# The Effects of Parental Education on Male Mortality: Evidence from the First Wave of Compulsory Schooling Laws*

Hamid Noghanibehambari[a]; Vikesh Amin[b]; Jason Fletcher[c]

**Abstract:** This paper investigates the causal impact of father's education on son's longevity by linking the full count 1940 US census to Social Security Administration death records and using the first wave of compulsory schooling laws from 1875-1912 as instruments for education. OLS estimates suggest small protective effects—conditional on children surviving until age 35, an extra year of father's education increases son's age at death by 0.66 months. IV estimates are substantially larger, with an extra year of father's education increasing his son's age at death by 5.82 months. We also find that an extra year of father's education increases son's education by 0.22 years, conditional on children surviving till 17 years. This suggests that intergenerational transmission of human capital is a channel linking father's education to children's longevity.

**Keywords**: Mortality, Longevity, Education, Intergenerational Effects, Historical Data

**JEL Codes**: I18, I26, H75

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[a] Corresponding Author, Austin Peay State University, Marion St, Clarksville, TN 37040 Email: noghanih@apsu.edu, ORCID ID: https://orcid.org/0000-0001-7868-2900

[b] Department of Economics, Central Michigan University, 1200 S Franklin St, Mt Pleasant, MI 48859, USA. Email: amin1v@cmich.edu

[c] La Follette School of Public Affairs, University of Wisconsin-Madison, 1225 Observatory Drive, Madison, WI 53706-1211, USA. Email: jason.fletcher@wisc.edu

## 1. Introduction

Research consistently documents striking education disparities in mortality. While these education-mortality gradients are robust, having been replicated across birth cohorts and countries, the evidence on the causal relationship is unclear. Several studies have used exogenous variation in schooling arising from changes in compulsory schooling laws (CSLs) to identify the causal relationship. Lleras-Muney (2005) constructed synthetic cohorts using US decennial censuses to compute 10-year mortality rates and used compulsory schooling and child labor laws from 1915-1939 as instruments for education. She found large protective effects—an extra year of education reduced 10-year mortality rates by over 6 percentage points. Fletcher (2015) used the NIH/AARP Diet and Health study and found that an extra year of education reduced the likelihood of death over a 10-year period by 6.9 percentage points, though the estimate was imprecise. However, Mazumder (2008) showed that the results in Lleras-Muney (2005) were not robust to the inclusion of state-specific trends. Outside the US, studies using CSLs have reported null effects for England (Clark & Royer 2013), France (Albouy & Lequien 2009) and Sweden (Meghir et al. 2018) but protective effects have been found in the Netherlands (van Kippersluis et al. 2011) and Romania (Malamud et al. 2021).

Other studies have identified the causal effect of education by comparing mortality and education outcomes within twin pairs. Using male twin pairs from linked complete-count 1920 and 1940 US Census and death records in the NUMIDENT file from the Social Security Administration, Halpern-Manners et al. (2020) found an extra year of education increased age at death by 0.35 years. Lleras-Muney et al. (2022) linked the complete-count 1940 US Census to death records from Family Tree and found a slightly larger effect in twin fixed-effect models—an extra year of education increased age at death by 0.45 years for both men and women. Twin studies for Scandinavia have reached different findings, with null effects for Denmark (Behrman et al. 2011) but protective effects for Sweden (Lundborg et al. 2016).

The evidence on the causal effects of education on mortality has focused exclusively on educational effects within the same generation (i.e., those directly affected). Little is known about whether there is a protective causal *intergenerational* effect of education on the mortality/longevity of the next generation. Studies have documented that higher parental education is associated with lower adult mortality risk (Huebener 2019; Lee & Ruff 2019; Montez & Hayward 2011), and there are several possible pathways linking parental education to adult mortality risk. Parental education can influence adult mortality risk directly through biological imprinting processes. Lower parental education is associated with higher probabilities of being born low birthweight (Chevalier & O'Sullivan 2007; Chou et al. 2010), which is a marker of inadequate nutrition and in turn is associated with higher risks of hypertension (Falkner 2002), stroke (Lawlor et al. 2005) and cardiovascular disease (Liang et al. 2021) in adulthood. More educated parents are more likely to have better access to health and medical care for their children. Parental education could also set in motion adult circumstances that directly affect health. For example, more educated parents are likely to live in more affluent neighborhoods and their children are likely to attend better schools, where peers may be less likely to engage in risky health behaviors (e.g., smoking) which affects their own health behaviors. In addition to attending higher quality schools, their children are more likely to attain high levels of education which is associated with better health and lower mortality.

Though the link between parental education and adult mortality risk is well established, it remains unclear whether the relationship is causal for at least two reasons. First, observed associations are likely confounded by other unobserved factors correlated with parental education and adult health/mortality.  More educated parents are likely to send their children to higher quality schools, but the choice of schools is not random. Parents that care strongly about education may elect to live in "good" neighborhoods with high quality schools and invest more in their children's education and health. More educated parents may have children with higher "innate" ability which is correlated with adult education and health/mortality. Second, a large literature in economics has estimated causal effects of parental education on children's health and education and found mixed results. In an influential study, Currie & Moretti (2003) used the availability of colleges in women's county of residence at age 17 as an instrument for maternal education. They found an additional year of college education for mothers reduced the risk of low birthweight by 1 percentage point. Other studies using variation in education from changes in CSLs have found no causal effects of parental education on child health outcomes from birth to age 16 (Arendt et al. 2021; Carneiro et al. 2013; Lindeboom et al. 2009; McCray & Royer 2011; Silles 2015). Reviewing the literature on causal effects of parental education on children's education, Holmulund et al. (2011) conclude that intergenerational schooling associations are largely driven by selection and that the causal effect is small at best. The inconclusive evidence on causal effects of parental education on children's health and education sheds further doubt on the causal relationship between parental education and adult mortality as these are two important pathways through which parental education affects adult mortality.

To our best knowledge only Noghanibehambari & Fletcher (2022) have attempted to identify the causal effect of paternal education on longevity. They constructed a longitudinal sample of 132,810 sibling fathers and their children based on the US 1940 full count census and Social Security Administration death records and employed cousin fixed-effect models. This approach relates the age at death of cousins to their (sibling) father's education, thereby controlling for a share of the unobserved shared family and genetic factors. Results from cousin fixed-effect models were similar to OLS estimates and showed that conditional on child survival till age 47, the age at death of children whose fathers have a high school (college) education was 2.6 (4.6) months higher than those with a father with elementary/no education. Their estimates though may still be biased by sibling-specific factors that are not differenced out. For example, genetic factors are not fully controlled for as siblings only share 50% of their genetic makeup. Parents may engage in discriminatory treatment of their children that is related to child characteristics, which in turn affects children's education.

This paper contributes to the literature by providing new evidence on the causal effect of parental education on adult longevity. We use the same data sources as Noghanibehambari & Fletcher (2022) but use variation in schooling arising from CSLs for identification. Employing CSLs to identify causal effects of education in the US is a common approach, having been used to estimate causal effects of education on wages, health, and crime (Lleras-Muney 2005; Lochner & Moretti 2004; Oreopoulos & Salvanes 2011; Stephens & Yang 2014). These studies employ the second wave of CSLs (between 1915-1939) that focused on high school attendance. In contrast, we use the first wave of CSLs between 1875-1912 for identification. Massachusetts was the first state to enact a CSL in 1852. The importance of education gained more attention in 1871 when the Republican Party launched a public-school crusade. Gradually states enacted CSLs, and by 1900 nearly all states except for those in the South had enacted CSLs. All states had CSLs by 1920. Initial laws set the entry and dropout ages as 8 and 14 years respectively. Over time, the entry age was mostly lowered while exit ages were increased. Figure 1 shows the geographic distribution of school exit ages over time. As can be seen by 1910 all states had CSLs, and the majority had an exit age between 16-18 years.[1] CSLs were complemented with child labor laws, that allowed employed children to stop attending school before the exit age in a CSL (usually after the child had attained a certain level of education). States often required working children to attend continuation school (part-time or evening school) that supplemented their employment. Enforcement of CSLs varied over time but was bolstered by after the enumeration of the 1900 Census revealed that 25% of children between ages 10-15 were employed. This led to increased public awareness and state-level changes to improve education and limit child labor.

We find small protective effects of father's education based on OLS regressions. Conditional on children surviving until age 35, an extra year of father's schooling increases son's age at death by 0.66 months. Our IV estimates are substantially larger than OLS estimates and show that conditional on children surviving till 35 years an extra year of father's education increasing age at death by 5.82 months.[2] In the presence of heterogenous treatment effects, IV estimates reflect a Local Average Treatment Effect (LATE) ) for individuals ("compliers" who increased their education because of the CSLs. (i.e., those whose schooling would have been lower in the absence of such laws). We note that our LATE estimates reflect the effect of completing primary schooling given that the first wave of CSLs focused on education up to age 14.

The LATE interpretation requires the satisfaction of exclusion restriction assumption- that the law changes are orthogonal to other policy and environmental changes that influence children's

[1] In Appendix Figure A1, we show the geographic distribution of school exit age, the minimum age for school leaving, across states and several years throughout the sample period.

[2] We focus on father's education because linking techniques used are reliable only for men, as women usually changed their names after marriage. We nevertheless present results for mother's education in the appendix and discuss these in section 3.

longevity and are correlated with parental education. These endogenous influences may produce pre-treatment effects and compulsory attendance laws could ride on those trends. We empirically test for this using an event-study estimation and find no evidence for this concern. In analyses to explore mechanisms, we find significant increases in children's educational attainment, suggesting that improvements in human capital is a likely mechanism channel. Therefore, this study also adds to literature on the intergenerational transmission of human capital by providing evidence based on the first wave of compulsory schooling laws in the US.

The rest of the paper is organized as follows. We describe the data sources in section 2 and outline our econometric approach in section 3. The results are presented in section 4. Finally, section 5 concludes the paper.

## 2. Data and Sample Selection

### 2.1 Data Sources

The primary source of data is Social Security Administration's Death Master Files (DMF) extracted from the CenSoc project (Goldstein et al. 2021). The DMF data contains death records of male individuals who died between 1975-2005. There are two advantages in using the DMF data. First, the DMF data can be linked to the full-count 1940 census, which means we can observe individual and parental characteristics in 1940. Specifically, it allows us to extract information on father's education and father's birth-state. Second, the DMF data contains millions of observations. This contrasts with many alternative data sources with much smaller sample sizes, such as Health and Retirement Study and National Longitudinal Mortality Study.

One issue with the DMF-1940-census linked data is that we only observe parental characteristics for those who live in their original households and with their parents. The limited cohort window of observation results in limited parental cohort window and less cross-cohort variation in schooling law exposure. We address this limitation by linking 1940 records to historical censuses 1900-1930 using cross-census linking rules provided by the Census Linking Project (Abramitzky et al. 2020). We start by linking individuals between 1940 and 1900. If there are parental information in 1900, we use them as the original household's characteristics. For the rest of observations, we move to linking 1940 and 1910 and repeat the process.

Although we can deduce parental birthplace and measures of socioeconomic status in 1900-1930, we do not have information on education as the 1940 census is the first census to report education. Therefore, we again employ cross-census linking to link parents back to their 1940 census records. We should note that they may or may not be observed in the same household as their children during the latter linking. This is not an issue as we already know the location of their children in 1940 and the household record number in 1900-1930 censuses for both parents and children. In fact, the sole purpose of the linking parents from 1900-1930 to 1940 is to extract the education information. Since linking techniques are reliable only for male individuals as females usually change their surnames after marriage, parental linking is possible only for fathers. For mothers, we only use the available information in the 1940 census.

The cross-census linking allows us to add more observations to the final sample by having more individuals with parental characteristics. However, there is another advantage of searching individuals in historical censuses. The full-count historical censuses report county-of-residence at the time of census. By observing individuals during their childhood years, we can infer their county-of-birth by proxying county of census-observation. We use the county-of-residence in the first census each individual appears in historical censuses as county of childhood. If the individual is not linkable to historical censuses, we use the county-of-residence in 1940 as the proxy. This information on place-of-birth is important given the growing evidence pointing to the relevance and influence of local area conditions during early-life on lifecycle outcomes and specifically old-age mortality and longevity (Schmitz & Duque 2022; Van Den Berg et al., 2006; Van Den Berg et al., 2009; Lindeboom et al., 2010; Modrek et al., 2022; Noghanibehambari & Engelman 2022).

The complexity of linking data across historical censuses to the 1940 census as well as the 1940 census to the Social Security death records brings concerns that the final selected sample is different from the original population in a way that confound our estimates. For instance, if individuals in states with stricter compulsory schooling laws are more likely to appear in the final sample than other states, the coefficients may overstate the effects by inflating through overrepresentation of the treated population. We can empirically test this concern by investigating the association between being in the final sample from the original 1940 population and parental exposure to state-level educational laws. In so doing, we focus on individuals born between 1900-1940 observed in the full count 1940 census. We implement similar sample selections and merge this data with our final sample of this study. We generate a dummy variable that indicates successful merging. We then regress this indicator on parental birth state educational laws conditional on fixed effects and covariates (similar to identification strategy of section 3). These results are reported in table A6. For both sample of mothers and fathers, we observe quite small and mostly insignificant associations between state-level educational laws and successful merging indicator. These results reduce the concern of endogenous data linking.

Data on CSLs and child labor laws comes from Clay et al. (2021). Their dataset builds on prior work by Lleras-Muney (2002) and Goldin & Katz (2011) by extending the previous coding to 1880. They also follow Stephens & Yang (2014) and use an iterative process to calculate years of required schooling for each state-year birth cohort.[3]

### 2.2. Sample Selection

The original DMF-census data contains approximately 7.8 million records. About 30 percent of observations either have parental information (parents are alive and children are living with their parents), or we can extract their parental information from historical censuses. Hence, the final sample contains 1,913,679 observations. Summary statistics of the final sample are reported in Table 1. We should highlight that the sample covers male individuals only. The average age at death in the final sample is 814.2 months (67.9 years). The sample covers cohorts who died between 1975-2005 and were born between 1885-1940 from fathers-mothers born between 1882-1915. About 7 percent of observations are nonwhite and 1.1 percent are Hispanic. The average father's years of schooling is 7.9 years. Fathers who were born in states with 1-5, 6, 7, 8, and 9-10 years of compulsory attendance law account for about 11, 25, 16, 12, and 1 percent of observations, respectively.[4]

## 3. Econometric Approach

Our main specification for estimating the effect of parental education relates the age at death of child $i$ born in cohort $b$ and in county $c$ whose parent is born in state $s$ in census region $r$ in year $y$ ($D_{ibcsry}$) to parental education ($PE_{sry}$) a vector of individual characteristics ($X_i$) that

[3] We refer readers to the data appendix in Clay et al. (2021) for specific procedure to calculate required years of schooling.

[4] In Table A11, we show the brief summary statistics of selected variables across states with different schooling policies.

includes race and ethnicity, birth cohort fixed-effects for the child ($\gamma_b$), county-of-birth fixed effects for the child ($\xi_c$), parental state-of-birth fixed ($\eta_s$), parental region-of-birth-by-birth-cohort fixed effects ($\gamma_{ry}$), and an error term ($u_{ibcsry}$):

$$(1)\ D_{ibcsry} = \beta_0 + \beta_1 PE_{sry} + X_i'\theta + \gamma_b + \xi_c + \eta_s + \gamma_{ry} + u_{ibcsry}$$

OLS estimates though are likely biased from unobserved factors correlated with parental education and children's longevity. We identify the causal effect by using variation use in CSLs to predict parental educational attainment by estimating the first stage equation:

$$(2)\ PE_{sry} = \alpha_0 + \alpha_1 LawYears_{sry} + X_i'\theta + \gamma_b + \xi_c + \eta_s + \gamma_{ry} + u_{ibcsry}$$

where $LawYears$ is a set of dummy variables for requiring (1) 1-5 years of schooling, (2) 6 years of schooling, (3) 7 years of schooling, (4) 8 years of schooling and (5) 9 years of schooling or more. Dummy variables are used for $LawYears$ because the effect of requiring one more year of schooling may not be linear throughout the distribution of required schooling. A key assumption to identify causal effects of education in this framework is that all other changes which occur across states and regions are uncorrelated with law changes, educational improvement, and the outcome (longevity in our case). We account for this through the inclusion of parental state of birth fixed-effects and region by year of parental birth cohort fixed- effects. These fixed-effects ensure that effects are identified by changes *within* states and region over time and that states are being compared to other states in their census region for the same birth cohorts.[5] We also present evidence from even-study analysis in the next section to support this identification assumption.

## 4. Results

### 4.1 First-Stage Results

First-stage estimates of the effect of compulsory schooling laws on parental education are shown in Table 2. All regressions control for the child's birth-county fixed effects and birth year fixed effects, parental birth-state fixed effects, and parental region-of-birth-by-birth-cohort fixed effects. Column 1 shows that compared to states with no compulsory schooling, those who were born in states with compulsory attendance laws have significantly higher educational attainment. Moreover, there is a monotonic pattern in marginal effects suggesting increases in the effects as the required years of education increases. For instance, those born in states with 1-5 required years of schooling have 0.01 additional years of schooling. Those born in states with 6 years of required schooling have an additional 0.09 years of schooling, and those born in states with 9-10 years of required schooling have 0.18 additional years schooling.

In columns 2 and 3, we explore the effects on two binary outcomes indicating father's education being more than 4 and 7 years, respectively. These thresholds point to finishing elementary and middle school education, though they are still arbitrary cut-off points as they do not account for grade repetitions. The compulsory schooling laws had positive and significant

[5] Stephens & Yang (2014) showed that return to schooling IV estimates for white men based on the 2nd wave of compulsory schooling laws become statistically insignificant and, in some cases, negative when including for region by year of birth effects in regressions to control for differential changes across states such as improvements in school quality. Their results highlight the importance of controlling for region by year birth effects.

effects for both outcomes. Laws requiring 7, 8 and 9-10 years of schooling also had much larger effects on the likelihood of having more than 7 years of schooling. For example, laws requiring 7 (8) years of schooling increased the probability of having more than 7 years of schooling by 1.6 (3.6) percentage points. In comparison, compulsory schooling laws for 7 (8) years only increased the probability of having more than 4 years of schooling by 1 (1.5) percentage points. This is what one would expect from laws that were enforced.

An important concern is that compulsory schooling laws were passed as educational attainment was improving and thus were not causing increased educational attainment. Figure 2 shows results from an event-study analysis to examine this concern. The event-time coefficients from OLS regressions are small, often negative and indistinguishable from 0 before the establishment of compulsory schooling laws. The coefficients start to rise and become statistically significant 6 years after the laws were implemented. A similar pattern is observed in the middle panel of Figure 2, which shows results from the Sun & Abraham (2021) specification for event-study analysis. The bottom panel of this figure shows the results of De Chaisemartin and D'haultfoeuille (2022) and suggests a very similar pattern in coefficients to those reported for OLS estimates. Overall, Figure 2 shows that educational attainment was not increasing prior to the implementation of compulsory schooling laws.

### 4.2 Main Results

Our main results are given in Table 3. The OLS estimate in column 1 shows that conditional on surviving to age 35 years an extra year of father's education is associated with a 0.66 month increase in children's (sons' as the sample covers male individuals only) age at death. This intergenerational OLS association is much smaller than OLS associations between education and longevity within the same generation. For example, OLS estimates in Halpern-Manners et al. (2020) and Lleras-Muney et al. (2022) from the 1940 full count US census indicate one extra year of education is associated with about 0.40 more years (or 4.8 months) of live. In contrast, the IV estimate in column 2 is over eight times larger—an extra year of father's education increases children's longevity by 5.82 months. The discrepancy between the OLS and IV estimates is likely because they represent different treatment effects. OLS estimates represent average treatment effects (ATE), whereas in the presence of heterogenous treatment effects, IV methods identify a LATE. Specifically, the IV estimates reflect the average effect of completing primary schooling for fathers (who increased their education because of the CSLs) on children's longevity. Columns 3-4 and 5-6 show results for white and nonwhite individuals respectively. The protective effect of father's education is predominantly concentrated on nonwhite individuals. Based on IV estimates an extra year of father's education increases longevity of white and nonwhite children by 5.35 and 9.13 months respectively.  The IV estimate for nonwhite individuals though is biased by weak IV, as indicated by the low first stage F-statistic.

As noted in section 2 we focus on father's education because linking techniques are not reliable for women. Nevertheless, results for women are presented in appendix table A1. The conclusions are similar to those found for father's education. IV estimates uncover large proactive effects of mothers. An extra year of mother's education increases children's longevity by 9.92 months conditional on surviving till age 35. This is substantially larger than the OLS estimate of 0.77 months. Similar to father's education, the effect of mother's education is concentrated on nonwhite individuals. An extra year of mother education increases children's longevity by 8.67 months for white individuals and 15.08 for nonwhite individuals.[6]

Our IV estimates for father's education are much larger than the sibling-based estimates reported in Noghanibehambari and Fletcher (2022). They found that conditional on child survival till age 47, an additional year of fathers' education is associated with 0.4 months higher longevity. We also note that our IV intergenerational effects for father's and mother's education are comparable in some instances to other "causal" estimates of education and longevity within the same generation. For example, family fixed-effect estimates in Halpern-Manners et al. (2020) and Lleras-Muney et al. (2022) indicate that an extra year of education is associated with a 4.8 month increase in age at death. Fletcher and Noghanibehambari (2021) explore the effects of college openings on college education and mortality. They estimate treatment-on-treated effect of college education on longevity of about 1 additional year. Therefore, the effect of college education is equivalent to about 2 years of additional father's education.

## 4.2 Robustness Checks

An important concern is that the treated groups are different in observable and unobservable ways with potential differences in health and longevity and that the results may partly pick up on these pre-existing differences. In table A7, we explore the association between race and ethnicity (as predetermined individual characteristics) on parental state-level educational policies. We observe that whites and non-Hispanics are more exposed to stricter educational policies. However, compared to the outcome mean, the observed associations are quite small in magnitude. For instance, exposure to compulsory attendance laws of 8 and 9-10 years is associated with 1.4 and

[6] The first-stage for nonwhite individuals is weak with a F-statistic of 7.54. There is no problem of weak instruments for white individuals or in the full sample. First stage results are given in appendix table A2.

1.6 percent higher likelihood of being white. The coefficient on white in column 2 of table 3 (i.e., the white-nonwhite difference in longevity conditional on covariates) is 3.6 (se=1.2). Therefore, the potential endogenous change in the sample's racial composition can explain most 0.05 months of longevity which is only 0.8% of the effects we find in table 3.

Further, in table A8, we construct a state by year panel (1880-1920) and examine the association between educational laws and other policies and state characteristics. We do not find consistent association between these policies and the share of dry counties in the state, birth registration laws, suffrage laws, and poll tax laws.[7] However, we observe negative association between these policies and measures of socioeconomic index (column 5) and share of literate people (column 7). The states that passed stricter policies are those with higher male labor force participation rate (column 6).

To complement the analysis of this section, we implement two robustness checks. First, we interact birth state fixed effects and birth year fixed effects with dummies for race and ethnicity to allow for flexibility of place and time effects across different racial groups. The results are reported in table A9. We observe set of coefficients that are very similar in magnitude to the main results. Second, we include several time varying state covariates including socioeconomic index, literacy rate, labor force participation rate, share of married people, share of different race groups, and share of different age groups. These results are reported in table A10. For the full sample and the sample of white people, we observe comparable coefficients to the main results. However, the coefficient on nonwhites is substantially larger than those reported in the paper, although very small first stage F-statistics makes this estimate unreliable.

[7] Several studies point to the influence of these policies on long-term health and mortality (Noghanibehambari and Noghani 2023; Noghanibehambari and Fletcher 2023b, 2023a).

### 4.3 Potential Mechanism

In this section we explore the intergenerational transmission of human capital as a mechanism linking father's education to children's longevity. We first take our final estimation sample and restrict it to children at least 17 years old to explore the impact of father's education on children's education. The OLS estimate in Table 4 column 1 shows one additional year of father's education is associated with 0.08 years increase in children's education. The IV estimate shows an increase of 0.22 years in children's education. Appendix Table A3 shows results for mother's education. OLS estimates show that one additional year of mother's education is associated with 0.09 years increase in children's education, whereas the IV estimate is 0.19. In both cases, the sample size though is quite small, and the first stage F-statistic just exceeds the threshold of 10 for weak instruments. Nevertheless, these are the first IV based estimates of the intergenerational transmission of human capital in the US and are similar to existing estimates from other research designs.[8] Using data on twin fathers and their children from the Minnesota Twin Registry, Behrman & Rosenzweig (2002) found an extra year of father's education increased children's education by 0.36 years, and no effect of mother's education.[9] Using data on parents and their adopted children from the Wisconsin Longitudinal Study, Plug (2004) found that additional year of either parents education increased children's education by about 0.27 years.

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[8] The only other IV paper to use compulsory schooling laws to estimate the intergenerational transmission of human capital is Oreopoulos et al. (2006). They examine grade repetition and use compulsory schooling laws between 1915-1969 as instruments. They found an extra year of father's education reduces the probability that a child repeats a grade by 2-4 percentage points.

[9] Antonovics & Goldberger (2005) argue that the results from Behrman & Rosenzweig (2002) are not robust to alternative coding schemes for schooling and alternative sample selection criteria. Behrman & Rosenzweig (2005) contest Antonovics and Goldberger's recoding and show that their results are robust to an independent coding scheme.

Using adopted children and parents from the National Longitudinal Study of Youth, Sacerdote (2000) found an extra year of father's (mother's) education increases children's education by 0.16 (0.22) years. We should note two facts in comparing our results with these studies. First, our IV results use a treatment effect that relies on changes in schooling policies that resulted in take up of primary and secondary schooling. This is in contrast with many of studies that examine compulsory schooling that enforces high school education and completion. Second, considering the relatively weak first stage effects and the focus of laws in an environment with fewer available schools and educational resources, the intergenerational impacts reported in column 2 of Table 4 is relatively large compared with other studies.

In the final sample and for those children with at least 17 years of age, about 97 percent of fathers are born prior to 1900. Therefore, we have much limited variation in exposure to schooling laws specifically for those of the early 20$^{th}$ century. On the other hand, most of children in 1940 census have not completed their education. One way to account for these limitations is to construct a measure that captures the educational level of each child in comparison with other children of the same age. We assume that school age starts at age 7 and each child in each year after that age can attain a maximum of one year of education (and a minimum of zero). Therefore, the ratio of actual attainment reveals a relative measure of education for age. Formally, we define this variable as: years of education/(Age -7). With this outcome an extra year of father's education increases children's expected education by 0.88, which is just larger than the OLS estimate of 0.32.

As another measure columns 5 and 6 use the Duncan socioeconomic index (SEI) of children as the outcome. The Duncan SEI is a measure of occupation status based upon the income level and educational attainment associated with each occupation in 1960. The IV estimate shows an extra year of father's education increases the Duncan SEI by 3 units, which represents an 14% effect relative to the mean SEI of 22.71.

## 5. Conclusion

A growing and relatively large literature evaluates the spillover effects of education across a wide array of areas and outcomes, including crime, civic engagement, political participation, smoking, drinking, and health (Tenn et al. 2010 ; Currie and Moretti 2003; Campbell et al. 2014; Lochner and Moretti 2004; Dee 2004). A narrow strand of this research evaluates the intergenerational impacts of education on mortality and examine the role of parental education on children's mortality outcomes during adulthood and old age (Noghanibehambari and Fletcher 2022; Huebener 2019). This paper adds to this literature by evaluating the impact of the first wave of CSLs on parental education and children's old-age longevity, during the late 19$^{th}$ century, when there were minimal to zero schooling policies in the US. The introduction of compulsory attendance policies was among the first policy-driven attempts to promote education. Since the late 19$^{th}$ and early 20$^{th}$ century US represents the developmental stages of many developing countries today, the results of this paper shed light on the relevance of schooling laws and their externalities for next generations' health outcomes.

We employ death records from Social Security Administration linked with the 1940 full-count census. This linked data provides a unique setting to extract information on parental characteristics as well as individuals' early-life and late-life outcomes. In addition, the unprecedented large sample size makes the data a distinct source to explore the research question. Our IV results using CSLs as the exogenous instrument suggest approximately 5.82 (5.92) months increase in children's longevity for one additional year of fathers' (mothers') schooling. These estimates are substantially larger than the OLS effects which implies about 0.66 (0.76) months rises in longevity for an additional year of father's (mother's) education. Further analyses suggest that increases in children's education and family socioeconomic status are likely mechanism channels. The nontrivial effects of education on nest generations' longevity adds to the benefits and positive externalities of policies that aim at promoting educational outcomes, specifically in an environment with limited established educational policies.

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**Figure 1: Geographic Distribution of School Exit Age**

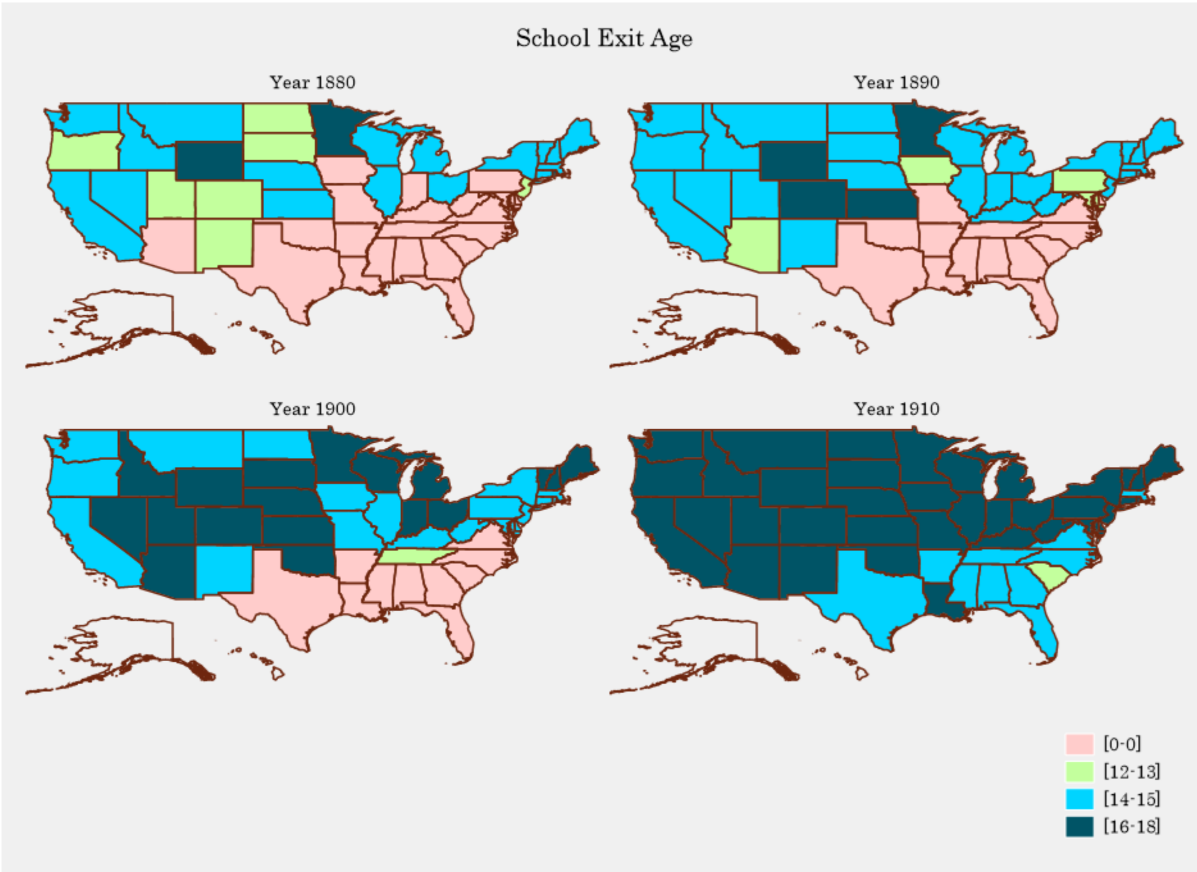

**Table 1 - Summary Statistics**

| | Mean | SD | Min | Max |
|---|---|---|---|---|
| Death Age (Months) | 814.18968 | 118.22665 | 418 | 1231 |
| Birth Year | 1925.7793 | 6.71001 | 1900 | 1940 |
| Death Year | 1993.6306 | 8.42834 | 1975 | 2005 |
| Nonwhite | .0657 | .24776 | 0 | 1 |
| Hispanic | .01105 | .10453 | 0 | 1 |
| Father's Birth Year | 1896.3628 | 7.81627 | 1882 | 1915 |
| Father's Years Schooling | 7.99661 | 3.32686 | 0 | 23 |
| Father's Years Schooling> 4 | .86323 | .3436 | 0 | 1 |
| Father's Years Schooling>7 | .62829 | .48326 | 0 | 1 |
| Father's Birth-State Compulsory Attendance 1-5 Years | .11049 | .3135 | 0 | 1 |
| Father's Birth-State Compulsory Attendance 6 Years | .25113 | .43366 | 0 | 1 |
| Father's Birth-State Compulsory Attendance 7 Years | .15607 | .36292 | 0 | 1 |
| Father's Birth-State Compulsory Attendance 8 Years | .12331 | .3288 | 0 | 1 |
| Father's Birth-State Compulsory Attendance 9-10 Years | .00928 | .09588 | 0 | 1 |
| Mother's Birth Year | 1899.9222 | 7.72657 | 1882 | 1915 |
| Mother's Years Schooling | 8.35566 | 2.96923 | 0 | 20 |
| Mother's Years Schooling> 4 | .9109 | .28489 | 0 | 1 |
| Mother's Years Schooling>7 | .67837 | .4671 | 0 | 1 |
| Mother's Birth-State Compulsory Attendance 1-5 Years | .11423 | .31809 | 0 | 1 |
| Mother's Birth-State Compulsory Attendance 6 Years | .21016 | .40742 | 0 | 1 |
| Mother's Birth-State Compulsory Attendance 7 Years | .16179 | .36826 | 0 | 1 |
| Mother's Birth-State Compulsory Attendance 8 Years | .16897 | .37472 | 0 | 1 |
| Mother's Birth-State Compulsory Attendance 9-10 Years | .0625 | .24205 | 0 | 1 |
| Own Schooling (Conditional on Age>7) | 7.07036 | 3.8783 | 0 | 20 |
| Schooling / (Age-5) (Conditional on Age>7) | 2.15555 | 3.43616 | 0 | 20 |
| Own Socioeconomic Index (Conditional on Age>16) | 22.53713 | 17.27492 | 3 | 96 |
| **Observations** | 1,913,593 | | | |

**Table 2 – Effect of Compulsory Schooling Laws on Father's Education**

| | **(1)** | **(2)** | **(3)** |
|---|---|---|---|
| **Outcome** | Years of Schooling | Years of Schooling>4 | Years of Schooling>7 |
| **Outcome Mean** | 7.997 | 0.863 | 0.628 |
| Law Requires 1-5 Years | .00946 | .00088 | .00163 |
| | (.01815) | (.00204) | (.00274) |
| Law Requires 6 Years | .0963*** | .01291*** | .01591*** |
| | (.01988) | (.00214) | (.00316) |
| Law Requires 7 Years | .08689*** | .01055*** | .01629*** |
| | (.02275) | (.00267) | (.00376) |
| Law Requires 8 Years | .20907*** | .01525*** | .03587*** |
| | (.02838) | (.00303) | (.0047) |
| Law Requires 9-10 Years | .1856*** | .00106 | .02552*** |
| | (.03903) | (.00455) | (.00571) |
| Observations | 1913592 | 1913592 | 1913592 |

Notes. Standard errors, clustered on father's birth-state-birth-year are in parentheses. Regressions include child's birth-county fixed effects, child's birth year fixed effects, parental birth-state fixed effects, and parental region-of-birth-by-birth-cohort fixed effects. Regressions also include race and ethnicity dummies as individual covariates.
*** p<0.01, ** p<0.05, * p<0.1

**Figure 2: Event Study Analysis of Compulsory Schooling Laws**

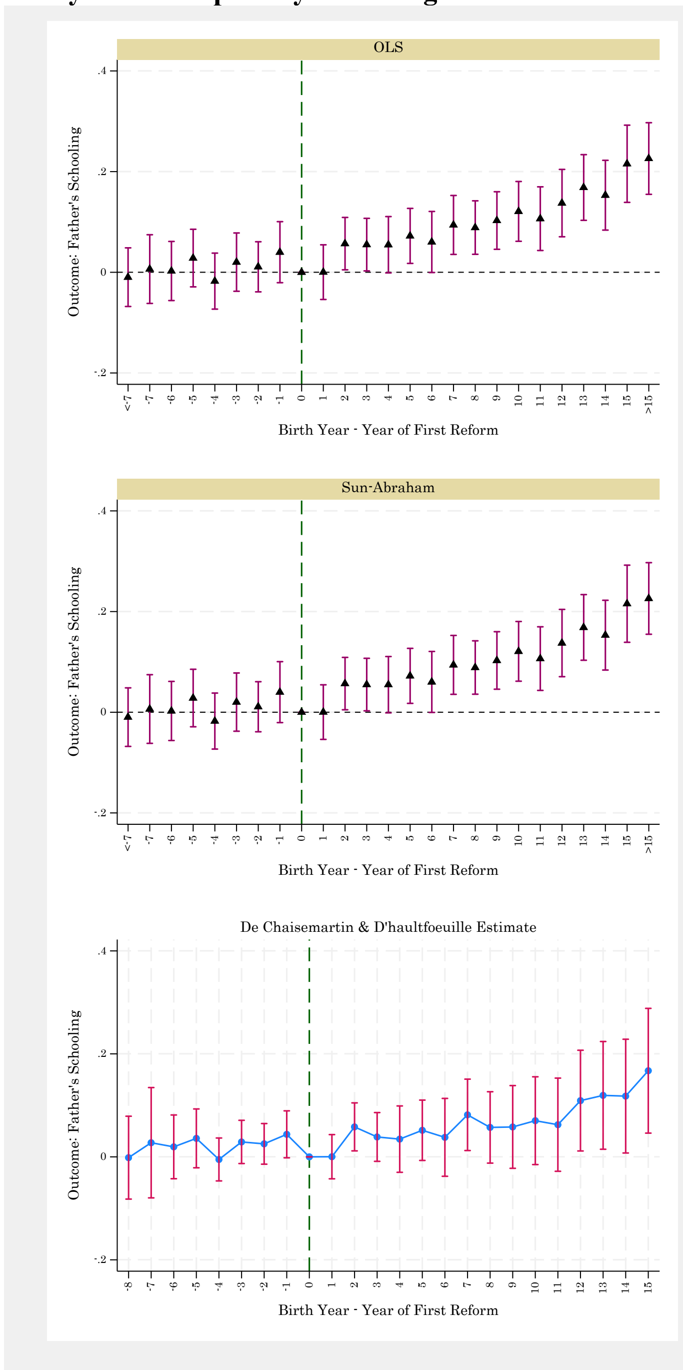

**Table 3 – Effect of Fathers' Education on Sons' Longevity**

| | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
| **Outcome** | Age at Death | Age at Death | Age at Death | Age at Death | Age at Death | Age at Death |
| **Outcome Mean** | 814.190 | 814.190 | 816.006 | 816.006 | 788.306 | 788.364 |
| **Sample** | Full | Full | Whites | Whites | Nonwhites | Nonwhites |
| **Method** | OLS | IV | OLS | IV | OLS | IV |
| Father's Years of Schooling | .65643*** | 5.81982** | .67216*** | 5.34596* | .52353*** | 9.15458* |
| | (.0253) | (2.56412) | (.02577) | (2.7414) | (.10253) | (5.52706) |
| Observations | 1913592 | 1913593 | 1787864 | 1787866 | 125474 | 125727 |
| First-Stage F-Statistic | | 14.725 | | 14.016 | | 6.286 |

Notes. Standard errors, clustered on father's birth-state-birth-year are in parentheses. Regressions include child's birth-county fixed effects, child's birth year fixed effects, parental birth-state fixed effects, and parental region-of-birth-by-birth-cohort fixed effects. Regressions also include race and ethnicity dummies as individual covariates. *** p<0.01, ** p<0.05, * p<0.1

**Table 4 – Effect of Fathers' Education on Sons' Education and Socioeconomic Status (SES)**

| | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
| **Outcome** | Schooling/(Age-5) Conditional on Age > 5 | Schooling/(Age-5) Conditional on Age > 5 | Schooling Conditional on Age > 17 | Schooling Conditional on Age > 17 | SES Score Conditional on Age > 17 | SES Score Conditional on Age > 17 |
| **Outcome Mean** | 2.156 | 2.156 | 9.764 | 9.764 | 22.714 | 22.714 |
| **Method** | OLS | IV | OLS | IV | OLS | IV |
| Father's Years of Schooling | .0797*** | .22213*** | .32517*** | .87755*** | 1.39678*** | 3.27321*** |
| | (.00192) | (.05002) | (.00393) | (.15548) | (.01317) | (1.02979) |
| Observations | 1555173 | 1555173 | 837781 | 837781 | 448731 | 448731 |
| First-Stage F-Statistic | | 12.616 | | 6.171 | | 4.734 |

Notes. Standard errors, clustered on father's birth-state-birth-year are in parentheses. Regressions include child's birth-county fixed effects, child's birth year fixed effects, parental birth-state fixed effects, and parental region-of-birth-by-birth-cohort fixed effects. Regressions also include race and ethnicity dummies as individual covariates. *** p<0.01, ** p<0.05, * p<0.1

# APPENDIX

**Figure A1: Compulsory Schooling Laws Across States and Time**

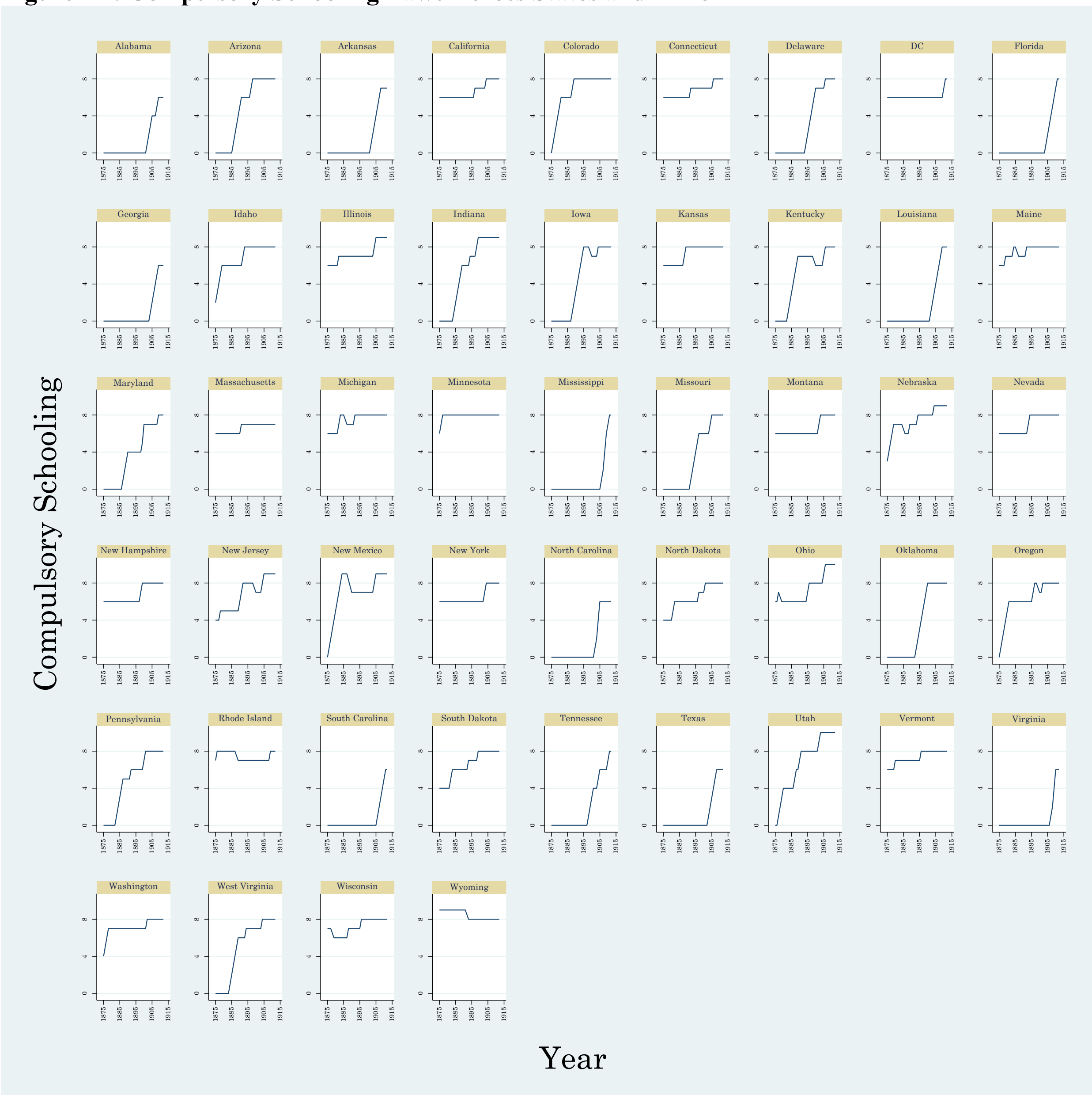

**Table A1 – Effect of Mothers' Education on Sons' Longevity**

| | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
| **Outcome** | Age at Death | Age at Death | Age at Death | Age at Death | Age at Death | Age at Death |
| **Outcome Mean** | 813.831 | 813.831 | 815.692 | 815.692 | 788.193 | 788.252 |
| **Sample** | Full | Full | Whites | Whites | Nonwhites | Nonwhites |
| **Method** | OLS | IV | OLS | IV | OLS | IV |
| Mother's Years of Schooling | .76818*** (.02924) | 9.91875*** (3.20347) | .81349*** (.03077) | 8.67247*** (3.36538) | .29076*** (.10238) | 14.97119*** (5.75783) |
| Observations | 1835832 | 1835834 | 1711306 | 1711309 | 124272 | 124525 |
| First-Stage F-Statistic | | 11.639 | | 11.200 | | 7.447 |

Notes. Standard errors, clustered on mother's birth-state-birth-year are in parentheses. Regressions include child's birth-county fixed effects, child's birth year fixed effects, parental birth-state fixed effects, and parental region-of-birth-by-birth-cohort fixed effects. Regressions also include race and ethnicity dummies as individual covariates. *** p<0.01, ** p<0.05, * p<0.1

**Table A2 – Effect of Compulsory Schooling Laws on Father's Schooling, by Race**

| | (1) | (2) |
|---|---|---|
| **Sample** | Whites | Nonwhites |
| **Outcome** | Years of Schooling | Years of Schooling |
| **Outcome Mean** | 8.212 | 4.929 |
| 1-5 Years | -.0209<br>(.01646) | .19958***<br>(.05776) |
| 6 Years | .04947***<br>(.01863) | .2697***<br>(.07902) |
| 7 Years | .02542<br>(.02087) | .40395***<br>(.09408) |
| 8 Years | .14208***<br>(.02632) | .50399***<br>(.12337) |
| 9-10 Years | .12059***<br>(.03824) | 1.12594***<br>(.28333) |
| Observations | 1787864 | 125474 |

Notes. Standard errors, clustered on father's birth-state-birth-year are in parentheses. Regressions include child's birth-county fixed effects, child's birth year fixed effects, parental birth-state fixed effects, and parental region-of birth-by-

by-birth-cohort fixed effects. Regressions also include race and ethnicity dummies as individual covariates. *** p<0.01, ** p<0.05, * p<0.1

**Table A3 – Effect of Compulsory Schooling Laws on Mother's Education**

| | (1) | (2) | (3) |
|---|---|---|---|
| **Outcome** | Years of Schooling | Years of Schooling>4 | Years of Schooling>7 |
| **Outcome Mean** | 8.383 | 0.913 | 0.680 |
| 1-5 Years | -.00589<br>(.01762) | -.00031<br>(.00166) | -.00008<br>(.00296) |
| 6 Years | .07021***<br>(.01937) | .00639***<br>(.00176) | .00824***<br>(.00317) |
| 7 Years | .08571***<br>(.02189) | .00395**<br>(.00193) | .00683*<br>(.0037) |
| 8 Years | .16375***<br>(.02501) | .00503**<br>(.00225) | .02014***<br>(.00431) |
| 9-10 Years | .14569***<br>(.03165) | -.00342<br>(.00326) | .01108**<br>(.00494) |
| Observations | 1835832 | 1843513 | 1843513 |

Notes. Standard errors, clustered on mother's birth-state-birth-year are in parentheses. Regressions include child's birth-county fixed effects, child's birth year fixed effects, parental birth-state fixed effects, and parental region-of-birth-by-birth-cohort fixed effects. Regressions also include race and ethnicity dummies as individual covariates. *** p<0.01, ** p<0.05, * p<0.1

**Table A4 – Effect of Mothers' Education on Sons' Education and Socioeconomic Status (SES)**

| | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
| **Outcome** | Schooling Conditional on Age > 17 | Schooling Conditional on Age > 17 | Schooling/(Age-5) Conditional on Age > 5 | Schooling/(Age-5) Conditional on Age > 5 | SES Score Conditional on Age > 17 | SES Score Conditional on Age > 17 |
| **Outcome Mean** | 2.140 | 2.140 | 9.983 | 9.983 | 22.573 | 22.573 |
| **Method** | OLS | IV | OLS | IV | OLS | IV |
| Mother's Years of Schooling | .09159*** | .18709*** | .40789*** | 1.15304*** | 1.48679*** | 4.6143** |
| | (.00208) | (.06285) | (.0045) | (.26088) | (.01439) | (1.95) |
| Observations | 1491332 | 1491332 | 699177 | 699177 | 428729 | 428729 |
| First-Stage F-Statistic | | 12.509 | | 2.756 | | 1.737 |

Notes. Standard errors, clustered on mother's birth-state-birth-year are in parentheses. Regressions include child's birth-county fixed effects, child's birth year fixed effects, parental birth-state by fixed effects, and parental region-of-birth-by-birth-cohort fixed effects. Regressions also include race and ethnicity dummies as individual covariates. *** p<0.01, ** p<0.05, * p<0.1

**Table A5 – Parental Education and Age-at-First-Birth of the Child**

| | **(1)** | **(2)** | **(3)** | **(4)** |
|---|---|---|---|---|
| **Outcome** | ***Age at Birth of the First Child*** | | | |
| **Model** | OLS | IV | OLS | IV |
| **Outcome Mean** | 26.989 | 26.989 | | |
| Father's Years of Schooling | .11611*** | -.62399*** | | |
| | (.00218) | (.15254) | | |
| Mother's Years of Schooling | | | .21136*** | -.7407** |
| | | | (.00252) | (.34539) |
| Observations | 1913592 | 1913592 | 1835832 | 1835832 |
| F-Stat | | 16.522 | | 9.530 |

**Table A6 – State-Level Educational Policies and Successful Data Linking from 1940-Census and Death Records**

| | **(1)** | **(2)** |
|---|---|---|
| **Sample** | Father Sample | Mother Sample |
| **Outcome** | Successful Merging between 1940-Census and Death Records | Successful Merging between 1940-Census and Death Records |
| **Outcome Mean** | 0.058 | 0.049 |
| 1-5 Years | -.00027<br>(.0006) | .00052*<br>(.00027) |
| 6 Years | -.0016<br>(.00101) | -.0001<br>(.00036) |
| 7 Years | -.00123<br>(.00098) | -.00025<br>(.00043) |
| 8 Years | -.00247**<br>(.00112) | -.00031<br>(.00044) |
| 9-10 Years | -.00229<br>(.00154) | -.00009<br>(.00055) |
| Observations | 31797923 | 34851073 |

**Table A7 - State-Level Educational Policies and Sample's Racial Composition Change**

| | **(1)** | **(2)** | **(3)** |
|---|---|---|---|
| **Outcome** | White | Black | Hispanic |
| **Outcome Mean** | 0.934 | 0.063 | 0.011 |
| 1-5 Years | .00796*** | -.00636*** | -.00022 |
| | (.00222) | (.00211) | (.0006) |
| 6 Years | .01034*** | -.00872*** | -.00225*** |
| | (.00223) | (.00213) | (.00068) |
| 7 Years | .01509*** | -.01308*** | -.00199*** |
| | (.00285) | (.00277) | (.00077) |
| 8 Years | .01427*** | -.01152*** | -.00376*** |
| | (.00298) | (.00278) | (.00111) |
| 9-10 Years | .01547*** | -.01378*** | -.00117 |
| | (.00373) | (.00348) | (.00294) |
| Observations | 1906959 | 1906959 | 1906959 |

**Table A8 - State-Level Educational Policies and State-Level Characteristics**

| | (1) | (2) | (3) | (4) | (5) | (6) | (7) |
|---|---|---|---|---|---|---|---|
| **Outcome** | Share of Dry Counties | Birth Registration Law Effective in State | Suffrage Law Effective in State | Poll Tax Effective in State | Socioeconomic Index | Male Labor Force Participation Rate | Share of Literate |
| **Outcome Mean** | 0.149 | 0.012 | 0.027 | 0.254 | 22.663 | 0.520 | 0.907 |
| 1-5 Years | .03186 | -.00214 | -.00392 | .00845 | -.15064*** | .00093 | .00061 |
| | (.0353) | (.00497) | (.00468) | (.02631) | (.03909) | (.00058) | (.0015) |
| 6 Years | .05204 | -.0085 | -.01818* | -.02295 | -.20405*** | .00094 | -.00459*** |
| | (.03615) | (.00644) | (.01036) | (.02666) | (.03873) | (.00063) | (.00156) |
| 7 Years | .02856 | -.00959 | -.0393** | -.15632*** | -.13995*** | .0014* | -.01027*** |
| | (.04227) | (.00894) | (.01969) | (.02929) | (.04645) | (.00083) | (.00208) |
| 8 Years | -.03236 | -.01546 | .00932 | -.07975** | -.17551*** | .00177* | -.01326*** |
| | (.04015) | (.01457) | (.0153) | (.03215) | (.05018) | (.00093) | (.00217) |
| 9-10 Years | -.04542 | -.10033* | .04443 | -.08751*** | -.18182** | .01051*** | -.01364*** |
| | (.05223) | (.05145) | (.06099) | (.03239) | (.07936) | (.00176) | (.00254) |
| Observations | 1603 | 1603 | 1637 | 1603 | 1637 | 1634 | 1637 |

**Table A9 – Robustness Check of Father's Schooling on Longevity: Adding Birth-State FE by Race/Ethnicity Dummies and Birth-Year FE by Race/Ethnicity Dummies**

| | **(1)** | **(2)** | **(3)** | **(4)** | **(5)** | **(6)** |
|---|---|---|---|---|---|---|
| **Outcome** | Age at Death | Age at Death | Age at Death | Age at Death | Age at Death | Age at Death |
| **Outcome Mean** | 814.190 | 814.190 | 816.006 | 816.006 | 788.323 | 788.364 |
| **Sample** | Full | Full | Whites | Whites | Nonwhites | Nonwhites |
| **Method** | OLS | IV | OLS | IV | OLS | IV |
| Father's Years of Schooling | .70728*** | 5.27593** | .7191*** | 4.85267* | .61835*** | 7.82871 |
| | (.02533) | (2.55891) | (.0259) | (2.60855) | (.10253) | (5.43456) |
| Observations | 1913585 | 1913593 | 1787858 | 1787866 | 125466 | 125727 |
| First-Stage F-Statistic | | 15.386 | | 15.354 | | 6.476 |

**Table A10 - Robustness Check of Father's Schooling on Longevity: Adding State Controls**

| | **(1)** | **(2)** | **(3)** | **(4)** | **(5)** | **(6)** |
|---|---|---|---|---|---|---|
| **Outcome** | Age at Death | Age at Death | Age at Death | Age at Death | Age at Death | Age at Death |
| **Outcome Mean** | 813.794 | 813.794 | 815.662 | 815.661 | 788.096 | 788.149 |
| **Sample** | Full | Full | Whites | Whites | Nonwhites | Nonwhites |
| **Method** | OLS | IV | OLS | IV | OLS | IV |
| Father's Years of Schooling | .6627*** | 5.76375* | .67909*** | 4.71133 | .51891*** | 14.63638 |
| | (.02611) | (3.46177) | (.02662) | (3.4665) | (.10396) | (13.41769) |
| Observations | 1834624 | 1834626 | 1710128 | 1710131 | 124246 | 124495 |
| First-Stage F-Statistic | | 11.747 | | 11.795 | | 1.709 |

**Table A11 – Summary Statistics of the Sample across States with Different Educational Policies**

| | State-Level Schooling Policies | | | | | | | | | |
|---|---|---|---|---|---|---|---|---|---|---|
| | 1-5 Years | | 6 Years | | 7 Years | | 8 Years | | 9-10 Years | |
| | Mean | SD | Mean | SD | Mean | SD | Mean | SD | Mean | SD |
| Death Age (Months) | 818.13729 | 117.85625 | 828.07543 | 117.12139 | 803.08922 | 116.09978 | 779.24145 | 115.41362 | 744.64254 | 111.95068 |
| Birth Year | 1925.4065 | 6.49454 | 1924.532 | 6.55174 | 1927.1377 | 6.21901 | 1929.6331 | 6.07036 | 1933.0246 | 5.01814 |
| Death Year | 1993.5874 | 8.41884 | 1993.54 | 8.42205 | 1994.0645 | 8.39134 | 1994.5685 | 8.31126 | 1995.0775 | 8.4 |
| White | .95212 | .21352 | .98907 | .10398 | .98962 | .10134 | .99085 | .09521 | .9884 | .10709 |
| Black | .04539 | .20817 | .00765 | .08711 | .00798 | .08897 | .00531 | .07268 | .00743 | .0859 |
| Hispanic | .01019 | .10042 | .00629 | .07906 | .01149 | .10658 | .00801 | .08916 | .06071 | .23881 |
| Father's Birth Year | 1896.5735 | 7.07681 | 1894.7078 | 7.08346 | 1899.0497 | 6.45531 | 1902.891 | 7.14554 | 1908.6531 | 6.32789 |
| Mother's Birth Year | 1899.7483 | 7.20175 | 1898.1661 | 7.31654 | 1902.0164 | 6.75373 | 1905.2908 | 6.94501 | 1909.9956 | 5.55013 |
| Father's Years Schooling | 7.95423 | 3.20168 | 8.69113 | 3.02304 | 8.601 | 3.00991 | 9.05992 | 2.82973 | 9.4207 | 2.92288 |
| Mother's Years Schooling | 8.27713 | 2.85783 | 8.8203 | 2.67466 | 8.85383 | 2.69292 | 9.38569 | 2.5753 | 9.63497 | 2.73657 |
| Observation | 211435 | | 480559 | | 298652 | | 235970 | | 17756 | |