Increased Schooling Reduces Hospitalization Later in Life: New Evidence with Optimal Instruments from the United States

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October 17, 2023

Abstract

We investigate the causal effect of education on hospitalization. We apply novel techniques to estimate a sparse model that uses the least absolute selection and shrinkage operator (LASSO) regression with a data-driven penalty to construct optimal cross-validated instrumental variables and select a parsimonious set of controls. This method yields consistent and more efficient estimates relative to conventional instrumental variable procedures and overcomes the limitations of previous studies using compulsory schooling laws in the United States. We also use an approach for a valid inference that allows instruments to be only plausibly exogenous. Using the 1992-2016 Health and Retirement Study, our results suggest that an additional year of schooling in early life lowers the likelihood of two-year hospitalizations later in life by 2.6 percentage points (or about 9.5%). This estimate is robust to different model specifications and plausible amounts of imperfect exogeneity and is similar to the local treatment effect among potential compliers.

Keywords: Education; Hospitalization; Optimal instruments; Sparse model; Plausibly exogenous

JEL: I10; I20; C36

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* We are grateful to Hans van Kippersluis for the insightful comments. We appreciate helpful comments from conference participants at the 2021 AcademyHealth Annual Research Meeting and the International Health Economics Association 2021 World Congress. We thank staff members of the Health and Retirement Study at the University of Michigan for providing the restrictive data and their assistance in using the MiCDA enclave. This project was partially supported by the California Center for Population Research at UCLA (CCPR), which receives core support (P2C- HD041022) from the Eunice Kennedy Shriver National Institute of Child Health and Human Development (NICHD). All errors are our own.

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1. Introduction

In the early twentieth century, many countries passed child attendance laws and child labor laws to establish a minimum number of years of schooling for school-aged children. Such legislation, often referred to as compulsory schooling law (CSL), offers researchers a quasi-experiment to study the causal effects of education on many outcomes. Since the seminal study of Lleras-Muney (2005), a substantial number of studies have been devoted to identifying health returns to education based on these laws across the globe (Galama et al., 2018; Hamad et al., 2018). Empirical studies from the United States that attempted to estimate the causal effects of education on health (typically measured by mortality) have found mixed results (Albarrán et al., 2020; Arcaya and Saiz, 2020; Galama et al., 2018; Xue et al., 2021). On average, recent studies find either small or null returns of education on health, though the evidence also suggests that the impact of education depends on the context.[1] Thus, whether education improves health, when, and for whom remains a question of empirical study.

In the United States, the main approach for estimating the causal effect of education on health using compulsory schooling legislation has been shown to suffer from multiple estimation problems. Using successive censuses to construct mortality rates and using changes in compulsory schooling as instruments for education, Lleras-Muney (2005) first documented that one additional year of schooling reduces 10-year mortality by 6.1 percentage points. However, a subsequent study by Mazumder (2008) showed that the IV estimate decreased substantially and became statistically insignificant after adjusting for state-specific linear trends. These trends were added to account for unobserved state-specific changes that may be related to both secondary schooling and health outcomes. However, this approach to controlling for unobservables has several limitations. It is unclear whether state-specific trends can capture the

[1] A significant education effect on health exists in some contexts such as those born 1924-1931 in Sweden (Fischer et al., 2013), men born around 1917 in Netherland (Van Kippersluis et al., 2011), but not in others, for example, Britain (Clark and Royer, 2013), France (Albouy and Lequien, 2009), Germany (Braakmann, 2011; Pischke and Von Wachter, 2008), Denmark (Arendt, 2005; Arendt, 2008), and birth cohorts 1940-1960 in Sweden (Lager and Torssander, 2012; Meghir et al., 2018). Lleras-Muney et al. (2022) find that the effects of education and health are different across places and systematically related to the social environment.

evolution of the state characteristics which likely follow a nonlinear path. More importantly, adding the state of birth and year of birth interactions may result in overfitting in the first stage, substantially decreasing the strength of these laws as instruments for education (Black et al., 2015; Lleras-Muney, 2005): the additional predictive power of these laws is low once state fixed effects and state-specific trends are included, leading to a weak instrument problem. More recent studies have re-estimated the effect of education on mortality using better data with more accurate measures of mortality and larger sample sizes. These studies concluded that state and cohort fixed effects account for most of the variation in mortality rates. Therefore, simply increasing the sample size cannot produce precise estimates of the causal effect of education on health with compulsory schooling laws as instruments (Black et al., 2015; Fletcher, 2015). As a result, the US literature is inconclusive: the instrumental variable (IV) estimates are large but imprecise.

In this study, we examine the relationship between years of schooling and hospitalizations using a longitudinal dataset. We make two innovations. First, we take advantage of newly developed machine learning methods to identify optimal instruments and a sparse set of controls. Using these methods, we can avoid the indiscriminate use of fixed effects and state-specific trends, which result in weak instruments. We show that these new methods substantially improve the efficiency of our estimators relative to conventional instrumental variables estimators (Belloni et al., 2012; Belloni et al., 2014; Chernozhukov et al., 2015). We also improve the instruments' credibility by investigating the validity of the exclusion restriction using various recently developed approaches.

Second, we investigate the effect of education on hospitalization in addition to mortality. Although there is extensive literature on education and health, causal evidence on how education impacts the use of healthcare, such as hospital admissions, remains scarce. Hospitalizations are also of interest for various reasons. First, they are costly. Hospitalizations account for the largest share of healthcare costs in the US healthcare system (Centers for Medicare & Medicaid Services, 2021). At the individual level, hospital admission poses considerable economic risks for adults, especially for non-elderly adults; it substantially reduces earnings (Dobkin et al., 2018). Finally, hospitalizations are of interest because they capture the quality of life, something that mortality measures do not (Albuquerque de Almeida et al., 2020). Unlike other measures of healthcare utilization, among the population over 65, hospitalizations likely reflect health (rather than ability to pay) and are predictive of mortality (Lehmann et al., 2018; Lin, 2017; Yue et al., 2021).[2] Yet, there is little empirical evidence of the causal effects of education on hospitalizations in the United States.[3]

Using a nationally representative panel survey of the middle-aged and older adults from the Health and Retirement Study (HRS), which includes repeated measures in hospitalization, we find that among low-education (high school or less) whites born in the continental US between 1905 and 1959, an additional year of schooling significantly reduces the likelihood of two-year hospitalizations in later life by 2.6 percentage points, approximately 9.5% relative to the mean. Our optimal instruments reject the null hypothesis of weak instrument based on the effective first-stage F-statistic of Montiel Olea and Pflueger (2013), which is robust to heteroscedasticity, serial correlation, and clustering (Andrews et al., 2019).

To obtain these results, we first use the least absolute selection and shrinkage operator (LASSO) regression with a data-driven penalty to identify a set of optimal cross-validated instrumental variables and a parsimonious set of controls. Using the selected instruments and controls (instead of including all state fixed effects and state-specific linear trends), we can maximize the first stage's predictive power, increase the instruments' strength, minimize model complexity, and obtain precise IV estimates. We then use the LASSO-selected instruments and controls in a two-stage least squares (2SLS) model to obtain the post-LASSO estimator, eliminating some of the LASSO estimator's shrinkage bias (non-zero coefficients are biased toward zero).

[2] Hospitalizations have low demand elasticities (Manning et al., 1987) and, as such, are more likely to reflect changes in health status and corresponding healthcare use compared to other measures such as outpatient care. Hospitalization is a strong predictor of mortality and may also capture the demand for healthcare and access to hospitals (Lehmann et al., 2018; Lin et al., 2017; Yue et al., 2021). A previous study focusing on the same sample finds that education affects hospitalization mainly through improved health rather than income (Yue et al., 2021).

[3] Bijwaard and van Kippersluis (2016) used data from a Dutch cohort born 1937-1941 and showed a higher survival rate after hospitalizations, but intelligence accounts for most of the association. Another working paper by Gehrsitz and Williams (2021) found that the 1972 school reforms in Britain led to significantly lower hospitalization rates for men but not for women.

Given that our model only includes a selected set of controls, we additionally adjust for potential violations of the exclusion restriction assumption by applying Conley et al.'s (2012) local-to-zero approach. This approach allows instruments to be only plausibly exogenous—it allows the exclusion restriction failure to be close to zero but perhaps not exactly zero. Further following van Kippersluis and Rietveld (2018), we draw estimates of the degree to which the exclusion restriction fails by investigating the reduced form impacts of compulsory schooling laws on hospitalizations for a "zero-first-stage" group who are ex-ante assumed to be unaffected by the instruments. We constructed this group as those with a high probability of graduating from college based on pre-determined characteristics. Intuitively, this approach amounts to finding a control group that is unaffected by compulsory schooling laws to eliminate potential bias in the affected group. This approach allows us to account for a wide variety of unobserved factors without imposing a linear functional form, as state-specific trends do.

Our main IV estimate ($\beta = -0.026$) is more than twice that of the Ordinary Least Squares (OLS) estimate ($\beta = -0.010$), but the difference is not statistically significant. To assess the extent to which 2SLS estimates reflect the local average treatment effect among compliers, we compare our IV estimate with the OLS estimate from the subsample of potential compliers (Abadie, 2003; Blandhol et al., 2022; Imbens and Angrist, 1994; Sloczynski, 2021). We consider potential compliers of compulsory schooling laws as individuals who are predicted to have schooling in the CSL ranges (5-10 years of schooling) or have education less than secondary school (< 9 years of schooling) based on pre-determined characteristics such as gender and parental education. Our analyses suggest that the discrepancy between IV and OLS estimates is likely due to the fact that the IV estimate is more likely to capture the local average treatment effect among compliers.

The remainder of this paper is organized as follows. Section 2 summarizes the related literature. Section 3 describes the data and the empirical analytical strategy. Section 4 presents the main results. Section 5 discusses the education effects on common causes of hospitalizations, self-reported health, and mortality. Section 6 concludes.

2. Related Literature

In the United States, Lleras-Muney (2005) first used US compulsory schooling laws as instrumental variables for educational attainment to examine the effect of education on mortality using census data to construct ten-year mortality rates. The author controls for several state characteristics that jointly influence secondary education and mortality and, importantly, allows for differential effects of the year of birth across four census regions. The study finds a large and statistically significant education effect on mortality; one additional year of schooling is estimated to reduce the 10-year mortality by 6.1 percentage points. Mazumder (2008) extended this study and found that this estimate is not robust to the inclusion of state-specific linear trends. A series of subsequent studies use 1960-1980 censuses data documenting low first-stage F-statistics once state fixed effects and/or region by cohort fixed effects are added to the model, which raises concerns about weak instrument bias in IV estimates (Black et al., 2015; Bound et al., 1995; Lleras-Muney, 2005). The fundamental limitation of these analyses is that in the US, compulsory schooling laws had a small effect on the average education of the population; one more year of compulsory schooling resulted in an increase of 0.05 years of additional schooling on average (Lleras-Muney, 2002, 2005).

Other studies in economics have revisited this topic using novel and arguably better data.[4] Black et al. (2015) improved the measurement of mortality by combining US census data with complete vital statistics records to compute mortality rates (instead of using the censuses). The authors failed to find any significant effects of education on mortality. They attributed their null findings to the fact that most of the variation in mortality is explained by state and cohort fixed effects. Fletcher (2015) used a novel dataset of 3.5 million individuals and also failed to show statistically significant effects on mortality, though the magnitude is large: Fletcher finds a 6.9 percentage points reduction in mortality per additional schooling year, which is comparable to that of Lleras-Muney (2005), but the standard error is large making the effect not statistically significant (the first-stage F-statistic on the excluded instruments is 18.3).[5] The author concludes

[4] Prior studies (e.g., Buckles, et al., 2016; Fletcher and Noghanibehambari, 2021) have shown significant effects of college education on mortality and longevity. In this section, we summarize studies that use compulsory schooling laws as instruments, which mainly impact secondary schooling.

[5] Applying Lee et al. (2022)'s tF adjustment further increases the standard error.

that “*further use of this methodology may require even larger, and potentially unattainable, sample sizes in the US*.”

Another strand of literature from public health uses two-sample instrumental variables, with the first stage estimated using census data, finding significant effects on dementia risks and cardiovascular diseases (Hamad et al., 2019; Nguyen et al., 2016). However, once state-of-birth fixed effects are included, the instruments become substantially weak, and many significant IV estimates become insignificant (Hamad et al., 2019). The fundamental issue with US studies is that, as noted above, once a large number of state fixed effects and state-specific trends are included, the instruments are weak, even in large samples, because the effects of compulsory schooling laws in education were small. Instead, better analytical methods are needed to provide consistent but more efficient estimates.

Fewer studies have assessed the causal effects of education on healthcare use. The studies that did give a mixed picture. Two studies in Denmark focused on hospitalizations. Arendt (2008) leveraged the change in urban-rural differences in education due to the 1958 school reform in Denmark as a quasi-experiment. The study found that having more than a primary schooling degree statistically significantly lowers the likelihood of being hospitalized for women and reduces the probability of hospitalizations due to lifestyle-related diagnoses for men. Tansel and Keskin (2017) found that more years of education reduced the number of days hospitalized in Turkey. Bijwaard (2021) used Swedish Military Conscription data to investigate the mediation effect of cardiovascular hospitalizations on the relationship between education and mortality. The author shows that education gains in the survival probability until 63 is the largest for those with only primary education (5.5 percentage points) and is 1.9 percentage points for those with post-secondary education. These gains are mainly due to educational differences in hospitalizations for the highly educated and due to other factors for the less educated. In contrast, Behrman et al. (2011) found no effect of education on hospitalization by comparing identical Danish twins with different levels of schooling, although the correlation was large and statistically significant. Meghir et al. (2018) used compulsory schooling reforms in Sweden and found no impact of education on hospitalization.

These studies vary in their methodology, and it's unclear whether they are all comparable. But more importantly, they do not study the US. It may well be that the effects of education in the US are different, given the substantial differences in the healthcare and welfare systems of these European countries. For example, the US has a lower health insurance coverage rate and worse access to care compared to other OECD (the Organization for Economic Co-operation and Development) countries (Rice et al., 2013). As such, the educational differences in hospitalizations may be larger in the US. One relevant study was conducted in the US. Mazumder (2008) used compulsory schooling laws as instruments for education and added state-specific linear trends in the model. The IV estimate of years of schooling on ever hospitalized in the last year was -0.0268, which is large but not statistically significant. The author does not report the first-stage partial F-statistic for the instruments used in this analysis.[6] Thus, we could not assess potential biases related to weak instruments.

3. Data and Empirical Methods

3.1 The US Health and Retirement Study

We draw on the US Health and Retirement Study (HRS) for individual-level data. The HRS is a national longitudinal panel study that biennially surveys a representative sample of individuals aged 51 years and above and their spouses. The HRS sample was built over time. From 1992 through 2016, the HRS obtained detailed information on respondents' childhood health and socioeconomic status. We obtained data from the RAND HRS Longitudinal File 2016 (V1), which contains cleaned and processed variables with consistent naming conventions across waves (Health and Retirement Study, 2020). We extracted respondents' childhood health status and childhood family financial situation variables from the RAND fat files. We further merged into the 1992-2016 HRS restricted data with state identifiers to gain information on the respondents' state of birth.

[6] The partial F-statistic on the first stage regression with state-specific linear trend using the 1960-1980 censuses for another analysis in this paper is 7.4. For the analysis of education and hospitalization, the author uses data from the Survey of Income and Program Participation (SIPP) with a much smaller sample size (number of observations in this analysis: 26,484).

We focus on white individuals who had an education of high school or less[7] and were born in 48 states and the District of Columbia (Hawaii and Alaska were not part of the Union). Owing to the availability of measures for compulsory schooling laws, we restricted the analysis to those born between 1905 and 1959.[8] This resulted in a sample of 30,898 persons and 207,172 person-years. We further restricted the sample to white respondents (who are most likely to be affected by compulsory schooling laws) with a high school degree or less[9] and without missing values for all variables included in the regression.[10] The final regression sample included 12,528 individuals and 85,887 person-years. **Figure A1** displays the sample flowchart.

The primary outcome of our study is hospitalization. It is a wave-specific measure available in all waves of the HRS data and represents whether the respondent had reported any overnight hospital stay since the last interview. For example, in 2010, the question was asked, "(Since R's LAST IW MONTH, YEAR/In the last two years), have you been a patient in a hospital overnight?"

Education was measured as the number of years of schooling. We included several individual-level covariates that confound the relationship between education and hospitalization based on a prior conceptual framework for education and healthcare use (Yue et al., 2021). These include the female gender, parents' years of education (the highest grade of completed education of the respondent's father or mother, which ranges from 0 to 17), and a cubic function of age, state of

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[7] Fletcher (2016) restricts the majority of analyses on the effects of education on health to lower-educated individuals. Oreopoulos (2006) conducts analyses separately for those with high school or less and those with education higher than high school; the study finds that the effect of education on earnings is only significant for low-education individuals. Since this sample selection is based on an endogenous variable, we will check our results' robustness to this selection later in this paper.

[8] Several studies focus on these birth cohorts. For example, Lochner and Moretti (2004) focus on men born 1900-1960, and Oreopoulos (2006) uses cohorts 1900-1961.

[9] Lleras-Muney (2002) finds no statistically significant effect of compulsory schooling laws on the educational levels of Blacks.

[10] The missing values are due to missingness in hospitalizations and parental education. We excluded 1,297 persons (7,905 person-years) with missing values in either of the two variables; 237 persons (241 person-years) missed hospitalizations only, 1,289 persons (7,632 person-years) missed parental education, and 31 persons (32 person-years) missed both. However, excluding them from analyses had no impact on our estimates. A sensitivity analysis adding another category of missingness to parental education, leading to a sample of 13,817 persons and 93,519 person-years, yields similar OLS estimate ($\beta = -0.010, s.e. = 0.001$), Lasso IV estimate ($\beta = -0.045, s.e. = 0.011$) and Plausibly exogenous IV estimates ($\beta = -0.027, s.e. = 0.009$).

birth, year of birth, and survey waves. Based on parents' years of education, we defined parents' educational attainment as less than high school, high school, some college, and college or above.

Table 1 documents the summary statistics of the included sample. The overall average rate of hospitalization over a two-year period was 27.5%. The respondents' average year of schooling was 10.9, which reflects a skewed distribution toward the higher end. More than half of the respondents were female (60.5%) and had parents who did not graduate from high school (66.5%). The average of their parents' highest education was 9.4 years. Over half of them were born after 1921 and in the South or Midwest. Regarding compulsory schooling laws, 63.8% of respondents were exposed to continuation laws. The average number of schooling years required for respondents, when they were 14 years, based on compulsory attendance laws, child labor laws, and accumulative required schooling, was 9.7, 7.8, and 8.3, respectively.

3.2 The US Compulsory Schooling Laws

3.2.1 Data sources for compulsory schooling laws

Compulsory schooling laws in the 1900s have been systematically compiled by various researchers. This study focuses on six key variables: 1) minimum age of compulsory schooling (Entry age); 2) maximum age of compulsory schooling (Dropout age); 3) education for exemption from maximum age rule (Education to Dropout); 4) age at which youth can obtain a work permit (Work Permit Age); 5) education required to receive a work permit (Education to Work); and 6) whether a state has mandatory continuation schools that force children at work to complete their education on a part-time basis (Continuation Laws).

In this study, we relied primarily on previous studies as sources of compulsory schooling laws. We extended the data series by collecting our own data for certain states and years missed by these sources. Specifically, we used datasets from Lleras-Muney (2002) and Goldin and Katz (2003) for 48 states (excluding Alaska, Hawaii, and Washington DC) for 1910-1939, and the dataset from Acemoglu and Angrist (2000) for 48 states (excluding Alaska and Hawaii) during 1940-1978, and Washington DC during 1915-1978. We collected additional data to impute 1) compulsory schooling laws for Washington DC during 1910-1914, 2) missing continuation schooling laws for Washington DC during 1915-1978, and 3) missing continuation schooling laws for 48 states (excluding Alaska and Hawaii) during 1940-1978. **Table A1** documents all the data sources for compulsory schooling laws.

3.2.2 Construction of instruments based on compulsory schooling laws

Following the literature, we construct several composite measures for compulsory schooling years. Similar to previous studies (e.g., Lleras-Muney, 2005; Stephens and Yang, 2014), the aspects of child attendance laws and child labor laws used to construct these measures are those prevailing in the individual's state when they were 14 years old,[11] except that entry age was assigned based on laws in place at age 6 following Goldin and Katz (2003). More specifically,

Years of compulsory schooling required by child attendance laws (CA_{sct}) for those born in state s in year c and 14 years old in year t are computed as

$$CA_{sct} = \min\left(Dropout\ Age_{sct} - Entry\ Age_{sc,t-8}, Education\ to\ Dropout_{sct}\right)$$

Years of compulsory schooling required by child labor laws (CL_{sct}) for those born in state s in year c and 14 years old in year t are computed as

$$CL_{sct} = \max\left(Work\ Permit\ Age_{sct} - Entry\ Age_{sc,t-8}, Education\ to\ Work_{sct}\right)$$

Leave age (LA_{sct}) for those born in state s in year c and 14 years in year t is computed as follows:

$$LA_{sct} = \min\left(Dropout\ Age_{sct}, Work\ Permit\ Age_{sct}\right)$$

We also adopted the "required schooling (RS_{sct})" constructed by Stephens and Young (2014), iterating through ages 6 to 17 to determine whether the child is required to attend school at that

[11] Most states required students to attend schools through age 14 for the birth cohorts included.

age based on the law in place that same year. In addition, we constructed several dummy variables to capture potential nonlinear effects. Cutoff values were used to ensure sufficient observations per group.[12]

Prior research has used various sets of instruments based on the CSLs.[13] For example, these instruments include dummies for continuation laws and years of compulsory schooling required by child labor (CL) laws (Lleras-Muney, 2005), categories for years of compulsory schooling required by child labor laws and attendance laws (Acemoglu and Angrist, 1999), categories for cumulative years of compulsory schooling (required schooling) (Stephens Jr and Yang, 2014), and dropout age (Oreopoulos, 2006). The performance of compulsory schooling laws as instruments varies across studies, with different model specifications, birth cohorts, survey years, and sample sizes; the F-statistic on instruments from the first stage ranges from 243.5 in Oreopoulos (2006), without controlling for state/region-specific trends [14] to 4.7 in Lleras-Muney (2005), adjusting for region by year-of-birth indicators.[15] Using all the aspects of CSLs and their transformations will generate many instruments, but IV estimators with many instruments are largely biased based on 2SLS and have too small standard errors (Chao and Swanson, 2005; Hansen et al., 2008). Thus, it is important to identify the optimal instruments for specific analyses.

3.2.3 Descriptive analyses of compulsory schooling laws

[12] To save notation, we have dropped the subscripts from the following equations: $CA6 = 1\ if\ CA \leq 6; CA7 = 1\ if\ CA = 7; CA8 = 1\ if\ CA = 8;\ CA9 = 1\ if\ CA = 9; CA10 = 1\ if\ CA \geq 10; CL6 = 1\ if\ CL \leq 6; CL7 = 1\ if\ CL = 7; CL8 = 1\ if\ CL = 8; CL9 = 1\ if\ CL \geq 9; RS6 = 1\ if\ RS \leq 6; RS7 = 1\ if\ RS = 7; RS8 = 1\ if\ RS = 8; RS9 = 1\ if\ RS \geq 9$.

[13] IV estimates are sensitive to the choices of instruments.

[14] Oreopoulos (2006) includes all US native-born individuals born 1901-1961 from 1950-2000 US censuses. The first stage includes birth year fixed effects, state of birth fixed effects, survey year fixed effects, a dummy variable for race, and state controls for fractions living in urban areas, black, in the labor force, in the manufacturing sector, female gender, and average age based on when a birth cohort was age 14.

[15] Lleras-Muney (2005) focuses on US born whites between 1901-1925 from the 1960-1980 censuses. The first stage adjusts for the female gender, state characteristics (state expenditures on education, the number of school buildings per acre, percent of the population that was living in urban areas, percent of the white population that was foreign born, percent of the population that was black, percent of the population employed in manufacturing, average annual wages in manufacturing per worker, average value of farm property per acre, and number of doctors per capita), and the interaction between region of birth and birth cohort dummies.

Figure 1 shows the trends in the average years of compulsory schooling by educational law over time when respondents were 14 years old. In general, states required more years of education as time went by. However, this was not always the case; there were some ups and downs over the study period. Moreover, the number of years of compulsory schooling, constructed in different ways, is moderately correlated.[16] It suggests that each of these constructed measures carries unique information about compulsory schooling. This motivates the generation of multiple different instrumental variables and the use of all of them. On the other hand, it is not clear which laws were effectively enforced—which laws best predict education attainment is an empirical matter.

As preliminary evidence, we plotted the average years of completed education by years of compulsory schooling and educational attainment (see **Figure 2**). These graphs imply that these laws were effective only in improving education for those who ultimately obtained low education levels, particularly those with education less than a high school degree, but not for those who eventually graduated from college. For those at the lower end distribution of education, the average education was higher for those in states requiring more years of compulsory schooling, which is consistent with the monotonicity assumption of instruments. In addition, these effects were not linear: one more year of compulsory schooling did not have the same impact when compulsory schooling required 6 years and when it required 10. To account for these features, we need to include a nonlinear transformation of these laws as instruments for education.

3.2.4 The strength of compulsory schooling laws as instruments

We first assess whether a large proportion of the variation in CSLs can be explained by state (e.g., state-of-birth) fixed effects, cohort (e.g., year of birth) fixed effects, and, in particular, region or state trends. Column (1) of **Table 2** shows the mean and standard deviation (SD) of the raw numbers. In Column (2), we purge out state fixed effects and cohort fixed effects by computing the residuals from regressing raw numbers on these fixed effects. We report the mean, SD, and percentage reductions in SD (↓SD %) compared to the raw numbers based on the

[16] The correlation coefficients range from 0.12 to 0.57, with most of the correlation less than 0.4.

computed residuals. We then add region/state linear trends to the regression models and calculate the same statistics in the remaining columns. In general, the variation in compulsory schooling laws is relatively robust to these fixed effects but not to the inclusion of state trends. For schooling by child labor laws and dropout age, one-third of the variation was explained by these fixed effects and trends. For three of the variables we use (continuation laws, schooling by child attendance laws, and required schooling), the dummies and state-specific trends account for approximately half of the remaining variation or more. Thus, we either have to use a large sample or have to be judicious when adding fixed effects and trends in regression models.

We then formally test the strength of compulsory schooling laws as instruments. We estimate a linear regression of schooling years on various sets of instruments used in prior studies, along with controls. We then report the first stage effective F-statistics proposed by Montiel Olea and Pflueger (2013). Considering that laws are most effective for those with a high school degree or less (Fletcher, 2015; Lleras-Muney, 2002), we conduct these analyses separately for the overall sample and a restrictive sample focusing on those with a high school degree or less. **Table 3** shows that regardless of the specific laws under study (we consider the specifications in four previous studies), the first stage effective F statistics are large without state of birth and year of birth fixed effects but become substantially small after adding the region and state trends to the model.[17]

3.3 Empirical Strategy

3.3.1 Pooled Ordinary Least Squares (OLS)

We first pool all the 1992-2016 HRS data and examine the association between education and hospitalization. We then estimate the following equation using a linear probability model:

$$Y_{itcs} = \alpha + \beta * X_{ics} + D_{ics}\theta + \mu_c + \lambda_s + l_{sc} + \epsilon_{itcs} \quad (1)$$

[17] Results on first-stage F statistics are very similar. We report the first stage effective F statistic because it is robust to heterogeneity and clustering (Montiel Olea and Pflueger, 2013).

where Y_{itcs} denotes the outcome (ever hospitalized in the past two years) for individual i from cohort c and born in state s. X_{ics} represents years of completed education for individual i. Depending on the model specifications, we include the following controls: individual characteristics in D_{ics} that contain female gender, age, age^2, age^3, parents' educational attainment, and survey wave dummies; μ_c is a set of birth cohort dummies; λ_s is a set of state-of-birth dummies; and l_{sc} represents state-of-birth linear trends. ϵ_{itcs} is the idiosyncratic error term. We also estimate Equation (1) using a logit model and report the corresponding marginal effects.[18]

Consistent identification of the coefficient θ from the OLS requires at least the following assumption: conditional on D_{ics}, μ_c, λ_s, and l_{sc}, X_{ics} is uncorrelated with ϵ_{itcs}. However, this is very likely to be violated because of omitted variables. For example, individual preferences or discount rates may be related to both educational attainment and outcomes. This motivates instrumental variable analyses.

3.3.2 Conventional two-stage least squares with panel data (2SLS)

The econometric model can be written as:

$$X_{ics} = \alpha + Z_{cs}\pi + D_{ics}\lambda + \mu_c + \gamma_s + l_{sc} + v_{itcs} \qquad (2)$$

$$Y_{itcs} = \alpha + \beta * X_{ics} + D_{ics}\theta + \mu_c + \gamma_s + l_{sc} + b_{ics} + \epsilon_{itcs} \qquad (3)$$

In the first step, we model years of completed education as a function of excluded instruments (Z_{cs}), together with other control variables. We then fit the second equation by replacing the years of completed education with the corresponding predicted values from the first step. The variance-covariance matrix of the predicted values was adjusted to produce consistent standard errors.

[18] Cameron and Miller (2015) suggest using the generalized estimating equations (GEE) approach to handle clustering for logit models. We estimated a GEE model with a binomial distribution family, a logit link function and the exchangeable within-cluster correlation structure. Marginal effects from the GEE model yield no meaningful changes. For example, the marginal effect of years of schooling on hospitalizations with all controls (β = -0.009, $s.e.$ =0.001) is similar to the marginal effect from a logistic regression in Column (8) of **Table 4**.

To better explain the IV estimates bias related to the strength of the instruments and exclusion restriction, we rewrite Equations (2) and (3) following the notation of Conley et al. (2012).

$$X = Z\Pi + V \qquad (4)$$

$$Y = X\beta + Z\gamma + \varepsilon \qquad (5)$$

Where X ($N \times 1$ vector) represents the years of completed education. Z is an $N \times r$ matrix with $r \geq 1$ excluded instrumental variables constructed based on compulsory schooling laws. Π is a vector of the first-stage coefficients. Y ($N \times 1$ vector) denotes the outcome (ever hospitalized in the past two years). β is the parameter of interest that measures the effect of education on hospitalization. γ represents the direct effect of the instruments on Y, which measures the degree of exclusion restriction violation. It is assumed to be zero ($\gamma = 0$) in conventional IV analyses. ε and V are $N \times 1$ composite error terms.

To obtain valid causal estimates from the 2SLS procedure, we need three regular assumptions (Angrist and Pischke, 2014; Van Kippersluis and Rietveld, 2018). First, relevance ($\Pi \neq 0$). We can assess relevance using partial (joint) F-statistic on the excluded instruments from the first stage (Bound et al., 1995; Stock and Yogo, 2005) or preferably the effective first-stage F-statistic (Montiel Olea and Pflueger, 2013).[19] Andrews, Stock, and Sun (2019) suggest that we should use the effective F-statistic to test weak instruments, and if the null hypothesis of a strong instrument is rejected, use weak-instrument-robust methods such as the Anderson-Rubin (AR) confidence intervals. Second, independence. Z is uncorrelated with any confounder in the treatment-outcome relationship, which can be assessed by over-identifying restriction tests. Third, there is an exclusion restriction (γ=0). Z influences Y only through the channel of X.

[19] Previous literature has suggested using the first-stage F statistics with 10 as the rule of thumb to assess weak instrument bias. However, recent studies have critiqued it and proposed new approaches. For example, Lee et al. (2022) argue that standard errors of 2SLS IV estimates need to be adjusted to obtain the true 95% confidence intervals, especially when the first-stage F statistic is less than 104. The authors propose to apply an adjustment factor (*tF* adjustment) to 2SLS standard errors based on the first-stage F statistic with the adjustment factor provided in their tables for confidence intervals. The procedure is developed under the context of a single instrument and is robust to heteroskedasticity and clustering. In our analyses, we use the "*weakivtest*" package to test for weak instruments based on effective F statistics and the "*twostepweakiv*" package to obtain the 95% AR confidence sets.

Note there is a trade-off between instrument strength and the degree of violation of the exclusion restriction. This can be seen easily in the special case where β and γ are scalars (Conley et al., 2012). In this case,

$$\hat{\beta} = (Z'X)^{-1}Z'Y \xrightarrow{p} \beta + \frac{\gamma}{\Pi} \quad (6)$$

On the one hand, a small Π or weak instrument, which is the case for conventional studies that use compulsory schooling laws as instruments (Section 3.4), could substantially amplify the biases from exclusion restriction failures. However, when Π is large, an appreciable deviation from the exact exclusion restriction ($\gamma = 0$) may lead to only minor losses in the precision of the estimates.[20] It thus motivates us to find strong instruments (large Π), but allows for small violations of the exclusion restriction (γ is not exactly zero but close to zero). In our analysis, we boost the strength of the instruments by using a sparse model that identifies and uses optimal instruments and parsimonious controls. We then assess the validity of the exclusion restriction assumption and the robustness of our IV estimates to various degrees of exclusion restriction violation.

3.3.3 Approximately sparse regression models with many instruments and many controls

To obtain the sparse model, we estimate LASSO regressions in the framework of causal inference to select instruments and controls (Belloni et al., 2012; Belloni et al., 2014; Chernozhukov et al., 2015).[21] This approach provides consistent estimators by selecting the optimal set of instruments and a sparse set of controls with a data-driven choice of the penalty level, given the assumption of approximate sparsity. The sparsity assumption assumes that the conditional expectation of endogenous variables given the instruments can be approximated well by a parsimonious yet unknown set of variables. The approximate sparsity assumption imposes a restriction that only some of the variables have nonzero associated coefficients and permits a

[20] It also suggests that the weak instrument issue is study-specific and should be evaluated case by case.
[21] This method has been previously applied in studies examining the impacts of incarceration using judge's characteristics as instruments. For example, Mueller-Smith (2015), and Leslie and Pope (2017).

nonzero approximation error. As such, it allows for model selection mistakes in the LASSO regression. The validity of the sparsity assumption can be assessed by the super-score test (Belloni et al., 2012). Specifically, a variant of the LASSO estimator (Belloni et al., 2012; Belloni et al., 2014) was used to select instruments and controls, as follows:

$$\hat{\beta} = \arg\min \sum_{i=1}^{n} \left(y_i - \sum_{j=1}^{p} x_{i,j} \right)^2 + \lambda \sum_{j=1}^{p} |b_i| \gamma_j \qquad (7)$$

Where λ is the "penalty level" and γ_j is the "penalty loadings."[22] The penalty loadings are estimated from the data to ensure the equivalence of coefficients estimates to a rescaling of $x_{i,j}$ and to address heteroskedasticity, clustering, and non-normality in model errors. Similarly, standard errors are clustered at the respondent level to address within-subject correlation (recall we are using panel data).[23] Using the notation in Equations (2) and (3), we illustrate the algorithm for the "Post-Double-Selection (PDS)" methodology (Belloni et al., 2012; Belloni et al., 2011, 2014; Chernozhukov et al., 2015) as follows:

Step 1. Estimate a LASSO regression with the focal independent variable (X_{ics}) as the dependent variable and all potential instruments (Z_{cs}) and controls ($D_{ics}, \mu_c, \lambda_s, l_{sc}$) as regressors. Obtain a selected set of instruments and controls.

Step 2. Estimate a LASSO regression with the outcome variable (Y_{itcs}) as the dependent variable (X_{ics}) and all control variables ($D_{ics}, \mu_c, \lambda_s, l_{sc}$) as regressors. We specify the LASSO model to partial out theoretically important confounding variables in D_{ics}: female gender, age, age^2, age^3, parents' educational attainment, and survey wave dummies. In other words, all the models contain these confounding variables, but the LASSO chooses among the remaining potential controls (year of birth dummies, state of birth dummies, and state of birth specific trends). Obtain a selected set of controls.

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[22] The approach developed by Belloni et al. (2012), used in this paper, provides a data-driven method for choosing the penalty for the LASSO regression, not a user-specified penalty.

[23] In the specification checks section, we conducted a sensitivity analysis to cluster the standard errors at both state of birth and individual level.

Step 3. Estimate a LASSO regression with the focal independent variable (X_{ics}) as the dependent variable and all control variables ($D_{ics}, \mu_c, \lambda_s, l_{sc}$) as regressors. Similarly, the LASSO model partials out the variables in D_{ics}. Obtain a selected set of controls.

Step 4. Estimate a 2SLS regression using the selected set of instruments from Step 1 and the union of selected sets of controls from Steps 2 and 3. This produces a post-LASSO IV estimator (LASSO-IV). The post-LASSO estimator discards the LASSO coefficient estimates and refits the regression via two-stage least squares to alleviate LASSO's shrinkage bias.

Another closely related approach is the "Post-Regularization" methodology (Chernozhukov et al., 2015), which uses selected variables to construct orthogonalized versions of the dependent variable (Y_{itcs}) and focal independent variable (X_{ics}). We report the estimators based on this method as a robustness check.[24]

In addition, we follow Baum, Schaffer, and Stillman (2017) to get IV estimators in the context of the generalized methods of moments approach (GMM-IV) using optimal instruments and selected controls in the sparse model.[25] The GMM-IV approach is more efficient and provides various tests for the validity of instruments, particularly the over-identifying restrictions test in the setting of clustered standard errors (Baum, 2006; Cameron and Miller, 2015; Hoxby and Paserman, 1998). It reports the J statistic of Hansen (Hansen, 1982) to test for over-identifying restrictions on grouped data; a rejection of the null hypothesis suggests that either the instruments are not truly exogenous or are being incorrectly excluded from the regression.

3.3.4 Plausibly exogenous IV estimate (main IV estimates)

[24] We use the "*ivlasso*" packages to compute these two IV estimators.

[25] Compared to P2SLS, the approach of estimating instrumental variable estimators using GMM yields consistent but more efficient estimators. One reason is that P2SLS needs to reduce the dimension of instruments to the same level as the primary regressor. In contrast, GMM-IV does not need to, which is important in this study since many instruments are available. Also, in the presence of non-i.i.d assumption of errors, which is the case in this study, GMM estimators are more efficient than P2SLS estimators.

The exclusion restriction assumption is generally not directly testable. Previous studies have spent much effort arguing for the plausibility of the condition in particular settings or adding various controls to test the robustness of the IV estimators. Recently, placebo tests have become popular for assessing this assumption (Lal et al., 2021). The rationale is that if we can find a subgroup in which the first stage is zero ($\Pi = 0$), then the reduced-form effect of the instruments on the outcome (regress Y on Z) should also be zero if the exclusion restriction is satisfied (Bound and Jaeger, 2000). In other words, in this "zero-first-stage" subgroup, any correlation between the instruments and the outcome represents a direct effect of instruments and suggests a violation of the exclusion restriction.[26]

Admittedly, it is not always easy to find a plausible zero-first-stage group. Our prior analyses show that compulsory schooling laws have minor or null effects on individuals who eventually graduate from college, which could be used as the "zero-first-stage" subgroup. However, there is a concern with using this group: it is selected based on the outcome. Theoretically, compulsory schooling laws could have had effects on individuals who were not directly targeted by the law. For example, students kept in high school because of these laws might end up in college—indeed, Lang and Kropp (1986) find evidence of spillover effects. Instead, we constructed a "zero-first-stage" group as those with a high probability (>50%) of graduating from college based on predetermined characteristics. We use a logit model to predict the probability of graduating from college as a function of gender and dummies for parents' years of schooling, state of birth, and year of birth. The use of predicted college graduates also mitigates concerns from the sample selection of low-education individuals in this paper.

Despite various tests of the exclusion restriction, we acknowledge that the perfect exogeneity assumption ($\gamma = 0$) is unlikely to hold. To correct our IV estimates' robustness for various degrees of exclusion restriction failures (or exogeneity errors), we implement Conley et al. 's (2012) local-to-zero approach to relax the exact exclusion restriction assumption by allowing

[26] For example, Nunn and Wantchekon (2011) investigate whether the slave trade caused a culture of mistrust in Africa using the distance to slave trade ports as an instrument. In their study, the exclusion restriction requires that the distance only affects mistrust through the slave-trade channel. If the assumption holds, then the negative relationship between distance to coast and mistrust should not exist in other parts of the world without slave trades (the zero-first-stage group).

compulsory schooling laws to directly affect later-life hospitalizations. Conley et al. (2012) show how one can obtain a consistent estimate (plausibly exogenous IV estimate) when γ in Equation 4, measuring the degree of exclusion restriction failure, is close to zero but perhaps not exactly zero. Assuming $\gamma \sim N(\mu_\gamma, \Omega_\gamma)$, the plausibly exogenous estimate takes the following form:

$$\hat{\beta} \sim N\left(\beta_{2SLS} + A\mu_\gamma, W_{2SLS} + A\Omega_\gamma A'\right) \qquad (8)$$

where $N(\cdot)$ is a normal distribution, $A = (X'Z(Z'Z)^{-1}Z'X)^{-1}(X'Z)$, β_{2SLS} is the conventional 2SLS point estimate, and W_{2SLS} is the variance-covariance matrix.

However, Conley et al. (2012) are unclear about how to specify μ_γ and Ω_γ in empirical analysis.[27] We further followed van Kippersluis and Rietveld (2018) to obtain consistent estimates of μ_γ and Ω_γ from a reduced-form regression of Z on Y among the zero-first-stage subsample.[28] Compared with previous studies using state-level controls (e.g., percent employed in manufacturing, per capita education expenditures) and state-specific trends to adjust for variables related to both secondary schooling and health, the advantage of this approach is that it allows us to account for a wide variety of factors in any functional form.[29]

Finally, for all the above analyses using HRS data, we clustered standard errors at the respondent level to address the within-subject correlation and potential heterogeneity problems; fully robust standard errors are robust to arbitrary correlations and heterogeneity (Wooldridge, 2010). We also present estimates clustering at both individual and state birth levels.

[27] Conley et al. (2012) suggest replacing the conventional assumption $\gamma = 0$ with a user-specified assumption on a plausible value, range, or distribution of γ based on prior belief in domain knowledge.
[28] μ_γ contains the reduced-form regression coefficients ($\hat{\gamma}$) of Z on Y among the zero-first-stage subsample. $\Omega_\gamma = \left(0.125\ sqrt(S_0^2 + S_{-0}^2)\right)^2$, where S_0 and S_{-0} denotes the standard error of $\hat{\gamma}$ in the zero-first-stage group and the reminder of the analytic sample, respectively; see van Kippersluis and Rietveld (2018) for more details.
[29] We used the user-written command *plausexog* to implement this approach .

In sum, our main estimates use the instruments and the controls identified by the LASSO procedure and then correct for violations of the exogeneity assumption using the results from the zero-first-stage group.

4. Results on the relationship between education and hospitalization

4.1 The association between years of schooling and hospitalization

As a benchmark, in **Table 4**, we pool all data and estimate several variants of Equation 1 using OLS. Overall, the OLS estimates are fairly robust across models with different controls. Column (1) shows that an additional year of schooling is statistically significantly associated with 1.5 percentage points (pp) reduction in the probability of two-year hospitalization rates among middle-aged and older US white adults without any controls. The association decreases to 1.0 pp after adding gender, parents' educational attainment, age, and survey-wave dummies in Column (2), but the decrease is not statistically significant. Additionally controlling for state of birth and year of birth fixed effects in Column (3) yields similar estimates (0.9 pp). Column (4) further allows the year of birth effects to vary by state and produces similar estimates (1.0 pp). Interestingly, a large majority of the coefficients of the state-of-birth dummies, year-of-birth dummies, and state-level trends are not statistically significant.

We then repeat the analyses using a logit model and report the marginal effects in Columns (5) to (6). The marginal effects are only 0.1 pp lower relative to OLS estimates, and the difference is not statistically significant. Therefore, we proceeded with our IV analysis using a linear probability model.

4.2 IV estimates of the education effect on hospitalization

4.2.1 Optimal instruments and selected controls

The optimal instruments selected by the sparse model using the PDS methodology included Continuation Laws, CA_{st}, $CA7$, $CL8$, $RS6$, and $RS8$. Selected controls include all individual-level characteristics (female gender, age, age^2, age^3, parents' educational attainment, and survey wave dummies) and six state birth dummies (Illinois, New York, Ohio, Tennessee, Texas, and Virginia). No year of birth dummy was selected. The sparse model does not pick up other fixed effects because of their limited empirical impact on years of hospitalization and substantial effects on instrumental variables (years of compulsory schooling). [30] Indeed, results from a regression of hospitalization on the full set of controls show that none of the coefficients on state-of-birth specific linear trends is statistically significant at the 0.05 level, and only the state fixed effect of the District of Columbia (not others) is statistically significant (p=0.021). On the other hand, the vast majority of state fixed effects and state-specific trends (statistically) significantly predict years of compulsory schooling measured by required schooling (RS_{sct}). Thus, most of the control variables that were not selected only influence years of compulsory schooling (instrumental variables) but have limited impacts on hospitalizations (the outcome). As a result, including these controls in the model would substantially reduce the variation in our instruments and lead to weak instruments and larger standard errors (because they are empirically not associated with hospitalization after controlling for LASSO-selected controls).[31]

To further check the validity of the PDS LASSO model, we conducted a placebo test examining the effect of individuals' years of schooling on their parents' education: the causal effect should be zero and not statistically significant. We first estimated a 2SLS model without controls using the optimal set of instruments (Continuation Laws, CA_{st}, $CA7$, $CL8$, $RS6$, and $RS8$) selected by the PDS LASSO model. The 2SLS estimate on individuals' years of schooling is 0.77 (95% CI: 0.76, 0.79). Adding all controls (gender, age, age2, age3, survey wave dummies, state fixed

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[30] This is part of the approximate sparsity assumption of the PDS LASSO method. There are at least three aspects influencing which states are included by the LASSO regression: the magnitude of changes in compulsory schooling laws, the frequency of these changes, and the number of HRS respondents born in a state. We do observe large and frequent changes in compulsory schooling laws in these six states, for example, continuation laws for Tennessee (**Figure A2**), child attendance laws and required schooling for New York, Texas, and Virginia (**Figures A3 and A4**), and child labor laws for Illinois and Ohio (**Figure A5**). Additionally, a large proportion of HRS respondents were born in the six selected states.

[31] Other studies have also included state and cohort specific economic, education, and demographic conditions to account for state-level changes associated with both compulsory schooling laws and health outcomes. For example, Oreopoulos (2006) includes the fraction living in an urban city, living on a farm, black, in the labor force, and working in the manufacturing industry. When we incorporate these variables into our analysis, we find that none of them are associated with compulsory schooling laws and hospitalization with two exceptions: the fraction living in an urban city and living on a farm were statistically significantly related to hospitalization. Therefore, we do not include these variables and rather rely on the 'zero-first-group' as a comparison group to adjust for changes in state conditions over time.

effects, state-specific trends, and year of birth dummies) to the model yields a 2SLS estimate of -0.37 (95% CI: 0.67, 0.07). Applying the PDS LASSO method to select instruments and controls, we obtained a LASSO-IV estimate of 0.07 (95% Anderson-Rubin CI: -0.31, 0.25).[32] It selected the same set of optimal instruments and a slightly different set of controls compared to those selected when hospitalization is the outcome. This exercise corroborates the validity of our LASSO-IV approach.

Table 5 documents the results of the LASSO-selected optimal instruments from the first stage (regressing X on Z) and from the reduced form regression (regressing Y on Z) for predicted white college graduates and our primary analytic sample (whites with a high school degree or less). For predicted college graduates, the first-stage coefficients on the instruments were small and not statistically significant. The joint F statistic from the reduced-form regression among the zero-first-stage group is 0.57 and is not statistically significant, indicating no detectable violation of the exact exclusion restriction ($\gamma = 0$). This finding supports our use of predicted college graduates as the zero-first-stage group.[33]

4.2.2 Main results

Our main IV estimate, shown in Column (1) of **Table 6**, suggests that an extra year of schooling lowers the likelihood of hospitalizations by 2.6 percentage points (pp) (95% CI: -0.043, -0.009), which is statistically significant. The IV estimate is larger but not statistically different from the OLS estimate (1.0 pp; 95% CI: -0.013 to -0.008) with LASSO-selected controls (note that the OLS estimate with LASSO-selected controls is also the same as the OLS estimate with the full set of controls in Column (4) of **Table 4)**. This implies that it is sufficient to include only selected instruments and controls. The plausibly exogenous IV estimate that accounts for

[32] The effective first-stage F-statistic is 10.58, which is less than the 14.77 critical value with tao=10% and alpha=0.05.

[33] Our results also show more years of schooling required by continuation laws were associated with more years of completed schooling and a reduced probability of being hospitalized later in life. The interpretation of coefficients on dummies of compulsory schooling (CL8, CA7, RS6, and RS8) is not straightforward because of both the multicollinearity and the reference groups that include both more and fewer years of compulsory schooling. For example, due to multicollinearity, the coefficients on RS6 and RS8 are not in the expected direction, though not statistically significant. If we rerun the reduced form of the analytic sample with only RS6 and RS8 as instruments, the coefficient on RS6 is 0.001 (0.011), and the coefficient on RS8 is -0.004 (0.005). Nonetheless, **Figure 2** shows that violation of the monotonicity assumption is not a big concern.

possible exogenous bias using the zero-first-stage group is slightly smaller than the LASSO-IV and GMM-IV estimates in Columns (3) and (4) of **Table 6**.

Although compulsory schooling laws have been considered legitimate instruments and are widely used to estimate the causal effects of education on health, we assess whether the assumptions needed for their validity hold in our study using the set of LASSO-selected optimal instruments. Column (3) of **Table 6** shows that the effective first-stage F-statistic for the optimal instruments with selected controls is 19.21, which rejects the null hypothesis of weak instruments. Moreover, the super-score test (Belloni et al., 2012) rejected weak identification and supported the sparsity assumption. This supports the relevance assumption. The LASSO-IV estimate is -0.046 (95% CI: -0.067 to -0.025) and statistically significant, implying that one additional year of schooling is related to 4.6 pp more reductions in two-year hospitalization rates.[34] The IV estimate on education from the "post-Regularization" methodology using post-LASSO orthogonalized variables (Chernozhukov et al., 2015) is -0.023 (95% CI: -0.052 to -0.049).

Since we cluster standard errors at the respondent level to account for within-subject correlation, the over-identification restrictions test must be computed following the cluster version of the two-step GMM estimation (Cameron and Miller, 2015; Hoxby and Paserman, 1998). We thus relied on the GMM model for testing the over-identification assumption.[35] The GMM approach also reports results on various tests of IV assumptions. We described them here as well. The GMM-IV estimate for schooling (β=-0.044, 95% CI: -0.0.065 to -0.022) in Column (4) of **Table 6** is very close to our LASSO-IV estimate. The Hansen J statistic is 8.19 (p = 0.146), suggesting that the instruments satisfy the over-identification restrictions, which supports the independence assumption for our instruments. In addition, GMM-IV tests for weak instrument-robust inference

[34] The weak-instrument-robust Anderson-Rubin 95% confidence set is (-0.071, -0.026), which is similar to the 95% Wald confidence interval (-0.067 to -0.025). It further confirms that our instruments are not weak.

[35] Tests for over-identifying restrictions after the 2SLS estimation include Sargan's (1958) and Basman's (1960) χ^2 tests and Woodridge's (1995) robust score test when robust standard errors were used. However, robust tests of over-identifying restrictions after the 2SLS estimation are not available with clustered standard errors (Stata, 2015) . Instead, if GMM estimator was used, Hansen's (1982) J statistic χ^2 test is reported, which is also available for GMM estimator with clustered standard errors. We clustered our standard errors at the individual level because the use of the HRS panel data.

using an Anderson-Rubin Wald test; the χ^2 statistic for this test is 28.33 ($p < 0.001$) and provides consistent evidence that the set of instruments is not subject to weak instrument bias.

In addition, we note that Keane and Neal (2023) show that the size inflation in the 2SLS t-test could be severe in over-identified models and strongly recommend the use of the Limited Information Maximum Likelihood (LIML) estimator combined with the conditional likelihood ratio test (CLR). Our LIML-IV estimate is -0.046 with a 95% CLR confidence interval of (-0.072, -0.026), which is similar to our Lasso-IV estimate.[36]

4.3 Specification checks

We conducted several sensitivity analyses to assess the robustness of our IV estimates to the various model specifications.

First, our primary analyses cluster regression standard errors at the individual level to address repeated measures among the HRS respondents. However, this approach does not account for the correlations across birth cohorts within states with different compulsory schooling laws. We conducted a sensitivity analysis by clustering standard errors at both the individual and state of birth levels, which is allowed in both Baum, Schaffer, and Stillman (2017) for IV estimation and Clarke Matta (2018) for plausibly exogenous IV estimation. To obtain two-way clustered standard errors for the first stage and reduced-form OLS estimation, we used methods proposed for models with high-dimensional fixed effects (Gaure, 2010; Guimaraes and Portugal, 2010).[37] Column (1) of **Table 7** reports the results and shows no significant changes.

As another robustness check, we collapsed our longitudinal data at the individual level to obtain one observation per person. We constructed a new measure for hospitalization, as the number of HRS survey waves a person reported ever being hospitalized divided by the number of waves the person responded to HRS. This reflects the average likelihood of hospitalization. We then

[36] Following Keane and Neal (2023), we used *"ivreg2"* and *"weakiv"* packages.
[37] We use the Stata command "*reghdfe*" to implement cluster OLS regression standard errors at both the individual and state of birth levels.

obtained 2SLS estimates using LASSO-selected instruments and controls and clustered standard errors at the state of birth level. The results are presented in Column (2) of **Table 7**. The main IV estimate ($\beta = -0.030$, $P < 0.01$) is similar to our primary results ($\beta = -0.026, \mathrm{P} < 0.01$).

Second, given the well-established relationships between these early life conditions and both educational attainment and health (Case et al., 2002; Currie, 2009), it is important to assess the potential omitted variable biases related to childhood health and family situation. If the instruments are truly exogenous, they should be insensitive to the inclusion of these controls. On the other hand, if CSLs are more likely to be implemented in places where other policies affecting health (such as water sanitation or vaccinations), then the IV estimates will be affected by their inclusion. The HRS asks respondents to recall their childhood health and financial situation. For example, *childhood family financial situation* (the question in HRS is asked as "Now think about your family when you were growing up, from birth to age 16. Would you say your family during that time was pretty well off financially, about average, or poor?" We recreated it as a dummy variable, which is set to 1 if the response is "poor" and 0 otherwise). The HRS has a question about respondents' *self-rated child health* status. The question asked in the HRS was, "Consider your health while you were growing up before you were 16 years old. Would you say that your health during that time was excellent, very good, good, fair, or poor?" It is available from Wave 4. This question was included as a proxy for the concept of child health as an ordinal variable (1=excellent, 2=very good, 3=good, 4=fair, and 5=poor). In this sensitivity analysis, we additionally controlled for the two measures of childhood health and childhood family socioeconomic conditions. Note that since these measures refer to the time when respondents were up to 16, some might argue that these early life conditions are after the effective compulsory schooling laws were applied, and so they are potential outcomes. Nonetheless, Column (3) of **Table 7** shows no material changes in any of the estimates.

Third, we used an alternative measure of hospitalization. Our measure of hospitalization in the primary analyses included hospitalizations due to any cause. However, some hospitalizations are not preventable and are not likely affected by educational attainment.[38] One example is

[38] Our data does not allow us to compute preventable hospitalizations that could have been avoided had the disease conditions been managed successfully by primary care providers in outpatient settings.

hospitalizations related to terminal conditions, which lead to imminent mortality. Following the spirit of a prior study (Behrman et al., 2011), we constructed a relevant outcome—hospitalizations other than those occurring in the two years before death—to ignore hospitalizations from terminal conditions. Column (4) of **Table 7** reports the estimates, which are consistent with our primary findings. HRS data does not allow us to distinguish preventive hospitalizations from curative hospitalizations. Admittedly, some types of hospitalizations (e.g., preventive surgeries like mastectomy for breast cancer, drug or alcohol detoxification programs that require close medical supervision, or observations for symptoms that could potentially evolve into a serious condition) may be necessary and used for preventive purposes. Prior studies have also found that people with higher educational attainment use more preventive care (Fletcher and Frisvold, 2009; Kenkel, 2006). Thus, our estimate of education on health could be biased downwards. However, it is not a big concern because hospitalizations are usually reserved for severe cases (McDermott and Roemer, 2021; Salah et al., 2021). Nevertheless, our estimates reflect the effect of schooling on overall hospitalizations.

Lastly, we assess the impact of selective mortality on our estimates, which is well-known in the literature to cause bias in the education-health gradient (Beckett, 2000; House et al., 1994; Kaestner et al., 2020; Lynch, 2003). Because HRS primarily includes middle-aged and older adults, we observe people only if they survived to middle age and became eligible for the HRS survey. Prior research has shown people with more years of schooling were more likely to survive and less likely to be hospitalized (Arendt, 2008; Behrman et al., 2011; Galama et al., 2018; Lleras-Muney, 2005; Yue et al., 2021). If we could include more individuals at younger ages, we would have more people with fewer years of schooling who use more hospitalizations. Therefore, the hospitalization gap between those with more and less schooling would grow. Since the HRS only includes relatively older adults, this sample selection will likely attenuate our estimates of education's effect on hospitalization towards zero. Selective mortality also causes the sample we observe to shrink over time, even among those that are included in HRS at baseline. Since the lower educated die at younger ages, the education gaps in hospitalization will also shrink as a result of selective mortality within the panel. Previous work has documented these patterns and found that in the HRS, attrition leads to an underestimated effect of education on hospitalization (Yue et al., 2023).

Conceptually, it is not entirely clear that we want to "correct" this bias from selective mortality: we only care about gaps among the living. However, one could argue that it is interesting to know what the gap in hospitalizations would have been had the less educated survived. In particular, one might wonder what the education gradient in hospitalizations would be as a result of CSLs, which might also affect mortality. To address this, we implement two approaches.

We first checked the sensitivity of the results to dropping the oldest cohorts. Note that this is not without a cost: the CSLs have been shown to have been more effective for these cohorts. Additionally, we lose variation in the CSLs to estimate our effects. Nevertheless, this provides a useful check in terms of the magnitude of the estimates. Column (5) of **Table 7** shows our estimates become slightly larger after dropping the oldest cohorts (1905-1914); the plausibly exogenous IV estimate is -0.033, P<0.01. This coefficient is indeed larger, consistent with the findings in the literature that selective mortality causes estimates to be downward biased (Beckett, 2000; Kaestner et al., 2020; Lynch, 2003). However, the difference in the coefficients (-0.033 with birth cohorts 1915-1959 vs. -0.026 with all cohorts 1905-1959) is not statistically significant.

We then used an inverse-probability-weighting (IPW) approach to obtain estimates that would have been observed had dropouts remained in the survey. This is a standard method used in many contexts to account for differential attrition (Bailey et al., 2020), including in studies that investigate the effect of education on health measures (Weuve et al., 2012; Yue et al., 2022). To obtain the IPW weights, we separately estimated the probabilities of being alive and uncensored in hospitalizations as a function of individual characteristics (one-wave lagged variables: female gender, marital status, census region of residence, number of people in the household, number of living children, whether a proxy conducted the interview, self-reported health status, body mass index (BMI), ever had cancer or heart disease or stroke or chronic lung disease, ever had hypertension or diabetes or emotional problems, educational attainment, house ownership, labor force status, individual earnings, and total household income).[39] We multiplied the probabilities and took the inverse as weights, thus adjusting for the attrition bias. The results of this exercise

[39] All these variables were statistically significantly associated with the probability of attrition at the 0.05 level.

are presented in Column (6) of **Table 7**. It increases the schooling effect for OLS and decreases the effect size for the plausibly exogenous IV estimate $\beta = -0.021, P < 0.01$, which is similar to our main results. Overall, any bias from the HRS sample selection and attrition will attenuate our estimates toward zero.

In summary, our main estimate based on plausibly exogenous methods is robust to all specification checks and indicates a significant negative effect of more years of schooling on the probability of being hospitalized in the middle and old life stages.

4.4 Robustness to violations of perfect exogeneity

In this section, we assess the robustness of our estimates to various degrees of exclusion restriction violations using Conley et al. 's (2012) framework for plausibly exogenous IV estimations. Specifically, we assess the robustness of our IV estimates by setting the degree of exclusion restriction violations (or the exogeneity error), measured by γ, to various percentages of the reduced form impacts of compulsory schooling laws on hospitalizations in our primary analytic sample. The results of this exercise are documented in Panel A of **Table 8**. It shows that the plausibly exogenous IV estimate decreases as the exogeneity error increases from 0% to 100% of the reduced form impacts. With an exogeneity error equal to zero, the plausible exogenous IV is -0.046, which is the same as LASSO-IV, assuming no exclusion restriction violations ($\gamma = 0$). The plausibly exogenous IV estimate remains negative and statistically significant at the 5% level until the exogeneity error reaches 60% and becomes close to zero only when the exogeneity error is 100%.[40]

We then draw plausible values of exogeneity errors from the reduced form among two subgroups: white college graduates and blacks with a high school degree or less. In other words, given that previous studies suggest that compulsory schooling laws have smaller (but not null) effects on years of schooling among these two groups, we consider the two subgroups as

[40] Ding et al. (2009) investigate the impact of poor health on academic performance using genetic markers as instruments for health outcomes. They conducted a similar exercise to assess their IV estimate's robustness to various degrees of exclusion restriction violations. Their IV estimate loses statistical significance after the exogeneity error reaches 50% of the reduced form impacts.

comparison groups (Fletcher, 2015; Lleras-Muney, 2002). This exercise assumes the impact of instruments on hospitalizations through channels other than years of schooling—exogeneity errors—among our primary sample amount to the reduced-form effects of instruments on hospitalizations among the two subgroups. The results of this sensitivity analysis are presented in Panel B of **Table 8,** with details regarding the first stage and the reduced-form regressions among these subgroups reported in **Table A2**. First, for white college graduates, compulsory schooling laws have minor and significant effects on education but a small and insignificant direct effect on hospitalizations. Incorporating the reduced-form impacts among white college graduates as exogeneity errors, the plausibly exogenous IV (β=-0.056, p < -0.01) becomes larger than that in our main analysis. Second, we can use Blacks with low education levels. Although prior studies document limited impacts of compulsory schooling laws among Blacks born between 1901-1925 (Lleras-Muney, 2002), our analyses of HRS and census data suggest that these laws might influence Blacks with a high school degree or less (see **Figures A6 and A7**).[41] Our results reveal significant and slightly smaller effects of these laws on both education and hospitalizations among low-educated Blacks compared to the low-educated whites included in our primary analyses. Including instrument coefficients from the reduced-form regression among low-educated Blacks, the plausibly exogenous IV (β=-0.016, p<0.01) is slightly smaller than that in the IV estimates in our primary analyses but remains statistically significant.

Based on these analyses, we conclude that even allowing for plausible amounts of imperfect exogeneity, we find a negative and statistically significant effect of schooling on hospitalization.

4.5 Comparison to OLS

Our main IV estimate ($\beta = -0.026$) is more than twice that of the OLS estimate ($\beta = -0.010$). Although the two estimates are not statistically significantly different from each other, this still raises the question of why the IV point estimate is larger. The previous section rules out weak instruments and violations of the exclusion restriction as explanations. In this section, we explore two additional explanations for the divergence between OLS and IV estimates.

[41] We use the IPUMS 1960 Census 1% sample, IPUMS 1970 Census 1% state samples, and IPUMS 1980 Census 5% sample to examine the impact of compulsory schooling laws on education (see **Figure A7**).

Omitted variable bias. The OLS estimates suffer from selection bias due to unobservables correlated with secondary schooling and hospitalization. To assess the likelihood that the estimates are biased by unobservables, we use the method proposed by Altonji, Elder, and Taber (2005) that one can use selection on observables to assess the potential bias from unobservables. To implement the method, we first obtain the regression coefficient ($\hat{\beta}^R$) of schooling on hospitalization with a restricted set of controls and then obtain the regression coefficient ($\hat{\beta}^F$) with a full set of controls. The ratio $\hat{\beta}^F/(\hat{\beta}^R - \hat{\beta}^F)$, is used to gauge how much stronger unobservables, relative to selection on observations, must fully account for the full estimated effect (Nunn and Wantchekon, 2011). The larger the numerator, $\hat{\beta}^F$, the greater the effect that needs to be explained by the selection of unobservables. The smaller the denominator, ($\hat{\beta}^R - \hat{\beta}^F$), the less the estimate is explained by the selection on observables; thus, we need a stronger selection on unobservables to explain the entire effect. For this exercise, we set the restricted set of covariates as in Column (1) of **Table 4** with no controls ($\hat{\beta}^R$=-0.015, p<0.01). The set of full covariates included all individual controls, state fixed effects, year of birth fixed effects, and state-of-birth-specific linear trends, as shown in Column (4) of **Table 4** ($\hat{\beta}^F$=-0.010, p<0.01). The corresponding ratio is two. It indicates that to explain the entire OLS estimate, the selection of unobservables would have to be twice the selection of observables. Therefore, it is likely that the estimated association between schooling and hospitalization based on OLS is fully driven by the unobservables. In other words, selection bias is plausible in the OLS estimates, which contributes to part of the divergence between the OLS and IV estimates. But why is OLS downward biased when we compare it to IV? The inclusion of controls lowers the OLS estimate, suggesting that unobserved variables lead OLS to be over-estimated. Thus, this seems an unlikely explanation for the results.

Local average treatment effects. A different explanation for the difference between OLS and IV estimates is that the effect of education on health differs within the population: in the presence of treatment effect heterogeneity, the OLS estimate reflects the average treatment effect among the population, whereas the IV estimate that captures the average treatment effect only among those affected by CSLs (compliers).[42] To investigate this issue we first attempt to identify compliers and then we investigate if the OLS coefficient differs for compliers and others, as suggested by previous work.[43]

It is impossible to precisely identify who was affected by CSLs (the compliers). However, these laws targeted individuals who were not likely to obtain a high school degree. Previous work has shown that the individuals most affected were at the margin – individuals obtaining 7 years of schooling would be affected by a law requiring 8, but those obtaining only 5 prior to the law would be much less affected (Imbens and Angrist, 1994; Imbens and Rubin, 1997). This is what a model of compliance would predict.[44] We identify likely compliers by estimating the probability that an individual would have obtained schooling in the CSL ranges (between 5 and 10 years of school). Alternatively, we estimate the probability that an individual would obtain less than secondary school (< 9 years of schooling). We do this by estimating a logit regression where the outcome is a dummy variable for having 5-10 years of school (or a dummy for having less than 9 years of school), and the explanatory variables include all predetermined characteristics (observed before age 5) observed in the HRS, including parental education, state of birth, year of birth and gender. We predict the probability of these events for all individuals. Then, we estimate OLS regressions of hospitalization on education for different groups based on their predicted probability of belonging to the complier group.

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[42] Recently, Blandhol et al. (2022) pointed out that the local average treatment effect (LATE) interpretation of 2SLS IV estimates was initially established for a model without covariates (Imbens and Angrist, 1994). The LATE interpretation only applies for 2SLS estimates with covariates when all covariates are saturated. For example, a 2SLS specification that fully interacts with both the treatment and instruments. However, Blandhol et al. (2022) also show that such interactions inevitably lead to "many instruments" bias. Nonetheless, when we used the selected set of instruments but excluded all covariates, the 2SLS IV estimate was similar (β=-0.032, $p<0.01$).

[43] Marbach and Hangartner (2020) propose a method to profile compliers and non-compliers but only for IV analyses with a binary treatment variable. Dahal et al. (2014) and Dobbie et al. (2018) characterized compliers with a single continuous instrumental variable. Since multiple instrumental variables are included in our IV analysis, we did not profile compliers. Instead, we compare our IV estimate with the OLS estimate from potential compliers; the HRS data allow us to predict potential compliers using early childhood conditions. As concluded by Sloczynski (2021) and Abadie (2003), under assumptions of relevance, independence, exclusion restriction, strong monotonicity, and saturated covariates, the 2SLS IV estimate is equivalent to the OLS estimate of outcome on primary independent variable adjusting for covariates among compliers.

[44] If an individual was obtaining only 7 years of school, they had judged that, in their case, the benefit of an extra year of school was outweighed by the cost of attending. When a law passes requiring individuals to obtain 8 years of school, this increases the benefit of attending school because individuals can avoid the fine this way. If the penalty for non-compliance is small, only those close to the margin find it worthwhile to comply. As the penalty increases, more and more people will comply.

The results of this exercise are summarized In **Figure 3**. **Panel A of Figure 3** plots OLS estimates among those with a predicted probability of having 5-10 years of schooling larger than or equal to various critical values. It shows that individuals who were more likely to be compliers had larger OLS estimates, with 95% confidence intervals covering our main IV estimates. Results are similar if we consider those that are predicted to have less than secondary school as potential compliers (**Panel B of Figure 3)**. Therefore, the differences between OLS and IV estimates may be entirely attributable to the effect of the heterogeneity of schooling across various segments of the population. These results would be consistent with a model in which education matters more for low levels of schooling (the relationship between health and education is concave), for example. Alternatively, it could also be the case that compliers were more likely to come from certain groups such as immigrant families (CSL were in part targeted towards immigrants): these children benefitted more than natives from schooling because school helped them to assimilate.[45]

4.6. Sex differences

Given the well-documented differential returns to education by sex (Arendt, 2008; Lleras-Muney et al., 2022), we conduct subgroup analysis by sex using selected instruments and controls.[46] We find the effect of education on hospitalization is only statistically significant for women ($\beta =$ - 4.0 pp, 95% CI: -6.1 pp to -1.8 pp) not for men ($\beta =$ - 0.8 pp, 95% CI: - 4.9 to 0.8) (see **Table A3**). The results are consistent with Arendt (2008) using Danish data. It is worth noting that we failed to reject the null hypothesis of weak instruments for both women and men, primarily due to the diminished sample size; the effective first-stage F statistic is 11.98 for women and 7.90 for men. However, we find the weak-instrument-robust Anderson-Rubin 95% confidence sets (-0.112, -0.033) are very close to the 95% Wald confidence interval (-0.091, -0.029) for the Lasso-IV estimates, suggesting weak instrument bias only leads to limited size distortions. Yet, it is hard to compute the weak-instrument-robust confidence sets for our plausibly exogenous IV

[45] Lleras-Muney and Schretzer (2015) show that "keeping children in school by increasing the age required to obtain a work permit resulted in higher educational attainment and earnings, and the effects are larger for immigrant children than for natives." We cannot verify this hypothesis in our study because HRS does not ask about the immigration status of the respondents' parents.

[46] Results are similar if we select optimal instruments and controls separately by sex.

estimates.[47] Therefore, unlike our main estimates, the IV estimates by sex should be considered suggestive.

5. Effects of education on common causes of hospitalizations, self-reported health, and mortality

In this section, we apply our optimal instruments and selected controls to explore the effect of education on other measures of healthcare utilization and health outcomes (**Table A4**).[48] We briefly describe our main IV estimates—plausibly exogenous IV—here.

5.1. Number of hospital stays, hospital nights, and common causes of hospitalizations

Like our main results on hospitalization, we found a significant (negative) effect of more years of schooling on both the number of hospital stays ($\beta =$ -0.093, $p < 0.01$) and the number of hospitalization nights over all stays ($\beta =$ -0.541, $p < 0.001$).

We investigated the effect of education on common health conditions causing hospitalizations. Among the 15 most frequent conditions leading to hospitalizations shown in Russo and Elixhauser (2006) among HRS respondents, [49] we can identify most of these conditions reported

[47] Alternatively, Lee et al. (2022) propose a *tF* adjustment factor for constructing the t-ratio for testing based on the first F statistic. We tentatively apply the adjustment for our plausibly exogenous IV. For women, the F-statistic is 10.66. Following Table 3A of Lee et al. (2022), the *tF* adjustment factor is 1.688 + (10.711-10.66)/(10.66-10.253)×(1.727-1.688)=1.693. Then, the 0.05 *tF* standard error for the plausibly exogenous IV for women is 1.693×0.011=0.019; the corresponding 95% *tF* confidence interval is (-0.077, -0.003). Similarly, the 95% *tF* confidence interval of the plausibly exogenous IV estimate for men is (-0.041, 0.025). However, the *tF* adjustment is limited to the case of the single instrument IV model. The extent to which it can be applied to over-identified models remains unknown.

[48] Results are similar when we use outcome-specific optimal instruments and controls, with one exception for mortality. It is worth noting that for the ten-year mortality outcome, our estimation of the plausibly exogenous IV is compromised by the small sample size and limited variation in vital status. In order to gain a reliable estimate of exclusion restriction failure (λ), we have to use both predicted college graduates and college graduates as the zero-first-stage group. The plausibly exogenous IV (β=-0.061; 95% CI: -0.097, -0.025) is close to our estimate in **Table A4**. Nonetheless, it confirms the prior literature that we need a large sample size to estimate the effect of schooling on mortality.

[49] The 15 most frequent conditions causing hospitalizations among older adults in 2003 include 1) congestive heart failure; 2) Pneumonia; 3) Coronary atherosclerosis; 4) Cardiac dysrhythmias; 5) Acute myocardial infarction; 6) Chronic obstructive pulmonary disease; 7) Stroke; 8) Osteoarthritis; 9) Rehabilitation care, fitting of prostheses, and adjustment of devices; 10) Fluid and electrolyte disorders; 11) Chest pain; 12) Urinary tract infections; 13) Hip fracture; 14) Complication of medical device, implant, or graft; 15) Septicemia.

in the HRS data. We analyzed the following self-reported health conditions that were ever diagnosed by doctors: heart problems (including heart attack, coronary heart disease, angina, congestive heart failure, or other heart problems), lung problems (including chronic lung disease (except asthma) such as chronic bronchitis or emphysema attack), stroke or transient ischemic, and arthritis or rheumatism.[50] Our IV results show a larger effect for women than men on heart problems; among them, many are among the top most common causes of hospitalizations in the US (Russo and Elixhauser, 2006; Salah et al., 2021). Our main plausibly exogenous IV estimates suggest that one additional year of schooling is related to reductions in the probability of ever having heart problems ($\beta =$-0.113), lung problems ($\beta =$-0.014), and arthritis or rheumatism ($\beta =$-0.017), which are all statistically significant. We do not find any significant effect of education on stroke or transient ischemic.

5.2 Self-reported health and ten-year mortality

We then turn to estimate the effect of schooling on health status. We first use respondents' self-reported health status (1 = excellent, 2 = very good, 3 = good, 4 = fair, 5 = poor) as a general measure of health status, as it is a powerful predictor of mortality and long term patterns of hospitalizations (Case et al., 2002; Kennedy et al., 2001). We use it as a continuous variable and find more years of schooling improves health status ($\beta =$ -0.263, $p < 0.01$), which is close to the significant IV estimate on self-reported health ($\beta =$-0.229) reported in Mazumder (2008). One additional year of schooling significantly reduces the probability of reporting fair or poor health by 9.7 percentage points.

Furthermore, to examine the effect of schooling on ten-year mortality, we restricted the sample to those who were first interviewed in 2006 or earlier so that we could track the respondent's vital status over 10 years. Among the 11,675 included samples, the average age is 63.3, the median year of birth is 1932, and the average ten-year mortality is 24.9%. Our results indicate

[50] Najafi et al. (2019) have documented that the accuracy of self-reported chronic conditions was higher for cardiovascular diseases and low for diabetes and particularly hypertension because of low awareness. Bergmann et al. (1998) compared interview reports with hospitalization records and showed the self-report accuracy was high for heart problems and moderate for lung problems and stroke. The authors also reported that the self-report accuracy improves with a higher level of education.

that one additional year of schooling significantly reduces ten-year mortality by six percentage points (β = - 0.060, p<0.001).

5.3 Revisiting education's effect on mortality using Census data.

Finally, we investigate whether our empirical strategy could improve the estimation of education's effect on mortality and address the inconclusive results in prior literature: the instrumental variable (IV) estimates are large but imprecise.

We employed the LASSO-IV approach to revisit Lleras-Muney (2005)'s estimation of the relationship between education and ten-year mortality using the 1960, 1970, and 1980 censuses. The first two rows of **Table A5** document the original estimates. We successfully replicated the results using data from 1960, 1970, and 1980 1 percent IPUMS censuses. Both our Weighted Least Square (WLS= -0.035) and 2SLS IV (IV=-0.067) estimates, shown in the third row of **Table A5**, are similar to the corrected estimates reported by Lleras-Muney (2006).[51] In the fourth row, we then replaced interactions between the birth region and cohorts with state-of-birth specific linear trends from the model, and found the WLS estimates (WLS=-0.037) remained unchanged, but the IV estimate (IV=-0.052), while large, becomes statistically insignificant. We hypothesize this result is caused by the fact that the variation left in the instruments after including linear trends is small (the first-stage F statistic is only 1.19 in this analysis), which causes the instrument to be weak and increases the standard error of the IV point estimate.[52]

In the fifth row of **Table A5**, we applied our LASSO regression methods. We reported WLS estimates with selected controls and IV estimates using both selected IVs and controls. The LASSO regressions selected only two instruments (10 years of compulsory schooling laws and continuation laws) among the instruments used by Lleras-Muney (2005). Although the WLS estimate (WLS=-0.044) of this exercise is slightly larger, the IV estimate (IV=-0.010) with a first-stage F statistic of 24.17 becomes much smaller and not statistically significant. Finally, we

[51] Lleras-Muney (2006) is an erratum that provides corrected WLS and IV estimates of Lleras-Muney (2005).
[52] Mazumder (2008) did not report the first-stage F statistic for the IV analyses adjusting for state of birth specific linear trends.

allow LASSO regression to select instruments from the broader and updated set of instruments compiled and constructed in this study (section 3.2.2). The LASSO regressions select five instruments. Using these instruments, we find that the IV estimate is -0.043 and statistically significant, indicated by the weak-instrument-robust Anderson-Rubin 95% confidence sets (-0.072, -0.004).[53] The IV estimate is similar to the WLS estimate (WLS=-0.047) with LASSO-selected controls. Unfortunately, there is not enough data on individuals' pre-determined characteristics to create a zero-first-stage group for this education-mortality analysis because the census does not have many characteristics of individuals that would be pre-determined prior to entering school. Unlike the HRS, it does not ask respondents to report their parents' education, a key covariate to predict who will likely be affected by compulsory schooling laws. Nevertheless, the LASSO-IV does select a subset of controls and instruments that are more parsimonious, resulting in a significantly stronger first stage. This exercise demonstrates that the LASSO methods can be successfully used to improve the estimation to have a stronger first stage.

6. Conclusion and Policy Implications

Our results suggest that additional years of schooling significantly reduced the likelihood of two-year hospitalizations among low-education whites born in the continental United States between 1905 and 1959. Our main IV estimates based on LASSO-selected instruments and controls suggest that one extra year of schooling significantly lowers the two-year probability of later-life hospitalizations by 2.6 percentage points (a 9.5% reduction relative to the mean level of hospitalization). Our estimate is robust to various robustness checks. Due to the HRS sample selection, our estimate of education effect on hospitalization should be generalized only to those who survive up to middle age.

Our finding of a significant education effect on hospitalization contrasts with the small/null results on the education-mortality relationship (Galama et al., 2018; Hamad et al., 2018). Our study takes full advantage of previously constructed instruments using compulsory schooling

[53] The first-stage F statistic on selected instruments is 15.86. Since we weight the 2SLS regression by the number of people in each cell, the *"weakivtest"* does not report effective first-stage F-statistic. Instead, we report the weak-instrument Anderson-Rubin confidence set, which is close to the 95% Wald confidence interval.

laws; however, our empirical strategy allows us to use a parsimonious model with optimal instruments. This approach enables our IV estimates to be precise. In addition, using hospitalization as an outcome may better capture the effects of education on health. Indeed, our results are consistent with those of prior studies showing a significant impact of education on health outcomes. For example, Fletcher (2015) reported significant educational effects on several health outcomes, such as self-reported health, cardiovascular outcomes, and weight outcomes.

Our findings differ from the results of another Danish study that used a within-twins study design. The study found a strong negative association between education and hospitalization but failed to detect any significant causal effects (Behrman et al., 2011). The authors further suggested that education might be a marker for parental family and individual endowments. However, this is not the case for the US sample (Lundborg, 2013). It is worth noting that although the within-twin design can account for family background and/or genetics, it cannot control for other confounding variables, such as those policy changes that influence twins' education opportunities and hospitalization differently. In addition, the family fixed effects identifying samples might have different characteristics from other samples and exhibit a different treatment effect (Miller et al., 2021).

Our results have several policy implications. Our study contributes to the growing body of literature on the social determinants of health. Our LASSO-selected optimal instruments pick up compulsory schooling law changes that best predict years of schooling. Our reduced form results suggest such law changes (continuation law, child labor law, and child attendance law) also had significant impacts on individuals' long-term health outcomes. As health policymakers and researchers seek solutions to reduce healthcare costs and health disparities associated with socioeconomic status, policy reforms that address the social determinants of health could be an effective option. Moreover, educational attainment is highly related to other socioeconomic factors such as income, wealth, and occupation; thus, the results of this study could indicate how social factors could be used as policy levers to improve health and reduce healthcare costs. In a broader context, investment in the educational system could be a cost-effective way to reduce intense healthcare use and healthcare costs.

Our study also provides rigorous evidence for current policy reforms that integrate social factors into the healthcare delivery system. One notable example is the ongoing US value-based payment reforms, which aim to shift the focus of care from quantity to quality by financially penalizing or rewarding healthcare providers based on their patients' health outcomes. However, since socially disadvantaged patients, such as those with less educational attainment, are often concentrated among a subset of providers, the quality of care from these providers would be underestimated if patients' characteristics were not appropriately adjusted. A recent report concluded that incorporating social factors into Medicare payment schemes would have great implications for quality improvements and cost control (National Academies of Sciences and Medicine, 2017). Simultaneously, it also highlights the absence of rigorous empirical evidence. Our results support these risk-adjustment models. Given the significant causal effect of education on hospitalization, educational attainment should be considered an important social factor in the risk adjustment model for value-based payment schemes. Admittedly, educational attainment is not readily available in Medicare and Medicaid claims data, which precludes such a practice. As most Medicare enrollees are aged 65 years or above, educational attainment can be collected in the enrollment stage. Given our results, collecting data on patients' educational attainment at enrollment will facilitate more effective risk adjustment for payment reforms and more targeted resource allocation.

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Table 1. Summary statistics of the Health and Retirement Study (HRS) sample

	Mean	Overall Standard Deviation
The probability of hospitalizations	0.275	0.447
Years of completed education	10.932	1.927
Female	0.605	0.489
Age	68.514	11.004
Parents' years of completed education	9.336	2.925
Parent educational attainment		
Less than high school	0.665	0.472
High school	0.265	0.441
Some college	0.041	0.198
College and above	0.029	0.167
Year of birth		
1905-1910	0.019	0.136
1911-1920	0.121	0.326
1921-1930	0.202	0.401
1931-1940	0.385	0.487
1941-1950	0.199	0.399
1951-1959	0.075	0.263
Region of birth		
South	0.334	0.472
Midwest	0.361	0.480
Northeast	0.221	0.415
West	0.084	0.277
Compulsory schooling laws		
Continuation laws	0.637	0.481
By Child labor laws	7.831	1.145
By child attendance laws	9.659	1.724
By required schooling	8.245	1.117
Persons	12,528	
Observations	85,887	

Notes: The analytic sample was restricted to white respondents in the 1992-2016 Health and Retirement Study who were born in the continental United States between 1905 and 1959. We also restricted to those with a high school degree or less. All variables are time-invariant except the probability of hospitalizations and age.

Table 2. Summary statistics of compulsory schooling laws

	Raw Data	Control for State & Year FE		Control for Region Trends		Control for State Trends	
	Mean	Mean	↓ SD %	Mean	↓ SD %	Mean	↓ SD %
Continuation laws	0.5 (0.5)	0(0.3)	-48.0	0 (0.3)	-50.0	0 (0.2)	-62.0
Schooling by child labor laws	7.6 (1.4)	0 (1.2)	-14.7	0 (1.2)	-14.7	0 (1.1)	-26.6
Schooling by child attendance laws	9.4 (1.9)	0 (1.4)	-26.4	0 (1.4)	-26.4	0 (1.0)	-46.6
Dropout age	14.4 (1.6)	0 (1.3)	-23.8	0 (1.2)	-25.0	0 (1.2)	-29.3
Required schooling	8.0 (1.4)	0 (1.0)	-29.9	0 (0.9)	-31.4	0 (0.7)	-49.6

Notes: Standard deviations in parentheses. The numbers for "control for State & Year FE" were the mean and standard deviation of predicted residuals from regressing laws on state-of-birth and year-of-birth dummies. The numbers for "Control for Region Trends" were the mean and standard deviation of predicted residuals from regressions with state-of-birth dummies, year-of-birth dummies, and region-specific linear time trends. Similarly, the numbers for "control for Region Trends" were the mean and standard deviation of predicted residuals from regressions with state-of-birth dummies, year-of-birth dummies, and state-of-birth-specific linear time trends. Data were state-level compulsory schooling laws between 1919 and 1973 in 49 states (including the District of Columbia); corresponding to birth cohort 1905-1959. The total number of observations is 2,695.

Table 3. Effective first stage F-statistics of compulsory schooling laws on years of schooling

	(1)	(2)	(3)	(4)
Panel A. Sample of all educational levels				
Sets of excluded instruments				
Acemoglu and Angrist (2000): CA dummies, and CL dummies	63.9	4.1	0.6	0.6
Lleras-Muney (2005): Continuation laws, CL dummies	153.7	5.0	0.1	0.3
Oreopoulos (2006): Dropout age	14.9	4.7	0.5	1.3
Stephens and Young (2014): Required schooling dummies	72.4	6.4	1.5	3.4
Persons	30,824			
Observations	206,859			
Panel B. Sample of high school or less				
Sets of excluded instruments				
Acemoglu and Angrist (2000): CA dummies, and CL dummies	86.8	9.1	2.7	2.1
Lleras-Muney (2005): Continuation laws, CL dummies	210.2	13.6	1.7	2.2
Oreopoulos (2006): Dropout age	18.2	14.2	0.0	0.0
Stephens and Young (2014): Required schooling dummies	97.1	12.4	1.4	4.0
Persons	18,261			
Observations	120,593			
Fixed effects				
State of birth	No	Yes	Yes	Yes
Year of birth	No	Yes	Yes	Yes
Region × year of birth dummies	No	No	Yes	No
State × year of birth specific linear trends	No	No	No	Yes
Additional controls	Gender			

Notes: We computed Montiel Olea and Pflueger (2013)'s effective F-statistics using the "*weakivtest*" package. The analytic sample was restricted to white respondents in the 1992-2016 Health and Retirement Study who were born in the continental United States between 1905 and 1959.

Table 4. Association between years of schooling and hospitalizations

	OLS Estimates				Marginal effects from a logit model			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Years of Schooling	-0.015***	-0.010***	-0.009***	-0.010***	-0.014***	-0.009***	-0.009***	-0.009***
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Female		-0.013***	-0.012***	-0.012**		-0.013***	-0.012***	-0.012***
		(0.005)	(0.005)	(0.005)		(0.005)	(0.005)	(0.005)
Parents Education (ref: Less than high school)								
High school		-0.003	0.004	0.004		-0.003	0.003	0.005
		(0.006)	(0.006)	(0.006)		(0.006)	(0.006)	(0.006)
Some College		-0.029***	-0.022**	-0.021**		-0.031***	-0.025**	-0.023**
		(0.010)	(0.010)	(0.010)		(0.011)	(0.011)	(0.011)
College or above		-0.021*	-0.013	-0.012		-0.023*	-0.016	-0.015
		(0.012)	(0.013)	(0.013)		(0.013)	(0.014)	(0.014)
Age		-0.058***	-0.048***	-0.046***		-0.058***	-0.047***	-0.045***
		(0.012)	(0.013)	(0.013)		(0.014)	(0.014)	(0.014)
Age^2		0.001***	0.001***	0.001***		0.001***	0.001***	0.001***
		(0.000)	(0.000)	(0.000)		(0.000)	(0.000)	(0.000)
Age^3		-0.000***	-0.000***	-0.000***		-0.000***	-0.000***	-0.000***
		(0.000)	(0.000)	(0.000)		(0.000)	(0.000)	(0.000)
Survey wave dummies	No	Yes	Yes	Yes	No	Yes	Yes	Yes
State of birth	No	No	Yes	Yes	No	No	Yes	Yes
Year of birth	No	No	Yes	Yes	No	No	Yes	Yes
State of birth specific linear trends	No	No	No	Yes	No	No	No	Yes
Constant	0.444***	1.469***	0.417	3.317				
	(0.014)	(0.272)	(0.428)	(5.237)				
Adjusted/Pseudo R-squared	0.0044	0.0426	0.0458	0.0463	0.0036	0.0371	0.0409	0.0419
Observations	85887	85887	85887	85887	85887	85887	85887	85887

Notes: The analytic sample was restricted to white respondents in the 1992-2016 Health and Retirement Study who were born in the continental United States between 1905 and 1959. We also restricted to those with a high school degree or less. In parentheses are standard errors clustered at the individual level. *** p<0.01, ** p<0.05, * p<0.1.

Table 5. Estimates from the first stage and reduced form regressions

	Zero-first-stage group: predicted college or above	Analytic Sample: High school or less
Panel A. First stage		
Continuation laws	0.037	0.231***
	(0.129)	(0.044)
Compulsory schooling by attendance laws	0.009	0.006
	(0.034)	(0.012)
Compulsory schooling by labor laws =8	0.020	0.136***
	(0.118)	(0.042)
Compulsory schooling by attendance laws=7	-0.050	-0.713***
	(0.585)	(0.187)
Required schooling=6	0.217	-0.378***
	(0.366)	(0.124)
Required schooling=8	-0.027	0.083*
	(0.139)	(0.048)
Joint F stats	0.09	18.86***
Persons	2065	12,528
Observations	13,977	85,887
Panel B. Reduced form		
Continuation laws	-0.009	-0.015***
	(0.012)	(0.006)
Compulsory schooling by attendance laws	0.003	0.000
	(0.003)	(0.002)
Compulsory schooling by labor laws =8	0.014	-0.011**
	(0.012)	(0.005)
Compulsory schooling by attendance laws=7	0.027	0.053***
	(0.056)	(0.016)
Required schooling=6	0.027	-0.009
	(0.032)	(0.012)
Required schooling=8	-0.011	0.003
	(0.013)	(0.006)
Joint F stats	0.57	4.72***
Persons	2067	12,528
Observations	13,949	85,887

Notes: Both the first stage regression and the reduced form regression control for gender, age, age^2, age^3, parents' educational attainment, survey wave dummies, and selected state fixed effects in Table 6. In parentheses are standard errors clustered at the individual level. *** p<0.01, ** p<0.05, * p<0.1.

Table 6. Effect of years of secondary schooling on hospitalizations

	Main estimates		Intermediate IV estimates	
	Plausibly Exogenous IV	OLS	Lasso-IV	GMM-IV
	(1)	(2)	(3)	(4)
Years of Schooling	-0.026***	-0.010***	-0.046***	-0.044***
	(0.008)	(0.001)	(0.011)	(0.011)
Female	-0.008*	-0.013***	-0.000	-0.001
	(0.004)	(0.005)	(0.006)	(0.006)
Parents Education (ref: Less than high school)				
High school	0.007	-0.002	0.019**	0.017**
	(0.006)	(0.006)	(0.008)	(0.008)
Some College	-0.018*	-0.029***	-0.004	-0.006
	(0.010)	(0.010)	(0.013)	(0.013)
College or above	-0.012	-0.022*	0.001	0.000
	(0.011)	(0.013)	(0.014)	(0.014)
Age	-0.060***	-0.058***	-0.061***	-0.060***
	(0.010)	(0.012)	(0.012)	(0.012)
Age^2	0.001***	0.001***	0.001***	0.001***
	(0.000)	(0.000)	(0.000)	(0.000)
Age^3	-0.000***	-0.000***	-0.000***	-0.000***
	(0.000)	(0.000)	(0.000)	(0.000)
Selected state of birth fixed effects				
Illinois	-0.008	-0.018*	-0.005	-0.005
	(0.008)	(0.010)	(0.011)	(0.011)
New York	-0.007	-0.010	0.000	-0.000
	(0.006)	(0.008)	(0.009)	(0.009)
Ohio	0.020**	0.015	0.027**	0.026**
	(0.008)	(0.011)	(0.012)	(0.012)
Tennessee	0.021**	0.031***	0.009	0.011
	(0.010)	(0.011)	(0.014)	(0.014)
Texas	-0.059***	-0.017	-0.082***	-0.078***
	(0.017)	(0.010)	(0.023)	(0.023)
Virginia	-0.019	0.001	-0.037*	-0.036*
	(0.013)	(0.015)	(0.019)	(0.019)
Survey wave dummies	Yes	Yes	Yes	Yes
Constant	1.651***	1.468***	1.922***	
	(0.259)	(0.270)	(0.303)	
Effective First-Stage F Statistic			19.21**	19.23**
Observations	85,887	85,887	85,887	85,887

Notes: The selected set of instruments includes continuation laws, years of compulsory schooling required by child attendance laws (CA_{sct}), $CA_{sct} = 7$, years of compulsory schooling required by child labor laws (CL_{sct})=8, years of required schooling (RS_{sct})=6, and $RS_{sct} = 8$. The selected set of controls are displayed in the table; no year of birth fixed effects were selected due to the inclusion of age. The effective first-stage F-statistics is 19.21, which rejects the null hypothesis of weak instruments at $\alpha = 0.05$ level with $\tau = 10\%$. Column (1) reports the plausibly exogenous IV estimate using those with a college degree or above as the "zero-first-stage" group. Column (2) reports OLS estimators of regressing hospitalization on selected controls. Column (3) reports Lasso-IV estimates using selected instruments and controls, respectively. In parentheses are standard errors clustered at the individual level. *** p<0.01, ** p<0.05, * p<0.1.

Table 7. Specification checks on main results

	Two-way clustering	Collapse HRS longitudinal panel data to individual level	Additionally control for childhood health and SES	Alternative measure of hospitalization	Dropping birth cohorts 1905-1914 from analyses	Accounting for Attrition using Inverse Probability Weighting (IPW)
	(1)	(2)	(3)	(4)	(5)	(6)
Plausibly Exogenous-IV	-0.026***	-0.030***	-0.024**	-0.020**	-0.033***	-0.021***
	(0.009)	(0.010)	(0.010)	(0.009)	(0.011)	(0.011)
OLS	-0.010***	-0.007***	-0.008***	-0.009***	-0.011***	-0.012***
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.002)
LASSO-IV	-0.046***	-0.032***	-0.047***	-0.023***	-0.061***	-0.041***
	(0.012)	(0.008)	(0.013)	(0.005)	(0.015)	(0.013)
Persons	12,528	12,528	10,724	12,528	11,287	12,528
Observations	85,887	12,528	82,606	85,887	81,449	85,887

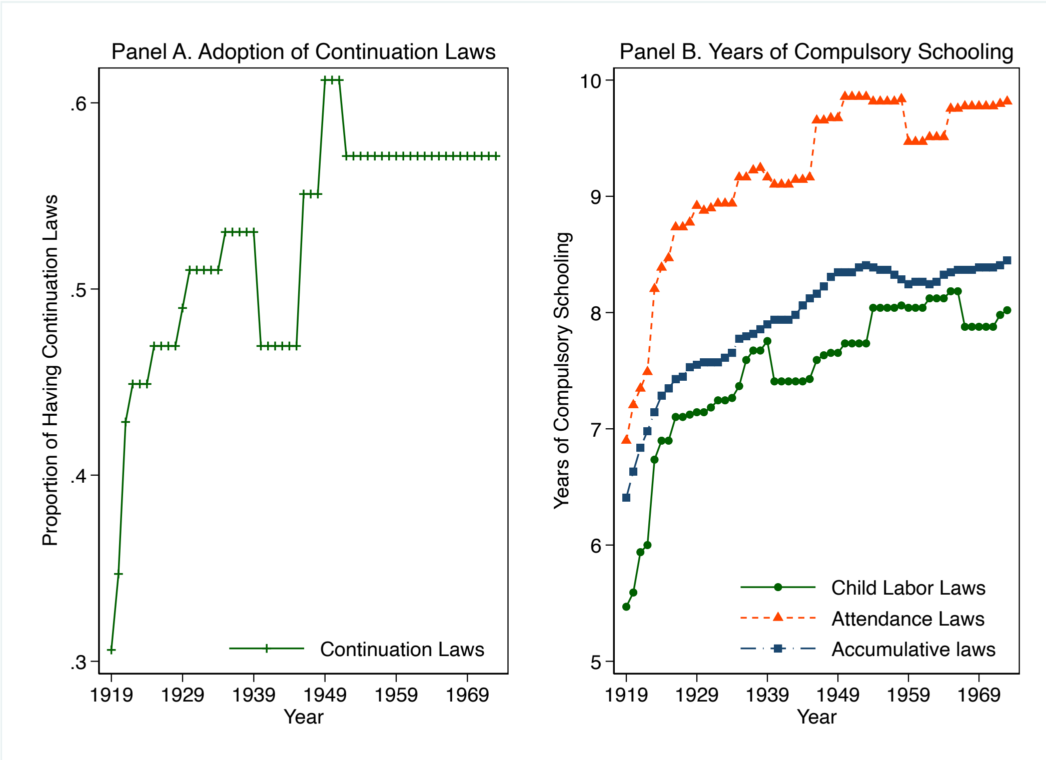
Notes: Selected instruments include continuation laws, years of compulsory schooling required by child attendance laws (CA_{sct}), $CA_{sct} = 7$, years of compulsory schooling required by child labor laws (CL_{sct})=8, years of required schooling (RS_{sct})=6, and $RS_{sct} = 8$. In parentheses are standard errors clustered at the individual and state of birth levels in Column (1), clustered at the state of birth level in Column (2), and clustered at the individual level in the remaining columns. *** p<0.01, ** p<0.05, * p<0.1.

Table 8. Robustness to violations of perfect exogeneity

	Plausibly Exogenous IV	
	Estimate	95% Confidence Intervals
Panel A. Exogeneity error (γ) as percent of coefficients on instruments from the reduced form among primary analytic sample		
0%	-0.046***	(-0.063, -0.029)
10%	-0.041***	(-0.058, -0.024)
30%	-0.032***	(-0.049, -0.015)
50%	-0.023***	(-0.040, -0.006)
60%	-0.018**	(-0.035, -0.001)
65%	-0.016*	(-0.033, 0.001)
70%	-0.014	(-0.031, 0.003)
80%	-0.009	(-0.026, 0.008)
90%	-0.004	(-0.021, 0.013)
100%	0.0003	(-0.017, 0.017)
Panel B. Exogeneity error (γ) as coefficients on instruments from the reduced form among different subgroups		
White college graduates	-0.056***	(-0.071, -0.041)
Blacks with a high school degree or less	-0.017***	(-0.025, -0.010)

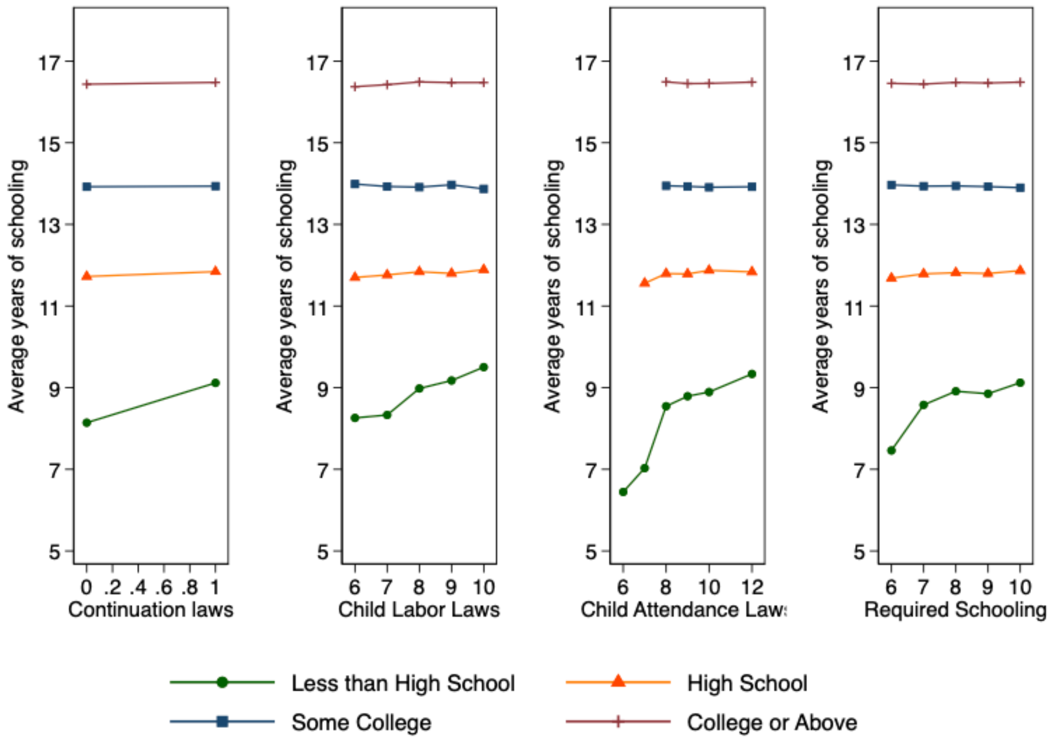
Notes: Shown are plausibly exogenous IV estimates following Conley, Hansen, and Rossi (2012)'s local-to-zero approach. *** p<0.01, ** p<0.05, * p<0.1.

Figure 1. Trends in compulsory schooling laws from 1919 to 1973



Notes: Shown are the aggregate average of state-level compulsory schooling laws and quality of school measures between 1919 and 1973 in 49 states (including the District of Columbia). Since we matched each individual to the laws that were in place in their state of birth when they were 14 years old, these trends correspond to the birth cohort 1905-1959. The total number of observations is 2,695, including 49 states for 55 years.

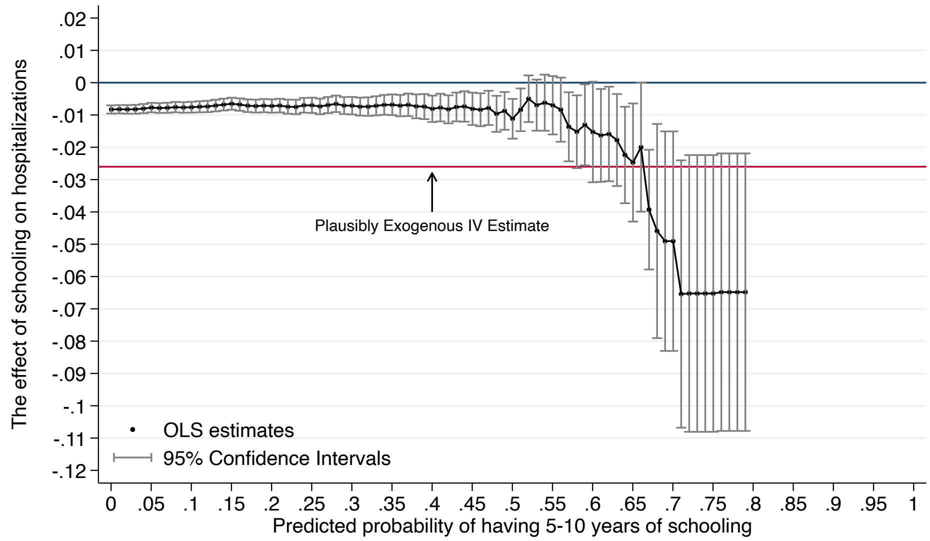
Figure 2. Average years of completed education and compulsory schooling years, by educational levels



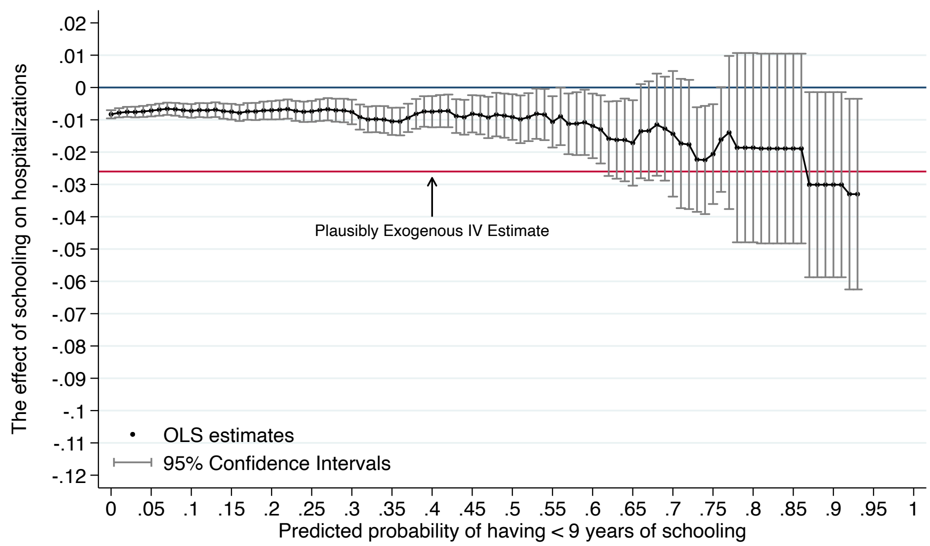
Notes: These graphs were based on the analytic sample that includes 10,724 unique respondents. Shown are the aggregate average of years of completed education by compulsory schooling and educational categories. To ensure stable estimates, only those "compulsory schooling" × "educational category" cells with 100 observations were included.

Figure 3. OLS estimates of the education effect on hospitalizations among potential compliers

Panel A. OLS estimates among those who are predicted to have schooling in CSL ranges (5-10 years of schooling)



Panel B. OLS estimates of the education effect on hospitalizations among those who are predicted to have schooling less than secondary school



Notes: Panel A shows OLS estimates along with its 95% confidence intervals restricting to those with a predicted probability of having 5-10 years of schooling larger than or equal to various critical values on the x-axis. Panel B shows OLS estimates along with its 95% confidence intervals restricting to those with a predicted probability of having <9 years of schooling larger than or equal to various critical values on the x-axis. We used a logistic regression model to make predictions based on HRS respondents' gender, dummies of parents' years of schooling, state of birth, and year of birth.

Increased Schooling Reduces Hospitalization Later in Life: New Evidence with Optimal Instruments from the United States

Appendix Tables and Figures

Dahai Yue, Ninez A. Ponce, Jack Needleman, Susan L. Ettner, Adriana Lleras-Muney

Table A1. Sources of compulsory schooling laws, 1910-1978

Birth cohorts	Year at 14 age	States	Continuation schooling laws	Child labor laws	Child attendance laws	Required schooling
1896-1964	1910-1939	48 states (excluding Alaska, Hawaii, and Washington D.C.)	Lleras-Muney (2002), Goldin & Katz (2003)	Lleras-Muney (2002), Goldin & Katz (2003)	Lleras-Muney (2002), Goldin & Katz (2003)	
1896-1900	1910-1914	Washington DC	Authors	Authors	Authors	
1911-1964	1915-1978	Washington DC	Authors	Acemoglu & Angrist (2000)	Acemoglu & Angrist (2000)	
1926-1964	1940-1978	48 states (excluding Alaska, and Hawaii)	Authors	Acemoglu & Angrist (2000)	Acemoglu & Angrist (2000)	
1905-1961	1919-1975	48 states (excluding Alaska, and Hawaii)				Stephens & Yang (2014)

Notes: "Authors" indicates data were collected by us from the following sources (for years without published reports, we carried forward the previous year's data):
1) US Office (Bureau) of Education. 1910. *Education Report*, 1910. (Annual Report of the Commissioner of Education). "Compulsory Education and Child-Labor Laws." Washington, DC: GPO.
2) US Department of the Interior, Bureau of Education. *Laws Relating to Compulsory Education*. Bulletin No. 20 by Ward W. Keesecker, US GPO 1929.
3) Alexander, K. & Jordan, K.F. (1973). *Legal Aspects of Educational Choice: Compulsory Attendance and Student Assignment*.
4) US Department of Labor, Division of Labor Standards (July 1946) *State Child-Labor Standards. A State-by-State summary of laws affecting the employment of minors under 18 years of age*.
5) US Department of Labor, Division of Labor Standards (Sep 1949) *State Child-Labor Standards. A State-by-State summary of laws affecting the employment of minors under 18 years of age.* Bulletin 114.
6) US Department of Labor, Division of Labor Standards (Apr 1952) *State Child-Labor Standards. A State-by-State summary of laws affecting the employment of minors under 18 years of age*. Bulletin 158.
7) US Department of Labor, Division of Labor Standards (Sep 1965) *State Child-Labor Standards. A State-by-State summary of laws affecting the employment of minors under 18 years of age.* Bulletin 158 (Revised 1965).

Table A2. Estimates on instrument from first stage and reduced form among different subgroups

	Analytic sample	Predicted college graduates	College graduates	low-education Blacks
	(1)	(2)	(3)	(4)
Panel A. First stage				
Continuation laws	0.231***	0.037	0.034*	0.318**
	(0.044)	(0.129)	(0.020)	(0.125)
Compulsory schooling by attendance laws	0.006	0.009	0.001	-0.009
	(0.012)	(0.034)	(0.005)	(0.026)
Compulsory schooling by labor laws =8	0.136***	0.020	0.031*	0.609***
	(0.042)	(0.118)	(0.018)	(0.133)
Compulsory schooling by attendance laws=7	-0.713***	-0.050	-0.096	-0.794***
	(0.187)	(0.585)	(0.062)	(0.255)
Required schooling=6	-0.378***	0.217	0.064	-0.274
	(0.124)	(0.366)	(0.051)	(0.242)
Required schooling=8	0.083*	-0.027	0.047**	-0.358**
	(0.048)	(0.139)	(0.021)	(0.140)
Joint F stats	18.86***	0.09	3.00***	8.87***
Persons	12,528	2065	5058	3,116
Observations	85,887	13,977	36,629	19,163
Panel B. Reduced form				
Continuation laws	-0.015***	-0.009	-0.012*	-0.015***
	(0.006)	(0.012)	(0.007)	(0.006)
Compulsory schooling by attendance laws	0.000	0.003	0.001	0.000
	(0.002)	(0.003)	(0.002)	(0.002)
Compulsory schooling by labor laws =8	-0.011**	0.014	0.004	-0.011**
	(0.005)	(0.012)	(0.007)	(0.005)
Compulsory schooling by attendance laws=7	0.053***	0.027	-0.037	0.053***
	(0.016)	(0.056)	(0.023)	(0.016)
Required schooling=6	-0.009	0.027	-0.009	-0.009
	(0.012)	(0.032)	(0.019)	(0.012)
Required schooling=8	0.003	-0.011	-0.015*	0.003
	(0.006)	(0.013)	(0.008)	(0.006)
Joint F stats	4.72***	0.57	1.45	0.85
Persons	12,528	2067	5104	3114
Observations	85,887	13,949	36761	19,098

Notes: Both first stage regression and reduced form regression controls for gender, age, age^2, age^3, parents' educational attainment, survey wave dummies, and selected state fixed effects in Table 6. In parentheses are standard errors clustered at the individual level. *** p<0.01, ** p<0.05, * p<0.1.

Table A3. IV estimates with optimal instruments by sex.

	Main estimates		Intermediate IV estimates	
	Plausibly Exogenous IV	OLS	Lasso-IV	GMM-IV
	(1)	(2)	(3)	(4)
Panel A. Women				
Years of Schooling				
Estimates	-0.040***	-0.012***	-0.060***	-0.060***
Standard errors	(0.011)	(0.002)	(0.016)	(0.016)
Wald 95% confidence sets	(-0.061, -0.018)	(-0.016, -0.009)	(-0.091, -0.029)	(-0.090, -0.029)
AR 95% confidence sets			[-0.112, -0.033]	
Observations	51,977	51,977	51,977	51,977
First-stage F statistic			10.66	
Effective first-stage F statistic			11.98	
Panel B. Men				
Years of Schooling				
Estimates	-0.008	-0.008***	-0.021	-0.020
Standard errors	(0.011)	(0.002)	(0.014)	(0.014)
Wald 95% confidence sets	(-0.030, 0.014)	(-0.049, 0.008)	(-0.049, 0.007)	(-0.048, 0.008)
AR 95% confidence sets			[-0.067, 0.017]	
Observations	33,910			
First-stage F statistic			8.23	
Effective first-stage F statistic			7.90	

Notes: The selected instruments include continuation laws, years of compulsory schooling required by child attendance laws (CA_{sct}), $CA_{sct} = 7$, years of compulsory schooling required by child labor laws (CL_{sct})=8, years of required schooling (RS_{sct})=6, and $RS_{sct} = 8$. The selected controls are displayed in the table; no year of birth fixed effects were selected due to the inclusion of age. Column (1) reports the plausibly exogenous IV estimate using those with a college degree or above as the “zero-first-stage” group. Column (2) reports OLS estimators of regressing hospitalization on selected controls. Columns (3) and (4) report Lasso-IV estimates GMM-IV estimators using selected instruments and controls, respectively. Standard errors clustered at the individual level.
*** p<0.01, ** p<0.05, * p<0.1.

Table A4. Estimates on other measures of healthcare utilization, common causes of hospitalizations, and health status

	OLS	Plausibly Exogenous IV	Lasso-IV	N (persons)	N (observations)
	(1)	(2)	(3)	(4)	(5)
Healthcare utilization					
No. of hospital stays (times)	-0.034***	-0.093***	-0.128***	12,527	85,717
	(0.001)	(0.026)	(0.033)		
No. of hospital nights over all stays	-0.188***	-0.541**	-0.658***	12,526	85,404
	(0.031)	(0.232)	(0.217)		
Common causes of hospitalizations					
Heart problems (0/1)	-0.012***	-0.113***	-0.055***	12,531	86,045
	(0.002)	(0.011)	(0.018)		
Lung disease (0/1)	-0.014***	-0.031***	-0.039***	12,533	86,046
	(0.002)	(0.007)	(0.013)		
Stroke (0/1)	-0.006***	-0.009	-0.022	12,534	86,063
	(0.001)	(0.007)	(0.011)		
Arthritis (0/1)	-0.017***	-0.053***	-0.047***	12,531	86,044
	(0.002)	(0.014)	(0.017)		
Health status					
Self-report of health (1=excellent, 2=very good, 3=good, 4=fair, 5=poor)	-0.112***	-0.263***	-0.297***	12,532	86,070
	(0.005)	(0.028)	(0.042)		
Fair or poor health status (0/1)	-0.045***	-0.097***	-0.122***	12,532	86,070
	(0.002)	(0.010)	(0.016)		
Ten-year mortality	-0.013***	-0.060***	-0.045***	11,675	11,675
	(0.002)	(0.015)	(0.016)		

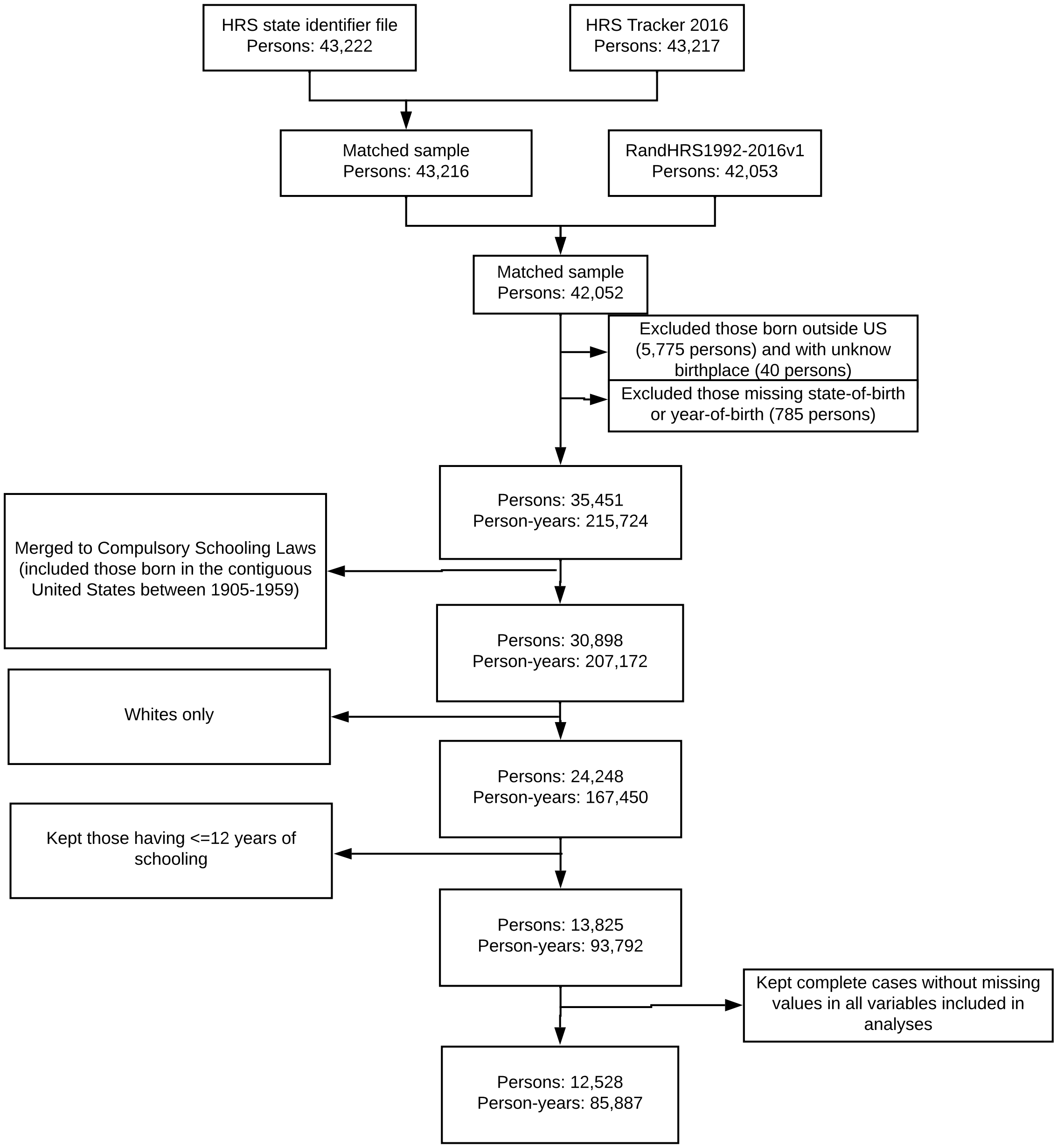
Notes: We applied the plausibly exogenous IV approach used for hospitalizations in the main analysis to examine various healthcare and health outcomes in the 1992-2016 US Health and Retirement Study. Applying selections on instruments and controls to each outcome yields no meaningful differences. Heart problems include heart attack, coronary heart disease, angina, congestive heart failure, or other heart problems. Lung disease includes chronic lung disease (except asthma) such as chronic bronchitis or emphysema attack. Stroke includes stroke or transient ischemic. Standard errors are clustered at the individual level to account for within-subject correlation, except that the standard error is clustered at the state of birth level for then-year mortality. For the analysis of ten-year mortality, we restricted the sample to those who were first interviewed in 2006 or earlier so that we could track the respondent's vital status over ten years.
*** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Table A5. Revisit Lleras-Muney (2005) on the relationship between education and mortality

	WLS	IV	First-Stage F-statistic	Anderson-Rubin 95% Confidence Sets	N	(selected) Instruments
1. Lleras-Muney (2005)	-0.017** (0.004)	-0.051** (0.026)	4.69	Not reported	4,792	childcom=(4,5,6,7,8,9,10), conts
2. Lleras-Muney (2006)	-0.036** (0.004)	-0.063** (0.024)	Not reported	Not reported	4,792	childcom=(4,5,6,7,8,9,10), conts
3. Replication of Lleras-Muney (2006)	-0.035** (0.004)	-0.067** (0.024)	5.87	(-0.209, -0.004)	4,796	childcom=(4,5,6,7,8,9,10), conts
4. Replication of Lleras-Muney (2006), replacing birth region and cohort interactions with state of birth specific linear trends	-0.037** (0.004)	-0.052 (0.080)	1.19	Entire Grid	4,796	childcom=(4,5,6,7,8,9,10), conts
5. Replication of Lleras-Muney (2006), replacing birth region and cohort interactions with state of birth specific linear trends: Lasso-IV to select both instruments and controls.	-0.041** (0.004)	-0.010 (0.021)	24.17	Null Set	4,796	Childcom=10, conts
6. Replication of Lleras-Muney (2006), replacing birth region and cohort interactions with state of birth specific linear trends, and adding updated instruments: Lasso-IV to select both instruments and controls.	-0.047** (0.004)	-0.043** (0.014)	15.86	(-0.072, -0.004)	4,032	CL_{sct}, CL6, CA_{sct}, RS_{sct}, RS6

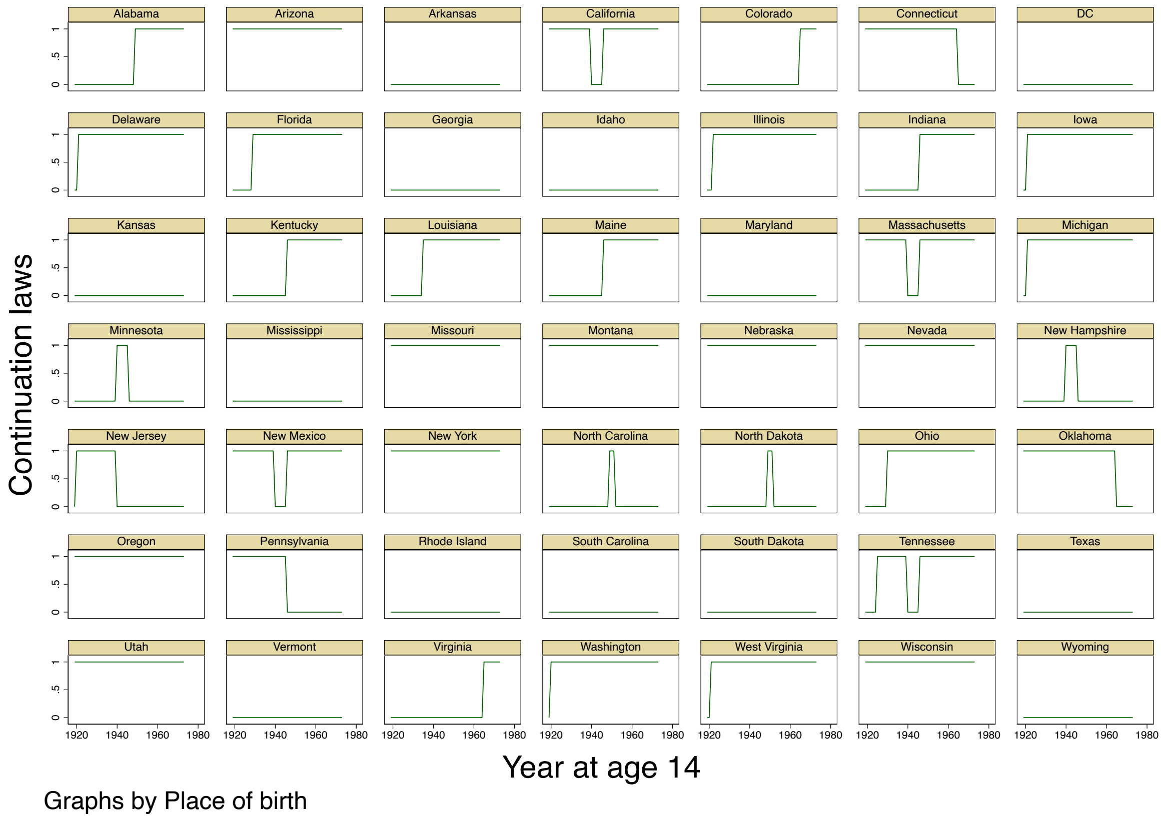
Notes: Lleras-Muney (2005) focuses on whites born in 48 continental US between 1901 and 1925 using data from 1960, 1970 and 1980 1% census. Lleras-Muney (2006) is an erratum of Lleras-Muney (2005) to compute education at base period. N represents state of birth, cohort, and gender cells. WLS denotes weighted least square. Childcom represents the year of compulsory schooling, and conts is continuation laws, used in Lleras-Muney (2005). CL_{sct}, CL6, CA_{sct}, RS_{sct}, RS6 are instruments constructed in section 3.2.2 of this study. Note, these instruments are only available for birth cohorts 1905-1925 because the entry age was assigned based on laws in place at age 6, which leads to a decreased sample size of 4032. However, restricting analyses of rows (1) to (5) to the birth cohorts 1905-1925 yielded no meaningful differences. All WLS and IV estimates were weighted by the number of observations in the cell in the base period. In parentheses are standard errors clustered at the state-of-birth and cohort levels.

Figure A1. Sample flowchart



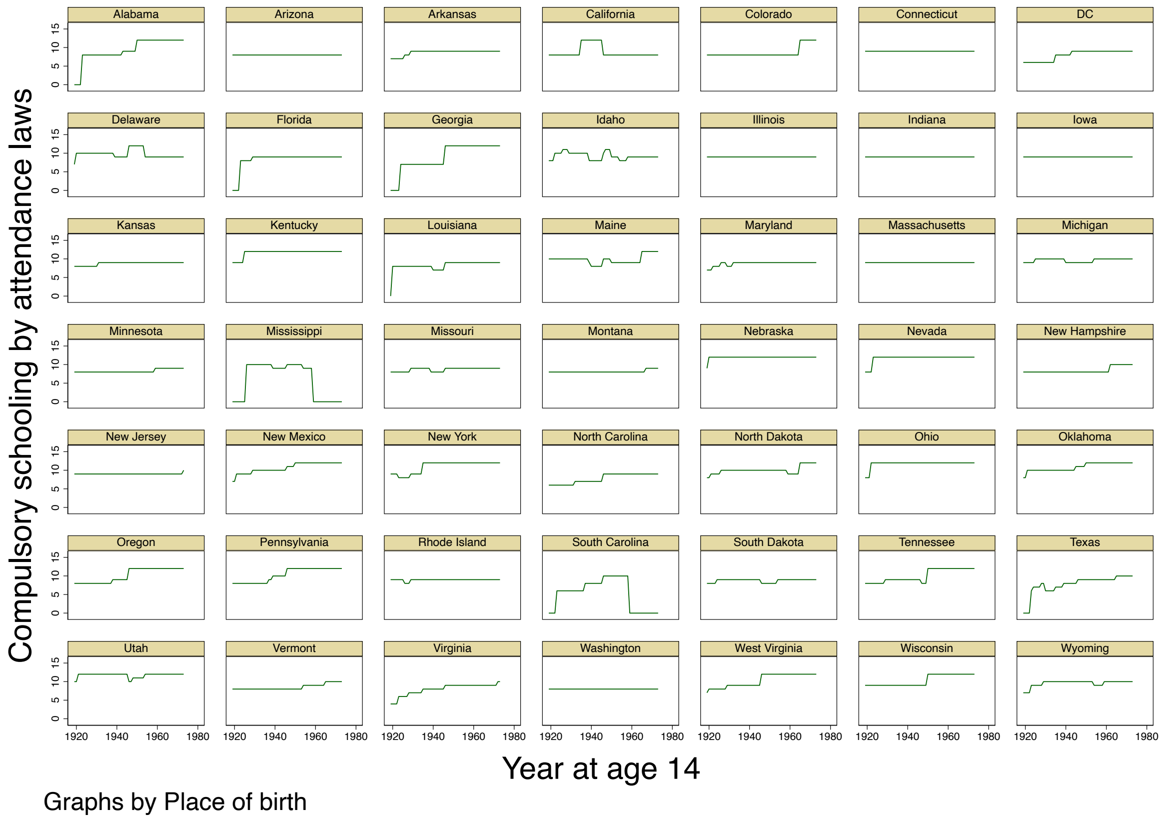
Notes: shown are the number of individuals excluded in each step. HRS denotes Health and Retirement Study. Our main analyses include 12,528 persons and 85,887 person-years.

Figure A2. Changes in continuation laws, by states



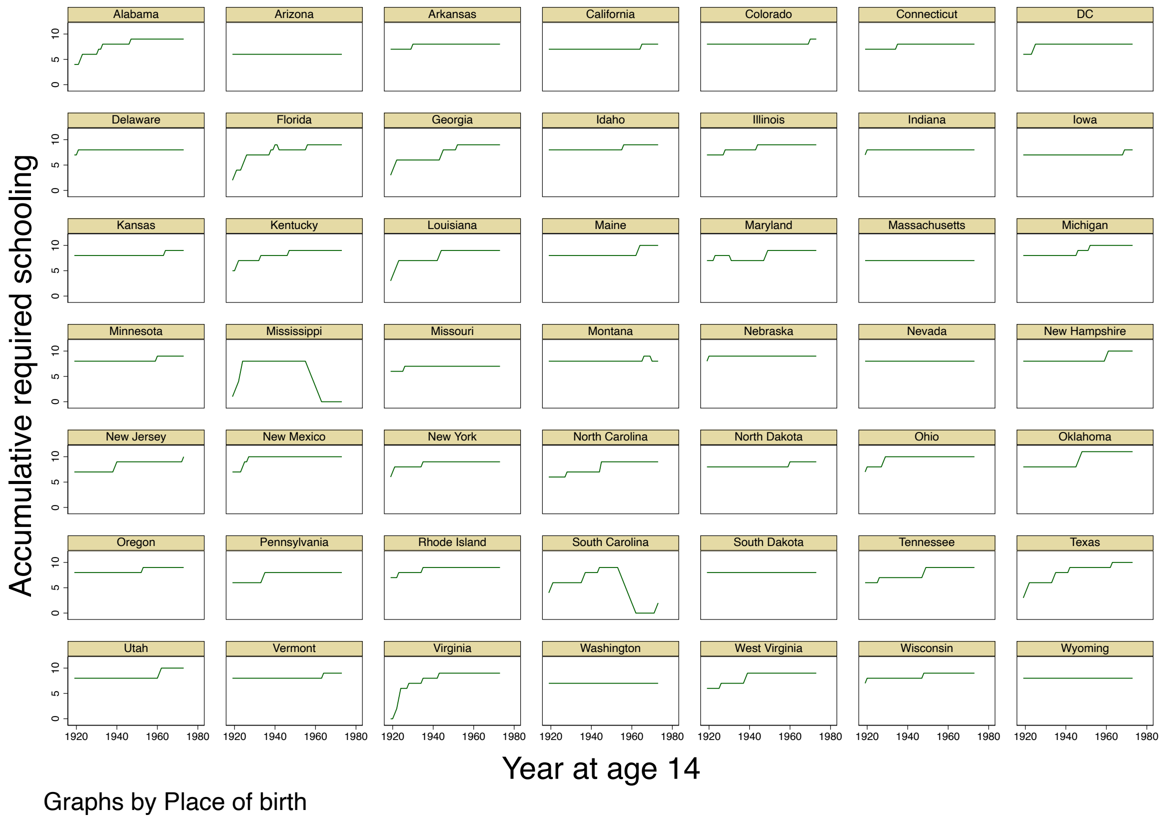
Notes: Data are state-level compulsory laws from various sources, as documented in Table A1.

Figure A3. Changes in compulsory schooling by child attendance laws, by states



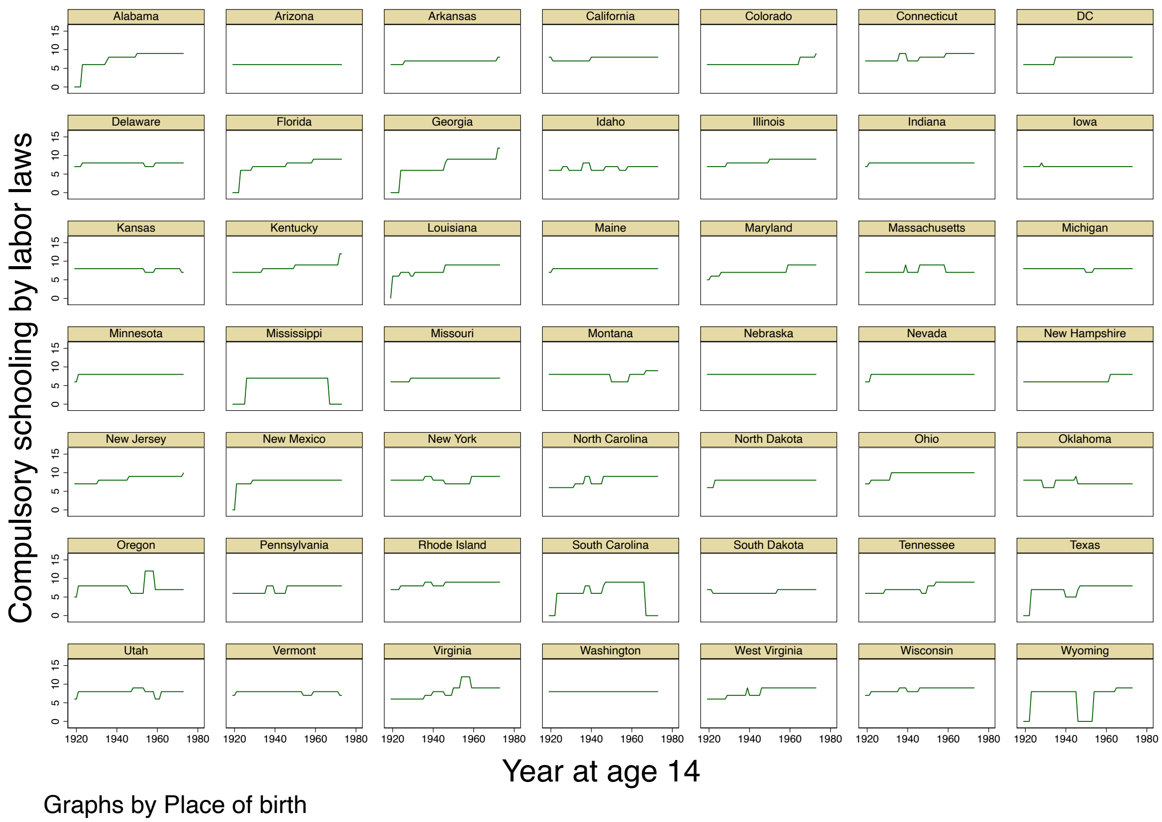
Notes: Data are state-level compulsory laws from various sources, as documented in Table A1.

Figure A4. Changes in accumulative required schooling, by states



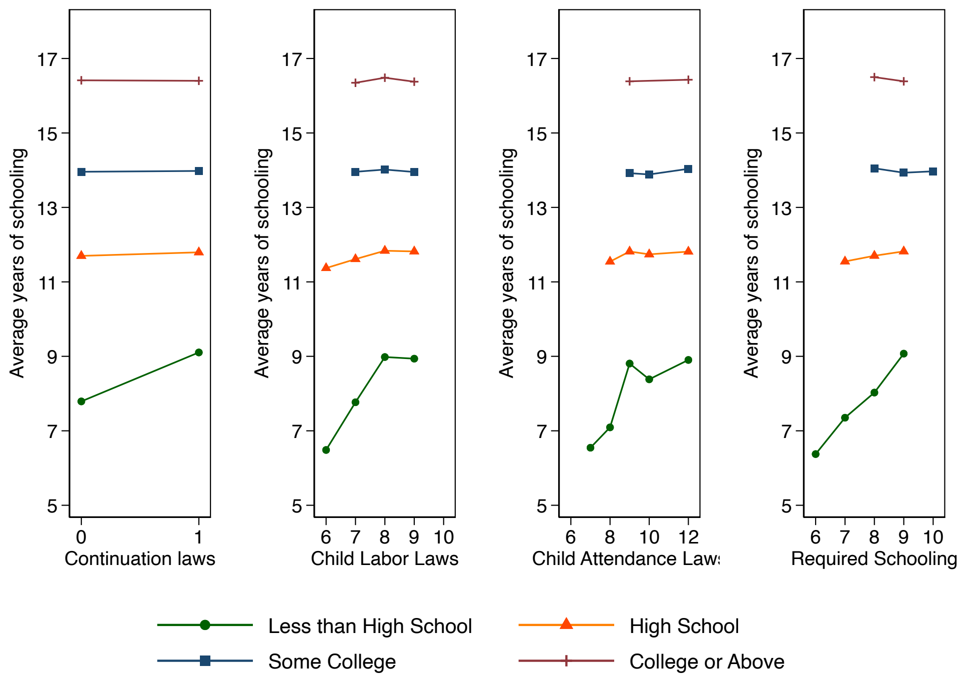
Notes: Data are state-level compulsory laws from various sources, as documented in Table A1.

Figure A5. Changes in compulsory schooling by child labor laws, by states



Notes: Data are state-level compulsory laws from various sources, as documented in Table A1.

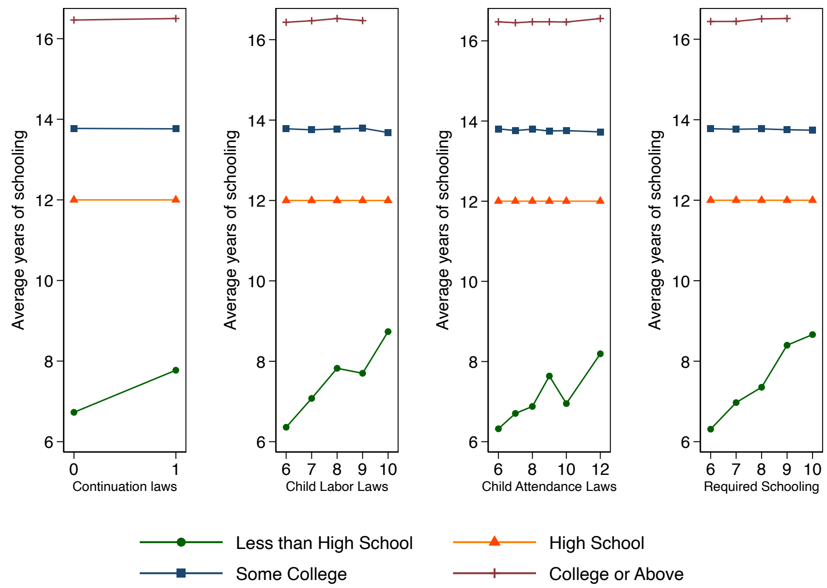
Figure A6. Average years of completed education and compulsory schooling years for black respondents in HRS, by educational categories



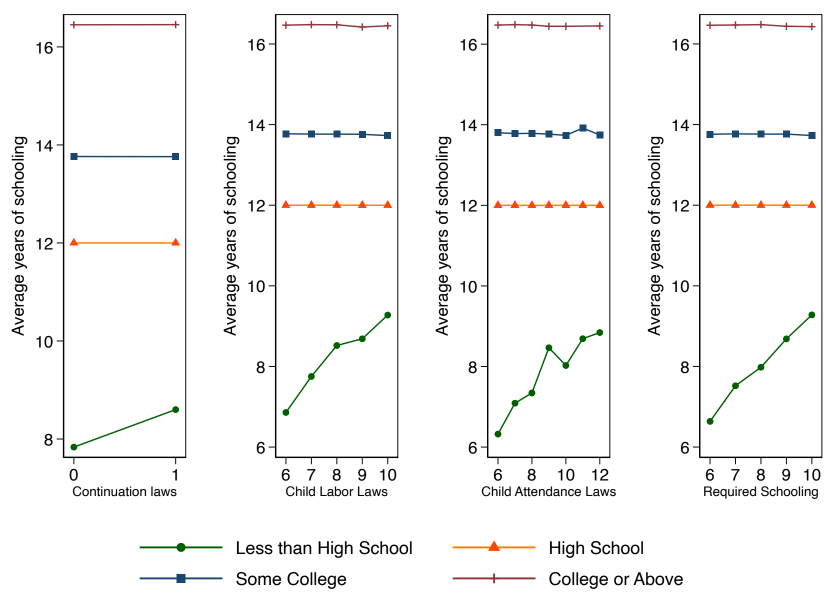
Notes: These graphs were based on the 1992-2016 Health and Retirement Study that includes 5,664 unique respondents (Blacks born in the continental United States between 1905 and 1959). Shown are the aggregate average of years of completed education by compulsory schooling and educational categories. To ensure stable estimates, only those "compulsory schooling" X "educational category" cells with 100 observations were included.

Figure A7. Average years of completed education and compulsory schooling years for black respondents in 1960-1980 censuses, by educational categories

Panel A. Blacks born 1901-1925



Panel B. Blacks born 1901-1959



Notes: These graphs were based on the 1960-1980 census that includes 93,934 black respondents born in the continental United States between 1901 and 1925 (Panel A) and 620,052 Blacks born between 1901 and 1959 (Panel B). We used the IPUMS 1960 Census 1% sample, IPUMS 1970 Census 1% state samples, and IPUMS 1980 Census 5% sample. Shown are the aggregate average of years of completed education by compulsory schooling and educational categories. To ensure stable estimates, only those "compulsory schooling" X "educational category" cells with 100 observations were included.