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ABSTRACT

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

Studies on the intergenerational transmission of human capital usually assume a one-way spillover from parents to children. But what if children 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; children's schooling has no significant effect on parents' survival. These results hold when we examine separate causes of death and when we restrict the sample to low-income and low-educated parents.

JEL Classification: I10, 126

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I Introduction

The intergenerational transmission of human capital has received a lot of attention recently in the economics literature and policy circles alike. If the level of human capital in one generation has causal effects on that of another, educational and health policies have outcomes beyond the generation they target. In that case, the spillovers from one generation to the other should be taken into consideration when designing policies (Björklund and Salvanes 2010; Black and Devereux 2011).[1]

Despite the widespread interest in this topic, previous studies have usually assumed a one-direction spillover and looked into the effect of parents' human capital on their offspring.[2] But what if children's human capital affects parents' human capital? While certain types of human capital, such as schooling, is more or less fixed in adulthood, other types, like health, are not. If children's education affects their parents' health, policy makers should account for any spillover effects of education on both former and later generations. In line with this, a number of recent studies provide evidence of a positive relationship between children's education and their parent's longevity (Torssander 2013; 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 is that the timing of the roll-out varied across municipalities, which 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), the effect of parental health on children's outcomes (Black et al. 2014; Persson and Rossin-Slater 2014), 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). 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.

[2] An exception is Kuziemko (2014) that 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.

cohorts. This provides us with plausibly exogenous variation in schooling.[3] We use data from the Swedish Cause of Death Register and proxy parents' health by their age at death.

There are several mechanisms through which children's education could affect parents' longevity. Well-educated children might 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).[4]

In a recent paper by Friedman and Mare (2014), the authors claim that sending children to college extends parents' lifespan. Controlling for financial resources and level of education, the authors compare the longevity of parents whose children have different levels of education and find that parents with college-educated children live longer.[5] 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 par-

[3]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.

[4]It is straightforward to extend the standard Demand-for-Health framework to the case where children can invest time and material resources in the production of their parents' health. Children face several incentives to invest; healthy parents can provide otherwise expensive services, such as child-care and home services, and may be wealthier due to having less health issues. There are also disincentives; well-educated children face a greater opportunity cost of providing time investments, such as informal care.

[5]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 paper was criticized in a New York Times column by Susan Dynarski (August 6, 2014), who argued that the causal claims made by the authors were unwarranted. The causal claims made by Friedman and Mare (2014) can be illustrated by the following quote from their abstract: "We show that adult offspring's educational attainments have independent effects on their parents' mortality, even after controlling for parents' own socioeconomic resources."

ents' 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.

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 healthier parents are better able to invest 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).[6]

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

[6]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 collected between 1971 and 1985, they find that greater intergenerational affect increases survival time among parents who experienced a loss in their social network, particularly among those who were widowed less than five years. 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. 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.

strong for mothers and fathers. Our instrumental variables estimates tell a very different story. We obtain small and insignificant IV estimates both when we pool all children and parents and when we consider separate effects by the gender of the parent and the child. In addition, we find no significant effects when we consider separate causes of death or when we focus on low-income or low-educated parents. 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. Moreover, our zero-results do not reflect the absence of a significant income return to schooling among the children; the returns are significant and positive among sons.

Our findings suggest that the positive cross-sectional relationship between children's education and elderly parents' health that we find most probably reflects the influence of unobserved factors that affect both children and parents. This questions whether the findings in previous studies reflect causal pathways. 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.[7] Many of them were financially vulnerable and it is reasonable to believe that they could potentially have benefited from having better-educated children.

The paper unfolds as follows. Section 2 discusses the compulsory schooling reform, while Section 3 describes the relevant institutional context. Section 4 describes our data. Section 5 outlines our empirical strategy and Section 6 presents our results. Section 7 then concludes.

II The Compulsory Schooling Reform

In 1948, a parliamentary committee proposed a comprehensive primary school-

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

ing reform in Sweden. The key feature of the proposal was to extend the number of mandatory years of schooling from seven to nine years.[8] 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, another feature of the reform was to change the way students were tracked in school. Before the reform, students were tracked in grade 6. The reform delayed tracking until the 9th grade, meaning that students with different capabilities were kept together for a longer period. However, the change in tracking was clearly less dramatic than it sounds, 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 Math and foreign languages. In fact, 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 school route and one junior secondary school route".

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 1955. Except for adding English as a compulsory subject, the

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

reform did not lead to any other changes in the total number of hours taught or to the distribution of hours designated to 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.[9]

III Institutional Context

The extent to which children affect their parents' health when old is likely to depend on the institutional context. In poor countries, with a small or no welfare system, parents rely to a large extent on their children. In Western countries, parents are less reliant on their children for material resources but may benefit from having educated children for the reasons discussed before. In order to help interpret our estimates, we next provide a brief description of the institutional context of our study.

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 no legal obligation to support and take care of their elderly parents, Swedish children provide quite extensive care to their parents.[10] For parents above 75 living

[9]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.

[10]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

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

of residence, after which the needs of the person will be 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.

provided schemes (Thörnqvist and Vardardottir, 2014).[11] For those who have had little or no income from work there is also a guaranteed pension, where the size 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 share receiving only guaranteed pension are much lower, reflecting the stronger labor market attachment of males in these cohorts. Only 10 percent of males aged 65-70 received guaranteed pension but 25 and and 50 percent, respectively, in the age groups 80-85 and 90 and above (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 next section, we illustrate the income for the generation of parents in our data.

IV Data

Our empirical analyses are based on a comprehensive dataset on all Swedish citizens born during the reform period. This dataset was created by merging a number of registers, including information on educational attainment, municipality

[11] 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 residence, basic demographic information, and causes of death. To create our reform sample, we started 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.[12] 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. The algorithm is able to assign reform exposure to 90 percent of individuals born 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. For non-missing cases, the reform algorithm is able to assign starting dates of the reform in different school districts using parish information.[13]

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 and Svensson (2008) shows that 88 percent of all children born in 1949 were in the right grade in 1961, reflecting both that some students repeated a class and that

[12]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.

[13]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. First, 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. Second, 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 among our biggest concerns.

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.[14] For parents, we use information on schooling from the 1960 census. For data on parents' income, we use the income and taxation register (IoT). In order to measure parental longevity, we use data from the Causes of Death Register. The register started in 1961 and covers all deaths among individuals who were permanently residing in Sweden, irrespective of whether the death took place in or outside Sweden.[15] 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 mother and father are observed for an individual. Table 1 provides summary statistics for the children and Table 2 for the parents.

The parent generation in our data includes fathers born between 1866 and 1940 and mothers born between 1889 and 1941. The average birth year for fathers and mothers is 1916 and 1920, respectively. As illustrated in Figure 1, there has been a dramatic increase in life expectancy at birth across these cohorts, from 45 to 67 years. We observe a large fraction of parents dying in our sample; 72 percent of mothers and 86 percent of fathers. Figures 2 and 3 show Kaplan-Meier survival plots for the mortality hazard of parents of high and low-educated children, defined

[14] 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.

[15] Since some parents may have not survived until 1961, we lack information on those cases. In the robustness section, we show that this is not a serious concern for our analyses.

here as having more or less than 9 years of schooling. The patterns suggest that those with lower-educated children have lower survival rates and that this pattern is most pronounced for fathers.

Table 2 shows that the parents in our sample had rather low incomes in the year 2000, where a large fraction had reached retirement age. Among mothers, the average before tax (pension) income was about 8,200 SEK monthly, 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 2846 for males and females, respectively. Figure 4 plots the distribution of income in year 2000 for parents and children.

In our data, we observe on average 3.3 children to each parent. Among mothers, we observe 1,309,304 children of 802,093 mothers. The corresponding numbers for fathers are 1,267,135 children distributed across 776,255 fathers. By including multiple children to each parent, our sample includes parents who have children that are both exposed and unexposed to the reform. As it turns out, 83 percent of the parents have children where all or none are exposed to the reform.

V IV methodology

We base our empirical analyses on the following two equations:

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

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

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 t_m denotes municipality-specific linear trends.[16] Equation (1) is our first-stage

[16] In our empirical analysis, we also check the sensitivity of our results to including county-by-year fixed effects. 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.

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 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.

VI Results

A Is Reform Exposure A Valid Instrument?

We start our empirical analysis 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. Those exposed to the reform have 0.70 additional years of schooling compared to unexposed males. Since reform exposure was not random, this specification might overstate the reform effects, 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 magnitude and in this specification, reform exposure increases years of schooling by 0.26 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 0.32 and the F-statistic to 186. 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. 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 parental schooling to the regression. 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 we use and F-statistics are suggesting that reform exposure is a strong instrument.

To check the validity of our DiD-estimates, a useful check is to try to predict reform exposure by parental schooling, using our preferred specification. This type of placebo-like test is particularly revealing in the context of this paper, since we must rule out that any relationship between children's schooling and parental mortality reflects an association between children's reform exposure and parental schooling. 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 that the schooling reform was not randomly implemented 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.

B Main Results

Next we show our main results. First, we replicate previous findings of a positive relationship between children's schooling and parental survival. Table 5 shows OLS estimates where we control for parental education and income (measured in 1970), in addition to birth year and municipality of residence at school age for children. In Panel A, we include both sons and daughters and both parents (if both are observed). Different columns show linear probability estimates of the relationship between children's schooling and parents' survival to various 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 show estimates separately by sons and daughters. Here, the point estimates are similar in magnitude; positive and significant for all ages considered. These results suggest that children's schooling is positively correlated with parents' longevity, even after controlling for parents' own socio-economic characteristics. These results are similar to those suggested by the recent papers by Friedman and Mare (2014) and Torssander (2013).[17]

[17]Since Friedman and Mare (2014) studied the effect of sending a child to college, we have also

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, without making any distinction about the gender of children or parents. The contrast with the OLS results 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 is only a quarter of the OLS estimate at the same age. These results are robust to including county-by-year fixed effects, as shown in Table 7. These results 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 children's schooling.

Next, we ask if the estimated zero-effects mask heterogeneity in the effect by the gender of the child. As previously discussed, such differences could be expected if, for instance, the returns to schooling differ across genders. The results for males, shown in Panel B of Tables 5 and 6, mirror those in panel A and the effects are again insignificant and small for the age-range 65 to 90. For daughters, most of the point estimates are insignificant but the at ages 75 and 80, there are some signs of a positive effect. In table 5, girls' schooling is found to increase parental survival until ages 75 and 80 by 1.2 and 1.4 percentage points, respectively. These estimates are close to the corresponding OLS estimates. However, these marginally significant effects disappear in Table 6. The overall impression of the estimates here is that there is almost no effect of children's schooling on parental longevity.

We can check for additional heterogeneity by studying if the effect of children's schooling differs by the gender of the parent. In the parent generation, the labor

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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 2.4 percentage points increase in the probability of the parent surviving to age 65. The corresponding number for survival until age 80 is 6.4 percentage points.

market participation rate of women was much lower than that of men. This suggests, that women's pension were on average substantially lower than that of men. 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. We investigate this possibility in Tables 8 and 9, where we consider the effect of the children's education on the survival rates of mothers and fathers separately.

Table 8 shows the effect of children's schooling on fathers' survival. In Panel A, the point estimates are again small and mostly insignificant. The same goes when we run regressions separately by the gender of the child; in none of the specifications we observe any significant effect, as shown in Panels B and C. These results suggest that children's schooling is of little consequence for their father's longevity.

Similarly, Table 9 shows the effect of children's schooling on mothers' survival. Pooling sons and daughters, the estimates in Panel A are small and insignificant at all ages. Panels B and C further suggest that the effects are also insignificant when the effects of male and female children are estimated separately.

In summary, our instrumental variables estimates do not provide any strong evidence that children's schooling positively affects parental life-length. To get a better understanding of these estimates, next we analyze possible mechanisms behind our results.

C Mechanisms

What could explain that children's schooling does not affect parental longevity? One explanation would be that any positive effects of schooling are offset by other effects working in the opposite direction. We next consider the possibility that higher education increases the distance to parents, due to the greater job opportunities associated with education.

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 larger 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 would be affected if children are important informal-care givers and if formal care does not fully substitute for informal care. 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 of the adult children residing in the same municipality as their parents. To do this, 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.

Table 10 shows IV regressions on the effect of schooling on a binary indicator of living in the same municipality as a parent at age 30. We find that increased schooling among females indeed increases the distance to one's parents, as measured through not living in the same municipality. The effect is much smaller and insignificant among males.

Another potential explanation for our zero-findings is that returns to schooling are small. If the income of the child is an important input in production of parental health, low or zero returns to schooling could explain why children's schooling does not matter. There will be an offsetting effect, however, if higher income also increases the opportunity cost of providing care for one's parents. We have estimated the income returns to schooling by gender, where the results suggest that males face a significant return to schooling, amounting to 3.7 percent (results available on request). For females, the point estimate is about 1 percent but insignificant.[18]

[18] 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. Only children surviving to the year 2000 are included

These results suggest monetary returns to education for males, while they live close to their parents, are not a likely source of positive effect on parental longevity. Also, the fact that girls are more likely to live further from their parents, because of getting more education, does not seem to affect parents' age at mortality.

C.1 Heterogeneity

In our main analyses, we focused on mortality without considering different causes of it. The zero-effects we have obtained may hide differences in the effect if children's education affects some causes of mortality but not others. Also, low-educated and low-income parents may gain more from having well-educated children than other types of parents. Below, we investigate these potential sources of heterogeneity in more detail, using our main IV specification and pooling all children together.

Panels A and B of Table 11 show estimates on the effect of schooling on parents belonging to the lower quartile of the income distribution. We measure 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 instead show results for low-educated parents. Note that a higher education was uncommon among the parent cohorts and about 73 percent only had primary school education (6 years of schooling for most). Unsurprisingly, the estimates do not change much when we restrict the sample to parents with the lowest possible schooling; the estimates are again small and insignificant. The results here suggest that not even the most financially vulnerable and low-educated groups of parents seem to benefit from having well-educated children, in terms of longevity.

The effect may also vary across time, since some older cohorts of children faced a period when they were legally obligated to take care of their elderly parents, as discussed in Section II. We therefore also tried restricting our sample to children born 1943-1949, where the *average* age of the parents in 1979, when a law change

in the analyses.

made children no longer responsible for their elderly parents, was 63. The estimates were still small and insignificant, however (results available on request).

C.2 Causes of Death

Another possibility is that 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 effect for lifestyle-related causes of death. Such effects could be hidden when one focuses on all-causes mortality. Next, we study the effects of education on specific causes of death.

In Table 12 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 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.

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 is probably the result of running a large number of regressions. Overall, we obtain no evidence that there are any differences in the effect of children's schooling on causes of parental death.

D Additional Robustness Checks

In a very small fraction of cases, we are unable to link children to one or both parents in our data and therefore drop those individuals in our main analysis. For two percent of the children, we lack information on mothers and for 4 percent we lack information on fathers. The main reason for this is that a small fraction of parents do not survive until 1961 when the causes of death register starts. If education has an effect on the probability of a parent surviving to 1961, our estimates may therefore 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 child schooling on the probability of having missing parental information, using our main IV specification. The results show that education indeed has a positive effect on the probability of observing a parent in data. The estimate is also significant and suggests that one additional year of schooling reduces the probability of having missing information on the mother and father by 1.2 and 1.3 percentage points, respectively.

We can deal with this finding in two ways. First, since the fraction of missing parents should decrease mechanically by the birth cohort of children, we can restrict our analyses to later years and examine to what extent the effect of schooling on the probability of observing a missing parent decreases. Doing so for mothers, we find that for birth cohorts born 1948 and onwards there is no longer any significant effect of education on the probability of observing a missing parent. We obtain the same pattern for missing information on fathers. Re-running our main specifications on the effect of schooling on parental survival, we find similar effects for the cohorts born 1948 and onwards as we found for our main sample that also included cohorts born 1943-1947. As a result, we conclude that the small fraction of cases with missing parental information does not bias our results to any great extent.

Second, we can deal with the missing data by treating the missing parents as being deceased by 1961 and including these cases in our main sample. Note that this means that we treat all missing as being parents who died prior to 1961. If we study survival until age 65 for instance, we can be sure that no parent that would be deceased by 1961 could have been more than 65 at the time of death. For a child born in the earliest cohort in our data, 1943, we can assume that a mother would have been at at most 45 years of age when having her last child. A mother in our sample could then at earliest been born in 1898, which means that she must have deceased at age 63 at the latest (since the mortality register starts in 1961). For later parent cohorts, they must have been even younger at the moment of death. When we include the missing parents in our sample, results do not change (results available on request). This provides us with the second piece of evidence that the small fraction of children with missing parents do not impact on our results in any important way.

E 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 only 7 years in school. This also means that our estimates are identified mainly on variation in schooling at lower end of the schooling distribution, which has consequences for the interpretation and external validity of our results.

First, 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 may have. Earlier studies, not relying on an IV-strategy, use variation across the entire schooling distribution. We should therefore be concerned about the external validity of our IV estimates. Second, it is reasonable to assume that the group of compliers in our study perceived the returns to schooling to be low, or else they would have obtained more schooling. If they face low returns, which is what we find for females, it is perhaps not surprising that we obtain insignificant effects of increased schooling on parental life length.

We can partly address this concern by examining whether the relationship between schooling and parental mortality differs across the schooling distribution in an OLS setting. In Table 13, we show OLS 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.[19] Note that it is variation in this end of the education distribution that mainly identifies our IV estimates. In Table 14, we instead focus on variation in schooling at 10 or more years of schooling. We see that the OLS estimates in most cases are larger when restricting the variation to the lower end of the education distribution. From this, we learn that our IV zero-results do not reflect an absence of a significant OLS relationship between schooling and parental mortality at the lower end of the schooling distribution. On the contrary, the relationship is stronger at that end.

VII 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 chil-

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

dren's schooling. While we can replicate previous findings of a positive relationship between children's schooling and parental longevity, our causal estimates are substantially smaller in magnitude and are statistically insignificant.

We acknowledge that our estimates only reflect variation in schooling at the lower end of the education distribution and that the causal effect might be different at higher levels of schooling, like the one used in Friedman and Mare (2014). We partly address this by showing that the cross-sectional relationship between children's schooling and parental longevity is at least as strong at the lower end as it is at the upper part of the education distribution. Future studies should aim at estimating the causal effect across different parts of the distribution.

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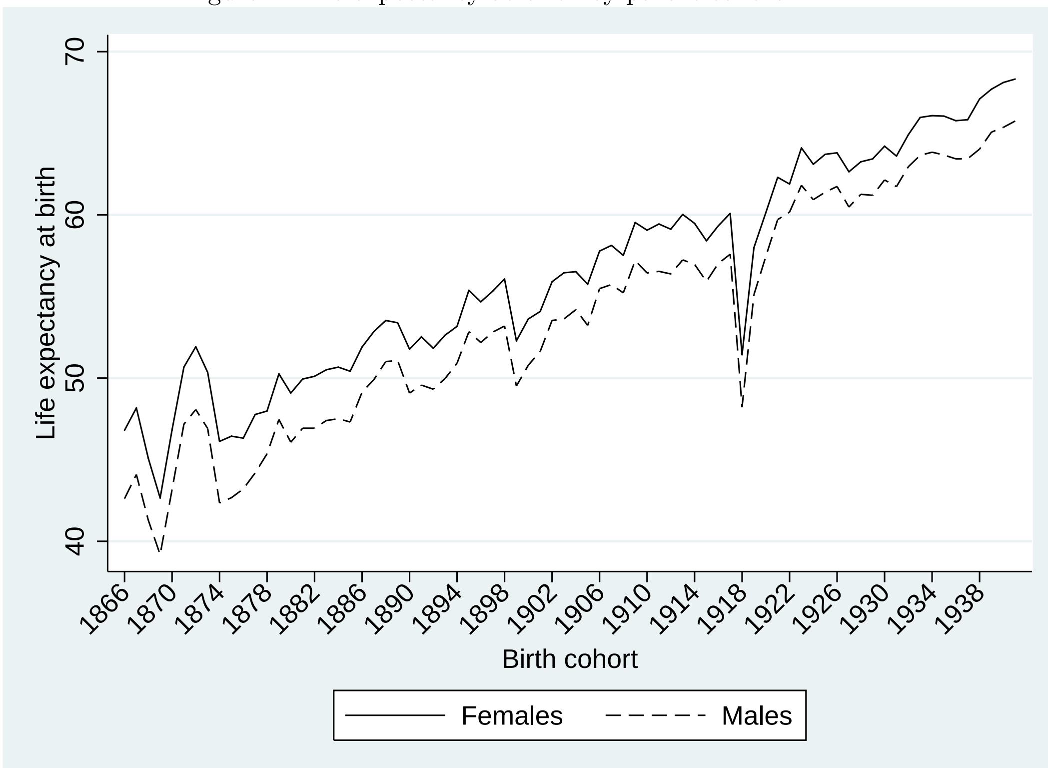
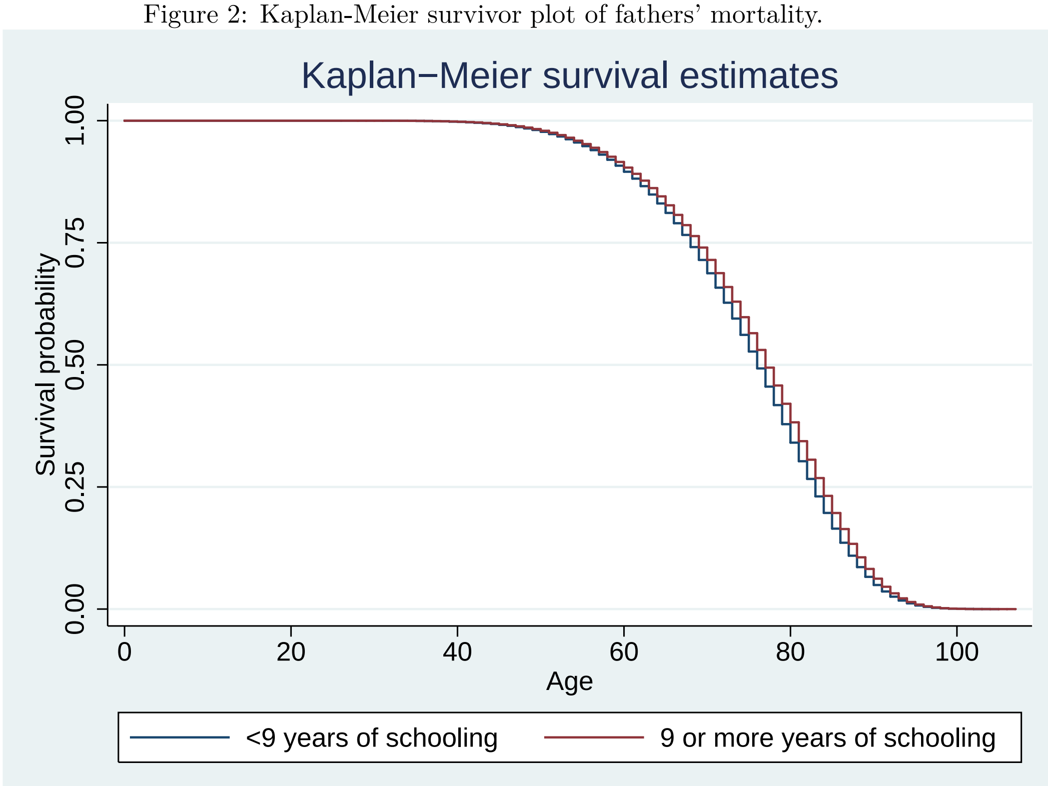
Figure 1: Life expectancy at birth by parent cohort



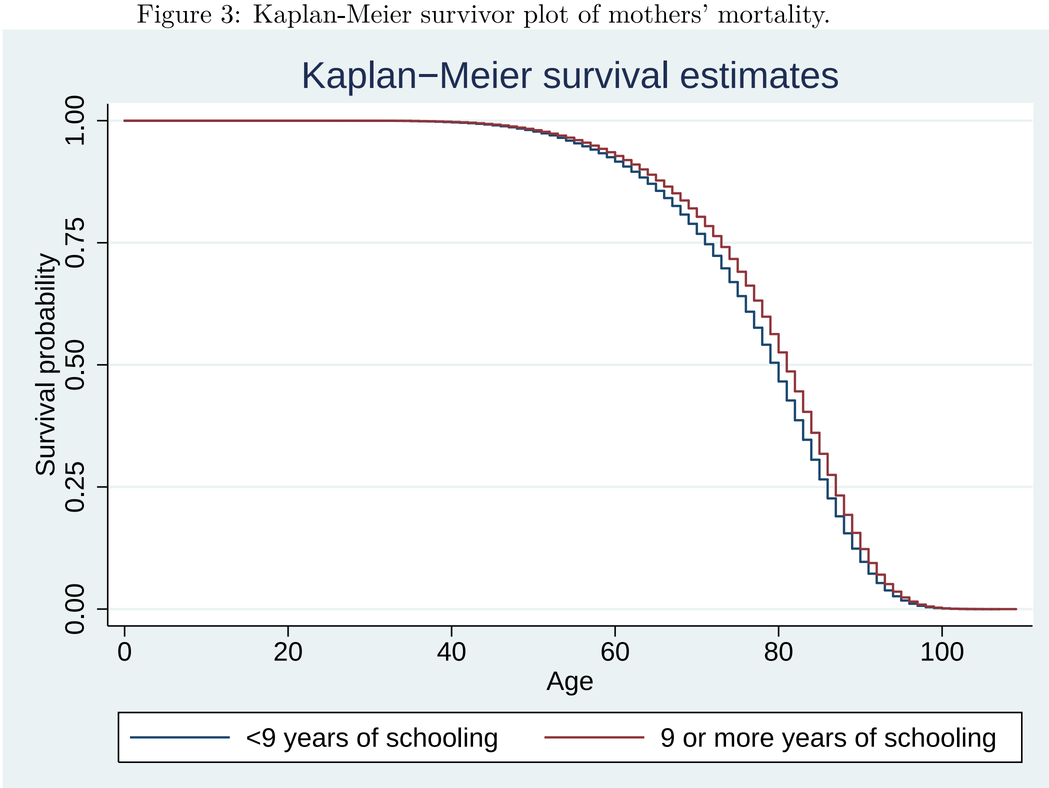
Notes: The graph show the life expecancy at birth for the parent cohorts born 1866-1941. Source: www.mortality.org.

Figure 2: Kaplan-Meier survivor plot of fathers' mortality.



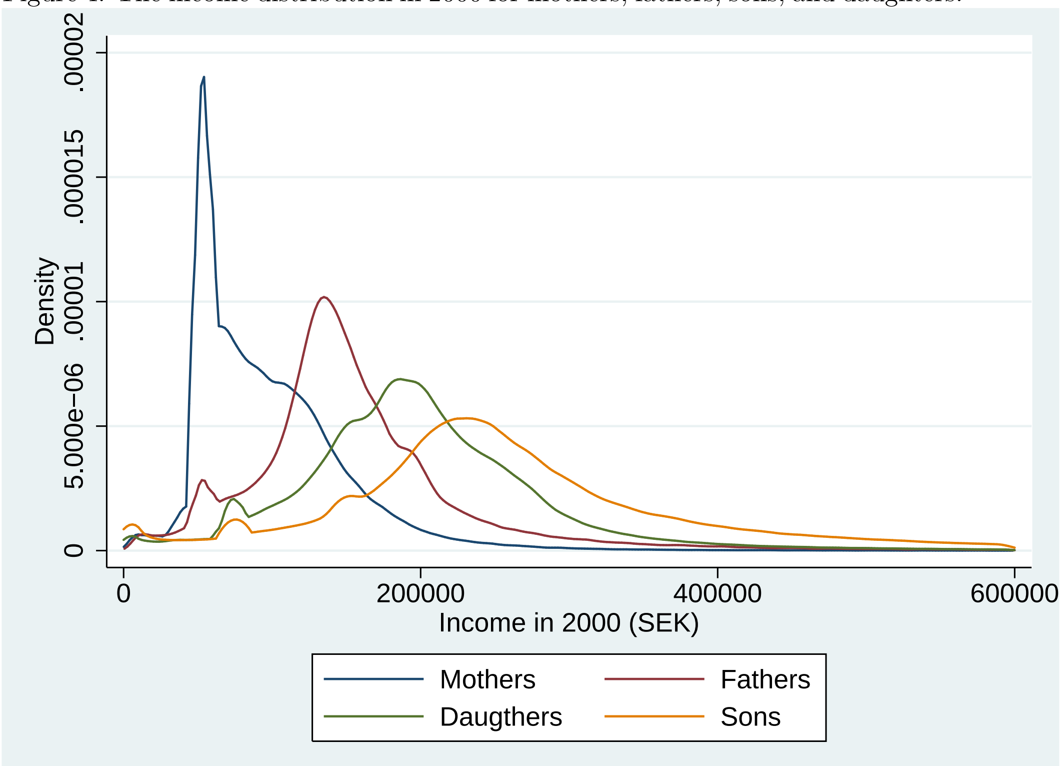
Notes: The graph shows Kaplan-Meier survivor estimates for fathers.

Figure 3: Kaplan-Meier survivor plot of mothers' mortality.



Notes: The graph shows Kaplan-Meier survivor estimates for fathers.

Figure 4: The income distribution in 2000 for mothers, fathers, sons, and daughters.



Notes: The graph shows density plots of the income in year 2000 for fathers, mothers, sons, and daughters. For sake of exposition, income is restricted to the range 0-600,000 SEK.

Table 1: Descriptives

	Mean	Sd	Mean	Sd
	Reform=0		Reform=1	
Years of schooling	11.206	3.024	11.933	2.5
Birth year	1946.9	2.967	1951.6	2.811
Female	0.489	0.5	0.488	0.5
Income year 2000	227131	208823	236555	198291
N	1516043		1060396	

Notes: This table shows decriptive statistics for the samples exposed and unexposed to the reform. Mean, standard deviations, and number of observations.

Table 2: Descriptives

	Mean	Sd	Mean	Sd
	Fathers		Mothers	
Birth year	1916.31	8.81	1919.93	8.09
Survival 65	0.892	0.311	0.892	0.311
Survival 70	0.820	0.384	0.820	0.384
Survival 75	0.72	0.449	0.72	0.449
Survival 80	0.587	0.492	0.587	0.492
Survival 85	0.435	0.496	0.435	0.496
Survival 90	0.301	0.459	0.301	0.459
Years of schooling	8.71	2.36	8.06	1.86
Income in 1968	195512	166218	72049	61142
Income in 2000	158982	103694	97899	55798
N	1,267,135		1,309,304	

Notes: This table shows decriptive statistics for the samples of mothers and fathers. Mean, standard deviations, and number of observations. Income is measured in year 2000 prices. The statistics are based on each mother and father appearing only once, whereas the number of observations at the bottom refers to the number of observations used in the regressions, where mothers and fathers with several children appear several times.

Table 3: First-stage regressions

Independent variable	(1)	(2)	(3)	(4)	(5)
	Panel A: First-stage regression, males				
Reform exposure	0.700	0.262	0.319	0.300	0.251
	(0.054)***	(0.044)***	(0.024)***	(0.000)	(0.043)***
N	1,316,753	1,316,753	13,075	1,316,753	1,316,753
F-stat.	171.03	38.79	186.33	177.67	46.42
	Panel B: First-stage regression, females				
Reform exposure	0.498	0.162	0.212	0.183	0.150
	(0.043)***	(0.033)***	(0.021)***	(0.000)	(0.034)***
N	1,259,686	1,259,686	1,259,686	1,259,686	1,259,686
F-stat	136.98	27.18	114.63	106.76	30.40
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. Columns (2) shows the effect of reform exposure on years of schooling from specifications including 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	Males	Females
	Panel A: limited controls		
Parental schooling	0.131	0.126	0.126
	(0.015)***	(0.014)***	(0.014)***
N	2,576,439	1,316,753	1,259,686
	Panel B: extended controls		
Parental schooling	0.011	0.007	0.004
	(0.023)	(0.016)	(0.016)
N	2,576,439	1,316,753	1,259,686

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

Independent variable	(1) 65	(2) 70	(3) 75	(4) 80	(5) 85	(6) 90
	Panel A: Males, females, and all parents					
Survival	0.004	0.007	0.010	0.012	0.012	0.008
	(0.000)***	(0.000)***	(0.000)***	(0.000)***	(0.000)***	(0.000)***
N	2,576,439	2,576,439	2,576,439	2,576,439	2,576,439	2,576,439
	Panel B: Males and all parents					
Survival	0.004	0.007	0.009	0.012	0.012	0.008
	(0.000)***	(0.000)***	(0.000)***	(0.000)***	(0.000)***	(0.000)***
N	1,316,753	1,316,753	1,316,753	1,316,753	1,316,753	1,316,753
	Panel C: Females and all parents					
Survival	0.005	0.008	0.011	0.013	0.013	0.009
	(0.000)***	(0.000)***	(0.000)***	(0.000)***	(0.000)***	(0.000)***
N	1,259,686	1,259,686	1,259,686	1,259,686	1,259,686	1,259,686

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 and birth cohort fixed effects 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 variable regressions.

Independent variable	(1) 65	(2) 70	(3) 75	(4) 80	(5) 85	(6) 90
	Panel A: Males, females, and all parents					
Survival	0.000	-0.001	0.005	0.004	0.002	0.003
	(0.003)	(0.003)	(0.004)	(0.004)	(0.004)	(0.004)
N	2,576,439	2,576,439	2,576,439	2,576,439	2,576,439	2,576,439
	Panel B: Males and all parents					
Survival	-0.003	-0.003	0.000	-0.002	-0.004	0.001
	(0.003)	(0.004)	(0.005)	(0.005)	(0.005)	(0.005)
N	1,316,753	1,316,753	1,316,753	1,316,753	1,316,753	1,316,753
	Panel C: Females and all parents					
Survival	0.004	0.002	0.012	0.014	0.012	0.005
	(0.005)	(0.006)	(0.007)*	(0.008)*	(0.008)	(0.007)
N	1,259,686	1,259,686	1,259,686	1,259,686	1,259,686	1,259,686

Notes: Panel A shows IV 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 parental survival: Results from instrumental variable regressions. Specifications include county-by-year fixed effects.

Independent variable	(1) 65	(2) 70	(3) 75	(4) 80	(5) 85	(6) 90
	Panel A: Males, females, and all parents					
Survival	0.003	-0.001	0.002	0.002	-0.005	-0.005
	(0.003)	(0.004)	(0.005)	(0.005)	(0.005)	(0.005)
N	2,576,439	2,576,439	2,576,439	2,576,439	2,576,439	2,576,439
	Panel B: Males and all parents					
Survival	-0.000	0.000	-0.002	-0.003	-0.007	-0.007
	(0.004)	(0.004)	(0.005)	(0.006)	(0.006)	(0.006)
N	1,316,753	1,316,753	1,316,753	1,316,753	1,316,753	1,316,753
	Panel C: Females and all parents					
Survival	0.008	-0.002	0.008	0.011	-0.000	-0.007
	(0.006)	(0.007)	(0.009)	(0.009)	(0.010)	(0.010)
N	1,259,686	1,259,686	1,259,686	1,259,686	1,259,686	1,259,686

Notes: Panel A shows IV 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 8: Effect of schooling on fathers' 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 fathers					
Survival	0.002	-0.001	0.010	0.005	0.003	0.001
	(0.004)	(0.005)	(0.005)*	(0.006)	(0.006)	(0.005)
N	1,267,135	1,267,135	1,267,135	1,267,135	1,267,135	1,267,135
	Panel B: Males and fathers					
Survival	0.002	-0.003	0.006	0.000	-0.002	-0.003
	(0.005)	(0.006)	(0.007)	(0.007)	(0.007)	(0.006)
N	647,741	647,741	647,741	647,741	647,741	647,741
	Panel C: Females and fathers					
Survival	0.002	0.001	0.015	0.011	0.011	0.006
	(0.008)	(0.010)	(0.011)	(0.011)	(0.010)	(0.009)
N	619,394	619,394	619,394	619,394	619,394	619,394

Notes: Panel A shows IV 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 9: 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					
Survival	-0.002	-0.000	-0.000	0.003	0.002	0.005
	(0.003)	(0.004)	(0.005)	(0.005)	(0.006)	(0.005)
N	1,309,304	1,309,304	1,309,304	1,309,304	1,309,304	1,309,304
	Panel B: Males and mothers					
Survival	-0.007	-0.002	-0.005	-0.004	-0.005	0.005
	(0.004)*	(0.005)	(0.006)	(0.007)	(0.007)	(0.007)
N	669,012	669,012	669,012	669,012	669,012	669,012
	Panel C: Females and mothers					
	0.006	0.003	0.009	0.016	0.012	0.003
	(0.007)	(0.008)	(0.010)	(0.011)	(0.012)	(0.010)
	640,292	640,292	640,292	640,292	640,292	640,292

Notes: Panel A shows IV 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 10: Instrumental variables estimates of living with parents.

	(1)	(2)	(3)	(4)
	Male children		*Female children*	
Living together at 30:	Mothers	Fathers	Mothers	Fathers
Schooling	-0.009	-0.010	-0.032	-0.027
	(0.010)	(0.009)	(0.013)**	(0.013)**
N	1,211,403	1,056,530	1,157,567	1,009,264

Notes: This table shows IV estimates of the effect of children's schooling on the probability of living in the same municipality as their parents at age 30. 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: Effect of schooling on parental survival: Results from instrumental variable 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			
Survival	-0.009	-0.009	0.001	0.001	-0.002	-0.000
	(0.008)	(0.009)	(0.010)	(0.009)	(0.008)	(0.006)
N	316,769	316,769	316,769	316,769	316,769	316,769
			Panel B: Low income, mothers			
Survival	-0.005	-0.003	0.000	0.000	0.009	0.006
	(0.006)	(0.008)	(0.010)	(0.011)	(0.012)	(0.011)
N	327,307	327,307	327,307	327,307	327,307	327,307
			Panel C: Low education, fathers			
Survival	0.007	0.005	0.018	0.013	0.005	0.002
	(0.005)	(0.006)	(0.007)***	(0.007)*	(0.006)	(0.006)
N	587,311	587,311	587,311	587,311	587,311	587,311
			Panel D: Low education, mothers			
Survival	0.001	0.000	0.003	-0.000	-0.001	0.002
	(0.003)	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)
N	810,948	810,948	810,948	810,948	810,948	810,948

Notes: This table shows IV estimates of the effect of children's schooling on parents' survival until ages 65-90. Results are for the sample of low-educated or low-income parents, see text for details. 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 12: Effect of schooling on the probability of parents dying because of a specific cause: Results from instrumental variable regressions.

Independent variable	(1) 65	(2) 70	(3) 75	(4) 80	(5) 85	(6) 90
			Panel A: Cancer			
Survival	-0.001	-0.002	-0.003	-0.001	-0.001	-0.002
	(0.002)	(0.003)	(0.003)	(0.004)	(0.005)	(0.005)
N	2,576,439	2,576,439	2,576,439	2,576,439	2,576,439	2,576,439
			Panel B: Heart Disease			
Survival	-0.000	-0.002	-0.005	-0.007	-0.010	-0.009
	(0.002)	(0.003)	(0.004)	(0.005)	(0.006)*	(0.007)
N	2,576,439	2,576,439	2,576,439	2,576,439	2,576,439	2,576,439
			Panel C: Respiratory Disease			
Survival	0.002	0.002	0.001	0.003	0.002	0.002
	(0.001)**	(0.001)*	(0.002)	(0.002)	(0.003)	(0.003)
N	2,576,439	2,576,439	2,576,439	2,576,439	2,576,439	2,576,439
			Panel D: Mental and behavioural disorders			
Survival	0.000	0.000	-0.000	-0.000	0.000	0.001
	(0.001)	(0.001)	(0.001)	(0.001)	(0.002)	(0.002)
N	2,576,439	2,576,439	2,576,439	2,576,439	2,576,439	2,576,439
			Panel E: Accidents and external causes			
	0.000	0.000	-0.001	0.000	-0.001	0.000
	(0.001)	(0.001)	(0.001)	(0.001)	(0.002)	(0.002)
N	2,576,439	2,576,439	2,576,439	2,576,439	2,576,439	2,576,439

Notes: This table shows IV 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 13: OLS results. Children with less than 10 years of schooling.

	(1)	(2)	(3)	(4)	(5)	(6)
Survival:	65	70	75	80	85	90
			Panel A: All children			
Schooling	0.005	0.008	0.012	0.016	0.016	0.012
	(0.001)***	(0.001)***	(0.001)***	(0.001)***	(0.001)***	(0.001)***
N	713,012	713,012	713,012	713,012	713,012	713,012
			Panel B: Male children			
Schooling	0.004	0.007	0.011	0.014	0.016	0.012
	(0.001)***	(0.001)***	(0.001)***	(0.001)***	(0.001)***	(0.001)***
N	402,552	402,552	402,552	402,552	402,552	402,552
			Panel C: Female children			
Schooling	0.006	0.010	0.014	0.018	0.018	0.013
	(0.001)***	(0.001)***	(0.001)***	(0.001)***	(0.001)***	(0.001)***
N	310,460	310,460	310,460	310,460	310,460	310,460

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;* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 14: OLS results. Children with 10 or more years of schooling.

	(1)	(2)	(3)	(4)	(5)	(6)
Survival:	65	70	75	80	85	90
			Panel A: All children			
Schooling	0.005	0.008	0.011	0.013	0.012	0.008
	(0.000)***	(0.000)***	(0.000)***	(0.000)***	(0.000)***	(0.000)***
N	1,863,427	1,863,427	1,863,427	1,863,427	1,863,427	1,863,427
			Panel B: Male children			
Schooling	0.005	0.008	0.011	0.013	0.012	0.008
	(0.000)***	(0.000)***	(0.000)***	(0.001)***	(0.001)***	(0.000)***
N	914,201	914,201	914,201	914,201	914,201	914,201
			Panel C: Female children			
Schooling	0.005	0.008	0.011	0.013	0.012	0.008
	(0.000)***	(0.000)***	(0.000)***	(0.000)***	(0.000)***	(0.000)***
N	949,226	949,226	949,226	949,226	949,226	949,226

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 10 or more 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). Standard errors clustered at the municipality level;* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.