202	1,282,198	1,172,391	941,229
F-statistic	125.85	125.85	125.98	130.62	132.04	97.60
			Panel C: Females and all parents			
Schooling	0.001	0.002	0.015	0.018	0.008	-0.009
	(0.006)	(0.007)	(0.009)*	(0.009)*	(0.011)	(0.010)
N	1,245,648	1,245,648	1,245,519	1,226,079	1,121,150	899,873
F-statistic	48.95	48.95	49.13	62.41	66.07	40.63

Notes: Panel A shows instrumental variables estimates of the effect of children's schooling on parents' survival until ages 65-90. Panel B shows IV estimates of the effect of sons' schooling on parents' survival. Panel C shows IV estimates of the effect of daughters' schooling on parents' survival. Specifications include municipality fixed effects, birth cohort fixed effects, and municipality-specific linear trends. Standard errors clustered at the municipality level;* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 7: Effect of schooling on fathers' survival: Results from instrumental variables regressions.

Independent variable:	(1) 65	(2) 70	(3) 75	(4) 80	(5) 85	(6) 90
			Panel A: Males, females, and fathers			
Schooling	0.004	0.006	0.021	0.013	0.010	0.000
	(0.006)	(0.007)	(0.007)***	(0.007)*	(0.008)	(0.007)
N	1,239,511	1,239,511	1,239,500	1,232,072	1,163,036	982,809
			Panel B: Males and fathers			
Schooling	0.003	-0.001	0.011	0.003	0.001	-0.002
	(0.007)	(0.008)	(0.009)	(0.008)	(0.009)	(0.008)
N	633,715	633,715	633,709	629,938	594,553	502,627
			Panel C: Females and fathers			
Schooling	0.005	0.009	0.023	0.022	0.018	-0.001
	(0.011)	(0.012)	(0.013)*	(0.013)*	(0.012)	(0.010)
N	605,796	605,796	605,791	602,134	568,483	480,182

Notes: Panel A shows instrumental variables estimates of the effect of children's schooling on fathers' survival until ages 65-90. Panel B shows IV estimates of the effect of sons' schooling on fathers' survival. Panel C shows IV estimates of the effect of daughters' schooling on fathers' survival. Specifications include municipality fixed effects and birth cohort fixed effects, and municipality-specific linear trends. Standard errors clustered at the municipality level;* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 8: Effect of schooling on mothers' survival: Results from instrumental variable regressions.

Independent variable:	(1) 65	(2) 70	(3) 75	(4) 80	(5) 85	(6) 90
	Panel A: Males, females, and mothers					
Schooling	-0.005	-0.004	-0.004	-0.001	-0.008	-0.003
	(0.006)	(0.006)	(0.007)	(0.008)	(0.010)	(0.010)
N	1,308,455	1,308,455	1,308,221	1,276,205	1,130,505	858,293
	Panel B: Males and mothers					
Schooling	-0.011	-0.008	-0.012	-0.011	-0.016	0.004
	(0.005)**	(0.006)	(0.008)	(0.009)	(0.011)	(0.011)
N	668,603	668,603	668,493	652,260	577,838	438,602
	Panel C: Females and mothers					
Schooling	0.002	-0.002	0.007	0.016	0.001	-0.020
	(0.009)	(0.010)	(0.012)	(0.013)	(0.016)	(0.015)
N	639,852	639,852	639,728	623,945	552,667	419,691

Notes: Panel A shows instrumental variables estimates of the effect of children's schooling on mothers' survival until ages 65-90. Panel B shows IV estimates of the effect of sons' schooling on mothers' survival. Panel C shows IV estimates of the effect of sons' schooling on mothers' survival. Specifications include municipality fixed effects and birth cohort fixed effects, and municipality-specific linear trends. Standard errors clustered at the municipality level;* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 9: Effect of schooling on parental survival: Results from instrumental variables regressions. Low-educated and low-income parents.

Independent variable:	(1) 65	(2) 70	(3) 75	(4) 80	(5) 85	(6) 90
			Panel A: Low inome, fathers			
Schooling	-0.015	-0.018	-0.006	0.003	-0.000	0.000
	(0.010)	(0.011)	(0.012)	(0.011)	(0.010)	(0.008)
N	309,871	309,871	309,869	308,370	296,261	265,691
			Panel B: Low income, mothers			
Schooling	-0.009	-0.007	0.002	-0.002	0.002	0.001
	(0.008)	(0.010)	(0.012)	(0.015)	(0.020)	(0.021)
N	327,089	327,089	327,015	317,781	279,895	212,787
			Panel C: Low education, fathers			
Schooling	0.009	0.013	0.029	0.020	0.012	-0.006
	(0.006)	(0.007)*	(0.008)***	(0.008)**	(0.009)	(0.007)
N	587,311	587,311	587,305	582,964	542,103	437,060
			Panel D: Low education, mothers			
Schooling	-0.002	-0.003	-0.006	-0.004	-0.009	0.004
	(0.005)	(0.006)	(0.007)	(0.007)	(0.009)	(0.009)
N	810,948	810,948	810,779	787,487	685,002	496,344

Notes: This table shows instrumental variables estimates of the effect of children's schooling on parents' survival until ages 65-90. Sample restricted restricted to low-income parents (Panels A and B) and low-educated parents (Panels C and D). Specifications include municipality fixed effects, birth cohort fixed effects, and municipality-specific linear trends. Standard errors clustered at the municipality level;* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 10: Instrumental variables and reduced form estimates of the effect of education on various potential mediators

Independent variable:	(1)	(2)	(3)	(4)
	Male children		*Female children*	
	Panel A: Living together with parents at age 30			
	Mothers	Fathers	Mothers	Fathers
Schooling	-0.006	-0.013	-0.038	-0.027
	(0.010)	(0.011)	(0.015)**	(0.014)**
N	616,623	524,527	589,435	500,852
	Panel B: Further studies			
	High school	University	High school	University
Reform exposure	0.016	0.005	0.007	0.003
	(0.004)***	(0.002)**	(0.004)*	(0.002)
N	1,302,318	1,302,318	1,245,648	1,308,355

Notes: Panel A shows instrumental variables estimates of the effect of schooling on the probability of living in the same municipality as one's parents at age 30. Panel B shows the effect of reform exposure on the probability of entering high school and university. Panel C shows the effect of schooling on earnings and labor market participation. Columns (1) and (2) show results for male children and columns (3) and (4) show results for female children. Specification includes municipality fixed effects and birth cohort fixed effects, and municipality-specific linear trends. Standard errors clustered at the municipality level;* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 11: Instrumental variables and reduced form estimates of the effect of education on various potential mediators

Independent	(1)	(2)	(3)	(4)
	Panel A: Female children			
	Income	Income>0	Spouse schooling	Marriage
Schooling	0.054	0.003	0.295	0.003
	(0.021)**	(0.002)	(0.102)***	(0..014)
N	1,253,841	1,253,841	759,247	1,245,650
	Panel B: Male children			
	Income	Income>0	Spouse schooling	Marriage
Schooling	-0.033	0.003	0.073	0.011
	(0.035)	(0.004)	(0.060)	(0.008)
N	1,210,503	1,210,503	750,791	1,302,318

Notes: Panel A shows instrumental variables estimates of the effect of schooling on the probability of living in the same municipality as one's parents at age 30. Panel B shows the effect of reform exposure on the probability of entering high school and university. Panel C shows the effect of schooling on earnings and labor market participation. Columns (1) and (2) show results for male children and columns (3) and (4) show results for female children. Specification includes municipality fixed effects and birth cohort fixed effects, and municipality-specific linear trends. Standard errors clustered at the municipality level;* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 12: Effect of schooling on parental outcomes: Results from instrumental variables regressions.

Independent variable:	(1) Wealth	(2) Labor market part.	(3) Earnings	(4) Retirement age
		Panel B: Fathers and all children		
Schooling	0.018	-0.004	0.005	0.000
	(0.055)	(0.002)*	0.014	(0.078)
N	376,024	1,025,982	1,019,312	811,369
		Panel C: Mother and all children		
Schooling	-0.038	0.015	0.028	0.012
	(0.053)	(0.005)***	(0.027)	(0.087)
N	658,370	1,171,022	1,09,0448	938,630

Notes: Panel A shows instrumental variables estimates of the effect of children's schooling on parents' wealth (in year 2000), average labor market participation between ages 50-60 (average of non-missing values), average earnings between ages 50-60 (average of non-missing values), and retirement age (age at which the parent received any pension income). Specifications include municipality fixed effects, birth cohort fixed effects, and municipality-specific linear trends. Standard errors clustered at the municipality level;* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Appendices for Online Publication

Table A.1: Effect of schooling on parental survival: Results from instrumental variables regressions. Specifications include county-by-year fixed effects.

Independent	(1)	(2)	(3)	(4)	(5)	(6)
variable:	65	70	75	80	85	90
	Panel A: Males, females, and all parents					
Schooling	0.004	0.000	0.008	0.005	0.000	0.001
	(0.004)	(0.005)	(0.006)	(0.007)	(0.007)	(0.006)
N	2,547,966	2,547,966	2,547,721	2,508,277	2,293,541	1,841,102
	Panel B: Males and all parents					
Schooling	-0.001	-0.003	-0.001	-0.003	-0.004	0.004
	(0.005)	(0.005)	(0.007)	(0.007)	(0.007)	(0.006)
N	1,302,318	1,302,318	1,302,202	1,282,198	1,172,391	941,229
	Panel C: Females and all parents					
Schooling	0.010	0.002	0.014	0.015	0.008	-0.006
	(0.008)	(0.009)	(0.011)	(0.011)	(0.011)	(0.010)
N	1,245,648	1,245,648	1,245,519	1,226,079	1,121,150	899,873

Notes: Panel A shows instrumental variables estimates of the effect of children's schooling on parents' survival until ages 65-90. Panel B shows IV estimates of the effect of sons' schooling on parents' survival. Panel C shows IV estimates of the effect of daughters' schooling on parents' survival. Specifications include municipality fixed effects, birth cohort fixed effects, and county-by-year fixed effects. Standard errors clustered at the municipality level;* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A.2: Effect of schooling on parental survival: Results from instrumental variables regressions. Low-educated fathers and female children.

Independent variable:	(1) 65	(2) 70	(3) 75	(4) 80	(5) 85	(6) 90
	Panel C: Low education, fathers and female children					
Schooling	0.013	0.024	0.042	0.037	0.034	-0.012
	(0.011)	(0.014)*	(0.016)**	(0.016)**	(0.015)**	(0.013)
N	287,066	287,066	287,063	284,977	265,027	213,468

Notes: This table shows instrumental variables estimates of the effect of female children's schooling on low-educated fathers' survival until ages 65-90. Specifications include municipality fixed effects, birth cohort fixed effects, and municipality-specific linear trends. Standard errors clustered at the municipality level;* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A.3: Effect of schooling on parental survival: Sample of one-child families.

Independent variable:	(1) 65	(2) 70	(3) 75	(4) 80	(5) 85	(6) 90
			Panel A: Males, females, and all parents			
Schooling	0.002	0.005	0.014	0.011	0.003	-0.008
	(0.008)	(0.009)	(0.012)	(0.013)	(0.016)	(0.014)
N	874,337	874,337	874,096	844,430	742,899	602,914
			Panel B: Males and all parents			
Schooling	-0.003	-0.005	-0.000	-0.006	-0.011	-0.005
	(0.006)	(0.007)	(0.008)	(0.009)	(0.010)	(0.010)
N	758,783	758,783	758,671	741,506	665,782	535,161
			Panel C: Females and all parents			
Schooling	0.001	0.004	0.016	0.026	0.010	-0.018
	(0.008)	(0.010)	(0.011)	(0.013)**	(0.014)	(0.013)
N	743,683	743,683	743,554	726,570	653,255	525,471

Notes: Panel A shows instrumental variables estimates of the effect of children's schooling on parents' survival until ages 65-90. Sample is restricted to one-child families. Panel B shows IV estimates of the effect of sons' schooling on parents' survival. Panel C shows IV estimates of the effect of daughters' schooling on parents' survival. Specifications include municipality fixed effects, birth cohort fixed effects, and municipality-specific linear trends. Standard errors clustered at the municipality level;* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A.4: Effect of schooling on parental outcomes: Results from instrumental variables regressions.

Independent variable:	(1) Wealth	(2) Labor Supply 50-60	(3) Income 50-60	(4) Retirement age
		Panel A: Females, fathers		
Schooling	-0.022	-0.002	-0.008	-0.129
	(0.090)	(0.004)	(0.021)	(0.132)
N	183,599	500,966	497,724	396,226
		Panel B: Females, mothers		
Schooling	-0.040	0.018	0.038	-0.060
	(0.070)	(0.009)**	(0.043)	(0.138)
N	322,515	572,612	533,646	458,745
		Panel C: Males, fathers		
Schooling	0.015	-0.004	0.008	0.051
	(0.056)	(0.002)*	(0.016)	(0.083)
N	192,245	524,991	521,588	415,143
		Panel D: Males, mothers		
Schooling	-0.010	0.011	0.022	-0.008
	(0.041)	(0.006)*	(0.029)	(0.092)
N	335,855	598,410	556,802	479,885

Notes: Panel A shows instrumental variables estimates of the effect of children's schooling on parents' wealth (at year 2000), average labor market participation between ages 50-60 (average of non-missing values), average earnings between ages 50-60 (average of non-missing values), and retirement age (age at which the parent received any pension income). Specifications include municipality fixed effects, birth cohort fixed effects, and municipality-specific linear trends. Standard errors clustered at the municipality level;* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A.5: Effect of schooling on the probability of parents dying because of a specific cause: Results from instrumental variables regressions.

Independent variable:	(1) 65	(2) 70	(3) 75	(4) 80	(5) 85	(6) 90
			Panel A: Cancer			
Schooling	-0.001	-0.003	-0.003	-0.003	-0.003	-0.005
	(0.003)	(0.004)	(0.004)	(0.006)	(0.007)	(0.007)
N	2,547,966	2,547,966	2,547,721	2,508,277	2,293,541	1,841,102
			Panel B: Heart Disease			
Schooling	-0.002	-0.004	-0.009	-0.013	-0.014	-0.011
	(0.003)	(0.004)	(0.005)*	(0.006)**	(0.008)*	(0.010)
N	2,547,966	2,547,966	2,547,721	2,508,277	2,293,541	1,841,102
			Panel C: Respiratory Disease			
Schooling	0.003	0.003	0.002	0.006	0.005	0.008
	(0.001)**	(0.002)*	(0.002)	(0.003)**	(0.004)	(0.005)
N	2,547,966	2,547,966	2,547,721	2,508,277	2,293,541	1,841,102
			Panel D: Mental and behavioural disorders			
Schooling	-0.000	0.000	-0.000	-0.001	0.001	0.001
	(0.001)	(0.001)	(0.001)	(0.001)	(0.002)	(0.003)
N	2,547,966	2,547,966	2,547,721	2,508,277	2,293,541	1,841,102
			Panel E: Accidents and external causes			
Schooling	-0.000	-0.000	-0.001	0.000	-0.002	-0.001
	(0.001)	(0.001)	(0.001)	(0.002)	(0.002)	(0.003)
N	2,547,966	2,547,966	2,547,721	2,508,277	2,293,541	1,841,102

Notes: This table shows instrumental variables estimates of the effect of children's schooling on the probability of parents dying before ages 65-90 for specific causes. Specifications include municipality fixed effects, birth cohort fixed effects, and municipality-specific linear trends. All children and all parents are included in the sample. Standard errors clustered at the municipality level;* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A.6: Effect of schooling on parental survival: Results from instrumental variables regressions. Additional control variables added.

Independent variable:	(1) 65	(2) 70	(3) 75	(4) 80	(5) 85	(6) 90
	Panel A: Males, females, and all parents					
Schooling	-0.003	-0.002	0.006	0.004	-0.002	-0.002
	(0.004)	(0.005)	(0.006)	(0.007)	(0.009)	(0.007)
N	2,547,966	2,547,966	2,547,721	2,508,277	2,293,541	1,841,102

Notes: Panel A shows instrumental variables estimates of the effect of children's schooling on parents' survival until ages 65-90. The specifications include an extented set of control variables including mother's birth year, father's birth year, mother's years of schooling, father's years of schooling, birth order (dummies) and mother's age at birth (dummies). In addition, the specifications include municipality fixed effects, birth cohort fixed effects, and municipality-specific linear trends. Standard errors clustered at the municipality level;* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A.7: Effect of schooling on parental survival: Results from instrumental variables regressions using various cutoffs on parents' birth year. All parents and all children.

Independent variable:	(1) 65	(2) 70	(3) 75	(4) 80	(5) 85	(6) 90
	Panel A: Parents born 1910 onwards					
Schooling	-0.003	-0.005	0.000	0.001	-0.006	-0.009
	(0.005)	(0.005)	(0.006)	(0.006)	(0.008)	(0.008)
N	2,134,530	2,134,530	2,134,285	2,094,841	1,880,105	1,427,666
	Panel B: Parents born 1920 onwards					
Schooling	0.009	0.010	0.013	0.014	0.015	0.002
	(0.007)	(0.007)	(0.008)	(0.009)	(0.011)	(0.014)
N	1,077,344	1,077,344	1,077,099	1,037,655	822,919	370,480
	Panel C: Parents born 1930 onwards					
Schooling	0.017	0.021	0.024	0.076	-	-
	(0.019)	(0.022)	(0.028)	(0.039)*	-	-
N	140,852	140,852	140,607	101,163	-	-

Notes: The table shows instrumental variables estimates of the effect of children's schooling on parents' survival until ages 65-90 using various restriction on parents' birth year.. Panel A: parents born 1910 onwards. Panel B: parents born 1920 onwards. Panel C: parents born 1930 onwards. All specifications include municipality fixed effects, birth cohort fixed effects, and municipality-specific linear trends. Standard errors clustered at the municipality level;* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A.8: Effect of schooling on parental survival: Results from instrumental variables regressions when dropping early and late cohorts.

Independent variable:	(1) 65	(2) 70	(3) 75	(4) 80	(5) 85	(6) 90
	Panel A: Males, females, and all parents, dropping cohorts born 1943-1944					
Schooling	-0.004	-0.004	-0.000	-0.000	-0.008	-0.011
	(0.005)	(0.006)	(0.007)	(0.007)	(0.010)	(0.009)
N	1,950,383	1,950,383	1,950,138	1,910,694	1,696,141	1,269,173
	Panel B: Males, females, and all parents, dropping cohorts born 1954-1955					
Schooling	-0.002	-0.002	0.013	0.011	0.003	0.001
	(0.005)	(0.006)	(0.007)**	(0.007)	(0.008)	(0.008)
N	2,176,344	2,176,344	2,176,343	2,165,822	2,039,245	1,689,767

Notes: Panel A shows instrumental variables estimates of the effect of children's schooling on parents' survival until ages 65-90. Panel A shows IV estimates when dropping children born 1943-1944 and Panel B show IV estimates when dropping those born 1954-1955. Specifications include municipality fixed effects, birth cohort fixed effects, and municipality-specific linear trends. Standard errors clustered at the municipality level;* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A.9: OLS results. Children with less than 10 years of schooling.

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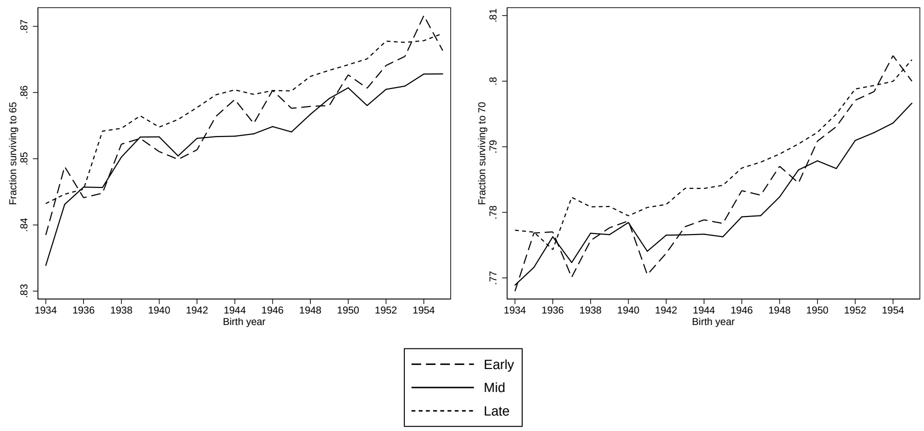
Independent variable:	(1) 65	(2) 70	(3) 75	(4) 80	(5) 85	(6) 90
			Panel A: All children			
Schooling	0.007	0.010	0.014	0.017	0.017	0.012
	(0.001)***	(0.001)***	(0.001)***	(0.001)***	(0.001)***	(0.001)***
N	703,024	703,024	702,947	691,146	632,221	513,532
			Panel B: Male children			
Schooling	0.006	0.008	0.012	0.016	0.017	0.013
	(0.001)***	(0.001)***	(0.001)***	(0.001)***	(0.001)***	(0.001)***
N	397,025	397,025	396,988	390,395	356,570	289,580
			Panel C: Female children			
Schooling	0.009	0.012	0.017	0.020	0.018	0.013
	(0.001)***	(0.001)***	(0.001)***	(0.001)***	(0.001)***	(0.001)***
N	305,999	305,999	305,959	300,751	275,651	223,952

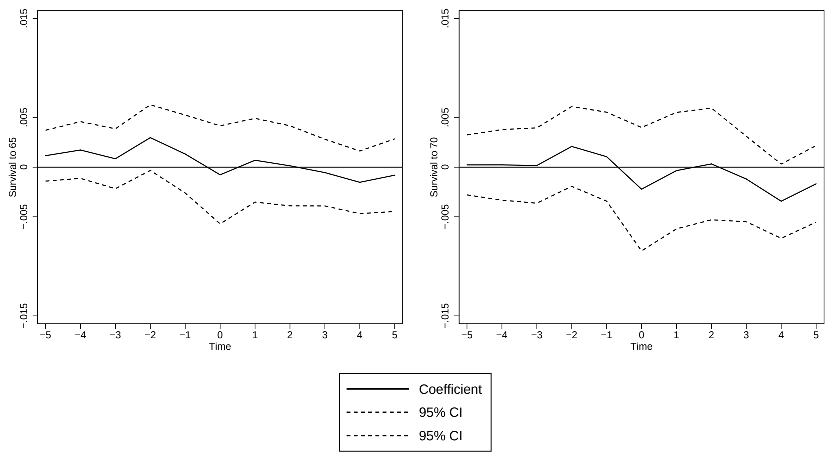
Notes: Panel A shows OLS estimates of the relationship between children's schooling and parents' survival until ages 65-90 for the sample of children having less than 10 years of schooling. Panel B shows OLS estimates on the relationship between sons' schooling and parents' survival. Panel C shows OLS estimates on the relationship between daughters' schooling and parents' survival.al. Specifications include municipality fixed effects and birth cohort fixed effects and controls for parental education and income (in 1970). Robust standard errors in parentheses;* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Figure A.1: Trends in parental survival to ages 65-70 for early, mid, and late adopting municipalities.



Notes: The graph plots mean parental survival by cohorts of children born 1934-1955 and by early, mid, and late adopting municipalities of the schooling reform.

Figure A.2: Event study graph. Effect of reform exposure on parents' probability of surviving to ages 65 and 70. All parents and all children



Notes: The graph plots estimates from a difference-in-differences regression on the effect of the timing of reform exposure on parental survival to ages 65 and 70. The regression includes dummies for cohorts born before and after the first cohort that was exposed to the reform in a municipality. See text for details.