The association between educational attainment and longevity using individual-level data from the 1940 census

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Abstract: We combine individual data from the 1940 full-count census with death records and other information available on the Family Tree at familysearch.org to create the largest individual dataset to date (17 million) to study the association between years of schooling and age at death. Conditional on surviving to age 35, one additional year of education is associated with roughly 0.4 more years of life for both men and women for cohorts born 1906-1915 and smaller for earlier cohorts. Focusing on the 1906-1915 cohort we find that this association is identical when we use sibling or twin fixed effects. This association varies substantially by place of birth. For men, the association is stronger in places with greater incomes, higher quality of school, and larger investments in public health. Women also exhibit great heterogeneity in the association, but our measures of the childhood environment do not explain it.

Keywords: Education; Longevity; Heterogeneity; Sex disparities; Individual data

JEL: I10; I20; J11

1. Introduction

Educational attainment is a strong predictor of longevity. Prior studies across different disciplines and in various countries show that those with more years of schooling have a lower mortality risk compared to those with less education (Baker et al., 2011; Buckles et al., 2016; Hummer and Hernandez, 2013; Kitagawa and Hauser, 1973; Lleras-Muney, 2005). In the US, these educational disparities in mortality have increased dramatically since the mid-1980s (Case and Deaton, 2015, 2017, 2021; Hayward et al., 2015; Meara et al., 2008; Montez et al., 2011; Olshansky et al., 2012; Sasson, 2016). Understanding the nature of this relationship and the reasons why it is so strong and persistent is a key issue for both researchers and policymakers.

In this paper, we investigate the relationship between education and longevity, which we refer to as the education gradient, using a novel and very large individual-level dataset for the US. We combine individual data from the 1940 full census with death records and information from family trees in FamilySearch to create a sample of more than 17 million white individuals born in the US between 1876 and 1915. FamilySearch has a wiki-style platform for recording genealogical information that includes profiles for 1.2 billion people. These profiles are linked together through family relationships which allows us to compare siblings or twins. We then focus on understanding whether education gradients vary as a function of the childhood environment and why. We do so by estimating education gradients by place of birth (and cohort if possible) and relating them to place characteristics for the 1906-1915 cohort.

We find large associations between education and longevity: conditional on surviving to age 35, one more year of schooling is associated with 0.4 additional years of life for both men and women born 1906-1915. The results are identical when we include sibling or twin fixed effects, alleviating concerns that our estimates are biased as a result of any omitted variables that are fixed within a family. While family fixed effects do not solve the issue of causality, these results suggest that if there are important omitted variables, they are individual ones, not contextual (state or local level) or family-level determinants of health (such as shared genes).[1] Of course, this does not imply that family level covariates do not matter for education or longevity—it

[1] There are many issues in interpreting the results of family and twin fixed effect models. For a review see Bound and Solon (1999) and the discussion in Galama et al. (2018).

simply suggests that the family component that is correlated with longevity is not the same as the family component that is correlated with education (Black et al., 2022).

We then document for the first time that there is substantial variation in the association between education and longevity based on place of birth (either state or city of birth). Children in the US grow up in vastly different environments. These environments have substantial effects on the education, earnings, health, and demographic outcomes of individuals (Almond et al., 2018; Chetty et al., 2014a; Chetty et al., 2014b; Dudovitz et al., 2018; Dudovitz et al., 2016). Previous research has shown that, in addition to affecting lifetime outcomes, these early environments also moderate the returns to education in the labor market.[2] We document that these conditions also moderate the returns to education on health. For men, the education gradient in longevity is *larger* if they were born in places with greater education, greater longevity, greater incomes, and better health resources. It is also larger for men who went to school in places where the quality of school was higher. Thus, for men the effect of education is larger in places where other resources that complement the production of human capital are larger during childhood. But there are no such relationships for women in our data—education is also a strong predictor of longevity for women, but we found no place-of-birth covariates that robustly predict the size of the gradient among women. We discuss these differences by sex and other implications of our findings.

There is a very large literature investigating the relationship between education and mortality that started with the seminal study of Kitagawa and Hauser (1973), who first documented in the US that mortality rates are lower for more educated groups. Since then, various studies have investigated many aspects of this relationship, including the functional form of the relationship, how it varies with age, and what mechanisms may explain it. Economic interest in this relationship began with the theoretical and empirical work of Grossman (1972), who first theorized reasons why the more educated might experience higher levels of health. In the last 20 years, economists have mostly focused on determining whether this relationship is causal by exploiting natural experiments, investigating either changes in compulsory schooling, or using twin designs. A series of recent reviews and meta-analysis in both economics (Galama et al.,

[2] For example, Card and Krueger (1992a) showed that the returns to school in the labor market are larger in places where the quality of school is greater.

2018; Grossman, 2015) and demography (Byhoff et al., 2017; Hamad et al., 2018; Xue et al., 2021) summarize the results of this body of work and conclude that it appears that education does not cause greater health and reduced mortality in all contexts and for all groups, but it is unclear why this heterogeneity exists.

This paper makes two contributions to this literature. First, we make use of the fact that our dataset is very large to provide systematic empirical evidence that the childhood environment modifies the education-longevity relationship. There are only a few studies that attempt to systematically understand the reason why education gradients vary across studies. Using a meta-analysis that collects estimates from many studies, Xue et al. (2021) conclude that methodological differences account for all the variation across studies and that education does not have a causal effect on mortality. But other studies (Barcellos et al., 2018, 2019; Gathmann et al., 2015; Kamhöfer et al., 2019) that estimate the causal effects of education, explicitly allowing for heterogeneity *within* the same study, find evidence that returns are indeed heterogeneous. These returns vary as a function of individual genetic endowments and baseline health (Barcellos et al., 2018) and are larger for those that had greater readiness or potential gains (Kamhöfer et al., 2019).

We investigate how environmental factors, rather than individual factors, modify the gradient. Montez et al. (2019a,b) document that education gradients vary by state of residence, but it is unclear why. Environmental conditions are of interest because they can be manipulated through public policy, which is often determined at the state and local level. We hypothesize that environmental conditions during childhood (measured by conditions in the place of birth) affect the health returns to years of schooling. As Galama et al. (2018) argue, education gradients in health are likely larger in places with more complementary education inputs because it is ultimately skill rather than time spent in school that likely matters for health. The intuition for this result is simple: time spent in school translates into greater skills when other resources, like good teachers, are available. Skills are also better developed when other conditions are also met. Children who are in good health are also more likely to absorb material, as are children who have access to other complementary inputs such as books and uniforms (Baumann and Krskova, 2016; Glewwe and Miguel, 2007). Our findings support this hypothesis for men.

In this area, our study is closest to Frisvold and Golberstein (2011) and Aaronson et al. (2021) who investigate the health returns to school quality among Black men. We extend their work by studying white men and women, controlling for family fixed effects and investigating other possible reasons why education gradients vary by state of birth.[3] We demonstrate that the findings for Black men extend to white men but not to white women, that they are robust to controlling for family fixed effects, and also show that school quality is not the only moderator of the education health gradient among men. Our work is also related to the literature that investigates how geography affects health outcomes (Deryugina and Molitor, 2020; Finkelstein et al., 2021), but we focus on how environments affect returns to human capital, not their direct impact on health. Our study is similar to Chetty et al. (2016) who document that in the US today there are large income gradients in adult mortality that vary widely across depending on where individuals live.

Our second contribution is to document that genealogical data can be leveraged to successfully advance research on the education-health relationship. In this area, our work is most closely related to that work by Halpern-Manners et al. (2020), who also use US census data matched to death certificates to estimate the effects of education on longevity and investigate the robustness of their findings to the inclusion of family fixed effects among siblings or twins. In the US, most studies on the education-mortality gradient rely on survey data or vital statistics where family identifiers are not available. Like Halpern-Manners et al. (2020), we find that our results are very similar whether or not we include sibling or twin fixed effects. This is similar to the results in Lundborg et al. (2016) for Sweden and stands in contrast to results of other studies with smaller sample sizes.[4]

Our work complements that of Halpern-Manners et al. (2020) in several ways. We show that the Family Tree data can confirm many of the stylized facts in the literature using period mortality,

[3] Unlike Aaronson et al. (2021), we do not find any evidence of a migration penalty: education-longevity gradients are almost identical for movers and stayers (the coefficient of education is 0.3986 for stayers and 0.4044 for movers). The migration penalty in Aaronson et al. (2021) appears to be a race-specific phenomenon.
[4] There are several papers that use twins to investigate the effect of education on mortality or other health outcomes outside of the US. See Galama et al. (2018) and Halpern-Manners et al. (2020) for a more detailed overview.

establishing that our data is of high quality. First, life expectancy derived from our data closely resembles the life expectancy in social security life tables. Second, we reproduce education gradients estimated in previous published papers, specifically in the seminal Kitagawa and Hauser (1973) study and the influential follow-up paper by Elo and Preston (1996). Third, we show that the education-longevity gradient exhibits well-known patterns estimated for mortality: it exhibits credentialing effects for men, it diminishes with age (Crimmins, 2005; Elo and Preston, 1996; Kaestner et al., 2020; Kitagawa and Hauser, 1973; Lynch, 2003), and it has become larger for more recently born cohorts (Kaestner et al., 2020; Meara et al., 2008; Montez et al., 2011; Montez et al., 2019b; Olshansky et al., 2012).

Our data does not suffer from truncation because death information is collected from many sources. We document that the education-longevity gradient is grossly underestimated when the data on deaths are truncated. Halpern-Manners et al. only observe deaths for men between 1973 and 2013. If we restrict our deaths to 1973-2013 the estimated coefficient on education for men falls from 0.38 to 0.28. Other studies use the public Social Security Numident files which only include deaths occurring between 1988 and 2005.[5] In our sample, the coefficient on education falls from 0.40 to only 0.33 if we exclude deaths in old age (after 2005, roughly age after age 95) and to only 0.10 when we exclude deaths in prime age years (between 1940 and 1988, roughly between ages 30 and 78). Thus, education plays a particularly important role on preventing early deaths, and truncated death records miss this important impact.

Finally, and perhaps most importantly, our data include women, which is another major strength of the Family Tree data, and is large enough to investigate spatial heterogeneity.[6] Halpern-Manners et al. (2020) cannot track women using their matching methods because women often change their names when they marry. We show that the education gradient of men and women follow different patterns, particularly when we break these based on place of birth. Halpern-

[5] Halpern-Manners et al. (2020) link individuals to the Death Master File which include deaths only from 1973 to 2013. Other studies using the same source of death data include Aizer et al. (2016), Rees et al. (2019), and Fletcher and Noghanibehambari (2021).
[6] Savelyev et al. (2022) combine data from the Minnesota Twin Registry to Mortality data, which include 558 pairs of twins for both males and females. The authors find a statistically significant effect of education on reductions in mortality for males. However, the effect is not precisely estimated for females, due to a lack of statistical power. Our Family Tree data include over 45,000 pairs of twins.

Manners et al. (2020) document that the education longevity gradient is larger for individuals who come from more advantageous backgrounds measured by parental occupation-based income. Our results for men are consistent with these findings, although we focus on broader childhood environments. Our results for women are different as we discuss extensively.

2. **Data and descriptive statistics**

2.1 Data

Primary micro-level data

The primary microdata comes from the 1940 full-count US census—the recently released full-count census and the first national census to collect information on years of schooling (in single years, ranging from 0 to 17) for the entire population. The census also collected name, place of birth, and year of birth for all respondents.

We study native white individuals who were 25 to 64 years old in 1940 (born 1876 to 1915) and whose education was reported in the 1940 census. We restrict attention to those over the age of 25 since at that age, education has almost always been completed. We exclude those born in Alaska and Hawaii (which were not states yet), the District of Columbia, or outside the US—we only have data on childhood conditions for individuals born in the 48 states. Because Blacks and individuals of other races are poorly represented in current family trees, we concentrate on individuals self-reporting as white in 1940.[7] There are 47.4 million individuals in the 1940 census that satisfy these criteria (**Figure A1**).

We then identified which of the people in this 1940 census sample were attached to a profile on the Family Tree at familysearch.org. The Family Tree is a genealogy platform with over 12.6 million registered users and profiles for over 1.2 billion deceased individuals. When people search their own family histories, they gather and attach sources and upload information to the profiles of their ancestors or other people they are researching. These sources include census

[7] Although about 10 percent of the population in the 1940 census is black, in the matched data they only account for 0.6 percent of the observations. Fully understanding the under-representation of black individuals in our data and its implications for the education gradient is beyond the scope of this paper.

records and death records from various sources (death certificates, obituaries, cemeteries, funeral home records, social security records) and the information that people upload regarding date of birth, date of death, and links between parents, spouses, and children. This database does not contain everyone in the US population at a point in time, potentially generating sample selection.[8] Of the 47 million people in our 1940 census who were whites and born in continental US between 1876 and 1915, we find that 26 million (55.3%) are attached to a profile on the Family Tree. We discuss the issue of sample selection below.

We drop individuals missing information on birth year or death year for whom the age at death—our outcome of interest—cannot be computed.[9] We also exclude individuals with age at death greater than 120. We also dropped about 2% of the individuals in the 1940 with missing values for education. **Figure A1** details of how we move from the original 1940 census data to the final data, which includes 17.6 million individuals born 1876-1915, approximately 13% of the original census population.

Our main explanatory variable of interest is years of schooling. Years of education are reported to the census enumerator while the person is still alive.[10] Because education is reported in years, we also overcome a limitation of many studies which can only observe education in categories. Previous research documents that the education of older cohorts is overstated in the 1940 census (Goldin, 1998), an issue we address in our analysis.

For our main analysis, we focus only on the youngest cohort born 1906 to 1915 for whom the data on education is of high quality and for whom we have data on the childhood conditions in their state of birth. We match individuals to conditions in childhood in their state of birth as reported in the 1940 census.[11] In order to make each of the birth cohorts comparable, we restrict

[8] Individuals interested in genealogy are more likely represented. Because the trees rely on vital registration records individuals living in times and places with complete registration systems are more likely represented. To our knowledge an assessment of the representativeness of the full set of family trees has never been completed.
[9] Month and day of birth and death are missing for a substantial number of cases, so we rely on year of birth and year of death only.
[10] By contrast, educational attainment available on death certificates is reported by funeral directors and other individuals instead of the deceased. As a result, the educational attainment reported on death records are rounded (heaped) at 12 years of schooling from both lower and higher-level education.
[11] We confirm state of birth with the information on the Family Tree profile—the two of which nearly always agree for people born in the United States.

attention to those who are alive at age 35 (thus allowing earliest-born cohorts to be alive in 1940).[12] This sample has about 5.5 million observations.

We also investigate how city-level conditions moderate education gradients. To do this we construct a subsample for whom we observe the city of birth (see Appendix section 1 for more details). About 2.5 million individuals in our sample of 5.5 million have detailed birthplace information in the family tree. Of these, about 1.3 million list a city as their place of birth. Many of these cities are rather small. We keep 276,142 individuals who are born in 351 large cities for which we are able to obtain quality of schooling measures. Alternatively, we keep 249,715 living in 215 large cities with information on city-level sanitation and health expenditures. In both cases we dropped individuals living in cities for which fewer than 100 individuals so we could obtain reasonable precise education gradients at the city level. These samples are much smaller than the original sample and likely suffer from more sample selection—we investigate this later in the paper.

State-level data for the 1906-1915 cohort

Quality of schooling by state. Based on issues of the Biennial Survey of Education, which contains the results of surveys conducted by the US Office of Education from 1918 to 1966, Card and Krueger (1992a) compiled a dataset measuring the quality of public schools based on the ratio of enrolled students to instructional staff in the state (pupil/teacher ratio), the average length of the school term (term length), and average annual teacher salaries. Stephens and Yang (2014) extended the data series to birth cohorts from 1905 to 1959, using various editions of the *Digest of Educational Statistics*.[13] These data vary by state and cohort. Specifically, we created a single measure for each state of birth and cohort by averaging the prevailing measures during the years in which that cohort aged 6 to 17.

Infant mortality, number of doctors and number of nurses from the 1910 census. We construct a measure of infant mortality using the 1910 census, which asked women the number

[12] The age restriction that we employ largely avoids any mortality related to World War II, since the US casualties in that war were mostly from the 1914 to1926 birth cohorts. This also means that the G.I. Bill is also unlikely to influence our measures of educational attainment (Bound and Turner, 2002).
[13] The dataset is available on the journal website: https://www.aeaweb.org/articles?id=10.1257/aer.104.6.1777

of children they ever had and the number of children they had that died. We use the fraction of children that died to women ages 16 to 45 as a proxy for child mortality in the state.[14] This measure ranges from 129 per thousand in Iowa to 294 per thousand in New Mexico, with an average of 185 deaths per thousand. Using the occupation questions in the census, we also compute the total number of doctors and nurses in each state, divided by the state population. The number of doctors per thousand ranges from 0.69 in South Carolina to 2.15 in Colorado, averaging 1.35 across states. The number of nurses per thousand ranges from 0.07 in Oklahoma to 0.65 in California, averaging 0.28 across states. Unfortunately, these measures are only available for 1910 and do not vary across cohorts.

Per capita income by state. We use estimates of state-level per capita income (in 1929 dollars) in 1900 and 1920, reported by the Bureau of Labor Statistics, to compute a predicted state-level income in 1910.[15] This per capita income ranges from $211 in Mississippi to $818 in Nevada, averaging of $478.4 across states. Again, this measure does not vary across cohorts.

City-level data for the 1906-1915 cohort

Quality of school data by city. We rely on the work by Schmick and Shertzer (2019) who collected school quality measures for public schools for a balanced panel of 385 cities from 1900 to 1930 based on the *Report of the Commissioner of Education* (1900-1916) and the *Biennial Survey of Education* (1918-1930). We use their data to construct 2 measures that mirror the measures we use at the state level as closely as possible. First, we constructed "Expenditures per pupil" as total expenditures on teachers, supervisors, capital, and other expenditures divided by the average daily attendance in a school. Second, we use pupil teacher ratios, as reported by Schmick and Shertzer (2019). We then average these measures across the years when an individual was 6-17 years old.

[14] Official infant mortality rates by state are only available for a few states with complete birth and death registration systems. We collected data for these states. The correlation between our measure and the 1900 infant mortality for the 8 states (CT, MA, ME, MI, NH, NY, RI, and VT) for which official measures are available is 0.97. The correlation in 1915 is 0.92, based on official measures from 10 states (CT, MA, ME, MI, MN, NH, NY, PA, RI, and VT).

[15] These data come from estimates produced by Kuznets, Miller and Easterlin (1960).

Sanitation and health expenditures. We collected data from various years of the Financial Characteristics of Cities (Ager et al., 2020; Curran, 1979; Hoehn-Velasco, 2018; Hoehn-Velasco and Wrigley-Field, 2022; Tabellini, 2020); the publication reported data for cities with population larger than 30,000 from 1905 (with 154 cities) to 1930 (with 310 cities). We focused on per capita sanitation expenses and per capita health expenses. Our two final measures are computed as the average expenditure from one year before birth and 5 years after birth to capture the full childhood environment.

2.2 Summary statistics

For the birth cohorts born between 1876 and 1915, the mean age at death is 75.6 but is five years greater for females (78.6) than for males (73.0) (**Table A1**). Conditional on surviving to age 65, the sex gap in longevity is larger for later cohorts, consistent with the growing survival advantage of women documented elsewhere (Barford et al., 2006; Beltran-Sanchez et al., 2015; Cullen et al., 2016; Goldin and Lleras-Muney, 2019; Preston and Wang, 2006).[16] As in other data, the distribution of longevity is not quite normal—there is a long-left tail of early deaths, particularly among males (**Figure A2)**. Notably, there is no evidence in the data that there is age heaping, suggesting that the information on vital dates is of high quality.[17]

The average year of schooling is nine for the full sample and increases to ten for the most recent 1906-1915 cohort. The distribution of education for the 1906-1915 cohort is roughly normal with large spikes at 8, 12, and 16 reflecting the large number of individuals completing primary school or obtaining high school and university degrees (**Figure A3**). Average education is slightly larger for women. Although more men graduate college, many more women graduate high school, consistent with (Goldin, 1998), who documented that women had higher high school graduation rates in every state for every year from roughly 1910 into the 1930s.

2.3 Representativeness

[16] The gap in average longevity between men and women at age 65 rises from 2.48 for the 1876-1885 cohort to 4.41 for the 1906-1915 cohort.
[17] Age heaping occurs when individuals round their reported ages or the dates when events occurred, resulting in spikes at the 10- and 5-year marks.

Our data appear to be representative of the targeted population in terms of education, sex, and geographic origin. The distribution of education in the 1940 census and in our matched 1906-1915 sample are very similar (**Figure A3**). We have more individuals with exactly 12 years of schooling, whereas there are more individuals with exactly 8 years in the full 1940 census. However, the differences are small. We cannot reject the null that the distributions are identical using the Kolmogorov–Smirnov test. (The p-value is 1 for both men and women.) Education in our sample is also representative for each birth cohort born between 1906 and 1915, though the differences are somewhat larger for older cohorts (**Table A2).**[18]

Our sample contains more males compared to the 1940 full-count data (52.3% versus 49.3%) (**Table A2**). In general, it is more difficult to trace women through historical records because their surnames change with marriage. However, our ability to track women is much higher than in other historical research (Hollingsworth, 1976; Kaplanis et al., 2018). We return to this selection issue when we discuss sex differences.

Lastly, spatial distribution in our data differs from the distribution in the census in some important ways (**Table A3**). Individuals from the Midwest are overrepresented in our data, whereas individuals from the Northeast are underrepresented. Most notably, the most populous states (CA, NY, NJ, PA) are underrepresented in our data, with the exception of Ohio.[19] These differences, however, are not statistically significant—we cannot reject that the distributions are equal (p-value is 0.582 for both males and females). We assess the sensitivity of our results to weighting schemes to make the data nationally representative.

To investigate sample selection more thoroughly, we estimate regressions to see if any of the 1940 baseline covariates predict whether or not an individual will end up in our sample. The results (**Table A4**) strongly suggest that there is strong selection into the sample: in general individuals from higher socio-economic status (married males with higher education) are more likely to end up in our sample, though individuals with higher incomes or occupation scores are

[18] For the 1915 cohort, mean education in the full census (10.24) is almost identical to mean education in our sample (10.31)—a difference of 0.07; for the 1906 cohort, the difference is larger (0.18). The education in our matched sample is a bit higher, particularly for older cohorts, which is consistent with the fact that genealogical records tend to represent higher SES better. Black et al. (2022) show that census data matched to Family Trees underrepresent children of immigrants, for example.

[19] This is likely due to the fact that historical records for these states are poor, whereas they are excellent in Ohio and other midwestern states like Illinois, Indiana, Iowa, and Idaho.

in fact less likely to be matched. State of residence, state of birth and cohort also significantly predict matches.

This selection is due to two facts. First, the family trees are not a complete set of trees – not all the US population at a point is time is included. They are also not likely to be representative for various reasons. For example, the trees are more complete in states that have older and more complete vital registration systems. Second, not all individuals are equally likely to be matched. Individuals with low levels of literacy for example make more spelling mistakes and thus are less likely to be matched across databases. In fact, no existing linking methods produce samples that are consistently representative of the linkable population (Bailey et al., 2020). But we can re-weight the sample using the inverse of the predicted probability of matching to make it representative and address potential selection bias as suggested by Bailey et al (2020). We present these results later in the paper.

We also investigate models where we include family and twin fixed effects, and it is important to establish whether these samples are representative of the broader population. Family and twin samples include larger family sizes by definition. In **Table A1** we document how these samples differ in other dimensions. The average longevity does not differ much across samples (it is around 75.4) but the sibling sample and the twin samples have lower educational attainment (9.8 and 9.5 years respectively) than the full sample (10 years). The samples are otherwise similar in terms of their distribution across space and the characteristics in the state of birth. We investigate the extent to which these education and family size differences affect the results.

2.4 Quality of death information

An important consideration is whether the age at death information in the Family Tree, which comes from various data sources, is of high quality. One way to assess this is to compare the average age at death in our data to the expected age at death reported in the Social Security Administration (SSA) cohort life tables. These tables show that for the 1910 cohort, conditional on surviving to age 35, average age at death is 71.61 for males and 78.54 for females.[20] In our data, the average age at death for this birth cohort is 71.80 for males and 78.89 for females, slightly higher than in the SSA data. This small difference is to be expected because immigrants

[20]These tables are available here: https://www.ssa.gov/OACT/NOTES/as116/as116_Tbl_7_1910.html#wp1081274

and low SES individuals are less likely to be included our data but are included in SSA computations.[21]

We also check the quality of our data against newly released version of the 1940 census matched to mortality records created by a research group at Berkeley, the CenSoc-Numident.[22] This database uses an algorithm to match all individuals observed in the 1940 census to death certificates of individuals who died between 1988 and 2005 in the Numident file held by the SSA. There are two main differences between our data and this database. First, our data contain information on the age at death from multiple sources, including deaths occurring before 1988 or after 2005. Second, our dataset is constructed differently, relying more on people who generated most of our matches to death records and not on algorithms alone. It is unclear if our data are more accurate for any given person or group. Restricting attention to those born in the contiguous 48 states categorized as white in the 1940 census, who died between 1988 and 2005, the mean age at death in both datasets is similar (differences are less than 0.8 years in all cases), exhibiting a similar (downward) trend for men and women (**Figure A4**).[23] The data differ in other dimensions.[24] We assess if our results differ with this alternative data source.

2.5 Quality of education gradients in mortality

Most importantly for our analysis, we check that the basic associations between education and mortality in our data match those in the literature. The cohorts that we study have been studied before. Specifically, the influential studies by Kitagawa and Hauser (1973) and Elo and Preston (1996) investigated education differentials in mortality rates in 1960 and in the 1980s, respectively. We now demonstrate that if we use our data to compute period mortality rates for these years, we can reproduce the education gradients in these publications.

[21] There could be other sources for the differences. The SSA computations are based only on data from states with registration systems. The SSA attempts to make these data representative by weighting the state-level data. Using weights does not materially change our averages (weighted longevity is 79.45 for females and 72.63 for males).
[22] These data are available here: https://censoc.berkeley.edu/.
[23] The age at death is falling in both data because both condition on being alive in 1940. So, although more recent cohorts live longer, older cohorts are only observed in 1940 if they lived long enough to be alive in 1940. This selection causes the downward trend observed in both datasets.
[24] **Table A5** shows how the two datasets compare. Longevity is greater in our data, despite similarities in the preceding figure, because of differences in the extent to which different cohorts are represented (The CenSoc-Numident data include a larger share of older cohorts). The geographic distribution of the CenSoc-Numident data is also different with more observations from the Northeast and fewer from the Midwest.

First, we compare our estimated gradients to those in Kitagawa and Hauser (1973), who matched deaths certificates to 1960 census records to compute age, sex, and education-specific annual mortality rates in 1960. We use our individual data to compute mortality rates using the same education and age groupings they employ. Our death rates are somewhat lower than theirs, consistent with our previous findings that the Census-Tree data contains longer-lived individuals (**Table A6**). In both datasets, however, rates fall monotonically with education.[25] We also report the ratio of the mortality rate of the least educated group (with less than 8 years of school) to the most educated group (those with at least one year of college). Although not identical, our ratios are similar, though a bit smaller. For example, among men (women) ages 65-74 the ratio is 1.1 (1.4) in our data, and it is 1.1 (1.6) in Kitagawa and Hauser (1973).

We also compare our estimated gradients to those reported by Elo and Preston (1996) using the National Longitudinal Mortality Survey (NLMS). The NLMS matches individuals in the Current Population Surveys, surveyed 1976-1980, to death certificates from the National Death Index from 1979 to 1985. They compute five-year mortality rates by education level for different cohorts and report results for individuals who were 65-89. We use our individual data to compute identical mortality rates for individuals that survive to 1979 and later, grouped into the same education categories available in the NLMS. Elo and Preston report that the ratio of the mortality rates of the least educated group (less than primary education) to the most educated group (college graduates or higher) is 1.46 for males and 1.38 for females. In our data, the ratios are similar though somewhat smaller: 1.34 for males and 1.20 for females (**Table A7**). In both replications, we find that our estimated gradients are somewhat smaller for women, possibly because of data issues, a question that we revisit later in the paper.

In sum, the Census-Tree data sample is somewhat longer-lived than the corresponding US population but provides a fairly accurate description of the associations between education and mortality rates at a point in time.

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[25]There is one exception. The death rates for males increase between those with 8 years of education and those with 9-11 years of education in the Census-Tree data.

3. Basic associations between education and longevity

We now investigate education gradients in completed longevity. For the 1906-1915 cohort, the average age at death increases with education almost linearly for both men and women (**Figure 1 Panel A**). **Figure A5** further shows the estimated density of the age at death conditional on surviving to age 35 for various education groups: no school (0), some elementary (1-7), some high school (8-11), some college (12-15), and college plus (16+). For both men and women, the density of longevity shifts right when education increases.

3.1 Empirical approach

We estimate how years of education relate to longevity, conditional on survival to age 35 and controlling for various covariates, using the following regression model:

$$y_{ifcs} = \beta_0 + \beta_1 education_{ics} + X_{ics}\theta + \mu_s + \gamma_c + \delta_{sc} + \rho_f + \varepsilon_{ifcs} \qquad \text{Eq. (1)}$$

Where the outcome y is the age at death for individual i born in family f, year c, and state s.[26] Education is the number of years of schooling in the 1940 census for the individual. The regression includes controls for sex (X_{ics}), birth cohort fixed effect (γ_c), state-of-birth fixed effects (μ_s), and state-of-birth specific linear cohort trends (δ_{sc}). We also control for family fixed effects (ρ_f) in samples that include only siblings or twins. We report Huber-White robust standard errors clustered at the state-of-birth level.

β_1 is the coefficient of interest, measuring the association between education and longevity or the education gradient in longevity. In regressions without family fixed effects, β_1 is identified by comparing the longevity of individuals of the same sex born in the same state and year but who differ in their level of education.

[26] We also estimate an Accelerated Failure Time model (AFT), common in demography, which uses the log of the age at death as the dependent variable. The results from this estimation (**Table A8**) lead to similar conclusions, with one more year of education increasing longevity by about 0.6 percent. We prefer the model in levels because the distribution of the age at death has a fat left tail, which is less consistent with the log normal assumption.

With family fixed effects, β_1 is identified by comparing the longevity of siblings or twins who obtain different education levels, thus netting out common genetic and family environments that affect both longevity and education. These omitted backgrounds are typically expected to bias the OLS estimates upwards: for example, richer families might provide more education and better health care access to their children. On the other hand, it is also possible that families that prioritize education make choices that are ultimately detrimental to their children's health. For example, poor parents tend to work more or move to guarantee their children's education (Heinrich, 2014; Hsueh and Yoshikawa, 2007). The family fixed effect estimates may also not be directly comparable to the full population estimates if the effects of education are heterogeneous-- for example, if they vary with family size or other unobserved characteristics (Miller et al., 2021). Lastly parents may compensate or reinforce investments on their children, which would also lead to fixed effects estimates that differ from population estimates.

3.2 Main results: OLS and family fixed effects

Table 1 shows the results for the 1906-1915 birth cohorts. Column 1 includes no covariates, column 2 controls for cohort fixed effects, column 3 adds state-of-birth fixed effects, and column 4 adds state-specific linear trends. The coefficient of education is positive and statistically significant in all regressions and for both men and women. The estimates are remarkably stable across columns. We find that one additional year of education is associated with roughly 0.4 more years of life. Women have slightly higher coefficients (0.41) than men (0.38), which is statistically significant (p-value=0.02). Relative to mean longevity (71.96 for men; 78.95 for women), this effect is roughly equivalent to a 0.5 percent increase in longevity for each additional year of schooling (with an elasticity of 0.05). Alternatively, an increase of one standard deviation in education would increase longevity by 1.2 years for both men and women.

As discussed in the data section, our sample is not representative of the baseline population we targeted in the census. We first re-weight the data to address geographic disparities. The estimates slightly increase if we weight the observations to make them representative of the nation using the 1940 state level population (Column 5); once we do this, there is no longer any difference between sexes. To further address selection, we re-estimate our models but weighting the observations using the inverse probability of the matching individuals between the census and Family Tree as suggested by Bailey et al. (2020). The estimates are very similar if we use the weights from a simple model that only uses education and sex (in addition to state of residence state of birth and cohort fixed effects) to predict a matches (Column 6) or if we also include labor force status, income, occupation scores, marital status and number of children in the household to predict matches (column 7).[27]

We now investigate the results with family fixed effects. Before presenting the results, it is worth discussing the variation that identifies these. The standard deviation of education is around 3 for the full 1906-1915 sample. This variation is not reduced by the inclusion of state of birth fixed effects, cohort fixed effects or state specific trends (**Table A9**)—it remains close to 3 years. However, the inclusion of family fixed effects reduces the variation substantially to 48% of the original among siblings and 45% among twins. While this is a substantial decrease, there remains a lot of variation within families. In the sibling (twin) sample 74% (68%) of families include siblings who differ in their years of education.[28]

Table 2 shows the results with family FE. The first column replicates the results from **Table 1** for reference and shows a gradient of 0.4. In Column 2, we estimate the model using OLS for a sample of families with at least 2 children. This OLS estimate is identical at 0.41, suggesting that associations are not different in this subsample of families. The estimate with family fixed effects is only slightly larger at 0.45. We then estimate results among twins, defined as siblings that are born within the same calendar year. The results for twins (shown in the last two columns) are very similar, slightly larger than OLS.

The slightly higher fixed effects estimates might reflect heterogeneous effects: perhaps the effect of education is different for individuals from larger families (Miller et al., 2021). The OLS estimates suggest this is the case for males, for whom the OLS coefficient rises as we move from the full sample into the sibling sample and the twin sample, but not for women. If we estimate

[27] To create our inverse probability weights, we estimate the probability of Census-Tree successful match as a function of year of birth dummies, years of schooling dummies, sex, race, household income, state of residence, state of birth, marriage status, number of kids, labor force participation, and occupation income scores.
[28] This variation is larger than that in Lundborg et al. (2016) though it is worth noting that we cannot distinguish fraternal and identical twins whereas Lundborg et al. do.

the coefficient of education separately for individuals of different family sizes, we do find a slightly higher effect for individuals in large families: for example, the OLS is 0.439 for individuals in families of 4 but only 0.412 for single children (**Table A10**). But the OLS coefficient falls for individuals in larger families and overall, we cannot reject that the coefficients are identical across families of different sizes (p-value=0.452).[29] The same is true if we estimate FE models by family size (**Table A10**). Although there may be other dimensions of unobserved heterogeneity across the sibling and twin samples, these results suggest that the fixed effects estimates are representative. Therefore the larger point estimates in the family FE models suggests that parents reinforce initial differences in endowments, consistent with the evidence in Aizer and Cunha (2012).[30]

Overall, the associations are mostly unaffected by the inclusion of fixed effects and hover around 0.4 for both sexes.[31] Our estimates without family fixed effects are almost identical to those found in Halpern-Manners et al. (2020), who link 1920 and 1940 records of males to mortality records in the NUMIDENT file from the social security administration. With family fixed effects, our estimates are somewhat larger than theirs—we return to this issue below and show the differences are caused by truncation in the death years included in the sample.

3.3 Data quality and effects of truncation

We now compare our results to the results one would obtain using the CenSoc-Numident data to assess if the fact that our data are mostly hand matched and includes more sources of death introduces a lot of error. If we restrict our data to include only individuals who died between 1988 and 2005, then our estimates are very similar: the coefficient of education is 0.096 using Family Tree data compared to 0.088 in the CenSoc-Numident data (**Table 3** columns 1 and 2).

In column 3, we show that when we include deaths from 1941 to 2005, the coefficient on education rises substantially from 0.10 to 0.34. Thus, including early deaths makes a very large

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[29] These results are only suggestive: our sibling counts are likely incomplete since our sample does not include individuals with missing data (date of birth or date of death).
[30] It is not clear why parents would reinforce more for daughters than for sons.
[31] We estimate this model by sex by interacting a dummy for female with years of education so we can include siblings of different sexes in the sample (instead of stratifying by sex).

difference to our estimates. If we include deaths up to 2020, the coefficient of education rises to 0.40, another substantial (but smaller) increase (column 4).[32] These results document that right and especially left censoring create a substantial attenuation in the estimates of the education gradient because more educated individuals are more likely to survive to 1988 and to live beyond 2005.

Censoring is also likely the reason our estimates for men are slightly larger than those in Halpern-Manners et al.[33] As shown in **Table A11**, our estimates on education for males using the untruncated data go from 0.38 in OLS to 0.46 in the twin sample. If instead we use the truncated data, the OLS coefficient is 0.28 (attenuated as discussed above) and the twin FE one is 0.20. Thus, in the truncated data the twin coefficient is smaller, as in Halpern-Manners et al. but in the untruncated data the coefficient is larger.[34]

3.4 Estimates by cohort and age

So far, our results only show associations for the youngest cohort in the data. **Figure 2** shows the associations by cohort and survival age. For each ten-year birth cohort, we restrict the sample so that everyone has survived to the same age: the 1906-1915 cohort is restricted to surviving to age 35, the 1896-1905 cohort is restricted to survive to age 45, etc. We then plot the gradients for different cohorts and conditional on surviving to different ages.

The relationship between education and longevity is greater for more recent cohorts than for older cohorts surviving to the same age (**Figure 2**). For example, conditional on surviving to age 55, one more year of education is associated with 0.32 years of life for the 1906-1915 cohort but with only 0.22 years of life for the 1896 cohort and 0.15 years of life for the 1886-1895 cohort. The gradients in longevity are about twice as large for the more recent cohort than for cohorts born 20 years earlier. More recent cohorts are also more highly educated—thus the gradient is

[32] There are some small differences by sex. The education gradients for men are very close on both datasets (0.0991 in the CenSoc-Numident data vs 0.1034 in the Census-Tree data). For women the CenSoc-Numident data produce lower estimates 0.0788 compared to 0.0882.
[33] Halpern-Manners et al. (2020) also conduct analysis that suggest that this censoring leads to attenuation.
[34] It is not clear why the truncation has a larger effect on the twin FE estimates than for the overall population estimates.

increasing with the level of education across cohorts.[35] These results do not appear to be driven by differential measurement error in education across cohorts, and they are identical if we use family fixed effects or estimate non-linear models.[36] These findings are in line with the narrative in the literature that education gradients in mortality rates have been rising in the US since mid-century. Indeed, if we compute annual mortality rates for 5-year age groups using our data, we find that the gap in mortality between those with high school or less and those with some college or more grew from 1950 to 1970 (**Figure A6**).

It is not clear why the gradients are largest for the 1906 to 1915 birth cohorts. These cohorts spent many of their early adult years in the Great depression and many of them served in WWII. Perhaps education was particularly valuable during these trying times especially in the determination of health. This interpretation would be consistent with the findings of Cutler et al. (2015) who document using more recent data that education gradients in various measures of health are larger for those graduating during bad times. We lack cohort data to more systematically explore these differences across cohorts.

We also find that the education gradient is lower at older ages. This is true for all cohorts. For example, for the 1906-1915 cohort, one more year of education is associated with 0.40 additional years of life for those surviving to age 35 but with only 0.26 years of life for those surviving to age 65. This evidence is consistent with the idea that education plays an important role in preventing early deaths. Analyses that condition on survival to old age will find lower estimates of the gradient. This result confirms observations in the literature based on alternative data and

[35] In the 1940 census, the mean education was 8.26 for the 1876-1885 cohort, 8.72 for the 1886-1895 cohort, 9.29 for the 1896-1905 cohort and 10.04 for the 1906-1915 cohort. These differences in educational attainment across cohorts are likely to be underestimated since those with lower education are less likely to have survived to 1940.

[36] In the 1940 census, educational attainment, particularly high school graduation, is overstated for cohorts older than 35 (those born before 1905; Goldin 1998), so caution is needed in interpreting these results. If individuals with low levels of schooling and who lived short lives reported higher levels of schooling, the relationship between years of school and longevity would likely be attenuated since the average longevity of the highly educated group would fall as a result of the misclassification. Goldin finds that these errors are smaller in states with higher educational attainment. In **Figure A7,** we show that the results are very similar among high education states, suggesting that the increase in the education gradient across cohorts is real rather than due to measurement error for older cohorts. The results with family fixed effects are in **Figure A8**. The gradients by cohort using non-parametric models are shown in **Figure A9** and use the methods described in the next section.

estimation methods, e.g. Kitagawa and Hauser (1973), Elo and Preston (1996) , Lynch (2003), and Crimmins (2005).[37]

3.5 Testing for linear relationships

So far, we have estimated linear relationships between education and longevity. However, a large literature has argued that there are important credentialing effects. Because in our data we observe education as a continuous measure ranging from 0 to 17, we can estimate non-parametric models of the education-longevity relationship and test whether this relationship is linear or subject to credentialing (or sheepskin) effects.

To investigate this, we estimate a fully non-parametric model for the youngest cohort for whom the quality of the education information is highest. We re-estimate eq (1) but replace years of schooling with dummies for every single year of school and use 0 as the left-out (reference) category. These estimates (plotted in **Figure 1 Panel B**) show that, for men, the relationship between education and longevity is best described as a series of step functions with increases at 1, 8, 12, and 16 and no increases in between. For women, the relationship is more linear before 8 years of school but becomes a step function thereafter.[38] The results with family fixed effects follow a similar pattern for men and women, though the point estimates tend to be larger (**Tables A12a and A12b**).

In **Table A13,** we present the fit of different models with and without family fixed effects: a fully non-parametric model, a linear model, and a model with splines at exactly 8, 12, and 16 years of school (corresponding to discrete changes in educational attainment). We show four measures of fit: the adjusted R-square, the AIC, the BIC, and the mean squared error from a cross-validation exercise.[39] The spline model provides the best fit (highest adjusted R-square, lowest AIC, BIC or MSE) for both men and women. However, the linear model still provides an

[37] These conclusions are identical if we estimate a linear or a log linear model as shown in **Figure A10**.
[38] **Table A14** shows that we reject the linear specification for both men and women for the full range but not for education levels between 1 and 6. Results from the CenSoc-Numident are similar (**Figure A11**).
[39] We use a 10-fold cross-validation process to compute the MSE. Specifically, we first randomly shuffle the dataset and split it into 10 groups. For each unique group, we take the group as a holdout, estimate the model on the remaining groups, compute the MSE based on all groups, and store the mean MSE. We then take the average of these 10 mean MSE.

excellent fit: our fit measures do not improve much by moving from the linear to the spline or the non-parametric model.

4. Heterogeneity by place-of-birth for the 1906-1915 cohort

In this section, we first document the variation in the gradient across place of birth and then correlate it with place-level covariates. We first study gradients using information on the state of birth, which is available for a very large sample. However, this information might be too coarse. As Wallis (2000) documents that at the beginning of the 20th century (and unlike today) most public expenditures and infrastructure were determined at the local level by local governments. Thus, we also investigate gradients at the city of birth level using a subsample for which this information is available.

4.1 Empirical approach to understanding heterogeneity

We proceed in two steps, following Card and Krueger (1992a) and the more recent work of Chetty et al. (2014). First, we estimate education gradients separately by place-of-birth and sex, controlling for birth-cohort dummies as in Eq. 1 above—we estimate linear relationships given that our results suggest this is an excellent approximation of the relationship between education and longevity. We describe these and how they vary by sex and across states.

Then, we correlate place-level gradients with observable place characteristics that have been hypothesized as plausibly moderating this relationship. We show the results visually and then we more formally test if the relationships are significant by estimating an OLS regression as follows:

$$\widehat{\beta_{pc}} = c + \gamma X_{pc} + \varepsilon_{pc} \qquad \text{Eq. (2)}$$

where $\widehat{\beta_{pc}}$ is the estimated association between education and longevity in a given place-of-birth p and (if possible) two-year cohort c, and $X_{spc}$is a place-level covariate measured for that cohort if cohort level variation exists—otherwise we correlate place-level gradients with place-level covariates. We estimate this regression using weighted least squares, using the inverse of the estimated variance of $\widehat{\beta_{pc}}$ as weights. We report whether the estimated associations $\hat{\gamma}$ are statistically significant. Because we only have place-level data for the most recent cohort (1906-1915), this analysis focuses on them.

For models that include family fixed effects we proceed somewhat differently and estimate these models as follows:

$$y_{ifcp} = \alpha + \sum_s \beta_s \left(education_{icp} \times \mu_p \times female\right) + \sum_p \delta_p \left(\mu_p \times female\right) + \sum_{pc} \lambda_{pc} (\mu_p \times \gamma_c \times female) + \rho_f + \varepsilon_{ifcp} \qquad \text{Eq. (3)}$$

Where y_{ifcp} is the age at death for individual *i* born in family *f*, year *c*, and place *p*. $education_{icp}$ represents years of schooling. $female$ equals to 1 for females and 0 for males. μ_p denotes place of birth dummies, and γ_c denotes birth cohort dummies. ρ_f denotes family fixed effects. ε_{ifcp} is the error term. β_s a vector of coefficients capturing the association between years of schooling and longevity for females and males by each birth state. We do this to include families with children of different sex and born in separate states who would be dropped if we estimated gradients separately for males and females, and state by state.

4.2 What childhood environments do we consider and why?

Theoretically, if skill (human capital) affects longevity, then other inputs into the production of skill that complement time spent in school would affect the return to time spent in school (years of education). We examine several potentially complementary inputs in the production of human capital.

First, we investigate if the gradient is larger in places where other school inputs are larger. Many authors have hypothesized that years of schooling do not fully capture the differences across individuals in their actual education (skill) levels. But measuring school quality is difficult, so evidence in support of this hypothesis is scant.[40] We focus on two others sets of inputs: school quality measures (teacher salaries, length term, and pupil teacher ratios), and peers' educational attainment. The school quality measures have been used before in several studies of the wage

[40] All the reviews cited in the introduction discuss this issue.

returns to education and have previously predicted larger health gradients among Blacks. We also investigate if the average education in the state-of-birth matters. Peers are an essential input into education. Individuals may benefit not only from having greater education themselves but from having family and peers who are better educated. Indeed Acemoglu and Angrist (2000) find that the returns to education on wages are (modestly) higher in places where average education is larger. We hypothesize that the same is true for health.

Second, we examine if the returns to years in school vary with measures of baseline health or, relatedly, with measures of health care access which presumably translate into better population health as predicted by Kipperluis and Galama (2014). We first investigate if gradients are larger in places where longevity is higher. Longevity is a summary measure of health and is heavily influenced by conditions early in life. Child health is likely complementary to time spent in school: unhealthy children are less likely to benefit from instruction (Glewwe and Miguel, 2007). But longevity captures more than child health, so we investigate whether infant mortality in the state of birth affects the gradient instead. This measure captures average health in the state early in life, before schooling starts. Alternatively, we look at how health care resources, measured by the number of doctors and number of nurses, affect the gradient. These two metrics inform us about the extent to which children would have access to medical treatment when they fell sick.

Greater health and health care resources may in fact result in *lower* rather that larger gradients. Many health investments during this time, such as water filtration and chlorination or the provision of sewers, often benefit individuals from both high and low socio-economic status. In this case we might expect education gradients to fall as health resources increase: having an education (and learning to boil water or clean hands) might be less helpful once everyone accesses clean water. Indeed, other widely available innovations have had greater impacts on initial poor populations lowering SES gradients.[41] Thus whether education gradients increase or fall with health resources is an empirical question.

[41] For example, Acemoglu and Johnson (2007) show that innovations like DDT (dichlorodiphenyl trichloroethylene) and penicillin increased longevity more in places with an initially large incidence of malaria and infectious disease that antibiotics treat. Becker, Philipson, and Soares (2005) show these innovations lowered inequality in longevity across countries of the world.

Third, we also explore if state level income affects education gradients. Today in the US, there are large correlations between state mortality and state level income (Couillard et al., 2021), possibly because richer states often invest more in their health infrastructure and in other social policies that benefit health. We hypothesize that during the early 20th century, individuals in richer states had greater resources to invest in their children in the form of nutrition, housing, etc. (Kornrich and Furstenberg, 2013). Equally importantly, in richer states, individuals likely had access to greater overall infrastructure such as better access to sewers and clean water which were rapidly disseminating during this period (Alsan and Goldin, 2019; Cutler and Miller, 2005). Because these resources would improve health, we expect that individuals in richer states would have larger education gradients. However, greater income per capita can also be associated with greater exposure to pollution (Selden and Song, 1994; Van Beeck et al., 2000) and other negative factors such as greater alcohol consumption or longer work hours, which can be detrimental to health. Thus, it is ultimately an empirical question whether or not children in rich states had greater or lower baseline health and whether education gradients would be higher as a result.

4.3 Documenting heterogeneity in the education gradient by state of birth

We first describe education gradients by state-of-birth by estimating Eq. 1 separately by state. The estimated coefficients on education are positive and statistically significant for both men and women in almost all states.[42] However, there is wide variation. **Table A15a** shows the top ten and bottom ten states ranked based on the education gradient. In Utah (the top state), one more year of education is associated with 0.67 additional years of life, whereas in Minnesota (the bottom state), one more year of education is associated with only 0.21 more years of life. Moving from the bottom 10th to the 90th percentile of the education distribution in the state yields 4.7 more years of life in Utah but only 1.3 in Minnesota. These large differences are statistically significant at the 5% level and similar if we estimate gradients based on a log model (Panel B). Thus, Utah remains one of the top states even when we account for the fact that life expectancy in Utah is high.

[42] The coefficients on education are positive but insignificant for men born in New Mexico and for women born in Louisiana, Nevada, New Hampshire, and Rhode Island.

Estimates by sex show some important differences. First, although the gradients are the same by sex when we average across the 48 states (0.401 for men and 0.413 for women), the variation by state of birth is larger for men than for women: the standard deviation of the gradients is 0.096 for women and 0.127 for men.[43] Moreover, the spatial distribution differs by sex **(Figure 3)**, though the difference in this gradient for the middle three groups do not differ in a statistically significant way from the estimates that report for the full sample in **Table 1**. This warrants some caution in the way we interpret state-level heterogeneity in our estimates.

To systematically compare across sexes, we plot the estimated education gradients by state of birth for women (y-axis) against the estimated gradients for men (x-axis) (**Figure A12**). In general, states with large associations for men also have large associations for women. But there are noticeable exceptions. In Rhode Island, Nevada, and Connecticut, the associations between education and longevity are very small for women but above average for men. Conversely, in Tennessee, Kentucky, and New Mexico, the associations are large for women but small for men. Overall and quite surprisingly, the correlation in the male and female education gradient across states is positive but smaller than one would expect (0.57). Thus, while the overall gradient is not different by sex, there appear to be large differences in why/when education is associated with longevity, differing by sex and space.

These results are similar but not identical if we include family fixed effects (**Table A15b**). **Figure A13** plots the state-specific gradients estimated with and without family FE. The correlation between them is 0.35 for females and 0.56 for males. As the figure makes clear, there is a lot more variation in the estimates with family fixed effects. These FE estimates are much noisier because of the small sample sizes for some states. This suggests that while the family FE might be useful to assess the extent of omitted variable biases at the family level, this approach has power limitations when exploring heterogeneity by state of birth. For the remainder of the analysis, we focus on results without family FE and report briefly if they matter.

[43] We exclude Delaware in these computations as it is clearly an outlier – see **Figure A13** for example. **Figure A14** shows the density of the estimated gradients by state. This Figure shows that the density is more tightly centered around the mean for women than for men.

We explore whether these gradients are stable over time by re-estimating the gradient, conditioning on survival to age 65 for all cohorts to make the estimates comparable across cohorts and without family FE given the sample size concerns. We correlate the gradient for the 1906-1915 cohort with the gradient for the oldest 1876-1885 cohort (**Figure A15**). Surprisingly, the gradients are not very related. The relationship is positive and statistically significant for males (regression coefficient = 0.33, p = 0.026) but not significant for females (regression coefficient = 0.156, p = 0.262). Thus, the states with high gradients for earlier cohorts are not the same states for more recent cohorts. This suggests that there were significant changes in environmental characteristics within states throughout the period affecting the association between education and longevity as suggested by (Hayward et al., 2015; Montez and Zajacova, 2014). We now explore reasons for this heterogeneity.

4.4 Why do gradients differ by state of birth?

First, we explore whether the returns to education vary with the quality of education. **Figure 4** plots the estimated state of birth and cohort-level education gradients by sex against our measures of quality: relative teacher wages, length of school term, and pupil-teacher ratios. For males, all three measures (which vary across cohorts) strongly predict education gradients (Panel A). Put otherwise, when teachers were well-paid, the school year was long, and pupil-teacher ratios were low, men who went to school longer benefited more from schooling in terms of their longevity. However, Panel B shows that the same does not hold true for women. If we regress education gradients on school measures, individually or jointly, the regressors are statistically significant predictors for men, with and without family FE (p-value<0.001). By contrast, quality of schooling measures are *negatively* and weakly related with the education gradient for women, and they are statistically insignificant when we include family FE (p-value=0.444) (**Table 4**).

We also test if the education gradients vary with the level of education in one's state of birth and cohort for the 1906-1915 cohort. **Figure 5 Panel A** plots the gradients by state-of-birth and cohort against the average level of education in the state for the same cohort and sex, where the cohorts are binned into 2-year age groups (N=48*5=240 observations).[44] For men, the education

[44] We plot the OLS results and report the coefficients in the figures (as well as in **Table A17**). The Fixed Effects results are in in **Table 4**.

gradients are larger in places where average education among men is large. This association is statistically significant ($\hat{\gamma} = 0.104, s.e. 0.008$); estimates with family FE ($\hat{\gamma} = 0.091, s.e. 0.009$) are similar. For women, the association of the gradient and average female education is substantially smaller ($\hat{\gamma}$ = 0.022, s.e. 0.0082) and statistically significant but not with family FE ($\hat{\gamma} = 0.042, s.e. 0.014$). These results are similar if we drop states with large Black populations or use race-specific measures of school quality (Card and Krueger, 1992b) (**Tables A16a and A16b**).

Next, we investigate if the education gradient varies by the level of health and health care resources. We start by looking at longevity. **Panel B of Figure 5** shows the estimated gradients by state of birth and two-year birth cohorts (N=240) against the average longevity for the same group in our data (conditional on surviving to age 35). For men, the education gradient is greater in places with greater longevity, and this association is statistically significant (without family FE: $\hat{\gamma} = 0.047, s.e. 0.008$; with family FE: $\hat{\gamma} = 0.070, s.e. 0.010$). For women, the education gradient decreases when longevity goes up ($\hat{\gamma} = -0.044, s.e. 0.009$), which is statistically significant but not with family fixed effects ($\hat{\gamma} = 0.004, s.e. 0.016$).

We use three alternative measures for the level of health in 1910: child mortality, number of doctors, and number of nurses (**Figure 6**). Because these measures do not vary at the cohort level, we show the relationship only at the state level. For men, we find that the education gradient is larger in places with better health resources, and the relationship is statistically significant for doctors ($\hat{\gamma} = 0.185, s.e. 0.001$) and nurses ($\hat{\gamma} = 0.255, s.e. 0.059$). These associations are much more muted for women: only the number of doctors predicts education gradients for women, and jointly, we cannot reject that all three measures do not matter for women's gradients (p-value: 0.458 without family FE, and 0.351 with family FE). However, we can reject the same test for men (p-value < 0.001 with or without family FE).

We end by exploring if gradients are larger in richer states, measured by per capita state-level income (**Figure 7**). Again, we find statistically significantly larger gradients for men born in richer locations ($\hat{\gamma}$ = 0.459, $s.e.$ 0.08 without family FE, and $\hat{\gamma}$ = 0.454, $s.e.$ 0.063 with family

FE) but not for women ($\hat{\gamma}$ = 0.056, $s.e.$ 0.08 without family FE, and $\hat{\gamma}$ = 0.010, $s.e.$ 0.069 with family FE).

Overall, we find education gradients for men are larger in places with higher incomes, more health care resources, and higher quality and quantity of education.[45] But the education gradients for women do not follow these patterns. We found no early life indicators moderating this variation for women. Indeed, if we regress the education gradients at the state-level on all state characteristics, we find that the r-squared in this regression is high for men (0.45 without family FE; 0.58 with family FE) and much lower for women (0.09 without family FE; 0.15 with family FE).

4.5 Gradients by city of birth

Our results by state of birth might be attenuated because state level measures do a poor job at capturing the actual environmental conditions individuals grew up under, since these were mostly determined by local environments. To assess this, we now replicate our analysis using the subsample of individuals that was known to be born in a large city for which we can obtain data on school quality and on education and health expenditures which reflect local policy choices.

From the onset we note that this analysis suffers from two limitations. First the samples are small and likely highly selected. Second, we cannot include family fixed effects as the identifying variation becomes very limited. We start by assessing the extent of sample selection. In **Table A19** we show that in our main sample (with state of birth information) the education gradient is 0.4 for all (column 1). In the sample with school quality measures or city expenditures it is 0.49 (columns 2 and 3). These results suggest the sample is biased towards greater gradients or alternatively that gradients were larger in large cities. Most of the increase in the gradient is actually driven by men (Panel B)—for women the gradients stay constant across samples.

[45] We also investigated whether education gradients were larger in states with more years of compulsory schooling and more spending on education. Similarly, we obtained statistically significant larger gradients for men but not for women, with and without family fixed effects (**Figures A16 and A17, Table A18**).

Next, we estimate gradients for each city. We find that the gradient overall is largest (greater than 1.3) in Elmira (New York), Piqua (Ohio), and Millville (New Jersey) and lowest, which is actually negative and lower than -0.5, for La Salle (Illinois), Adams (Massachusetts), and Milford (Massachusetts). **Figure A18** shows the distribution of these gradients across cities by sex. The mean is 0.51 (0.41) and the standard deviation is 0.43(0.47) for males (females)—overall these distributions are similar to the ones we estimated by state though the levels are somewhat higher and more imprecisely estimated. The mean is similar for men and women but again the correlation across sexes is 0.0567, much lower than one might expect.

In **Figures A19 and A20**, we plot the estimated gradients against our measures of education and health. Similar to our findings for states, we see that gradients are larger in places with more resources for men, but not for women. **Table A20** confirms these visual results. For men the quality of schooling measures (panel B) jointly predict gradients (p=0.0335), whereas the same is not true for women (p=0.9). Health expenditures are positively correlated with gradients for men mirroring the state results, but the results are not statistically significant. For women the coefficients are negative also statistically insignificant.

Overall the city-level results confirm our state-level results: gradients are greater for men in places with more resources but the same is not true for women.

4.6 Differences by sex

Why is the education gradient in longevity moderated by childhood conditions for men but not women? We discuss some hypotheses now.

We hypothesize that women are less sensitive to conditions while growing up and that this might explain why gradients are unaffected by these conditions. It has long been hypothesized that males are less biologically buffered than females against the environment during growth and development (Stinson, 1985). Recent papers document that the impact of childhood environments on education and other lifetime economic outcomes differs substantially by sex.[46] A few recent studies find some support for this hypothesis for health outcomes. For example, Doblhammer et al. (2013) find that the effects of being born during the 1866-68 Finnish famine on longevity were large for men but not noticeable for women. Similarly, Lindeboom et al. (2010) find that children exposed in utero to the Dutch Potato famine of 1846-47 had lower longevity, with much larger effects for men. Van den Berg et al. (2016) find that undernutrition between conception and age 4 lowers heights among adult men but not adult women. This male-sensitivity hypothesis is related to the large differences in longevity by sex—it is well documented that there are biological differences that disadvantage males in many health domains (Kraemer, 2000).

Even if the male-sensitivity hypothesis is borne out, it still provides no insight as to why there is variation in the education gradients for women. We hypothesize that, for women, factors like marriage markets and fertility play a large role in moderating the relationship between education and mortality. For example, until the mid 1930s, maternal mortality was very large in the US and varied substantially across states. If more educated women had fewer births, and had them at different ages, then this would affect the education gradient in mortality, both directly through maternal mortality and indirectly since births can have long-term health consequences (like fistulae) among women who survive past reproductive ages.

We investigate this using cross sectional measures of fertility and maternal mortality. The first fertility measure is the number of children ever born to women ages 16-45 as reported in the 1910 census. This measure captures fertility at the time of birth in the state of birth. We also use two variables derived from Vital statistics in 1933 (the first year for which all states reported these variables): the birth rate by state and the maternal mortality rate (number of deaths per 1,000 live births). These two measures capture the fertility behavior of the cohorts we study around the time they start reproducing: the 1906-1915 cohort starts procreating around 1930.

[46] Bertrand et al. (2013) find that boys do particularly poorly in disadvantaged environments, and they appear to be more sensitive to inputs. Consistent with these findings, Chetty and Hendren (2018a,b) find that neighborhood characteristics affect lifetime outcomes more for boys than for girls. Autor et al. (2019) find that childhood environments appear to affect boys' educational attainment more than girls'.

The education gradient for women is smaller in places where total fertility in 1910 was high (**Table A21**). This effect is negative and statistically significant when we include family fixed effects. For men, the association is insignificant in the fixed effects models. The variables measuring the actual behavior of the cohorts around 1933 are never significant. Thus, while not completely robust, the results hint at the possibility that education gradients are lower for women born in states with large fertility rates.

Differences between men and women could also be due to their differential attachment to the labor market and the differences in how they would obtain income. The association of education and longevity exhibits more credentialing effects for men and is rather linear among women. This suggests that the signaling associated with obtaining a degree might play a particularly important role in the labor market for men. White women in contrast had very low rates of labor market participation in the first half of the century, and they relied mostly on their husband's income (Bailey et al., 2012). Education might have affected who they married rather than how much they earned. Surprisingly, the financial returns to school (estimated using the 1940 census) are not correlated with education gradients for either men or women ($\hat{\gamma}$ is in fact negative and statistically insignificant for men and close to zero for women), and this holds true whether we use labor market earnings or household income to estimate the financial returns to education (**Figure A21**).

Finally, sex differences could be due to differences in the quality of the data by sex. Women are less well-represented in our data compared to men. Our comparisons with Kitagawa and Hauser (1973) and with Elo and Preston (1996) do show smaller gradients for women than they estimated, suggesting data quality might be an issue. However, in our data, states with lower attrition do not have smaller or larger gradients for men or women.[47] We do find that migration is a function of sex, education, and the interaction between them (**Table A22**). Women are in fact more likely to move, and thus our measures of the environment they grow up in might be mismatched. This is one possible explanation for our results.

[47] If we regress our estimates of the education gradients on the fraction of the population that is represented in our data, we find that the gradients are smaller in states for which our coverage is better for both women (beta = -0.0245, p value: 0.809) and men (beta=-0.0978, p value: 0.350), but the coefficients are not statistically significant.

5. Conclusion

This paper uses a new individual-level dataset to study the association between years of schooling and longevity in the US across white cohorts born between 1870 and 1915 using OLS and family fixed effect models. We find large associations that are mostly unaffected by the inclusion of family fixed effects. We also find that education gradients are large, grow across cohorts, and get smaller as individuals age. The relationship is close to linear, though there are important credentialing effects, particularly for men. This new evidence on the education-longevity relationship is mostly consistent with existing evidence that uses mortality rates.

We document extensive heterogeneity in this association by sex, cohort, and place of birth. As Hayward et al. (2015) notes "…there is no inherent causal association between educational attainment and adult mortality; instead, the causal association is dependent upon time, place, and social environment under study." Our findings are very much in line with this observation. We are able to make progress in understanding this heterogeneity for men. Men who are born in richer, more educated places with more health care resources and higher quality of schooling have larger gradients, consistent with the idea that these are important complementary inputs in the creation of human capital.

However, the dynamics of the gradient are substantially different for men and women. Although on average the education gradient is the same for men and women, the places where education gradients are large for men are not the same for women. Moreover, none of the observable characteristics of the childhood environment we observe appear to have large impacts on the education gradient for women. We do find some suggestive evidence, however, that differences in the level of migration or fertility may explain the different education gradients by sex. Future research should further examine this puzzle.

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Table 1. Regression results of longevity and education by sex.

	OLS estimates						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A. Full Sample							
Education	0.42***	0.41***	0.40***	0.40***	0.42***	0.39***	0.39***
	(0.01)	(0.01)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)
Female	6.90***	6.90***	6.91***	6.91***	6.72***	6.93***	6.96***
	(0.09)	(0.09)	(0.10)	(0.10)	(0.12)	(0.10)	(0.10)
Observations (individuals)	5,476,138	5,476,138	5,476,138	5,476,138	5,476,138	5,476,122	5,473,945
Adjust-R	.0701	.0702	.0719	.0720	.0674	0.0713	0.0723
Panel B. Male							
Education	0.42***	0.42***	0.38***	0.38***	0.42***	0.37***	0.37***
	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)
Observations	2,863,851	2,863,851	2,863,851	2,863,851	2,863,851	2,863,835	2,862,783
Adjust-R	.0094	.0097	.0123	.0124	.0117	.0118	.0119
Panel C. Female							
Education	0.42***	0.41***	0.41***	0.41***	0.42***	0.41***	0.41***
	(0.01)	(0.01)	(0.01)	(0.01)	(0.02)	(0.01)	(0.02)
Observations	2,612,287	2,612,287	2,612,287	2,612,287	2,612,287	2,612,287	2,611,162
Adjust-R	.0081	.0082	.0099	.0100	.0092	.0096	.0103
State fixed effects	No	No	Yes	Yes	Yes	Yes	Yes
Cohort fixed effects	No	Yes	Yes	Yes	Yes	Yes	Yes
State-specific linear trends	No	No	No	Yes	Yes	Yes	Yes
Weights	No	No	No	No	1940 state population size	Inverse probability of Census-Tree matching (Limited controls)	Inverse probability of Census-Tree matching (full set of controls)

Notes: Regression sample includes whites born in the 48 states between 1906 and 1915 and conditional on being alive at age 35. N=5,476,138. Column 5 shows weighted estimates with state population as weights, making the sample representative of the US population in 1940. The sample size here falls somewhat because there are a few missing values in the probability of matching. All estimates are from a linear regression model with Huber-White robust standard errors clustered at the state-of-birth level. *$p < .05$, **$p < .01$, ***$p < .001$.

Table 2. Education gradients in OLS and family fixed effects models.

	Full Sample	Sibling sample		Twin Sample	
	OLS	OLS	FE	OLS	FE
Panel A. Full Sample					
Education	0.40***	0.41***	0.45***	0.40***	0.44***
	(0.02)	(0.02)	(0.01)	(0.02)	(0.03)
Female	6.91***	7.01***	6.72***	6.99***	7.08***
	(0.10)	(0.10)	(0.03)	(0.14)	(0.12)
Adjusted R-squared	0.072	0.075	0.184	0.077	0.193
Observations (individuals)	5,476,138	2,296,597	2,296,597	91,282	91,282
# Families	3,613,946~	977,794	977,794	45,515	45,515
Panel B. Male					
Education	0.38***	0.42***	0.45***	0.43***	0.44***
	(0.02)	(0.02)	(0.01)	(0.03)	(0.04)
Adjusted R-squared	0.012	0.075	0.184	0.076	0.193
Observations	2,863,851	2,296,597	2,296,597	91,282	91,282
# Families	2,203,491	977,794	977,794	45,515	45,515
Panel C. Female					
Education	0.41***	0.40***	0.44***	0.36***	0.43***
	(0.01)	(0.02)	(0.01)	(0.03)	(0.04)
Adjusted R-squared	0.010	0.075	0.184	0.076	0.193
Observations	2,612,287	2,296,597	2,296,597	91,282	91,282
# Families	1,959,748	977,794	977,794	45,515	45,515
State fixed effects	Yes	Yes	Yes	Yes	Yes
Cohort fixed effects	Yes	Yes	Yes	No	No
State-specific linear trends	Yes	Yes	Yes	Yes	Yes
Weights	No	No	No	No	No

Notes: Regression sample includes whites born in the 48 states between 1906 and 1915 and conditional on being alive at age 35. The sibling sample includes only individuals that are in families with at least 2 children. The twin sample includes only siblings born within the same birth year. The sample size for the sibling and twin models is identical across sex because we report the results from models where education is interacted with sex instead of estimating the models separately, so we can include siblings of different sex, which is particularly important in the sibling sample. ~The number of families here is underestimated because some individuals (roughly 10%) do not have family identifiers.

Table 3. Estimates of the education gradient in alternative data sets, with and without truncation.

	Without Family Fixed Effects			
	CenSoc-Numident: Death year ∈[1988, 2005] (1)	Census-Tree: Death year ∈[1988, 2005] (2)	Census-Tree: Death year ∈[1941, 2005] (3)	Census-Tree: Death year ∈[1941, 2020] (4)
Panel A. Full Sample				
Education	0.0882***	0.0961***	0.3354***	0.3955***
	(0.0035)	(0.0030)	(0.0142)	(0.0152)
Female	1.1618***	1.5509***	5.9532***	6.9075***
	(0.0199)	(0.0173)	(0.1008)	(0.0949)
Observations	1,134,687	2,473,528	5,176,827	5,476,138
Adjust-R	0.1833	0.1824	0.0594	0.0720
Panel B. Male				
Education	0.0991***	0.1034***	0.3296***	0.3792***
	(0.0047)	(0.0035)	(0.0172)	(0.0184)
Observations	455,230	1,059,499	2,785,319	2,863,851
Adjust-R	0.1347	0.1594	.0102	0.0124
Panel C. Female				
Education	0.0788***	0.0888***	0.3360***	0.4101***
	(0.0036)	(0.0030)	(0.0124)	(0.0138)
Observations	679,457	1,414,029	2,391,508	2,612,287
Adjust-R	0.1671	0.1550	0.0117	0.0100

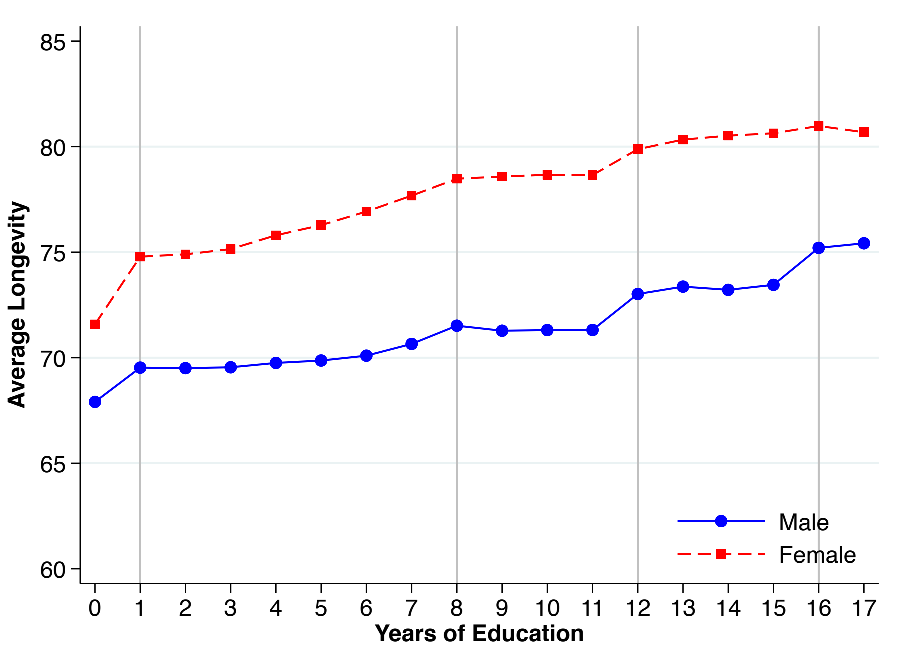
Note: Analytic samples from CenSoc-Numident and Census-Tree data include whites born 1906-1915 in the 48 states. CenSoc-Numident data only include deaths from 1988 to 2005. All regressions include state-of-birth dummies, birth cohort dummies, and state-of-birth specific time trends. Standard errors are clustered at the state-of-birth level. $^{*}p < .05$, $^{**}p < .01$, $^{***}p < .001$.

Table 4. The effect of state quality of school, average education and longevity, health resources, and per capita income on education gradients estimates with family FE.

	Male				Female			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: Quality of Schooling								
Relative Teachers' Wages	0.3241***			0.2702***	0.0171			0.0794
	(0.0573)			(0.0747)	(0.0559)			(0.0844)
Length of Term		0.0039***		0.0000		0.0001		-0.0016
		(0.0007)		(0.0011)		(0.0007)		(0.0013)
Pupil Teacher Ratio			-0.0085***	-0.0066**			-0.0018	-0.0039
			(0.0018)	(0.0021)			(0.0016)	(0.0024)
Adjusted R^2	0.3978	0.4225	0.3029	0.5589	-0.0197	-0.0215	0.0041	-0.0058
P value of joint significance				<0.001				0.4444
N	48	48	48	48	48	48	48	48
Panel B: Average years of education								
Average education	0.091***				0.042			
	(0.009)				(0.014)			
Adjusted R^2	0.7010				-0.0197			
N	48				48			
Panel C: Average Longevity								
Average longevity	0.070***				0.004			
	(0.010)				(0.016)			
Adjusted R^2	0.5304				-0.0202			
N	48				48			
Panel D: Public Health								
Child mortality	-0.0013**			-0.0008*	-0.0003			-0.0002
	(0.0004)			(0.0003)	(0.0003)			(0.0003)
Number of Physicians		0.1887***		0.0826		0.0600		0.0762
		(0.0491)		(0.0478)		(0.0411)		(0.0478)
Number of Nurses			0.2619***	0.1876**			-0.0039	-0.0544
			(0.0500)	(0.0537)			(0.0475)	(0.0543)
Adjusted R^2	0.1666	0.2269	0.3601	0.4519	-0.0063	0.0237	-0.0216	0.0076
P value of joint significance				<0.001				0.3513
N	48	48	48	48	48	48	48	48
Panel E: Economy								
Per Capita Income	0.4541***				0.0099			
	(0.0628)				(0.0689)			
Adjusted R^2	0.5219				-0.0213			
N	48				48			

Notes: The dependent variable in this table is the education coefficient from a regression of longevity on education estimated separately by state. Estimates are from a weighted OLS regression of education gradients on state-level measures, with the weights equal to the inverse of standard errors of estimated gradients. We computed state-specific child mortality (the number of deaths per 1000 live births) as the fraction of children that died among the number of children women ages 16-45 ever had based on the 1910 full census. We also calculated the average state-specific number of physicians/surgeons and nurses per 1000 population based on the occupation variable using the 1910 and 1920 full census. State-level per capita income is the average per capita income between 1900 and 1920. Analytic samples from Census-Tree data include whites born 1906-1915 in the 48 states. *$p < .05$, **$p < .01$, ***$p < .001$.

Panel A. Education and mean longevity



Panel B. Non-parametric estimates of the education-longevity relationship

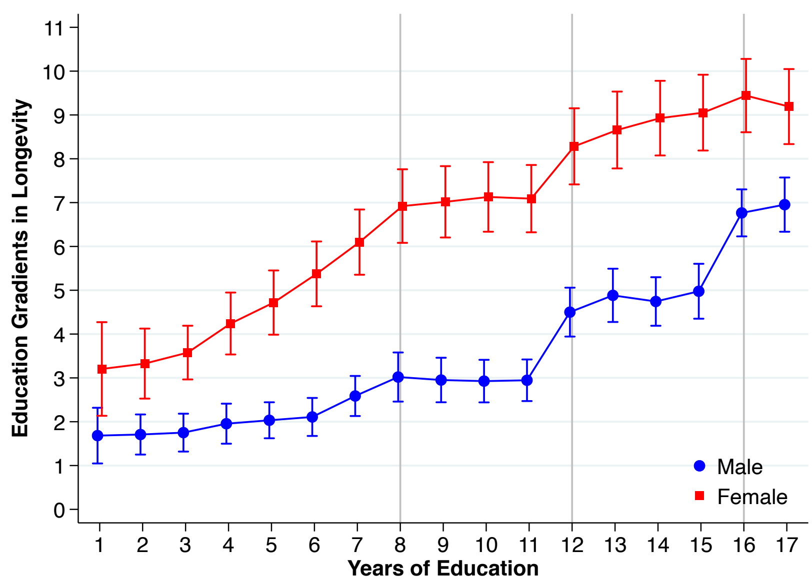


Figure 1. Longevity for the 1906-1915 birth cohort by sex and by educational levels.
Notes: Panel A sample includes only whites born 1906-1915 in the 48 states of the US and survived to age 35. See text for details. N=5,476,138. Panel B reports the coefficients from a regression of age at death on dummies for each single year of school, controlling for state of birth dummies, year of birth dummies, and state of birth specific linear trend. The excluded category is 0 years of school. The sample includes only whites born 1906-1915 in the 48 states of the US who survived to age 35. N=5,476,138.

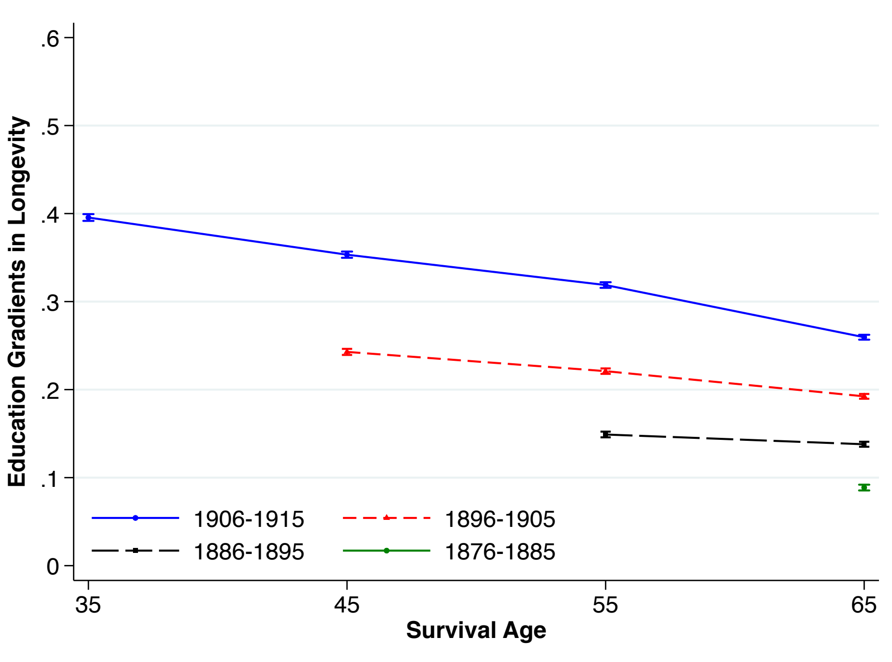
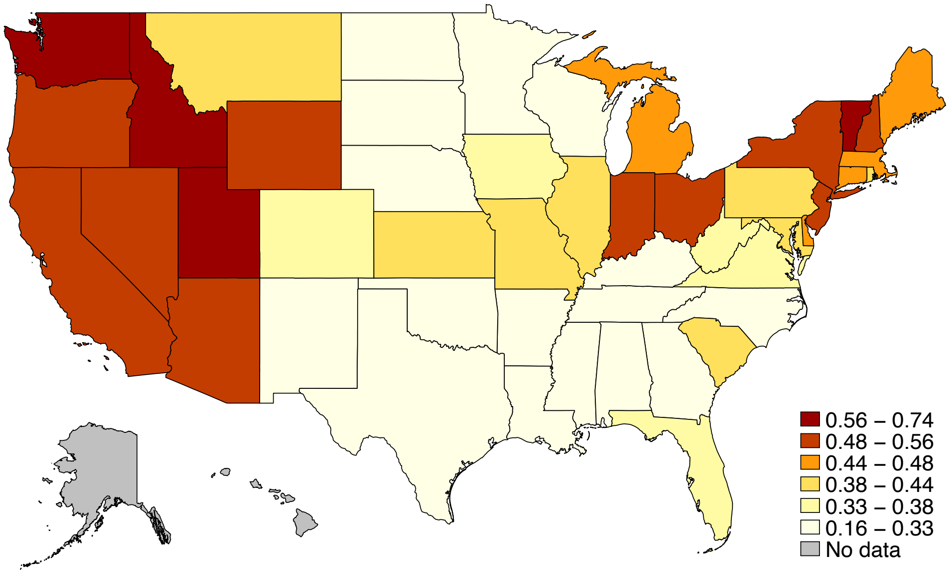


Figure 2. Associations by age and birth cohort, across ten-year birth cohorts, conditional on being alive at age 35, 45, 55, and 65.

Notes: The figure shows the coefficient of education and its corresponding 95% confidence intervals from regressions controlling for sex, birth cohort dummies, state of birth dummies, and state of birth specific time trends. Each estimate comes from a separate regression without family fixed effects. We always restrict the sample so that all the individuals in a given cohort are restricted to have survived to the same age.

Panel A. MALES



Panel B. FEMALES

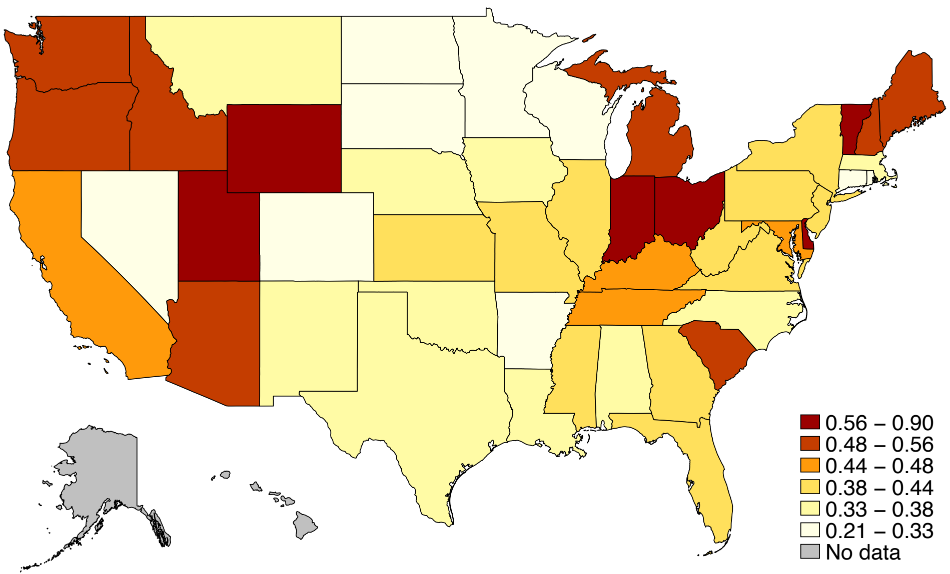
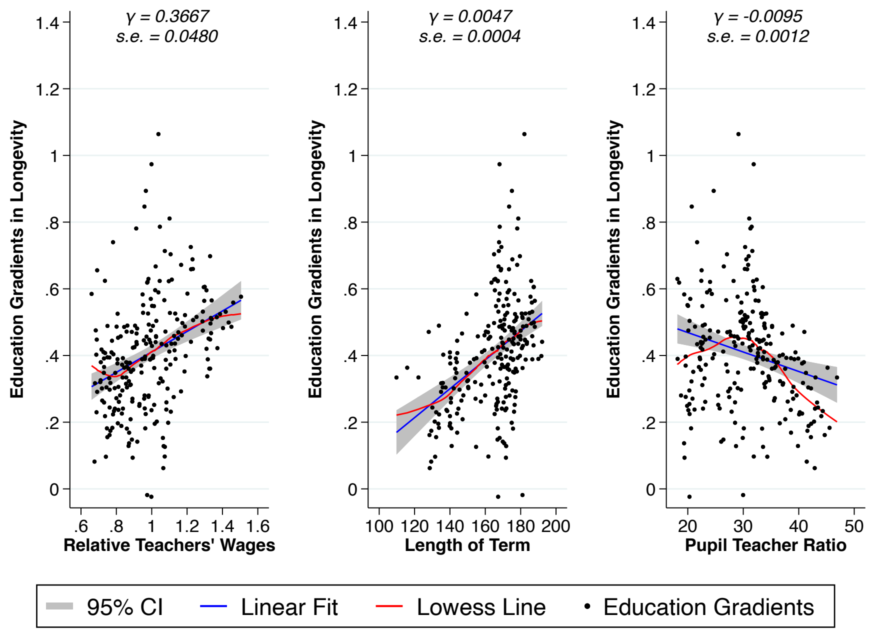


Figure 3. Education gradients by state-of-birth, for the 1906-1915 cohort who were alive at 35.

Notes: The figure shows the association between education and longevity for each state. Specifically, we estimate a regression of longevity on years of education controlling for birth cohort and sex. The figure shows the coefficients for education for each state and sex. These coefficients are statistically significant for each state, and they are different from each other. Sample includes only whites born 1906-1915 in the 48 states of the US and survived to age 35. See text for details. N=5,476,138.

Panel A. For MALES



Panel B. For FEMALES

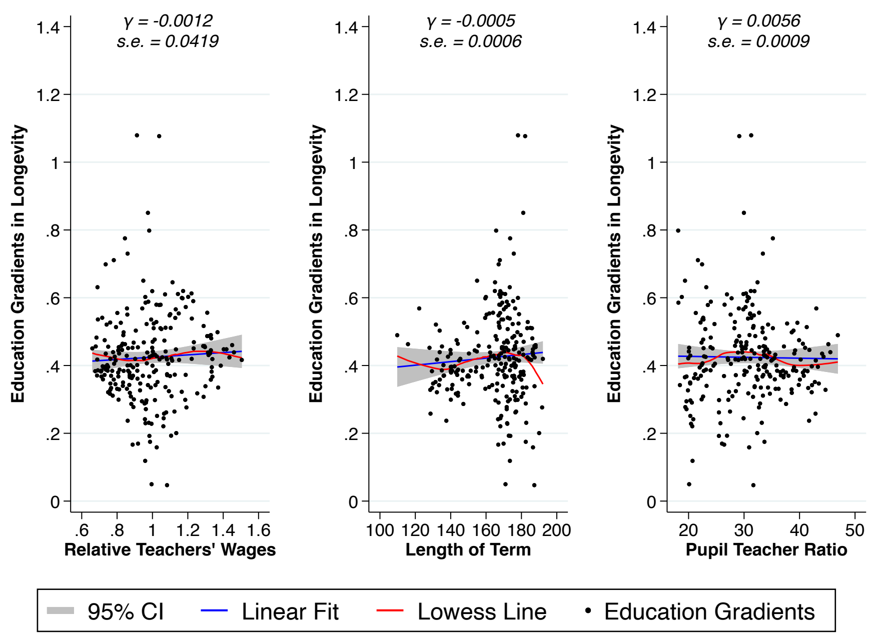
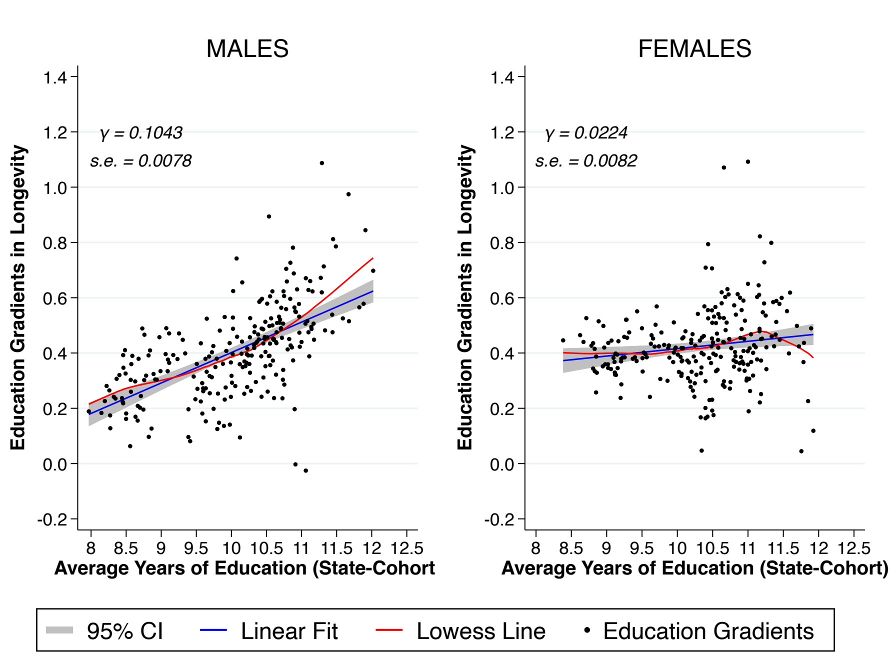


Figure 4. Education gradients and state-level quality of schooling.

Notes: Sample includes only whites born 1906-1915 in the 48 states of the US and survived to age 35. A point in the figure shows returns to education for specific state*cohort cell (48 states by 5 birth cohort groups with two birth cohorts per group). Coefficients (γ) and standard errors (*s.e.*) are from a weighted OLS regression of education gradients on quality measures, with the weights equal to the inverse of standard errors of estimated gradients. The lowess (locally weighted scatterplot smoothing) line provides a smooth function based on non-parametric techniques.

Panel A. By education levels



Panel B. By longevity levels

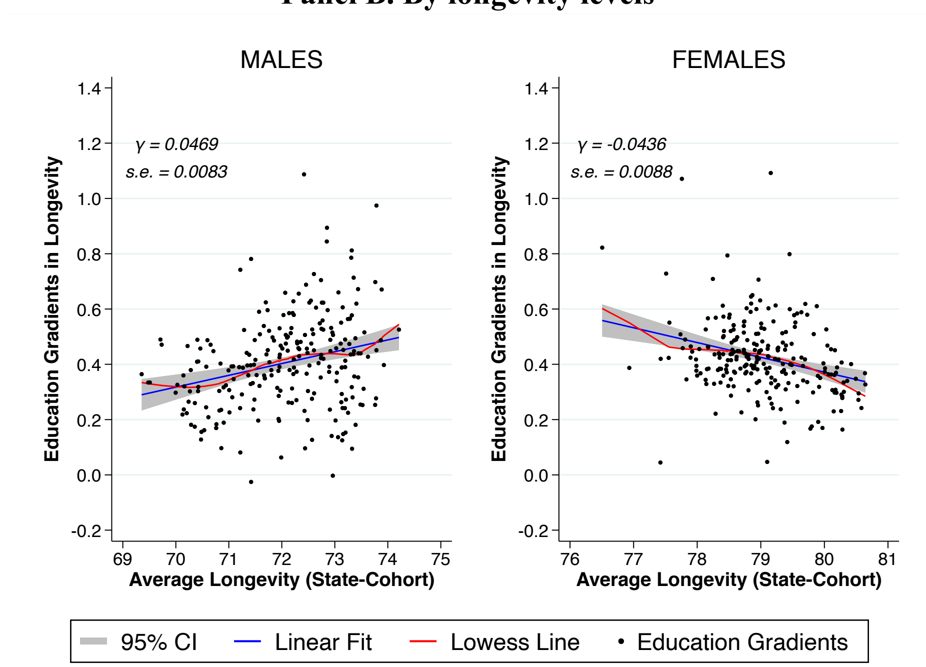
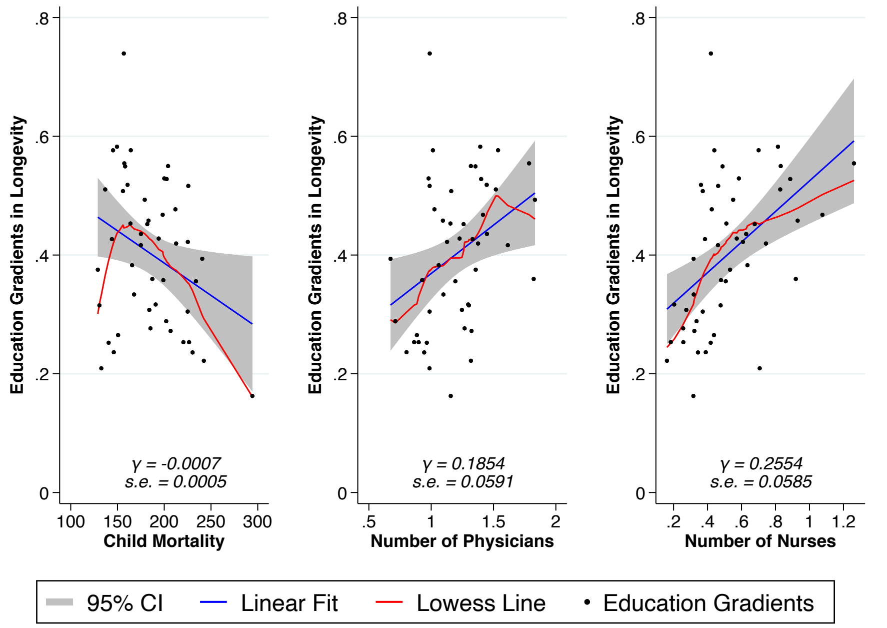


Figure 5. Returns to education on longevity by level of education and longevity.
Notes: Estimates by state of birth and cohort. Each point denotes returns to education for specific state*cohort cell (48 states by 5 birth cohort groups with two birth cohorts per group). Sample includes only whites born 1906-1915 in the 48 states of the US and survived to age 35. Coefficients (γ) and standard errors (*s.e.*) are from a weighted OLS regression of education gradients on state-cohort level average education or longevity, with the weights are equal to the inverse of standard errors of estimated gradients. The lowess (locally weighted scatterplot smoothing) line provides a smooth function based on non-parametric techniques.

Panel A. For MALES



Panel B. For FEMALES

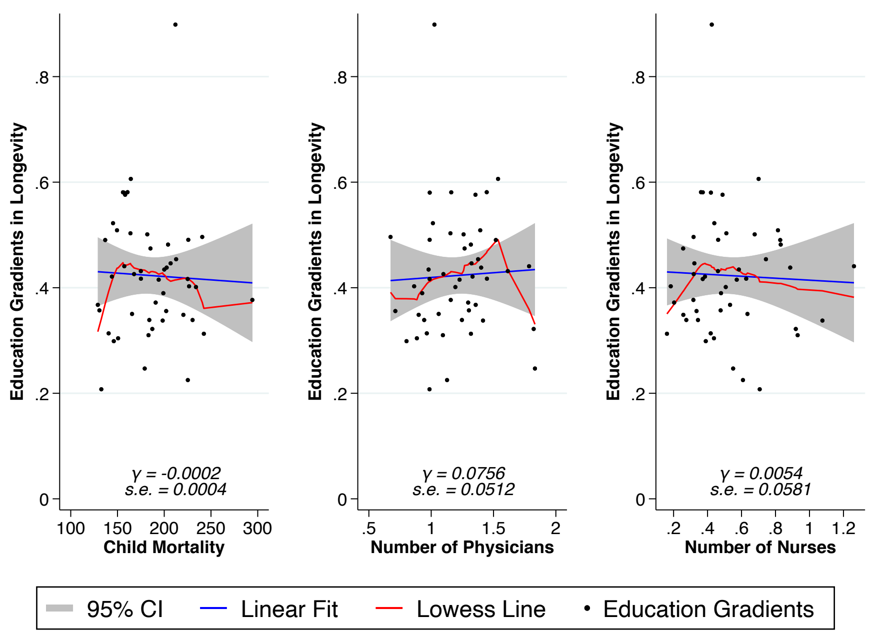


Figure 6. Education gradients and state-level proxies of health resources.

Notes: Sample includes only whites born 1906-1915 in the 48 states of the US and survived to age 35. A point in the figure shows the state-level coefficient of education. Coefficients (γ) and standard errors (*s.e.*) are from a weighted OLS regression of education gradients on health measures, with the weights are equal to the inverse of standard errors of estimated gradients. The lowess (locally weighted scatterplot smoothing) line provides a smooth function based on non-parametric techniques.

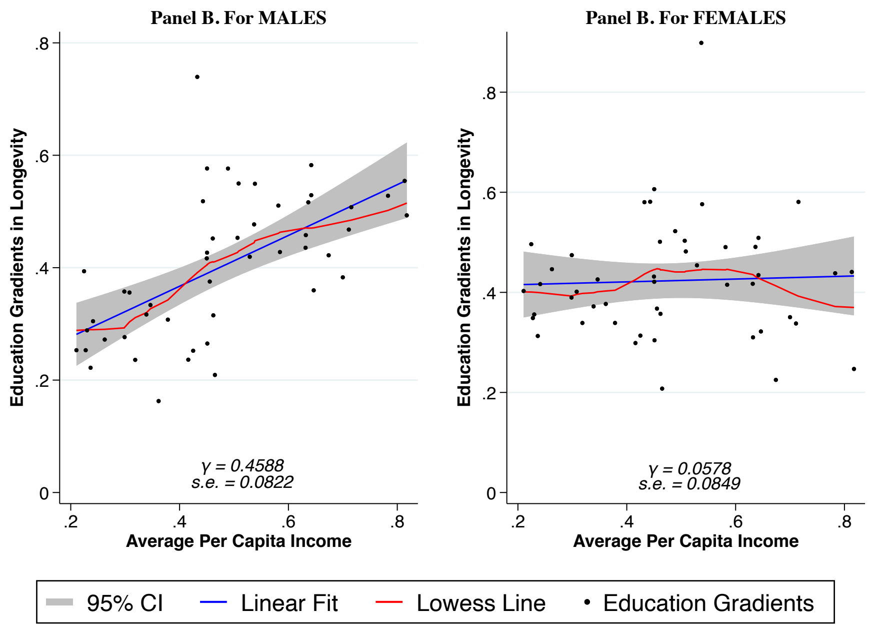


Figure 7. Education gradients and per capita income.

Notes: Sample from the Census-Tree data for estimating longevity returns includes only whites born 1906-1915 in the 48 states of the US and survived to age 35. State-level per capita income is the average per capita income between 1900 and 1920. A point in the figure shows the state-level coefficient of education plotted against per capita income. Coefficients (γ) and standard errors (*s.e.*) are from a weighted OLS regression of education gradients on state-level per capita income, with the weights equal to the inverse of standard errors of estimated gradients. The lowess (locally weighted scatterplot smoothing) line provides a smooth function based on non-parametric techniques.

Appendix

Section 1. Data construction for analyses with the city of birth variables

Census-Tree data with city of birth. Our census-tree-linked data with detailed place of birth information includes 2,403,217 people born from 1906 to 1915. We first included 2,394,727 (99.65%) individuals born in the US. After dropping records without the state of birth, we kept 2,388,226 records. We further obtained 1,302,454 records containing the city of birth information; many of these cities are rather small.

Data source for city-level school of quality measures. We rely on the work by Schmick and Shertzer (2019) who collected school quality measures for public schools for a balanced panel of 385 cities from 1900 to 1930 based on the Report of the Commissioner of Education (1900-1916) and the Biennial Survey of Education (1918-1930). We use their data to construct 2 measures that mirror the measures we use at the state level as closely as possible. First, we constructed "Expenditures per pupil" as total expenditures on teachers, supervisors, capital, and other expenditures divided by the average daily attendance in a school. Second, we use pupil teacher ratios, as reported by Schmick and Shertzer (2019). We then average these measures across the years when an individual was 6-17 years old.

Analytic sample for analyses with city-level school of quality measures. We first merge our census-tree dataset with city of birth to city codes from the 1940 IPUMS Census codebook for cities. It yields 387,629 individuals from 1,049 cities left in the dataset. Next, we matched this data to city-level school quality measures described above using the city code in census codebook (which was also used by Schmick and Shertzer, 2019). We obtained 289,723 individuals from 379 cities that can be matched to the city school quality measures.

We then merged the linked data on 289,723 individuals from 379 cities with city-level quality of schooling measures into our primary analytic sample (whites born in the 48 states between 1906 and 1915 and conditional on being alive at age 35. N=5,476,138) in the main text; we obtained 277,824 records from 379 cities. We further dropped 28 cities that have fewer than 100 observations in the sample. Finally, we included **276,142 people from 351 cities** in the analyses with city-level school of quality measures.

Data source for city-level sanitation and health expenditures. We collected data from various years of the Financial Characteristics of Cities (Ager et al., 2020; Curran, 2022; Hoehn-Velasco, 2018; Hoehn-Velasco and Wrigley-Field, 2022; Tabellini, 2020); the publication reported data for cities with a population larger than 30,000 from 1905 (with 154 cities) to 1930 (with 310 cities). We focused on per capita sanitation expenses and per capita health expenses. Our two final measures are computed as the average expenditure from one year before birth and 5 years after birth to capture the full childhood environment.

Analytic sample for analyses with city-level sanitation and health expenditures. We first merged our census-tree data for 387,629 individuals from 1,049 cities with the codebook to cities with available data on financial characteristics. We have 282,324 records from 307 cities matched. We further dropped several years (1906, 1908, 1919, 1922, and 1926) of data from Curran (2022) database for reasons like some years did not separate health and sanitation expenses. It leads to 261,306 people from 226 cities left in the data.

We then merged the data on 261,306 people from 226 cities with city-level quality of schooling measures into our primary analytic sample (whites born in the 48 states between 1906 and 1915 and conditional on being alive at age 35. N=5,476,138) in the manuscript, we obtained 250,357 individuals from 226 cities. We further dropped 28 cities that have fewer than 100 observations in the sample. Finally, we included **249,715 people from 215 cities** in the analyses with city-level expenses on health and sanitation.

Table A1. Summary statistics for whites born in the 48 states.

	Full Sample	Siblings Sample	Twins Sample	Male	Female
Panel A. Birth cohorts 1876-1915 surviving to age 35					
Age at death	75.55(12.89)			72.95(12.41)	78.55(12.78)
Years of education	9.21(3.25)			9.09(3.36)	9.36(3.12)
Year of birth	1898			1898	1899
Year of death	1974			1971	1977
Region (%)					
Northeast	17.00			17.25	16.70
Midwest	41.74			41.88	41.59
South	34.43			34.15	34.76
West	6.83			6.72	6.96
Observations	17,554,584			9,388,556	8,166,028
Panel B. Birth cohorts 1906-1915 surviving to age 35					
Longevity	75.29(14.06)	75.57(14.01)	75.41(13.99)	71.96(13.67)	78.95(13.55)
Years of education	10.03(3.06)	9.77(2.98)	9.47(3.08)	9.91(3.17)	10.15(2.94)
Year of birth	1910	1910	1910	1910	1910
Year of death	1986	1986	1986	1982	1989
Region (%)					
Northeast	16.76	13.90	13.87	17.03	16.48
Midwest	38.48	38.48	35.08	38.76	38.16
South	35.52	37.10	40.19	34.98	36.12
West	9.24	10.51	10.87	9.23	9.25
Observations	5,476,138	2,296,597	91,282	2,863,851	2,612,287
Panel C. State-level measures					
Relative Teachers' Wages	1.00	0.98	0.98	1.00	1.00
Length of Term	163.72	162.71	161.44	163.96	163.47
Pupil Teacher Ratio	32.18	32.09	32.55	32.12	32.24
Infant Mortality per 1000 live births	180.00	178.76	180.66	179.80	180.21
Number of Physicians per 1000 population	1.23	1.21	1.20	1.23	1.23
Number of Nurses per 1000 population	0.50	0.48	0.48	0.50	0.49
Per Capita Income (Thousands in 1929 US dollars)	0.46	0.45	0.44	0.46	0.46
Observations	5,476,138	2,296,597	91,282	2,863,851	2,612,287

Note: Descriptive statistics were calculated based on whites born in the 48 states and conditional on being alive at age 35. Table reports means. In parentheses are standard deviations for selected variables. We computed state specific child mortality (the number of deaths per 1000 live births) as the fraction of children that died among the number of children women ages 16-45 ever had based on the 1910 full census. We also calculated the average state-specific number of physicians/surgeons and nurses per 1000 population based on the occupation variable using the 1910 and 1920 full census. State-level per capita income is the average per capita income between 1900 and 1920.

Table A2. Descriptive statistics of 1906-1915 analytic sample in comparison to the 1940 full-count census.

	Census-Tree linked data						1940 full-count data					
	ALL		Male		Female		ALL		Male		Female	
Year of birth	N	Edu	%	Edu	%	Edu	N	Edu	%	Edu	%	Edu
1906	564,127	9.74	52.44	9.60	47.56	9.88	1,710,279	9.56	49.87	9.44	50.13	9.68
1907	552,526	9.85	52.17	9.71	47.83	9.99	1,691,639	9.68	49.70	9.56	50.30	9.79
1908	589,586	9.89	51.84	9.77	48.16	10.02	1,888,340	9.74	49.28	9.63	50.72	9.85
1909	529,470	10.03	52.29	9.92	47.71	10.16	1,687,964	9.91	49.72	9.81	50.28	10.01
1910	577,721	10.00	51.65	9.87	48.35	10.14	1,993,185	9.86	48.81	9.72	51.19	9.99
1911	537,529	10.04	52.53	9.92	47.47	10.17	1,852,255	9.94	49.66	9.83	50.34	10.05
1912	537,793	10.07	52.36	9.96	47.64	10.19	1,934,501	9.98	49.10	9.87	50.90	10.08
1913	527,406	10.16	52.85	10.04	47.15	10.28	1,907,268	10.08	49.53	9.97	50.47	10.19
1914	529,104	10.23	52.51	10.12	47.49	10.34	1,955,931	10.16	49.00	10.06	51.00	10.25
1915	530,876	10.31	52.43	10.23	47.57	10.39	2,007,208	10.24	48.72	10.15	51.28	10.31

Notes: To be consistent with 1940 full census, estimates from the Census-Tree data include whites born 1906-1915 in the 48 states. N=5,476,138.

Table A3. Representativeness of the sample data by state-of-birth: comparing the distribution of observations in the 1940 census and in the census-tree data for the 1906-1915 cohort.

		Census-Tree Data			1940 Full Census		
FIPS	State	All (%)	Male (%)	Female (%)	All (%)	Male (%)	Female (%)
1	Alabama	2.26	2.22	2.31	1.58	1.57	1.59
4	Arizona	0.24	0.24	0.24	0.37	0.39	0.36
5	Arkansas	2.17	2.12	2.21	1.2	1.19	1.21
6	California	2.17	2.24	2.10	5.97	6.12	5.83
8	Colorado	1.00	1.00	1.01	0.91	0.92	0.91
9	Connecticut	0.60	0.61	0.59	1.45	1.44	1.46
10	Delaware	0.03	0.03	0.04	0.2	0.2	0.2
12	Florida	0.59	0.59	0.60	1.21	1.19	1.22
13	Georgia	2.30	2.26	2.34	1.8	1.8	1.79
16	Idaho	0.84	0.82	0.87	0.43	0.46	0.41
17	Illinois	5.67	5.75	5.58	6.59	6.52	6.66
18	Indiana	3.87	3.85	3.90	2.68	2.71	2.66
19	Iowa	3.76	3.80	3.72	1.94	1.96	1.93
20	Kansas	2.45	2.44	2.46	1.36	1.35	1.36
21	Kentucky	3.45	3.40	3.51	2.05	2.07	2.02
22	Louisiana	1.57	1.56	1.59	1.36	1.36	1.35
23	Maine	0.77	0.79	0.75	0.63	0.64	0.63
24	Maryland	0.85	0.87	0.84	1.34	1.37	1.32
25	Massachusetts	1.96	2.04	1.87	3.47	3.37	3.57
26	Michigan	3.36	3.39	3.33	4.25	4.31	4.19
27	Minnesota	2.74	2.79	2.70	2.24	2.26	2.21
28	Mississippi	1.28	1.24	1.31	0.94	0.94	0.95
29	Missouri	4.14	4.12	4.15	2.9	2.86	2.93
30	Montana	0.59	0.59	0.59	0.45	0.48	0.42
31	Nebraska	2.00	2.00	2.00	1.01	1.01	1.01
32	Nevada	0.08	0.08	0.09	0.1	0.11	0.09
33	New Hampshire	0.36	0.38	0.35	0.38	0.38	0.38
34	New Jersey	1.09	1.11	1.06	3.48	3.45	3.51
35	New Mexico	0.30	0.29	0.31	0.41	0.41	0.41
36	New York	4.07	4.15	3.99	11.52	11.23	11.8
37	North Carolina	3.59	3.52	3.66	2.21	2.2	2.22
38	North Dakota	1.13	1.14	1.13	0.5	0.52	0.48
39	Ohio	5.47	5.56	5.37	5.45	5.43	5.47
40	Oklahoma	2.77	2.73	2.82	1.75	1.74	1.76
41	Oregon	0.92	0.93	0.91	0.91	0.94	0.88
42	Pennsylvania	7.13	7.16	7.11	7.94	7.94	7.94
44	Rhode Island	0.28	0.29	0.27	0.59	0.56	0.62
45	South Carolina	1.28	1.24	1.33	0.93	0.93	0.93
46	South Dakota	1.03	1.02	1.03	0.47	0.47	0.46
47	Tennessee	2.76	2.71	2.82	2.03	2.01	2.05
48	Texas	5.90	5.82	5.99	4.9	4.91	4.9
49	Utah	1.44	1.37	1.52	0.43	0.43	0.43
50	Vermont	0.49	0.49	0.48	0.27	0.28	0.26
51	Virginia	2.53	2.50	2.56	1.76	1.79	1.72
53	Washington	1.39	1.41	1.36	1.45	1.5	1.4
54	West Virginia	2.18	2.17	2.18	1.45	1.47	1.43
55	Wisconsin	2.86	2.92	2.80	2.51	2.55	2.47
56	Wyoming	0.25	0.24	0.26	0.22	0.23	0.2
	Northeast	16.76	17.03	16.48	29.73	29.29	30.17
	Midwest	38.48	38.76	38.16	31.9	31.95	31.83
	South	35.52	34.98	36.12	26.71	26.74	26.66
	West	9.24	9.23	9.25	11.65	11.99	11.34
	N	5,476,138	2,863,851	2,612,287	19,098,225	9,442,561	9,655,664

Notes: Sample includes only whites born 1906-1915 in the 48 states of the US.

Table A4. Weights regression models of predicting a successful census-family tree match.

	Limited Controls			Full Set of Controls		
	(1) All	(2) Men	(3) Women	(4) All	(5) Men	(6) Women
Dummies of education						
years of schooling=1	-0.0396***	-0.0326***	-0.0471***	-0.0620***	-0.0648***	-0.0763***
	(0.0027)	(0.0036)	(0.0041)	(0.0026)	(0.0036)	(0.0040)
years of schooling=2	-0.0280***	-0.0218***	-0.0344***	-0.0571***	-0.0600***	-0.0723***
	(0.0019)	(0.0025)	(0.0029)	(0.0019)	(0.0025)	(0.0029)
years of schooling=3	0.0008	0.0086***	-0.0079***	-0.0315***	-0.0311***	-0.0524***
	(0.0014)	(0.0019)	(0.0022)	(0.0014)	(0.0019)	(0.0022)
years of schooling=4	0.0265***	0.0284***	0.0259***	-0.0038***	-0.0088***	-0.0171***
	(0.0011)	-0.0015	-0.0017	(0.0011)	(0.0016)	(0.0017)
years of schooling=5	0.0444***	0.0459***	0.0432***	0.0155***	0.0121***	-0.0000
	(0.0011)	-0.0015	-0.0015	(0.0010)	(0.0015)	(0.0015)
years of schooling=6	0.0471***	0.0476***	0.0467***	0.0251***	0.0197***	0.0120***
	(0.0009)	-0.0013	-0.0013	(0.0009)	(0.0013)	(0.0013)
years of schooling=7	0.0685***	0.0699***	0.0667***	0.0504***	0.0445***	0.0372***
	(0.0008)	-0.0012	-0.0012	(0.0008)	(0.0012)	(0.0012)
years of schooling=8	0.0580***	0.0615***	0.0541***	0.0536***	0.0450***	0.0435***
	(0.0007)	-0.0011	-0.0011	(0.0007)	(0.0011)	(0.0011)
years of schooling=9	0.0571***	0.0604***	0.0530***	0.0554***	0.0464***	0.0464***
	(0.0008)	-0.0012	-0.0012	(0.0008)	(0.0012)	(0.0011)
years of schooling=10	0.0490***	0.0545***	0.0431***	0.0548***	0.0459***	0.0458***
	(0.0008)	-0.0011	-0.0011	(0.0008)	(0.0012)	(0.0011)
years of schooling=11	0.0638***	0.0674***	0.0598***	0.0692***	0.0596***	0.0610***
	(0.0008)	-0.0012	-0.0012	(0.0008)	(0.0012)	(0.0012)
years of schooling=12	0.0515***	0.0628***	0.0411***	0.0759***	0.0667***	0.0675***
	(0.0007)	-0.0011	-0.001	(0.0007)	(0.0011)	(0.0010)
years of schooling=13	0.0579***	0.0617***	0.0538***	0.0860***	0.0709***	0.0833***
	(0.001)	-0.0014	-0.0013	(0.0010)	(0.0014)	(0.0013)
years of schooling=14	0.0483***	0.0476***	0.0474***	0.0831***	0.0614***	0.0847***
	(0.0009)	-0.0013	-0.0013	(0.0009)	(0.0014)	(0.0013)
years of schooling=15	0.0457***	0.0516***	0.0395***	0.0887***	0.0709***	0.0891***
	(0.0011)	-0.0016	-0.0015	(0.0011)	(0.0017)	(0.0015)
years of schooling=16	0.0377***	0.0510***	0.0236***	0.0887***	0.0784***	0.0826***
	(0.0009)	-0.0012	-0.0012	(0.0009)	(0.0013)	(0.0012)
years of schooling=17	0.0288***	0.0484***	-0.0095***	0.0951***	0.0969***	0.0753***
	(0.0012)	-0.0015	-0.002	(0.0012)	(0.0016)	(0.0020)
years of schooling=18	0.0397***	0.0539***	0.0085	0.0932***	0.0935***	0.0748***
	(0.0032)	-0.0038	-0.0062	(0.0032)	(0.0038)	(0.0061)
years of schooling=19	0.0707***	0.0822***	0.0381***	0.1120***	0.1207***	0.0614***
	(0.0039)	-0.0045	-0.009	(0.0039)	(0.0044)	(0.0089)
years of schooling=20	0.0378***	0.0443***	0.0307***	0.0790***	0.0877***	0.0455***
	(0.0033)	-0.004	-0.0063	(0.0033)	(0.0040)	(0.0062)
Male	0.0376***	-	-	0.0813***	-	-
	(0.0002)	-	-	(0.0003)	-	-
Household Income	-	-	-	-0.0000***	-0.0000***	-0.0000***
	-	-	-	(0.0000)	(0.0000)	(0.0000)
Married	-	-	-	0.0681***	0.0712***	0.0546***
	-	-	-	(0.0003)	(0.0004)	(0.0004)
Occupation Score	-	-	-	-0.0009***	-0.0013***	0.0006***
	-	-	-	(0.0000)	(0.0000)	(0.0000)
Number of kids in 1940	-	-	-	0.0396***	0.0370***	0.0410***
	-	-	-	(0.0001)	(0.0001)	(0.0001)
In the labor force in 1940 or not	-	-	-	-0.0152***	0.0443***	-0.0630***
	-	-	-	(0.0004)	(0.0008)	(0.0007)
State of birth effects	Yes	Yes	Yes	Yes	Yes	Yes
Year of birth effects	Yes	Yes	Yes	Yes	Yes	Yes
State of residence effects	Yes	Yes	Yes	Yes	Yes	Yes
R-squared	0.0612	0.0604	0.0596	0.0929	0.0866	0.0983
Observations	16,808,100	8,335,542	8,472,558	16,805,284	8,334,132	8,471,152

Notes: Sample includes only whites born 1906-1915 in the 48 states of the US. The outcome is a binary variable with a successful census-tree match=1; 0 otherwise. In parentheses are standard errors clustered at the state of birth level. *$p < .05$, **$p < .01$, ***$p < .001$.

Table A5. Summary statistics for whites born 1906-1915 in the 48 states and died 1988-2005 using Berkeley's CenSoc-Numident data and our Census-Tree data.

	CenSoc-Numident		Census-Tree	
	Male	Female	Male	Female
Longevity	82.71 (5.23)	84.75 (5.32)	83.97 (5.11)	85.83 (5.25)
Min Longevity	72	72	73	73
Max Longevity	99	99	99	99
Years of education	10.79 (3.12)	10.54 (2.89)	10.25 (3.18)	10.27 (2.90)
Year of birth (%)				
1906	30.79	5.12	7.14	9.08
1907	37.46	5.75	7.72	9.47
1908	23.24	6.06	8.98	10.60
1909	8.51	6.65	8.91	9.75
1910	1.82	7.29	10.24	10.99
1911	2.16	8.22	10.39	10.18
1912	2.56	11.40	11.02	10.26
1913	3.12	12.95	11.45	9.95
1914	3.59	15.76	11.86	9.90
1915	5.42	20.79	12.29	9.83
Region (%)				
Northeast	30.79	29.42	17.25	16.11
Midwest	37.46	35.55	40.13	38.42
South	23.24	28.13	32.45	36.16
West	8.51	6.9	10.16	9.31
Observations	455,230	679,457	1,059,499	1,414,029

Notes: In parentheses are standard deviations. The CenSoc-Numident data include 6,824,036 individuals, among which 50% are females, and 93.19% are whites. Since this data only provides death records between 1988 and 2005, the mean age at death is 75.75, with a standard deviation equals to 9.07 and a range (47, 121). We first restricted the CenSoc-Numident dataset to those born 1906-1915 in 48 U.S. states (N= 1,214,847) and then merged it to the 1940 Full Census. All 1,214,847 observations from the CenSoc-Numident can be matched to the census. However, there are disagreements with respect to race between Census and CenSoc data; 94.75% (N= 1,151,073) identify themselves as white based on Census, while 86.63% (N=1,052,530) people consider themselves as white in both Census and CenSoc. To be consistent with the definition of our Census-Tree data, we restricted to whites using the self-reported race from the Census only. We further excluded those missing years of education and included 2,473,528 in the analyses.

Table A6. Death rates (per 1000) by educational attainment, United States 1960, males and females aged 45-74.

	45-54 years		55-64 years		65-74 years	
	Census-Tree	Kitagawa and Hauser	Census-Tree	Kitagawa and Hauser	Census-Tree	Kitagawa and Hauser
Males						
0-7 years	10.2	11.4	21.6	23.6	46.5	50.7
8 years	9.3	9.7	20.1	23.5	44.0	49.1
9-11 years	9.4	9.6	21.2	22.4	44.2	46.5
12 years	8.1	8.4	20.7	21.8	45.2	46.5
13+ years	7.6	7.4	18.7	18.2	41.5	46.1
Ratio (0-7 years)/(13+)	1.3	1.5	1.2	1.3	1.1	1.1
Females						
0-7 years	5.2	5.7	11.8	13.9	30.7	32.1
8 years	4.4	5.3	10.5	11.7	26.9	29.4
9-11 years	4.4	4.2	9.8	9.1	25.9	26.5
12 years	3.9	4.2	9.5	9.6	24.6	26.5
13+ years	3.5	3.7	8.4	7.9	22.5	19.9
Ratio (0-7 years)/(13+)	1.5	1.5	1.4	1.8	1.4	1.6

Notes: Death rates for the Census-Tree were calculated as the number of individuals died in 1960 divided by the number of individuals that were alive in 1960 within age groups. Death rates from Kitagawa and Hauser were taken from Table 2.8 in the Chapter 2 of Kitagawa and Hauser (1973). Death rates are not age-adjusted. Results are similar after age-adjustment, but Kitagawa and Hauser report fewer age categories for age-adjusted death rates.

Table A7. Death rates (per 1000) by educational attainment, United States 1979-1985, males and females aged 65-89.

Age and years of school completed	Males: Census-Tree	Males: Elo and Preston (1996)	Females: Census-Tree	Females: Elo and Preston (1996)
0-7 years	58.78	59.29	43.30	32.77
8 years	54.64	61.91	39.42	30.05
9-11 years	54.77	59.20	38.91	29.21
12 years	50.24	53.42	36.21	29.66
13-15 years	48.71	49.04	38.91	25.89
16+ years	43.86	40.66	36.21	23.80
Ratio (0-7 years)/(13-15)	1.21	1.21	1.11	1.27
Ratio (0-7 years)/(16+)	1.34	1.46	1.20	1.38
Sample size	2,986,604	25,270	3,886,330	35,231

Notes: Age-adjusted death rates for the Census-Tree were calculated from age-specific death rates between 1979-1985 with the U.S. population by age groups—estimated based on 1980 5% Census—as the standard. Age-adjusted death rates of Elo and Preston (1996), based on the public use sample of the National Longitudinal Mortality Survey, were calculated from age-specific death rates within five-year age groups with the total U.S. population on 1 July 1983 as the standard. These are reported in Table 1 of Elo and Preston (1996).

Table A8. OLS of log of age at death on a continuous measure of education for the 1906-1915 birth cohort who were alive at age 35.

	Without Family Fixed Effects						Within Siblings	Within Twins
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A. Full Sample								
Education	0.0058***	0.0057***	0.0055***	0.0055***	0.0058***	0.0054***	0.0063***	0.0062**
	(0.0002)	(0.0002)	(0.0002)	(0.0002)	(0.0003)	(0.0002)	(0.0001)	(0.0005)
Female	0.0942***	0.0942***	0.0943***	0.0943***	0.0917***	0.0950***	0.0947***	0.0968***
	(0.0014)	(0.0014)	(0.0014)	(0.0014)	(0.0018)	(0.0014)	(0.0003)	(0.0018)
Observations (individuals)	5,476,138	5,476,138	5,476,138	5,476,138	5,476,138	5,473,945	2,296,597	91,282
# Families							977,794	45,515
Adjust-R	.0604	.0606	.0621	.0622	0.0582	0.0623	0.0632	0.0419
Panel B. Male								
Education	0.0059***	0.0058***	0.0053***	0.0053***	0.0059***	0.0051***	0.0064***	0.0064***
	(0.0002)	(0.0002)	(0.0003)	(0.0003)	(0.0003)	(0.0003)	(0.0001)	(0.0005)
Observations (individuals)	2,863,851	2,863,851	2,863,851	2,863,851	2,863,851	2,862,783	2,296,597	91,282
# Families							977,794	45,515
Adjust-R	.0081	.0084	.0108	.0109	0.0103	0.0103	0.0632	0.0419
Panel C. Female								
Education	0.0056***	0.0056***	0.0056***	0.0056***	0.0057***	0.0056***	0.0062***	0.0060***
	(0.0002)	(0.0002)	(0.0002)	(0.0002)	(0.0002)	(0.0002)	(0.0001)	(0.0006)
Observations (individuals)	2,612,287	2,612,287	2,612,287	2,612,287	2,612,287	2,611,162	2,296,597	91,282
# Families							977,794	45,515
Adjust-R	.0073	.0074	.0089	.0090	0.0084	0.0092	0.0632	0.0419
State fixed effects	No	No	Yes	Yes	Yes	Yes	Yes	Yes
Cohort fixed effects	No	Yes	Yes	Yes	Yes	Yes	Yes	No
State-specific linear trends	No	No	No	Yes	Yes	Yes	Yes	Yes
Weights	No	No	No	No	1940 state population size	Inverse probability of Census-Tree matching	No	No

Notes: Sample includes only whites born 1906-1915 in the 48 states of the US and survived to age 35. Standard errors are clustered at the state-of-birth level. *$p < .05$, **$p < .01$, ***$p < .001$.

Table A9. Variations used in identification.

	(1)	(2)	(3)	(4)	(5)
	Raw	Cohort FE	Cohort and state FE	Cohort and state FE + state specific trends	Cohort and state FE + state specific trends +family FE
Panel A: Full Sample					
SD on education/residuals	3.063	3.059	2.950	2.949	NA
Observations (individuals)	5,476,138				
Panel B: Siblings Sample					
SD on education/residuals	2.983	2.979	2.870	2.870	1.386
No. Families	977,794				
Observations (individuals)	2,296,597				
Panel C: Twins Sample					
SD on education/residuals	3.082	3.075	2.930	2.929	1.309
No. Families	45,515				
Observations (individuals)	91,282				

Notes: Column 1 shows standard deviation (SD) of education in the raw data. Column 2 shows the standard deviation of residual education after regressing education on cohort dummies. Column 3 shows the standard deviation of residual education after regressing education on cohort and state-of-birth fixed effects. Column 4 shows the standard deviation of residual education after regressing education on cohort and state-of-birth fixed effects, and state of birth specific linear trends.Column 5 shows the standard deviation of residual education after regressing education on cohort and state-of-birth fixed effects, state of birth specific linear trends, and family fixed effects.

Table A10. The association between education and longevity, by family size.

	Full sample			Males			Females		
Family Size	N	OLS	FE	N	OLS	FE	N	OLS	FE
1	2,636,152	0.412		1,407,626	0.397		1,228,526	0.427	
		(0.016)			(0.020)			(0.013)	
2	1,422,454	0.429	0.457	1,422,454	0.429	0.457	1,422,454	0.407	0.447
		(0.020)	(0.009)		(0.020)	(0.009)		(0.017)	(0.010)
3	613,176	0.432	0.445	613,176	0.432	0.445	613,176	0.408	0.436
		(0.022)	(0.012)		(0.022)	(0.012)		(0.015)	(0.014)
4	205,632	0.439	0.444	205,632	0.439	0.444	205,632	0.423	0.447
		(0.024)	(0.020)		(0.024)	(0.020)		(0.025)	(0.022)
5	47,050	0.405	0.441	47,050	0.405	0.441	47,050	0.385	0.384
		(0.049)	(0.042)		(0.049)	(0.042)		(0.052)	(0.046)
6	8,285	0.292	0.328	8,285	0.292	0.328	8,285	0.333	0.322
		(0.089)	(0.102)		(0.089)	(0.102)		(0.085)	(0.110)

Notes: shown are OLS estimates and Family FE estimates of the association between education and longevity, by family size. All regressions include state-of-birth dummies, birth cohort dummies, and state-of-birth specific time trends. Standard errors are clustered at the state-of-birth level.

Table A11. Estimates of the education gradient with and without truncation.

	Census-Tree: Death year ∈[1941, 2020]	Census-Tree: Death year ∈[1973, 2013]
	(1)	(2)
Panel A. OLS		
Education	0.38	0.28
	(0.02)	(0.01)
Observations (individuals)	2,863,851	2,170,305
Panel B. Siblings FE		
Education	0.43	0.28
	(0.01)	(0.01)
Observations (individuals)	799,676	504,670
# Families	367,380	235,425
Panel C. Twins FE		
Education	0.46	0.20
	(0.06)	(0.05)
Observations (individuals)	26,108	15,801
# Families	13,033	7,888

Notes: Analytic samples from the Census-Tree data include white males born 1906-1915 in the 48 states. All regressions include state-of-birth dummies, birth cohort dummies, and state-of-birth specific time trends. Standard errors are clustered at the state-of-birth level. Column 2 truncate death years to 1973-2013 like Halpern-Manners et al (2020).

Table A12a. Estimates from a non-parametric model.

	Males		Females	
	beta	95% CI	beta	95% CI
Years of Schooling				
(0 = reference)				
1	1.68	(1.05, 2.32)	3.20	(2.14, 4.27)
2	1.71	(1.25, 2.17)	3.33	(2.53, 4.13)
3	1.75	(1.32, 2.18)	3.58	(2.96, 4.19)
4	1.95	(1.50, 2.41)	4.24	(3.53, 4.95)
5	2.03	(1.62, 2.44)	4.72	(3.99, 5.45)
6	2.11	(1.67, 2.54)	5.37	(4.63, 6.11)
7	2.59	(2.13, 3.04)	6.10	(5.35, 6.84)
8	3.02	(2.46, 3.58)	6.92	(6.08, 7.76)
9	2.95	(2.44, 3.46)	7.02	(6.20, 7.83)
10	2.93	(2.44, 3.41)	7.13	(6.34, 7.92)
11	2.95	(2.47, 3.42)	7.09	(6.32, 7.86)
12	4.50	(3.94, 5.06)	8.28	(7.42, 9.15)
13	4.88	(4.28, 5.49)	8.66	(7.78, 9.53)
14	4.74	(4.19, 5.30)	8.93	(8.08, 9.78)
15	4.98	(4.35, 5.60)	9.05	(8.19, 9.92)
16	6.77	(6.23, 7.30)	9.44	(8.61, 10.28)
17+	6.95	(6.33, 7.57)	9.19	(8.34, 10.05)
State of birth	Yes		Yes	
Year of birth	Yes		Yes	
State-of-birth specific linear trend	Yes		Yes	
Constant	280.65	(277.56, 283.74)	192.65	(189.47, 195.83)
Observations	2,932,417		2,668,435	
Adjusted R^2	0.01		0.01	
AIC	23,069,160		21,002,847	
BIC	23,069,533		21,003,230	

Notes: CI represents the Confidence Interval. The estimates are from a regression of age at death on dummies for each single year of school, controlling for state of birth dummies, year of birth dummies and state-of-birth specific linear trend. The excluded category is 0 years of school. The sample includes only whites born 1906-1915 in the 48 states of the US who survived to age 35. Standard errors are clustered by state-of-birth.

Table A12b. Estimates from a non-parametric model, with family fixed effects.

	Males		Females	
	beta	95% CI	beta	95% CI
Years of Schooling				
(0 = reference)				
1	1.76	(0.95,2.57)	4.50	(3.43,5.56)
2	2.45	(1.84,3.06)	3.85	(3.03,4.66)
3	2.23	(1.72,2.75)	4.52	(3.84,5.2)
4	2.55	(2.08,3.03)	5.17	(4.57,5.78)
5	2.68	(2.22,3.15)	5.56	(4.98,6.14)
6	2.74	(2.29,3.19)	6.39	(5.83,6.96)
7	3.07	(2.63,3.51)	6.95	(6.4,7.51)
8	3.33	(2.89,3.77)	7.54	(6.99,8.09)
9	3.69	(3.25,4.13)	7.99	(7.44,8.55)
10	3.80	(3.36,4.24)	8.29	(7.74,8.84)
11	4.03	(3.58,4.48)	8.48	(7.92,9.03)
12	5.26	(4.82,5.70)	9.27	(8.73,9.82)
13	5.78	(5.30,6.25)	9.57	(8.99,10.14)
14	5.95	(5.48,6.42)	9.90	(9.33,10.47)
15	5.99	(5.48,6.51)	10.07	(9.46,10.67)
16	7.71	(7.24,8.17)	10.53	(9.95,11.11)
17+	7.81	(7.31,8.30)	9.88	(9.2,10.57)
State of birth	Yes		Yes	Yes
Year of birth	Yes		Yes	Yes
State-of-birth specific linear trend	Yes		Yes	Yes
	313.62	(-72.82, 700.06)	442.88	(-63.79, 949.55)
Constant				
Observations	2,296,597		2,296,597	
Adjusted R^2	-0.6078		-0.6078	
AIC	16,900,000		16,900,000	
BIC	16,900,000		16,900,000	

Notes: CI represents the Confidence Interval. The estimates are from a family fixed effect regression of age at death on dummies for each single year of school, controlling for state of birth dummies, year of birth dummies and state-of-birth specific linear trend. The excluded category is 0 years of school. The sample includes only whites born 1906-1915 in the 48 states of the US who survived to age 35.

Table A13. Fit measures for different models;
OLS, various splines and fully non-parametric model.

Models	Non-parametric	Linear	Spline
Panel A. No Family Fixed Effects			
Full Sample			
Adjusted R^2	0.0128	0.0119	0.0127
MSE	195.0914	195.2652	195.0975
AIC	44,418,660	44,423,558	44,418,830
BIC	44,419,120	44,423,923	44,419,155
Males			
Adjusted R^2	0.0135	0.0135	0.0135
MSE	184.4705	184.6754	184.4737
AIC	23,069,160	23,069,160	23,069,212
BIC	23,069,533	23,069,533	23,069,482
Females			
Adjusted R^2	0.0108	0.0108	0.0108
MSE	181.6743	181.813	181.6756
AIC	21,002,845	21,002,847	21,002,870
BIC	21,003,215	21,003,230	21,003,151
Panel B. With Family Fixed Effects			
Full Sample			
Adjusted R^2	-0.725	-0.7267	-0.7252
MSE	182.6554	182.7848	182.6465
AIC	17,061,784	17,063,940	17,061,986
BIC	17,063,314	17,065,268	17,063,402
Males			
Adjusted R^2	0.0135	-1.8278	-1.8278
MSE	182.6551	182.7841	182.6461
AIC	23,069,160	8,403,790	8,403,799
BIC	23,069,533	8,405,245	8,405,146
Females			
Adjusted R^2	0.0108	-2.1563	-2.1564
MSE	182.656	182.7855	182.6471
AIC	21,002,845	7,208,424	7,208,462
BIC	21,003,215	7,209,862	7,209,793

Notes: Analytic sample includes whites born between 1906 and 1915 in the 48 states surviving to age 35. We included dummies of exact years of education (no education as the reference) in the non-parametric model, included a continuous measure of years of education in the linear model, and used the following knots for the spline regression model: years of education=1, 7, 8, 11, 12, 15, and 16. All models control for gender (not in the stratified analyses by gender), state of birth fixed effects, year of birth fixed effects, and state of birth specific linear trends. Standard errors are clustered at the state-of-birth level. MSE stands for Mean Squared Error, which is calculated from 10-fold cross validation. AIC denotes Akaike Information Criterion, and BIC represents Bayesian Information Criterion. Based on BIC measures, the spline model is the preferred model to depict the relationship between educational attainment and longevity.

Table A14. Test for linearity for the 1906-1915 cohort.

	Male		Female	
	F-stat	P-value	F-stat	P-value
sch2 - sch1 = sch3 - sch2	0.004	0.951	0.047	0.829
sch3 - sch2 = sch4 - sch3	0.632	0.431	1.643	0.206
sch4 - sch3 = sch5 - sch4	0.637	0.429	0.697	0.408
sch5 - sch4 = sch6 - sch5	0.001	0.979	1.419	0.240
sch6 - sch5 = sch7 - sch6	25.420	<0.001	0.323	0.573
sch7 - sch6 = sch8 - sch7	0.158	0.693	0.342	0.562
sch8 - sch7 = sch9 - sch8	10.374	0.002	14.664	<0.001
sch9 - sch8 = sch10 - sch9	0.233	0.632	0.023	0.881
sch10 - sch9 = sch11 - sch10	0.452	0.505	4.819	0.033
sch11 - sch10 = sch12 - sch11	90.126	<0.001	78.967	<0.001
sch12 - sch11 = sch13 - sch12	82.996	<0.001	53.072	<0.001
sch13 - sch12 = sch14 - sch13	10.824	0.002	0.806	0.374
sch14 - sch13 = sch15 - sch14	4.781	0.034	1.144	0.290
sch15 - sch14 = sch16 - sch15	69.215	<0.001	2.315	0.135
sch16 - sch15 = sch17 - sch16	103.619	<0.001	17.265	<0.001
P-value of the joint test (linearity)	638.43	<0.001	224.89	<0.001

Notes: The estimates are F test for coefficients from a regression of age at death on dummies for each single year of school, controlling for state of birth dummies, year of birth dummies and state-of-birth specific linear trend. The excluded category is 0 years of school. The sample includes only whites born 1906-1915 in the 48 states of the US who survived to age 35. Standard errors are clustered by state-of-birth.

Table A15a: Education gradients in longevity by state of birth (no family FE). Top and bottom ten states.

Top 10 States			Bottom 10 States		
State	Education Gradients	10p-90p increases	State	Education Gradients	10p-90p increase
Panel A. Levels					
1. Utah	0.6714	4.6996	39. North Carolina	0.3206	2.5645
2. Delaware	0.6530	5.8767	40. Mississippi	0.3193	2.5540
3. Vermont	0.5905	3.5428	41. Alabama	0.2972	2.3778
4. Ohio	0.5608	3.3647	42. Louisiana	0.2832	2.8319
5. Idaho	0.5543	3.3259	43. Wisconsin	0.2821	1.9748
6. Washington	0.5529	3.8706	44. South Dakota	0.2796	1.6774
7. Indiana	0.5462	3.2772	45. North Dakota	0.2657	1.8599
8. Wyoming	0.5392	3.2351	46. New Mexico	0.2649	2.3839
9. New Hampshire	0.5195	3.1171	47. Arkansas	0.2625	1.8375
10. California	0.5071	4.0566	48. Minnesota	0.2084	1.2506
Panel B. Logs					
1. Delaware	0.0096	0.0865	39. North Carolina	0.0044	0.0354
2. Utah	0.0094	0.0658	40. Mississippi	0.0043	0.0341
3. Vermont	0.0082	0.0494	41. Alabama	0.0041	0.0325
4. Ohio	0.0079	0.0474	42. Louisiana	0.0039	0.0394
5. Wyoming	0.0078	0.0469	43. Wisconsin	0.0039	0.0271
6. Washington	0.0077	0.0542	44. New Mexico	0.0039	0.0347
7. Idaho	0.0076	0.0458	45. South Dakota	0.0038	0.0229
8. Indiana	0.0076	0.0455	46. North Dakota	0.0036	0.0252
9. New Hampshire	0.0074	0.0446	47. Arkansas	0.0036	0.0250
10. Arizona	0.0072	0.0579	48. Minnesota	0.0028	0.0167

Notes: Analytic sample includes whites born between 1806 and 1915 in the 48 states who were alive at age 35 (N=5,476,138). We estimate a linear regression model stratified by state of birth to estimate the state specific returns to education, and to report the increase in longevity when education increases from the 10th percentile to 90th percentile. Estimates of state-specific returns to education on longevity reported in the table adjust for gender, and birth cohort fixed effects.

Table A15b: Education gradients in longevity by state of birth (with family FE). Top and bottom ten states.

Top 10 States			Bottom 10 States		
State	Education Gradients	10p-90p increases	State	Education Gradients	10p-90p increase
Panel A. Levels					
1. Oregon	0.6760	4.7319	39. Mississippi	0.3966	3.1728
2. Wyoming	0.5837	3.5021	40. Minnesota	0.3882	2.3290
3. California	0.5768	4.6142	41. Alabama	0.3910	3.1283
4. Colorado	0.5895	4.1268	42. Maryland	0.3259	2.6075
5. Arizona	0.6251	5.0011	43. North Carolina	0.3606	2.8850
6. New Mexico	0.5457	4.9115	44. Connecticut	0.3730	2.6113
7. Washington	0.6100	4.2700	45. Georgia	0.3501	2.8009
8. New Hampshire	0.6306	3.7835	46. South Carolina	0.3587	3.2283
9. Delaware	0.0760	0.6840	47. West Virginia	0.2986	2.3884
10. Utah	0.5068	3.5473	48. Nevada	0.1286	1.0291
Panel B. Logs					
Oregon	0.0096	0.0677	49. Mississippi	0.0057	0.0462
Wyoming	0.009	0.0521	50. Alabama	0.0055	0.0462
Delaware	0.0089	0.0084	51. Minnesota	0.0053	0.0321
Colorado	0.0086	0.0587	52. Maryland	0.0051	0.0360
California	0.0085	0.0651	53. North Carolina	0.0051	0.0405
Arizona	0.0084	0.0706	54. Georgia	0.0050	0.0395
New Mexico	0.0081	0.0701	55. Connecticut	0.0047	0.0348
New Hampshire	0.008	0.0553	56. South Carolina	0.0045	0.0448
Washington	0.0079	0.0605	57. West Virginia	0.0044	0.0335
Vermont	0.0075	0.0423	58. Nevada	0.0041	0.0131

Notes: Analytic sample includes whites born between 1806 and 1915 in the 48 states who were alive at age 35 (N=5,476,138). We estimate a linear regression model with interactions between state of birth and education to estimate the state specific returns to education (see equation 3 in the main text). We do this to include families with children born in separate states (they would be dropped if we estimated gradients state by state). We then report the increase in longevity when education increases from the 10th percentile to 90th percentile. Estimates of state-specific returns to education on longevity reported in the table adjust for gender, and birth cohort fixed effects.

Table 16a. Schooling quality measures for all races and those for whites in seventeen southern states.

	All Race (1906-1915 birth cohort)			All Race (1910-1919 birth cohort)			White (1910-1919 birth cohorts)		
State Name	Teacher salary	Pupils/ teachers	Term length	Teacher salary	Pupils/ teachers	Term length	Teacher salary	Pupils/ teachers	Term length
ALABAMA	557.1	42.1	133.3	651.96	39.74	139.36	1482	35.4	146.9
ARKANSAS	616.6	42.0	134.2	637.12	40.38	139.70	1278	37.8	143.8
DELAWARE	1066.1	31.8	178.1	1329.08	29.48	181.45	NA	29.0	NA
FLORIDA	631.8	33.7	140.2	765.24	33.25	149.39	NA	30.4	NA
GEORGIA	526.8	42.1	142.0	610.59	39.93	143.33	1520	35.8	147.4
KENTUCKY	655.7	39.9	147.4	765.40	38.16	154.56	NA	39.2	NA
LOUISIANA	776.3	37.7	146.7	867.17	36.23	151.01	2046	31.1	170.6
MARYLAND	1094.2	35.5	181.5	1311.30	33.91	184.40	2636	32.0	186.5
MISSISSIPPI	419.3	39.0	133.6	497.34	37.77	135.57	NA	29.7	NA
MISSOURI	948.3	31.4	167.8	1093.84	29.60	171.75	NA	29.4	NA
NORTH CAROLINA	596.1	39.0	138.2	718.41	37.39	146.30	1575	34.4	148.2
OKLAHOMA	856.8	37.2	158.0	956.98	35.85	160.95	NA	35.9	NA
SOUTH CAROLINA	568.9	43.0	121.9	669.01	39.39	133.82	1740	30.8	160.1
TENNESSEE	597.1	42.7	144.0	721.99	39.44	153.15	NA	37.8	NA
TEXAS	722.5	34.6	142.3	815.82	32.64	146.24	NA	31.2	NA
VIRGINIA	632.3	34.4	154.2	746.41	33.85	160.76	1590	31.7	165
WEST VIRGINIA	831.0	28.9	150.6	989.27	27.84	159.89	1778	27.6	NA

Notes. Schooling quality measures for all races were from the data sources described in the main text. Schooling quality measures for whites born in the southern states were from the Appendix 3 of Card and Krueger (1992b).

Table A16b. Robustness checks on race-specific schooling quality measures.

	Male				Female			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: Measures for All Race								
Relative Teachers' Wages	0.3241***			0.2702***	0.0171			0.0794
	(0.0573)			(0.0747)	(0.0559)			(0.0844)
Length of Term		0.0039***		0.0000		0.0001		-0.0016
		(0.0007)		(0.0011)		(0.0007)		(0.0013)
Pupil Teacher Ratio			-0.0085***	-0.0066**			-0.0018	-0.0039
			(0.0018)	(0.0021)			(0.0016)	(0.0024)
Adjusted R^2	0.3978	0.4225	0.3029	0.5589	-0.0197	-0.0215	0.0041	-0.0058
P value of joint significance				<0.001				0.4444
N	48	48	48	48	48	48	48	48
Panel B: Drop States with more than 20% Blacks in 1910								
Relative Teachers' Wages	0.2549***			0.3338***	0.0148			0.1408
	(0.0685)			(0.0921)	(0.0705)			(0.0998)
Length of Term		0.0032*		-0.0013		-0.0006		-0.0027
		(0.0012)		(0.0016)		(0.0012)		(0.0017)
Pupil Teacher Ratio			-0.0048	-0.0072**			-0.0029	-0.0051
			(0.0028)	(0.0026)			(0.0025)	(0.0028)
Adjusted R^2	0.2575	0.1344	0.0495	0.3650	-0.0265	-0.0212	0.0097	0.0274
P value of joint significance				<0.001				0.2752
N	38	38	38	38	38	38	38	38
Panel C. Replace measures for southern states with white schools								
Length of Term		0.0040***		0.0035***		-0.0003		-0.0007
		(0.0009)		(0.0009)		(0.0008)		(0.0009)
Pupil Teacher Ratio			-0.0077**	-0.0040			-0.0021	-0.0028
			(0.0027)	(0.0026)			(0.0022)	(0.0024)
Adjusted R^2		0.3000	0.1288	0.3207		-0.0180	-0.0018	-0.0081
P value of joint significance				<0.001				0.4503
N		48	48	48		48	48	48

Notes: Panel A replicates results from Table 4 in the main text. Panel B excludes ten states with more than 20% Black residents in 1910 including Mississippi, South Carolina, Georgia, Louisiana, Alabama, Florida, Virginia, North Carolina, Arkansas, and Tennessee. Panel C replaces schooling quality measures for individuals born in southern states with data for white schools whenever they are available in Table A16a, which is based on Appendix 3 of Card and Krueger (1992b). *p < .05, **p < .01, ***p < .001

Table A17. Heterogeneity in the education-longevity gradient – no family FE.

	Male				Female			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: Quality of Schooling								
Relative Teachers' Wages	0.3667***			0.0869	-0.0012			-0.1954***
	(0.0480)			(0.0594)	(0.0419)			(0.0524)
Length of Term		0.0047***		0.0043***		-0.0005		0.0027***
		(0.0004)		(0.0008)		(0.0006)		(0.0008)
Pupil Teacher Ratio			-0.0095***	0.0003			0.0056***	0.0083***
			(0.0012)	(0.0019)			(0.0009)	(0.0011)
Adjusted R^2	0.1936	0.4046	0.2091	0.4079	-0.0042	-0.0008	0.1458	0.1930
P value of joint significance				<0.001				<0.001
N	240	240	240	240	240	240	240	240
Panel B: Average years of education								
Average education	0.1043***				0.0224**			
	(0.0078)				(0.0082)			
Adjusted R^2	0.4278				0.0262			
N	240				240			
Panel C: Average Longevity								
Average longevity	0.0469***				-0.0436***			
	(0.0083)				(0.0088)			
Adjusted R^2	0.1138				0.0887			
N	240				240			
Panel D: Public Health								
Child mortality	-0.0007			-0.0003	-0.0002			-0.0001
	(0.0005)			(0.0004)	(0.0004)			(0.0004)
Number of Physicians		0.1854**		0.0782		0.0756		0.0948
		(0.0591)		(0.0643)		(0.0512)		(0.0608)
Number of Nurses			0.2554***	0.2022**			0.0054	-0.0487
			(0.0585)	(0.0688)			(0.0581)	(0.0668)
Adjusted R^2	0.0278	0.1581	0.2779	0.2835	-0.0171	0.0245	-0.0215	-0.0076
P value of joint significance				<0.001				0.4577
N	48	48	48	48	48	48	48	48
Panel E: Economy								
Per Capita Income	0.4588***				0.0578			
	(0.0822)				(0.0849)			
Adjusted R^2	0.3907				0.0100			
N	48				48			

Note. The dependent variable in this table is the education coefficient from a regression of longevity on education estimated separately by two-year birth cohorts (panel A, B, and C) and also by state (panel D and E). The cohorts are binned into 2-year age groups (N=48*5=240 observations for the 1906-1915 cohort). Estimates are from a weighted OLS regression of education gradients on state-level measures, with the weights equal to the inverse of standard errors of estimated gradients. We computed state specific child mortality (the number of deaths per 1000 live births) as the fraction of children that died among the number of children women ages 16-45 ever had based on the 1910 full census. We also calculated the average state-specific number of physicians/surgeons and nurses per 1000 population based on the occupation variable using the 1910 and 1920 full census. State-level per capita income is the average per capita income between 1900 and 1920. Analytic samples from Census-Tree data include whites born 1906-1915 in the 48 states. *$p < .05$, **$p < .01$, ***$p < .001$.

Table A18. The Effect of compulsory schooling laws and education expenditure on education gradients estimates with family FE.

	Males					Females				
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Years of compulsory schooling required by attendance laws	0.0222**				-0.0026	0.0060				0.0072
	(0.0071)				(0.0094)	(0.0057)				(0.0106)
Years of compulsory schooling required by child labor laws		0.0378***			0.0037		0.0038			-0.0141
		(0.0101)			(0.0142)		(0.0086)			(0.0159)
Cumulative years of compulsory schooling			0.0490***		0.0010			0.0132		0.0183
			(0.0125)		(0.0165)			(0.0105)		(0.0185)
Per capita education expenditures				0.0023***	0.0023***				0.0002	-0.0001
				(0.0003)	(0.0004)				(0.0003)	(0.0005)
Adjusted R^2	0.1579	0.2181	0.2344	0.5732	0.5444	0.0021	-0.0174	0.0119	-0.0162	-0.0330
P value of joint significance				<0.001						0.65
N	48	48	48	48	48	48	48	48	48	48

Note. The dependent variable in this table is the education coefficient from a regression of longevity on education estimated with family fixed effects. Estimates are from a weighted OLS regression of education gradients (with family fixed effects) on state-level measures, with the weights equal to the inverse of standard errors of estimated gradients. Data on Years of compulsory schooling required by attendance laws, Years of compulsory schooling required by child labor laws, and Per capita education expenditures were from Lleras-Muney (2005). Data on Cumulative years of compulsory schooling were from Stephens and Yang (2014). Analytic samples from Census-Tree data include whites born 1906-1915 in the 48 states. *$p < .05$, **$p < .01$, ***$p < .001$.

Table A19. Regression results of longevity and education by gender, and by sample with available birthplace information.

	(1)	(2)	(3)
	Samples with state of birth	Samples matched to city-level school quality measures	Samples matched to city-level financial characteristics
Panel A. Full Sample			
Education	0.40***	0.49***	0.49***
	(0.02)	(0.02)	(0.02)
Female	6.91***	6.86***	6.83***
	(0.10)	(0.10)	(0.10)
Observations (individuals)	5,476,138	276,142	249,715
Adjust-R	0.0720	0.0696	0.0694
Panel B. Male			
Education	0.38***	0.54***	0.55***
	(0.02)	(0.02)	(0.02)
Observations	2,863,851	147,775	133,732
Adjust-R	0.0124	0.0194	0.0203
Panel C. Female			
Education	0.41***	0.40***	0.40***
	(0.01)	(0.02)	(0.02)
Observations	2,612,287	128,367	115,983
Adjust-R	0.0100		0.0091
Number of cities	NA	351	215
State fixed effects	Yes	Yes	Yes
Cohort fixed effects	Yes	Yes	Yes
State-specific linear trends	Yes	Yes	Yes
Weights	No	No	No

Notes: Regression sample includes whites born in the 48 states between 1906 and 1915 and conditional on being alive at age 35. N=5,476,138. Column 2 use data from Schmick and Shertzer (2019); it includes school quality measures for public schools from 385 cities. Column 3 data are from the Financial Characteristics of Cities; it includes cities with population larger than 30,000 in 1910-1930. All estimates are from a linear regression model with Huber-White robust standard errors clustered at the state-of-birth level. *$p < .05$, **$p < .01$, ***$p < .001$.

Table A20. City-level measures and education gradients estimates without family FE.

	Male			Female		
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A. City financial characteristics						
Per capita sanitation expense	0.0549		0.0506	-0.0179		0.0391
	(0.0363)		(0.0500)	(0.0415)		(0.0579)
Per capita health expense		0.1244	0.0192		-0.1637	-0.2467
		(0.1101)	(0.1515)		(0.1252)	(0.1756)
Adjusted R^2	0.0060	0.0013	0.0014	-0.0038	0.0033	0.0008
P value of joint significance			0.3184			0.3413
N (number if cities)	215	215	215	215	215	215
Panel B. City quality of schooling						
Expenditures per pupil	0.0016*		0.0017*	-0.00004		0.0001
	(0.0006)		(0.0010)	(0.0007)		(0.0008)
Pupil Teacher Ratio		-0.0035	0.0024		0.0016	0.0018
		(0.0049)	(0.0054)		(0.0055)	(0.0061)
Adjusted R^2	0.0160	-0.0014	0.0137	-0.0029	-0.0026	-0.0055
P value of joint significance			0.0335			0.9550
N (number if cities)	351	351	351	351	351	351

Note: Panel A data are from the Financial Characteristics of Cities; it includes data for cities with population larger than 30,000 from 1905 (with 154 cities) to 1930 (with 310 cities). Per capita sanitation expense and Per capita health expense were computed as the average of years ranging from one year before birth and 5 years after birth. Panel B use data from Schmick and Shertzer (2019); it includes school quality measures for public schools from 385 cities (351 cities are matched to our sample). Expenditures per pupil was computed as total expenditures on teachers, supervisors, capital, and other expenditures divided by the average daily attendance in a school. Expenditures per pupil and Pupil Teacher Ratio were calculated as the average for years when an individual was 6-17 years old. *p < .05, **p < .01, ***p < .001.

Table A21. The effect of fertility and maternal mortality on education gradients.

	Females		Males	
	OLS	Family FE	OLS	Family FE
Average number of children ever born among women ages 16-45 in 1910	-0.0865	-0.2611*	-0.1675*	0.0185
	(0.0832)	(0.1059)	(0.0814)	(0.0806)
Maternal Mortality (deaths per 1,000 live births) 1933	0.0071	-0.0008	0.0081	-0.0126
	(0.0170)	(0.0220)	(0.0159)	(0.0166)
Birth rate (number of births per 1,000 individuals) in 1933	0.0042	0.0103	-0.0061	-0.0107
	(0.0093)	(0.0117)	(0.0092)	(0.0090)
p-value of joint test	0.6141	0.0076	<0.001	0.2138
N	48	48	48	48
Adjusted R^2	-0.0257	0.1832	0.3315	0.0342

Notes: The dependent variable in this table is the education coefficient from a regression of longevity on education estimated separately by state. Estimates are from a weighted OLS regression of education gradients on state-level measures, with the weights equal to the inverse of standard errors of estimated gradients. Analytic samples from Census-Tree data include whites born 1906-1915 in the 48 states. **p* < .05, ***p* < .01, ****p* < .001.

Table A22. Regression of migration on education and gender.

	Migration
Years of schooling	0.0102***
	(0.0017)
Female	0.0200***
	(0.0052)
Female # Years of schooling	-0.0015**
	(0.0006)
State fixed effects	Yes
Cohort fixed effects	Yes
State-specific linear trends	Yes
Constant	-15.6552***
	(0.0245)
Observations	17,645,460
R-squared	0.1325
Adjusted Rsq	.1325

Notes: We use data from 1940 full census and defined migration=1 if a respondent's state of residence is different from state of birth. Regression sample include whites born in the 48 states between 1906 and 1915. All estimates are from a linear regression model with Huber-White robust standard errors clustered at the state-of-birth level. *$p < .05$, **$p < .01$, ***$p < .001$.

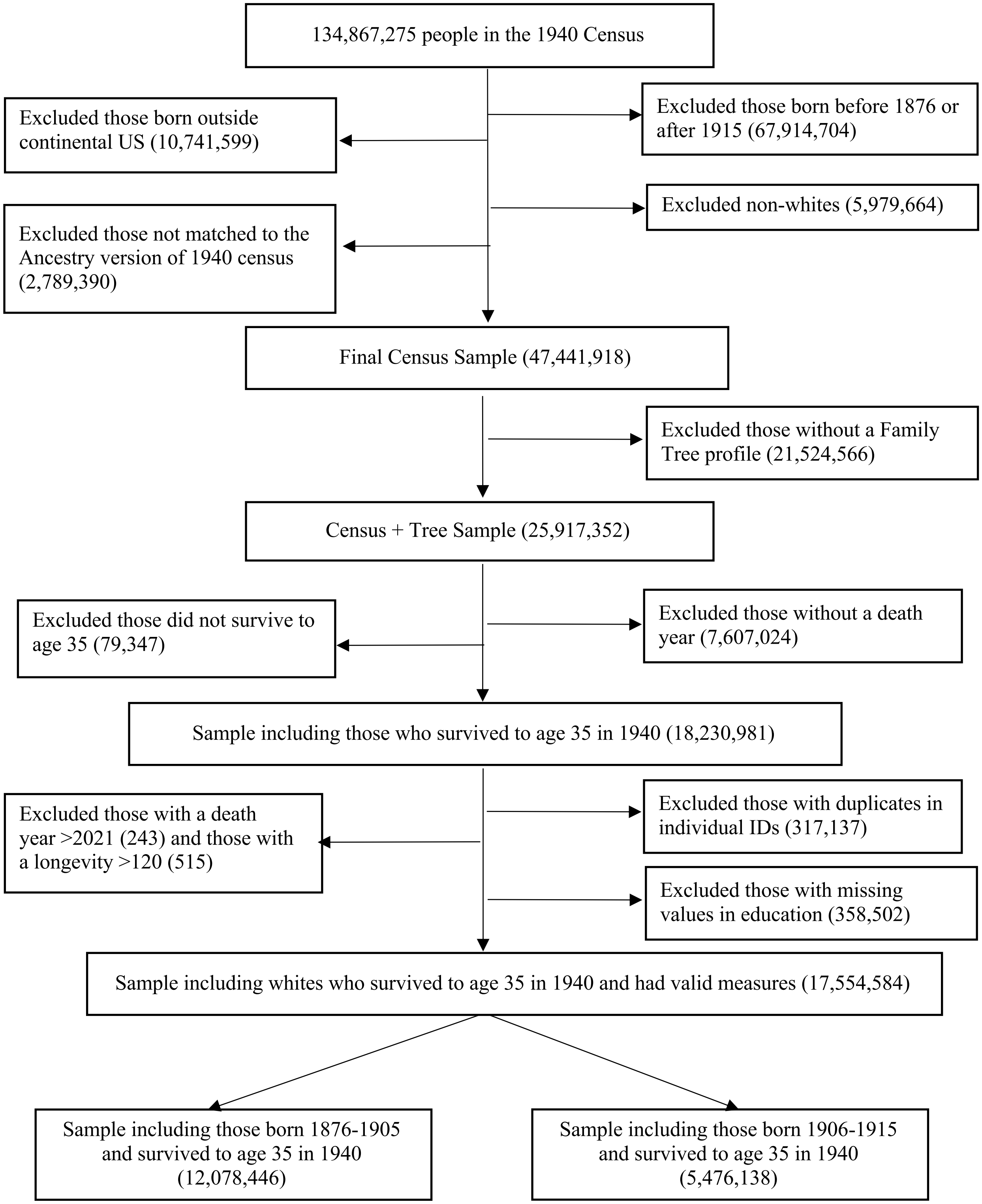


Figure A1. Sample Flowchart.
Notes: In parentheses are sample size.

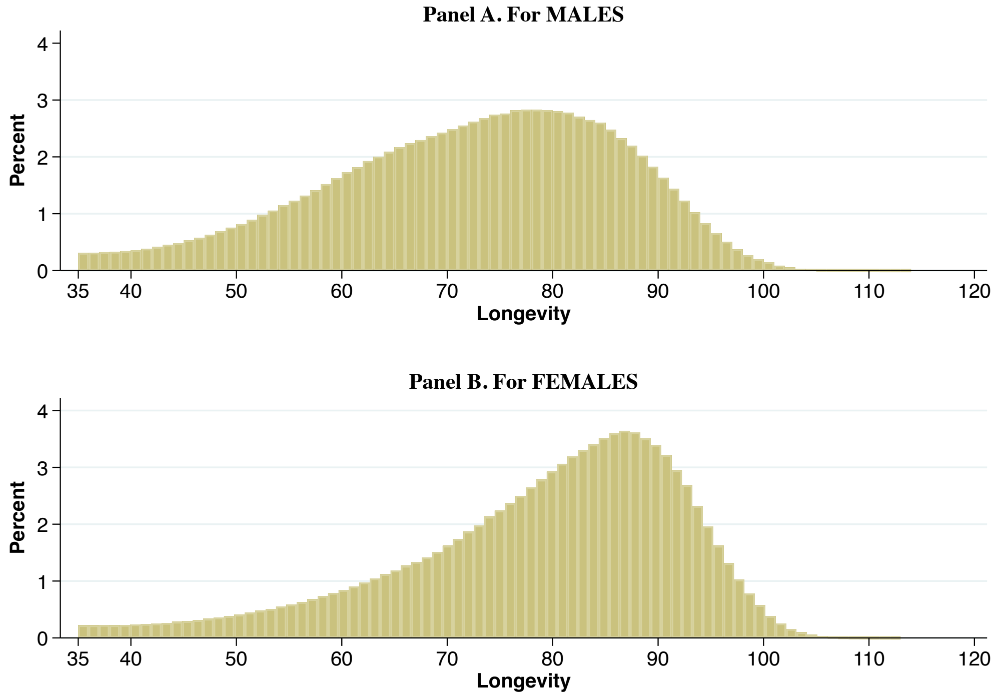


Figure A2. Distribution of longevity for the 1906-1915 birth cohort by gender.
Notes: The sample includes only whites born 1906-1915 in the 48 states of the US who survived to age 35. N=5,476,138.

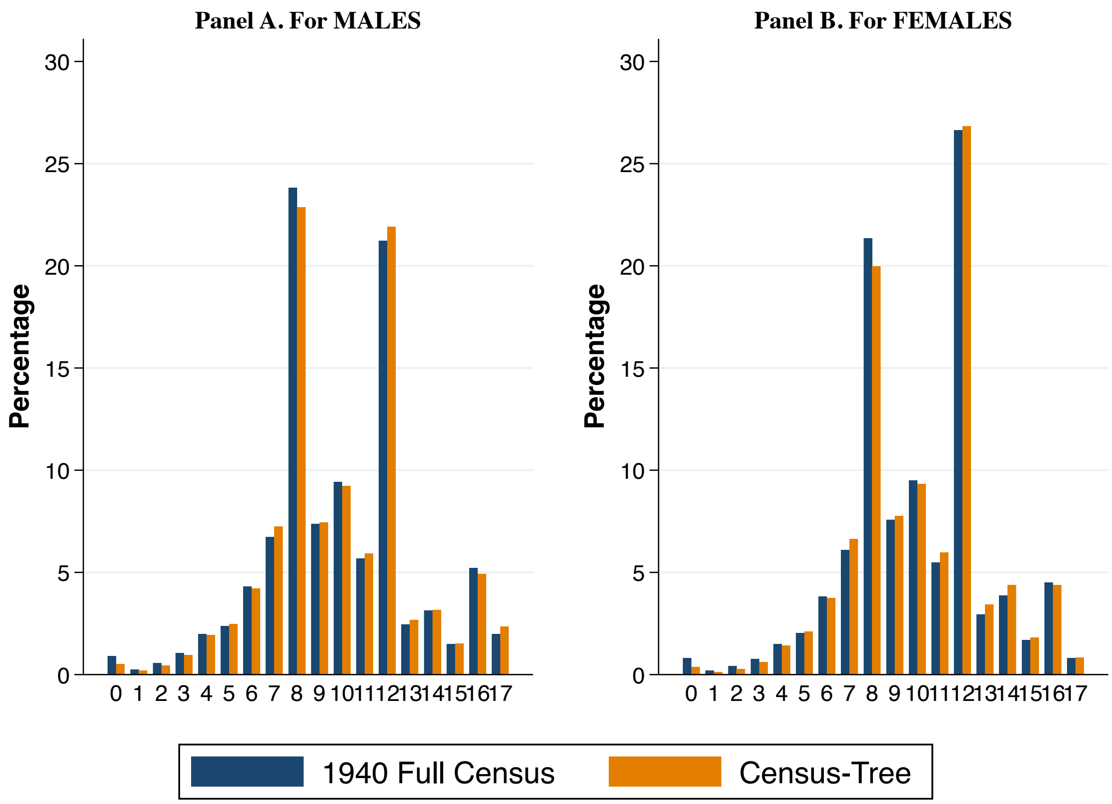


Figure A3. Distribution of years of education in the 1906-1915 birth cohort.
Notes: Matched 1940-family tree data. To be consistent with 1940 full census, estimates from the Census-Tree data include whites born 1906-1915 in the 48 states. N=5,476,138.

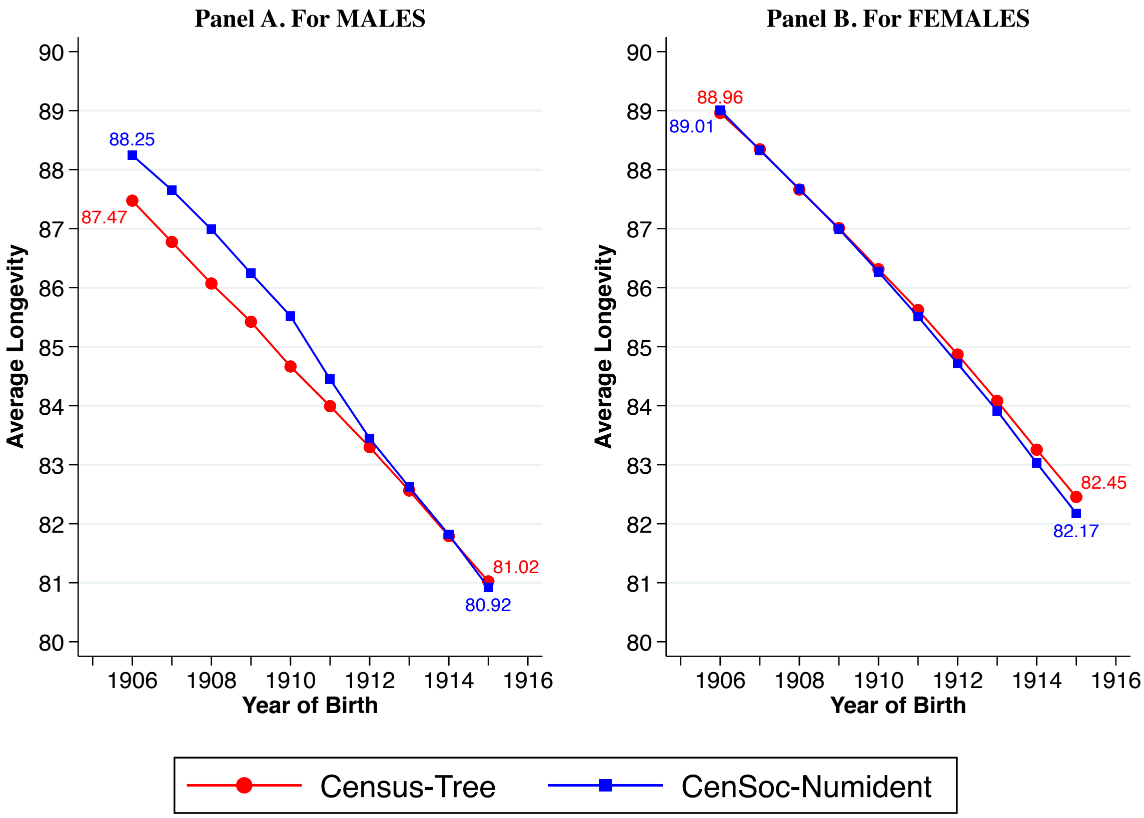


Figure A4. Restrict to whites in CenSoc-Numident based on self-reported race in the 1940 Census.

Notes: Estimates are based on CenSoc-Numident from UC Berkeley and Census-Tree data used in this study. The analytic sample include whites born 1906-1915 in the 48 states, and who died between 1988 and 2005. In the Censoc-Numident data and in our data the race is defined using the race reported in the 1940 census. There are 1,134,687 observations in CenSoc-Numident, and 627,200 observations in Census Tree. This figure shows a downwards trend in average longevity by birth cohorts. The primary reason is the survival bias. Those born in 1870 were 70 years old in the 1940 census, while those born in 1910 were only 30.

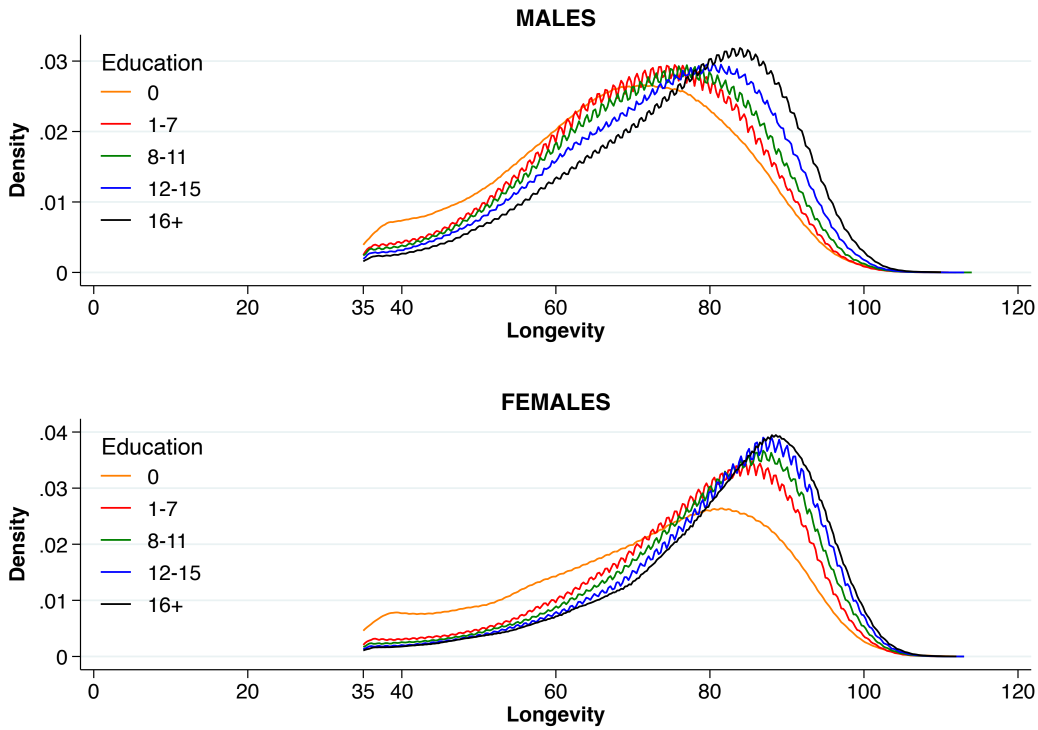


Figure A5. Panel B. Distribution of longevity for the 1906-1915 birth cohort by gender and by educational levels.

Notes: Sample includes only whites born 1906-1915 in the 48 states of the US and survived to age 35. See text for details. N=5,476,138.

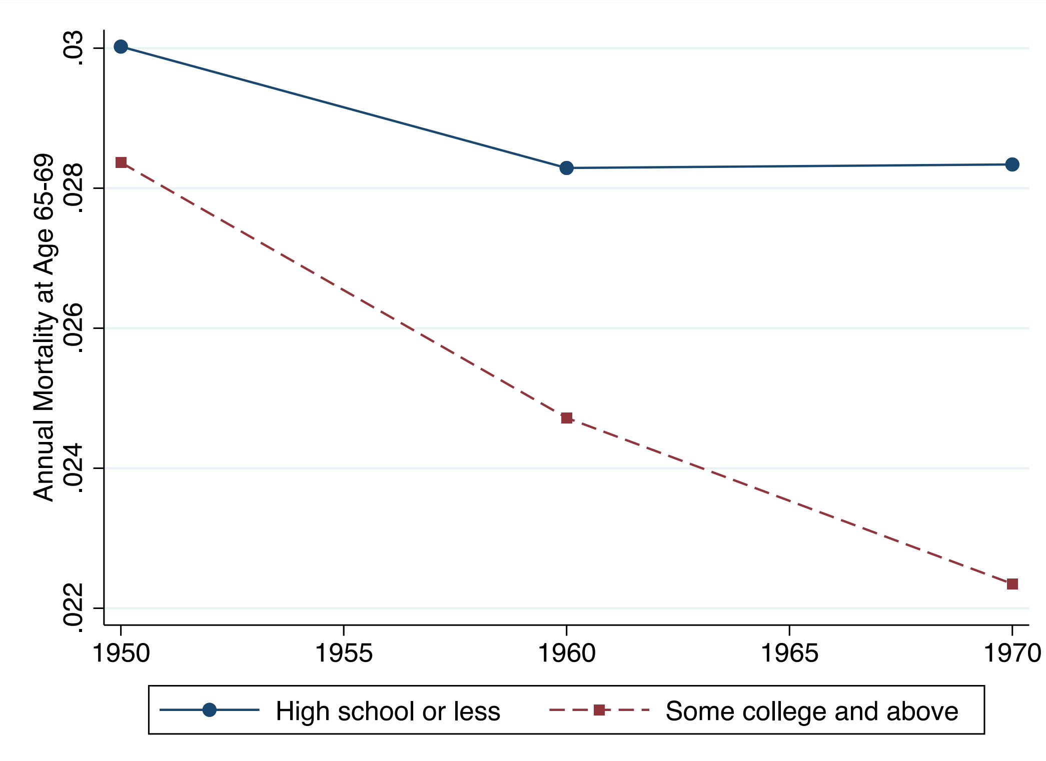


Figure A6. Annual mortality rates for 5-year age groups.
Notes: Analytic sample include whites born 1881-1885 in our Census-Tree data. Annual mortality rates for those age 65-69 in, for example, 1950 were computed as the number of those who died in 1950 divided by the number of those who are alive in 1950. We calculated the death rates separately for those with a high school or less and those with some college and above.

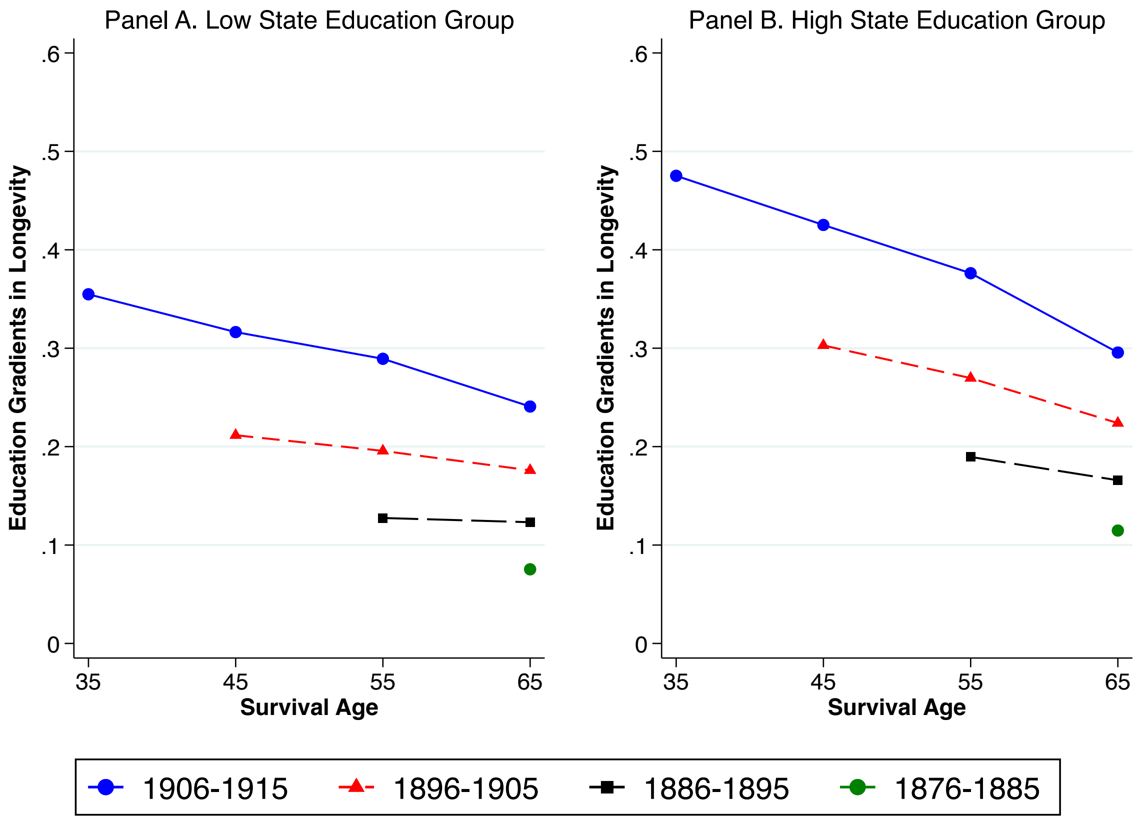


Figure A7. Associations by age and birth cohort, across ten-year birth cohorts, conditional on being alive at age 35, 45, 55, and 65, and by quartiles of state-level average years of education.

Notes: Both figures show the coefficient of education, from regressions controlling for gender, birth cohort dummies, state of birth dummies, and state of birth specific time trends. Each estimate comes from a separate regression. We always restrict the sample so that all the individuals in a given cohort are restricted to have survived to the same age. States with above-median average education are included in the high state education group.

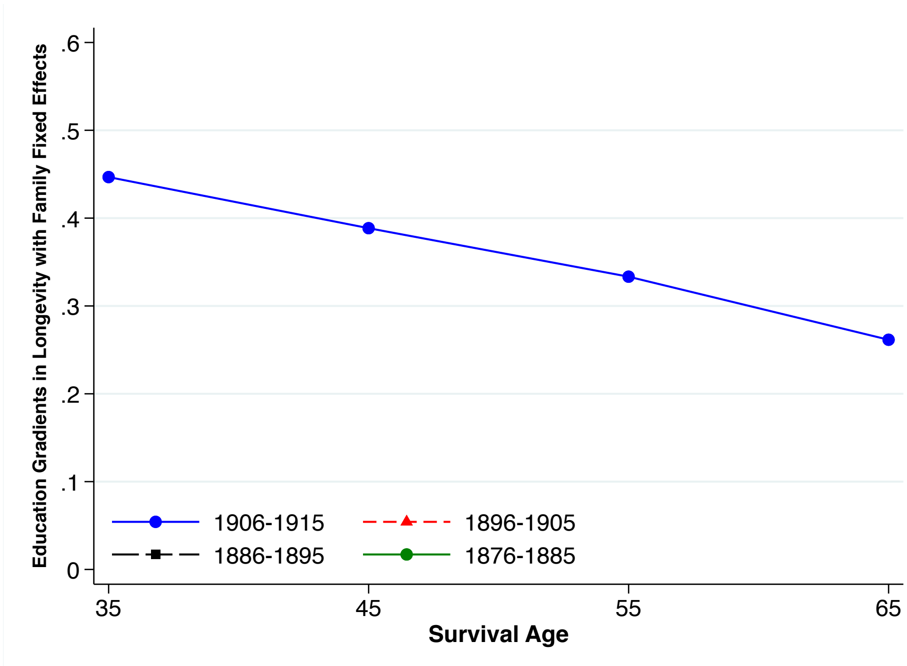


Figure A8. Associations by age and birth cohort, across ten-year birth cohorts, conditional on being alive at age 35, 45, 55, and 65.

Notes: The figure shows the coefficient of education, from regressions controlling for gender, birth cohort dummies, state of birth dummies, and state of birth specific time trends. Each estimate comes from a separate regression with family fixed effects. We always restrict the sample so that all the individuals in a given cohort are restricted to have survived to the same age. We only have family identifiers for the 1906-1915 birth cohorts.

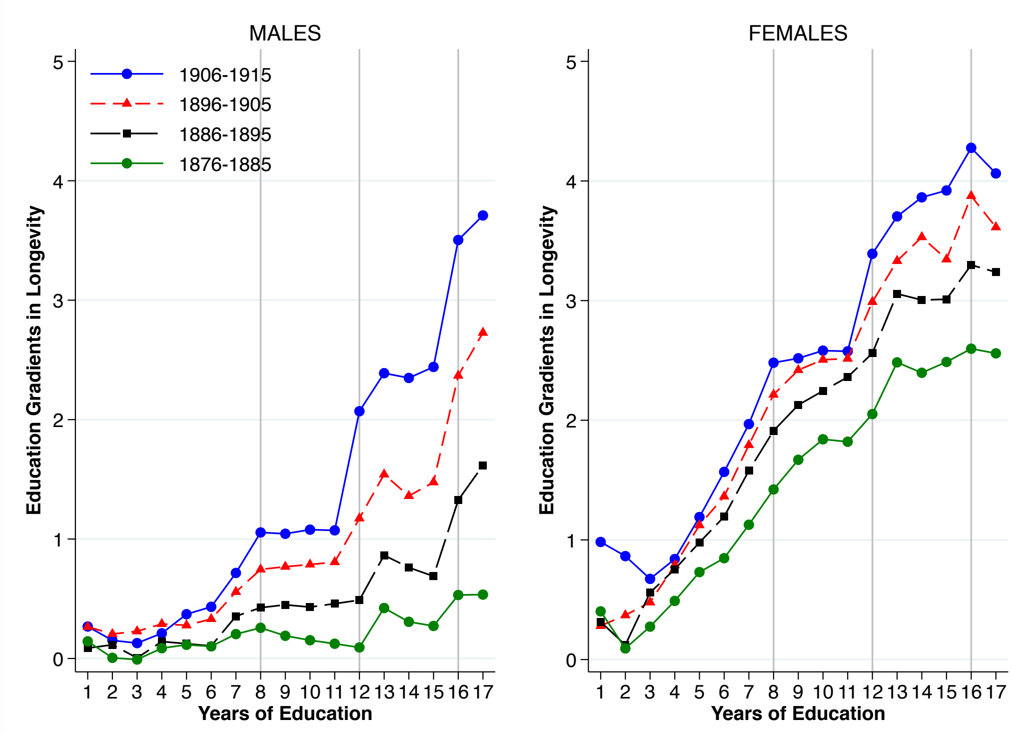


Figure A9. Education gradients by birth cohorts.
Notes: Shown are coefficients from a regression of age at death on dummies for each single year of school, controlling for state of birth dummies, year of birth dummies and state-of-birth specific linear trend. The excluded category is 0 years of school. The 1906-1915/1986-1905/1886-1895/1876-1885 samples include only whites born in the 48 states of the US who survived to age 65.

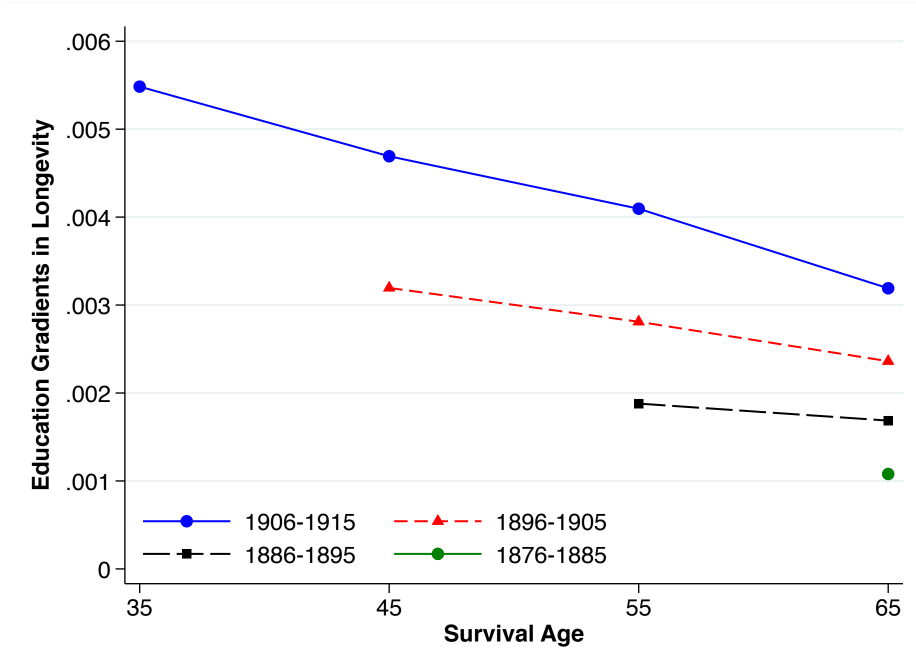
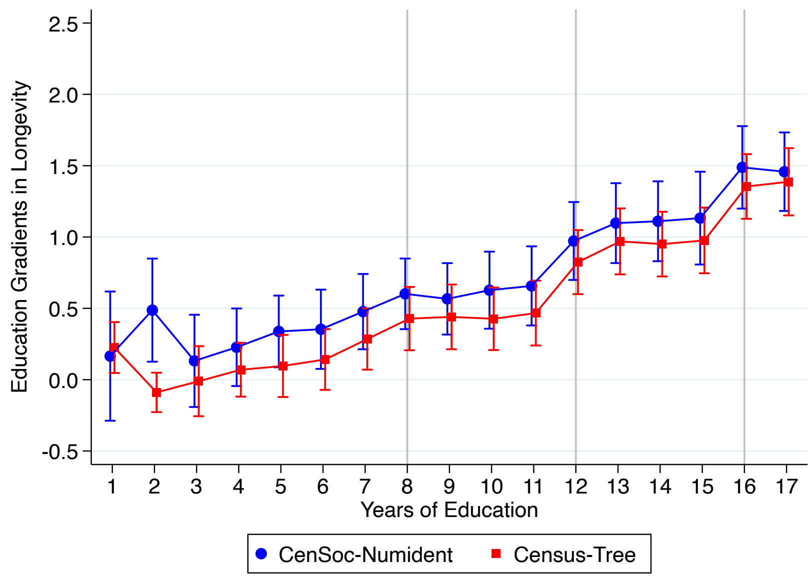


Figure A10. Associations by age and birth cohort, across ten-year birth cohorts, conditional on being alive at age 35, 45, 55, and 65.

Notes: The figure shows the coefficient of education, from regressions of log transformed longevity on gender, birth cohort dummies, state of birth dummies, and state of birth specific time trends. We always restrict the sample so that all the individuals in a given cohort are restricted to have survived to the same age.

Panel A. For MALES



Panel B. For FEMALES

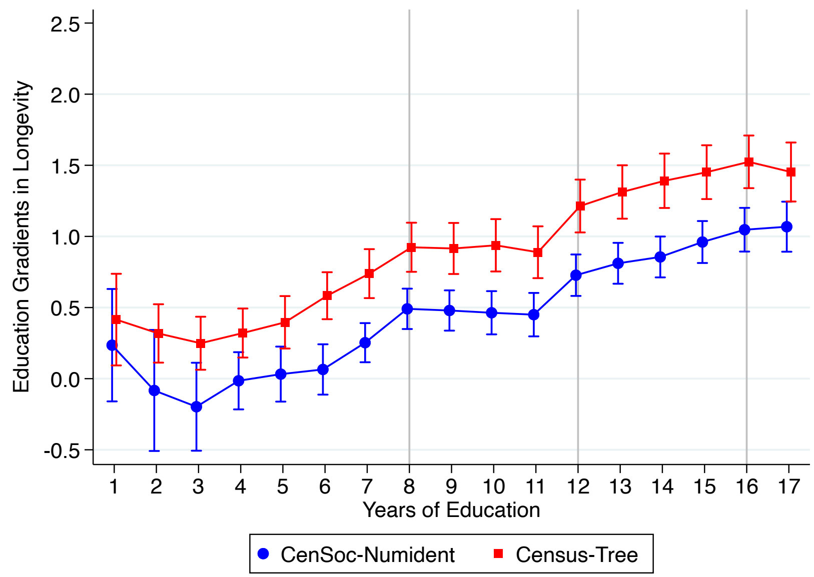


Figure A11. Education gradients in longevity using CenSoc-Numident and Census-Tree Data.
Notes: Analytic samples from CenSoc-Numident and Census-Tree data include whites born 1906-1915 in the 48 states and died 1988-2005. All models include a complete set of dummies for exact years of education (zero year of education as the reference) and adjust for state-of-birth fixed effects, year-of-birth fixed effects, and state-of-birth specific linear trends

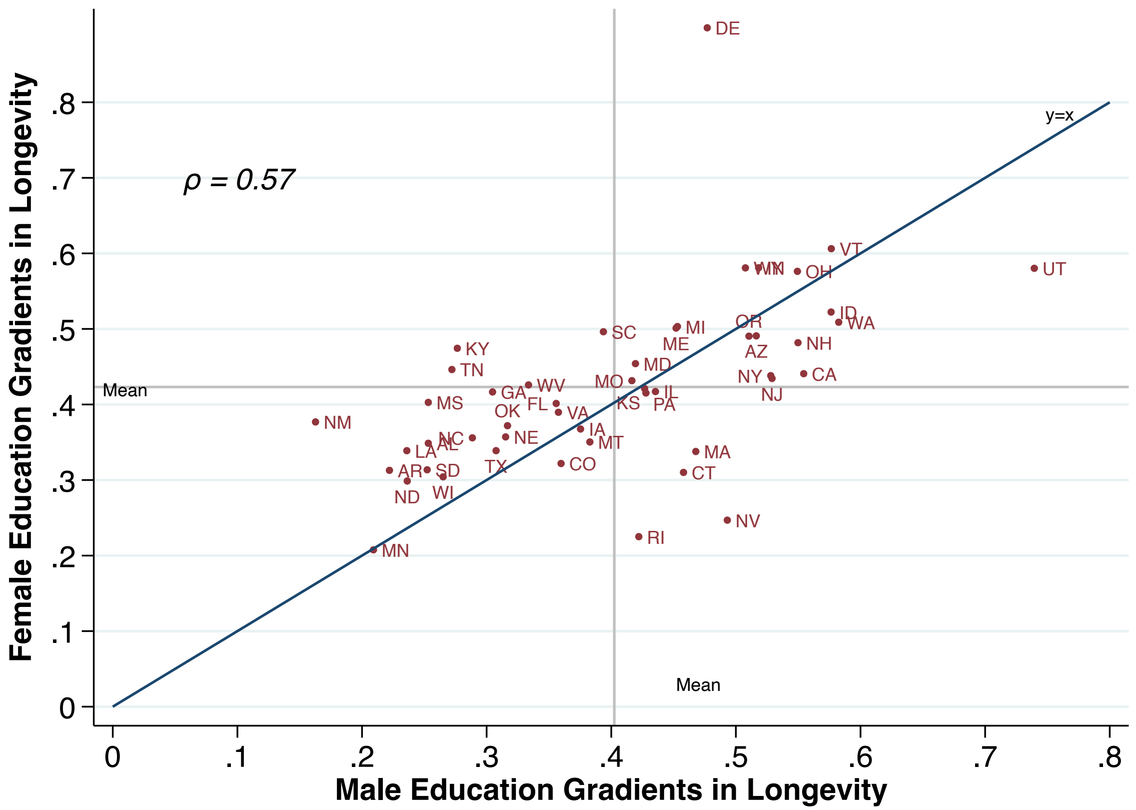


Figure A12. Correlation of female and male returns to education by state for the 1906-1915 cohort.
Notes: Matched 1940-family tree data. Sample includes only whites born 1906-1915 in the 48 states of the US and survived to age 35. See text for details. N=1,362,469. Coefficients for males and for females estimated separately for each state. Regressions control for year of birth. The correlation (ρ) between male and female coefficients is 0.26.

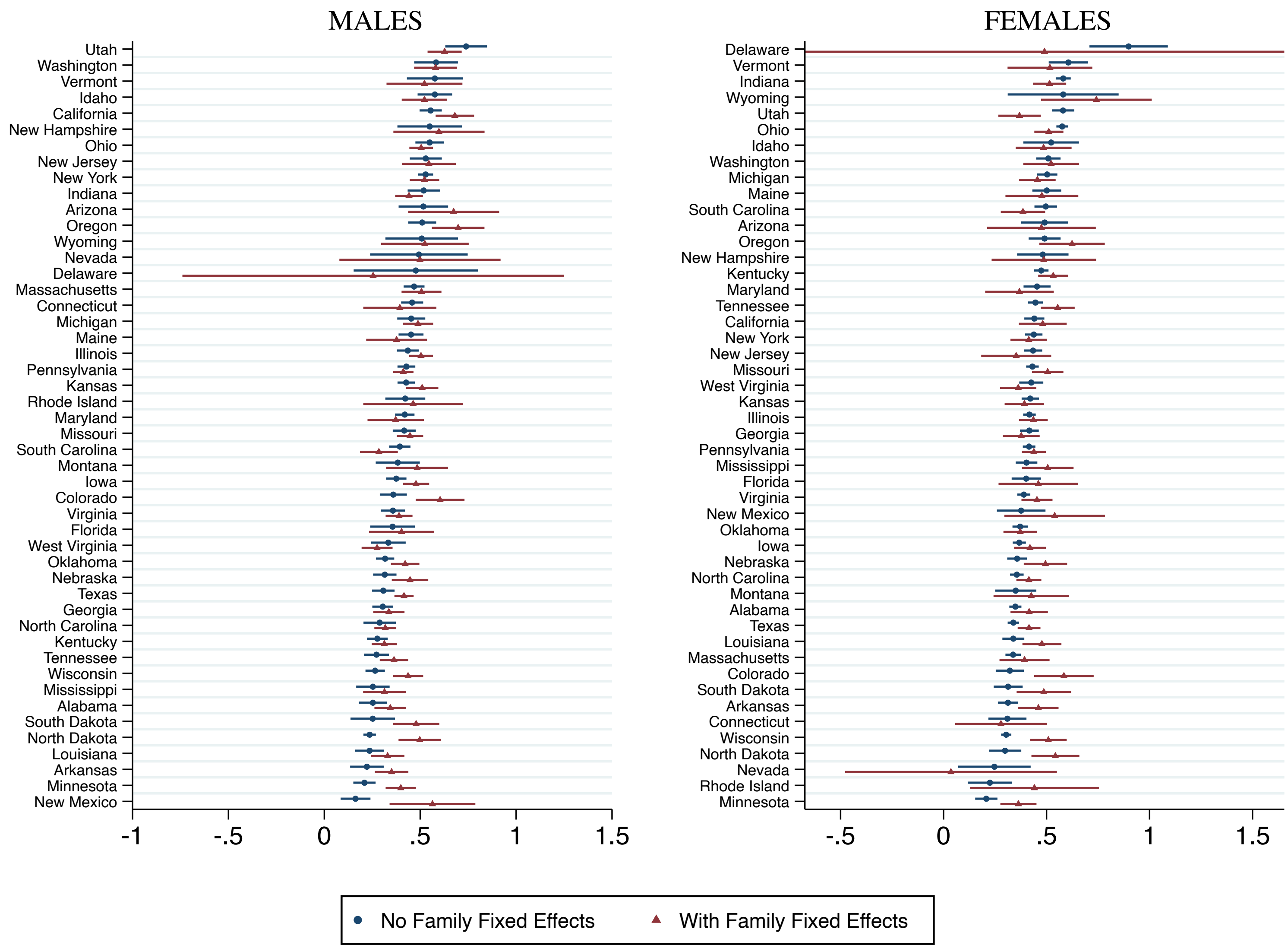


Figure A13. Education gradient in longevity by gender and state of birth.

Notes: The sample includes only whites born 1906-1915 in the 48 states of the US who survived to age 35. The horizontal lines correspond to 95% confidence intervals.

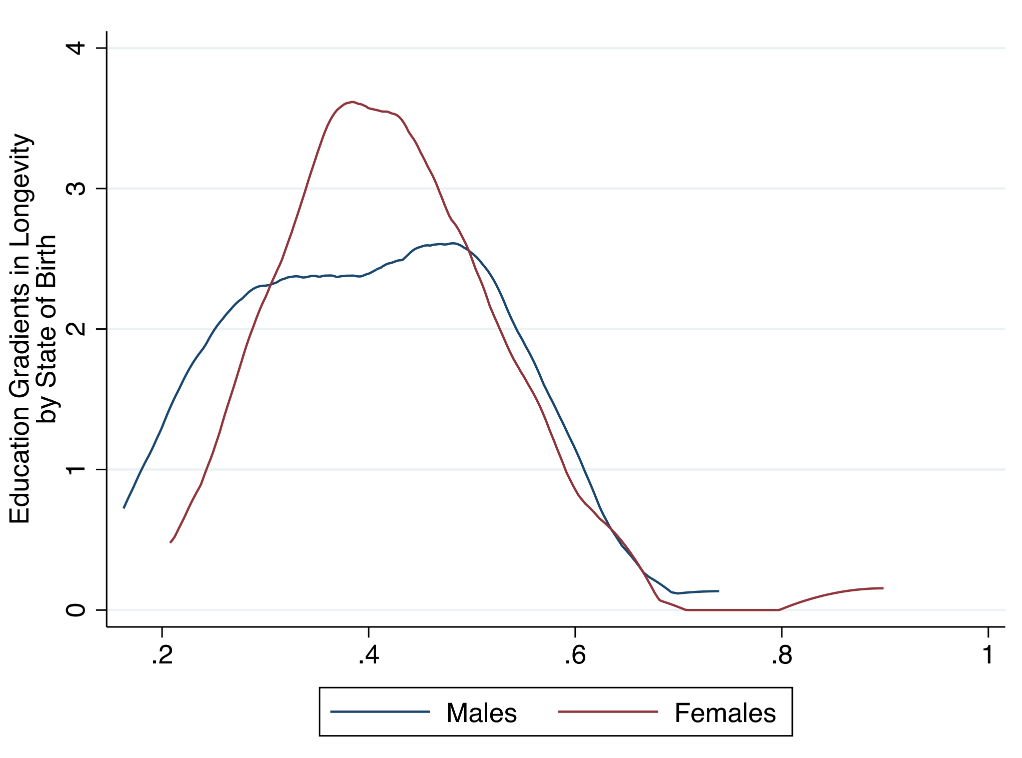
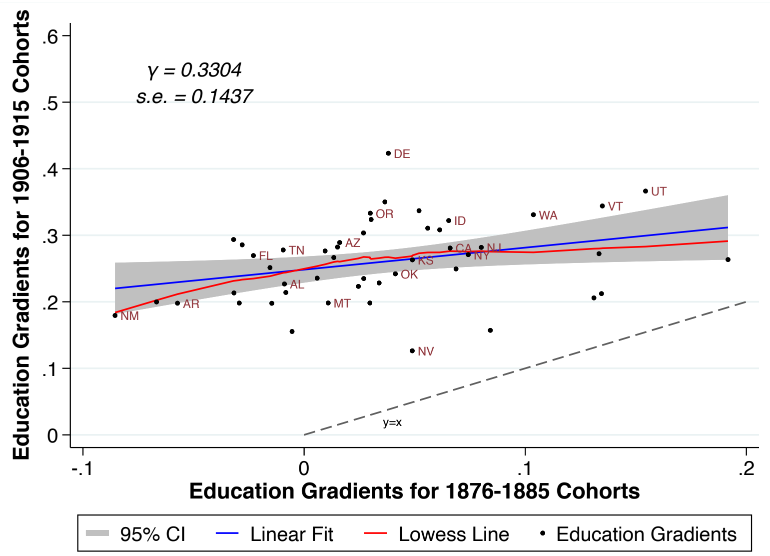


Figure A14. Distribution of state-level education gradients, by gender.
Notes: The estimates were based on whites born 1906-1915 in the 48 states of the US who survived to age 35.

Panel A. For MALES



Panel B. For FEMALES

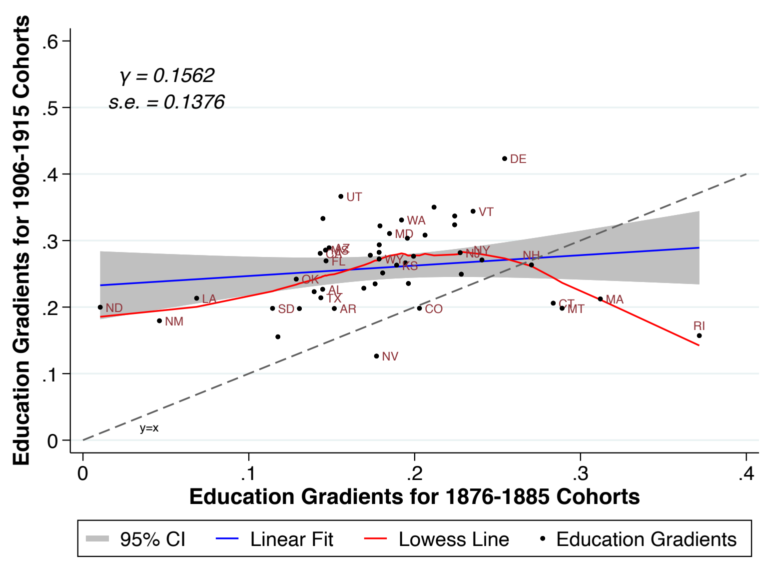
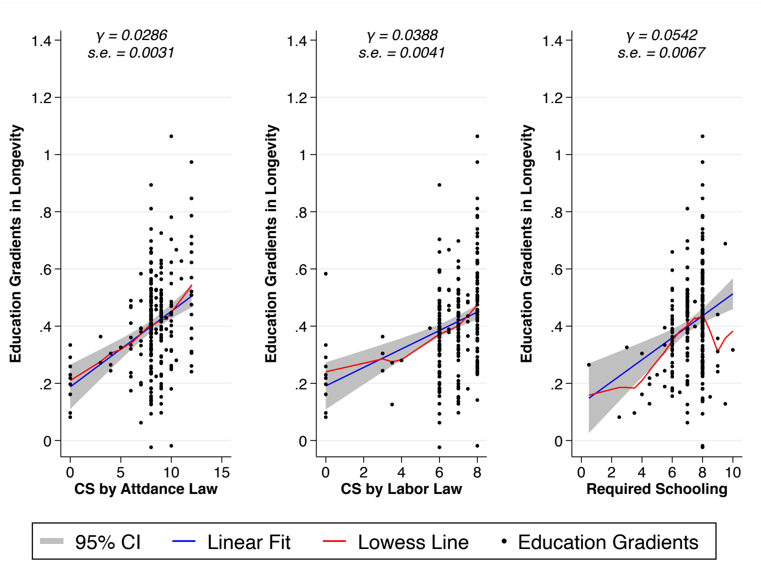


Figure A15. Education gradients at age 65 by birth cohorts.

Notes: Education gradients in longevity are estimated those born in 1906-1915 and 1876-1885. For both birth cohorts, analytic sample includes whites born in 48 states and survived to age 65. Coefficients (γ) and standard errors (*s.e.*) are from a weighted OLS regression of education gradients for 1906-1915 cohorts on education gradients for 1876-1885 cohorts

Panel A. Males



Panel B. Females

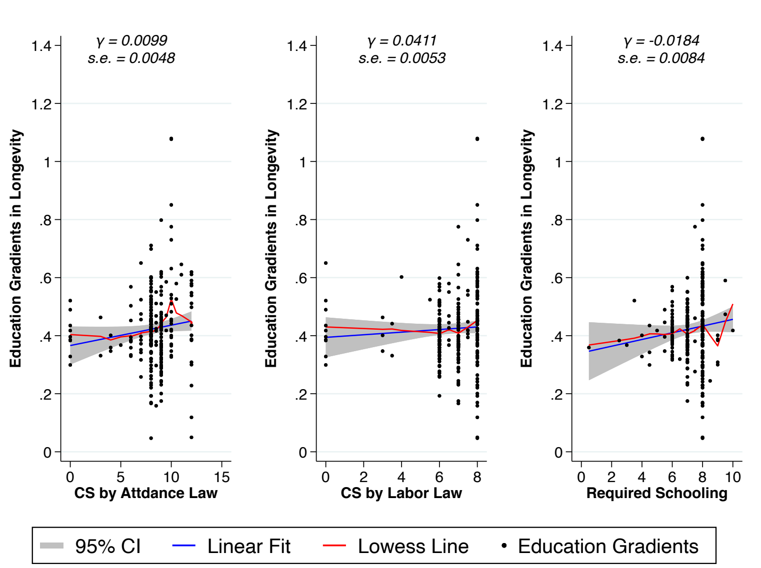


Figure A16. The Effect of compulsory schooling laws on education gradients—estimates without family fixed effects.

Notes. Estimates by state of birth and cohort. Each point denotes returns to education for specific state*cohort cell (48 states by 5 birth cohort groups with two birth cohorts per group). Sample includes only whites born 1906-1915 in the 48 states of the US and survived to age 35. Coefficients (γ) and standard errors (s.e.) are from a weighted OLS regression of education gradients on state-cohort level average education or longevity, with the weights are equal to the inverse of standard errors of estimated gradients. The lowess (locally weighted scatterplot smoothing) line provides a smooth function based on non-parametric techniques. Data on Years of compulsory schooling required by attendance laws, and years of compulsory schooling required by child labor laws were from Lleras-Muney (2005). Data on Cumulative years of compulsory schooling were from Stephens and Yang (2014).

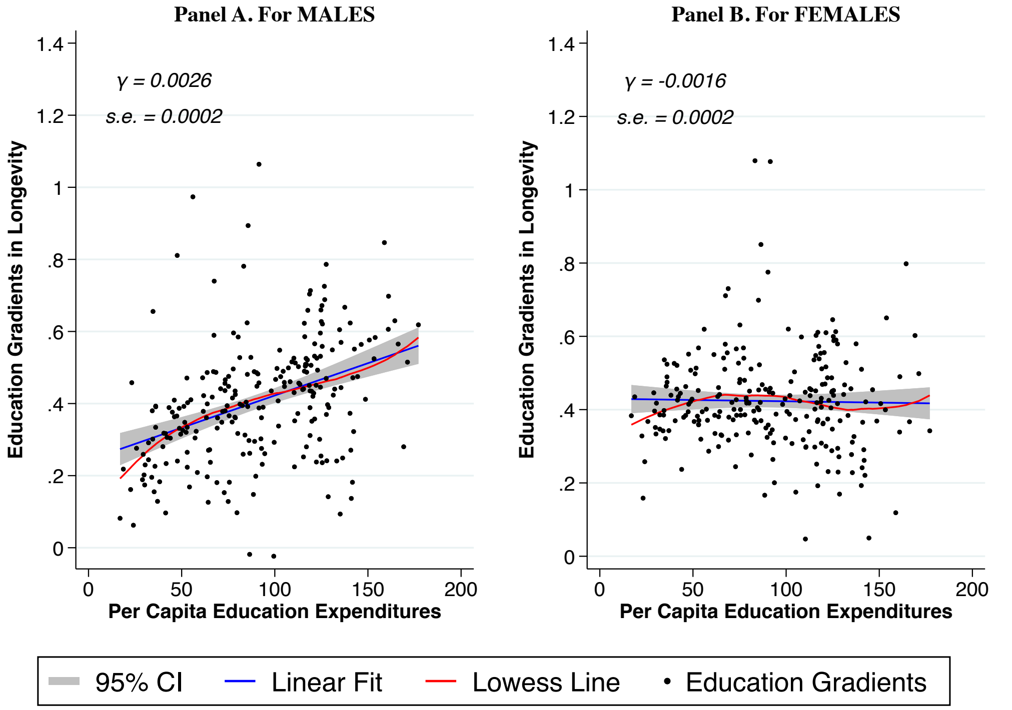


Figure A17. The effect of per capita education expenditure on education gradients—estimates without family fixed effects.
Notes. Estimates by state of birth and cohort. Each point denotes returns to education for specific state*cohort cell (48 states by 5 birth cohort groups with two birth cohorts per group). Sample includes only whites born 1906-1915 in the 48 states of the US and survived to age 35. Coefficients (γ) and standard errors (s.e.) are from a weighted OLS regression of education gradients on state-cohort level average education or longevity, with the weights are equal to the inverse of standard errors of estimated gradients. The lowess (locally weighted scatterplot smoothing) line provides a smooth function based on non-parametric techniques. Data on per capita education expenditures were from Lleras-Muney (2005).

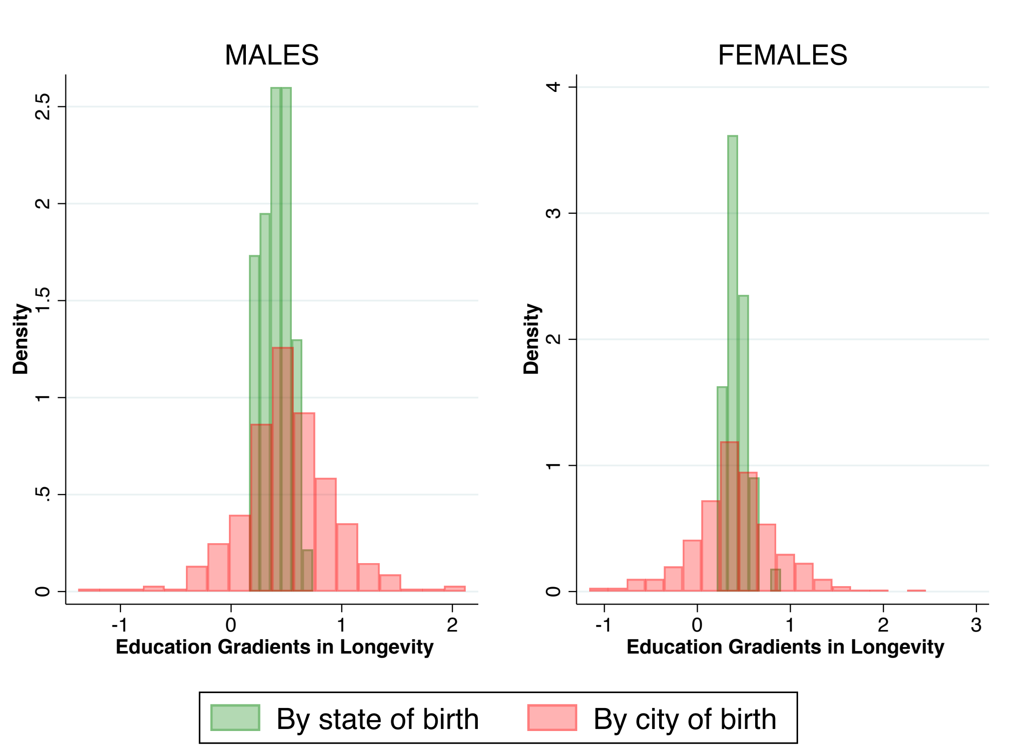
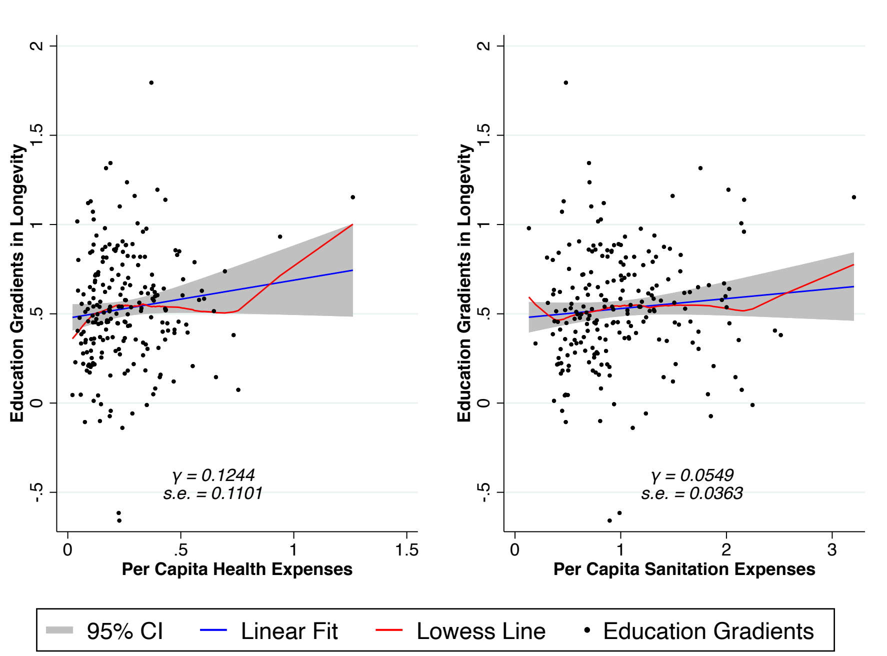


Figure A18. Distribution of education gradients by state of birth and by city of birth.
Notes: Education gradients by city of birth were estimated based on individuals from 351 cities with school quality measures. Distribution of education gradients by state of birth were based on 5.5 million individuals from 48 states in the main text.

Panel A. For MALES



Panel A. For FEMALES

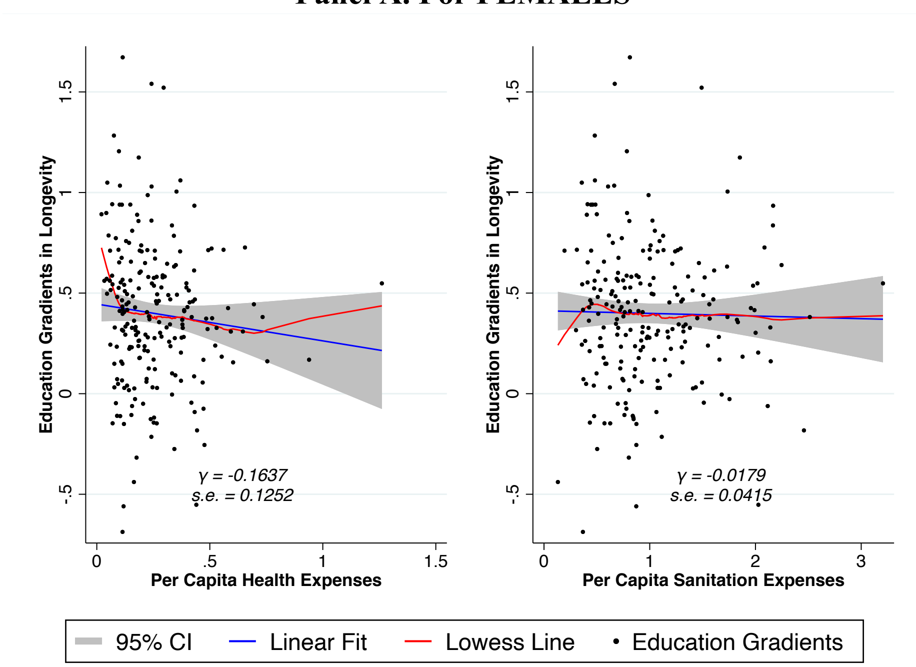
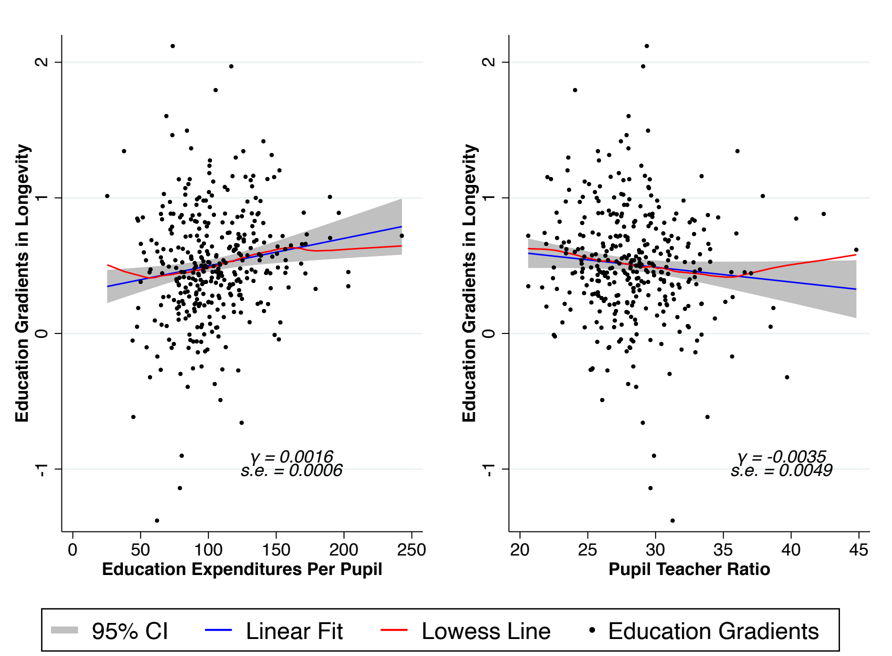


Figure A19. Education gradients and city health and sanitation expenses.
Notes: Sample includes only whites born 1906-1915 in the 215 large US cities and survived to age 35. A point in the figure shows the city-level coefficient of education. Coefficients (γ) and standard errors (*s.e.*) are from a weighted OLS regression of education gradients on health measures, with the weights are equal to the inverse of standard errors of estimated gradients. The lowess (locally weighted scatterplot smoothing) line provides a smooth function based on non-parametric techniques.

Panel A. For MALES



Panel B. For FEMALES

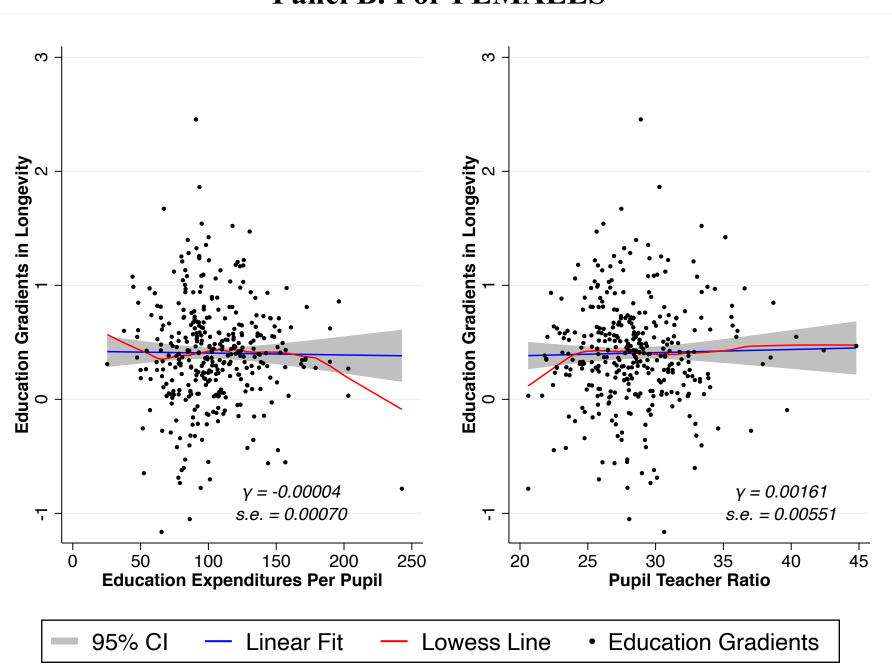
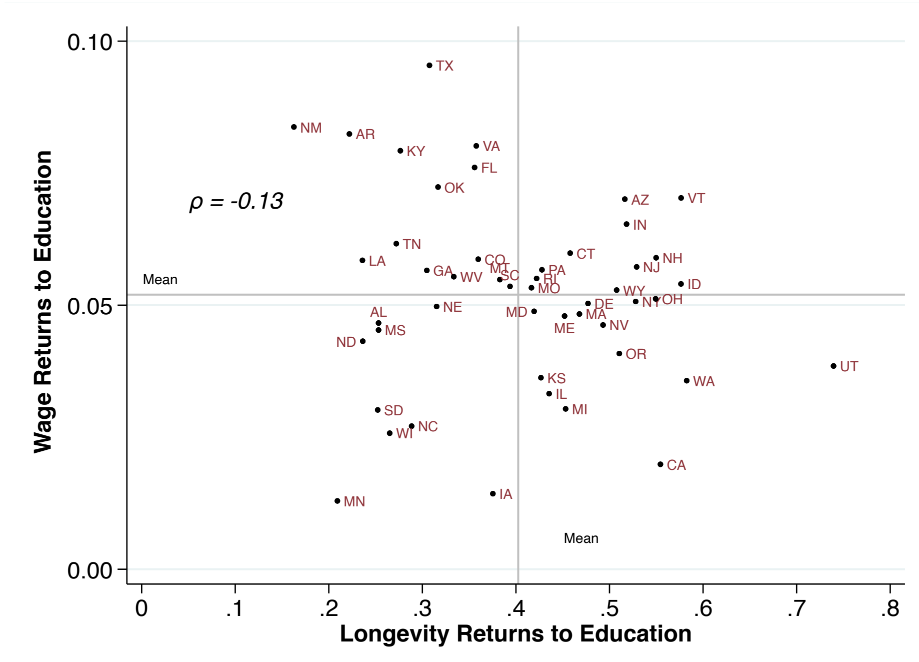


Figure A20. Education gradients and city quality of schooling measures.

Notes: Sample includes only whites born 1906-1915 in the 351 large US cities and survived to age 35. A point in the figure shows the city-level coefficient of education. Coefficients (γ) and standard errors (*s.e.*) are from a weighted OLS regression of education gradients on health measures, with the weights are equal to the inverse of standard errors of estimated gradients. The lowess (locally weighted scatterplot smoothing) line provides a smooth function based on non-parametric techniques.

Panel A. For MALES



Panel B. For FEMALES

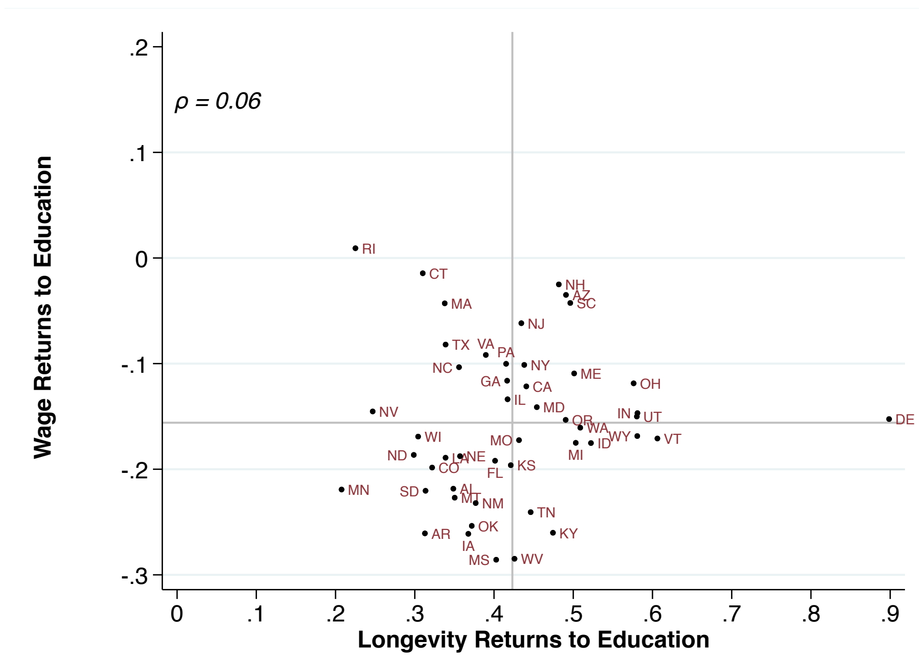


Figure A21. Correlation between wage returns and longevity returns to education.
Notes: We estimated the wage returns from the 1940 full census data. Sample includes only whites born 1906-1915 in the 48 states of the US.