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New evidence of the effects of education on health in the US: Compulsory schooling laws revisited

Jason M. Fletcher^{a,b,c,*}

^aLa Follette School of Public Affairs, University of Wisconsin—Madison, United States

^bDepartment of Sociology, University of Wisconsin—Madison, United States

^cCenter for Demography and Ecology, University of Wisconsin—Madison, United States

Abstract

Estimating the effects of education on health and mortality has been the subject of intense debate and competing findings and summaries. The original Lleras-Muney (2005) methods utilizing state compulsory schooling laws as instrumental variables for completed education and US data to establish effects of education on mortality have been extended to several countries, with mixed and often null findings. However, additional US studies have lagged behind due to small samples and/or lack of mortality information in many available datasets. This paper uses a large, novel survey from the AARP on several hundred thousand respondents to present new evidence of the effects of education on a variety of health outcomes. Results suggest that education may have a role in improving several dimensions of health, such as self reports, cardiovascular outcomes, and weight outcomes. Other results appear underpowered, suggesting that further use of this methodology may require even larger, and potentially unattainable, sample sizes in the US.

Keywords

Education; Mortality; Instrumental variables; Health status; Causality

1. Introduction

The statistical associations between education and health outcomes are among the most robust and replicated in the social sciences. However, until recently, most research did not attempt to address issues of reverse causality and omitted variables cited by Fuchs (1982), among others. The relationship between education and mortality has been of specific interest. A well known article by Lleras-Muney (2005) suggested the use of compulsory schooling laws in the early 20th century in the US as a way to allow causal effects to be estimated. The findings were important and large—one additional year of schooling was suggested to reduce 10-year mortality rates among older individuals by over 6 percentage points. Many studies have now used compulsory schooling laws in the US and many European countries, with a much more complicated set of findings. Mazumder (2011)

*Corresponding author. La Follette School of Public Affairs, University of Wisconsin—Madison, 1225 Observatory Drive, Madison, WI 53706, United States. jmfletcher@wisc.edu.

reviews this literature and suggests that much of the more recent research shows little evidence of causal effects, particularly outside of the US. This has been puzzling given the many theorized pathways linking education to health and mortality, such as income, knowledge, occupation, etc. A drawback in comparing results across countries is the large amount of variation in the context—differences in laws, time periods, health care settings, populations—which could in part explain differences in results.

A limited role of education on mortality in Europe, though, does not necessarily eliminate the potential role in the US. However, in attempting to focus back on the US context and examine their potential robustness and mechanisms, researchers confront several obstacles. Few datasets in the US are sufficiently large to undertake these analyses with the requisite power. However, even explorations unable to definitively show mortality effects may be able to provide evidence for or against mechanisms between education and mortality.

This paper utilizes untapped data from the epidemiology literature that surveyed several hundred thousand elderly respondents from the US in the 1990s. The focus of the survey was on diet and health outcomes and the respondents have been linked to the national death index. Information on state and year of birth allows compulsory schooling laws to be used as instruments for completed schooling to examine the effects of education on mortality and health outcomes. Findings suggest that education may have a role in improving several dimensions of health, such as self reports, cardiovascular outcomes, and weight outcomes. Other results appear underpowered, suggesting that further use of this methodology may require even larger, and potentially unattainable, sample sizes in the US.

2. Background literature

Beginning with Angrist and Krueger (1991), economists and other social scientists have utilized state and year variation in compulsory schooling (and related) laws as an instrumental variable for estimating impacts of education on later outcomes. The main idea is to leverage differences across states and over time (i.e. quasi-natural experiments) in a set of laws meant to induce educational attainment increases in these cohorts. In particular, in the early to mid 1900s, states adjusted their laws related to child labor and compulsory schooling, which dictate a minimum and maximum age between which school attendance is required, a minimum period of attendance, penalties for non-compliance of the laws, and exemption conditions. The argument is that these laws forced adolescents to stay in school longer than they would have otherwise, particularly adolescents who would have dropped out of school (and likely would have limited impacts on those who planned on attending college). Evaluations of these laws have suggested that only three aspects had an impact on educational attainment—the dictated school entry age, the age at which a child could get a work permit and leave school, and whether the state required children with work permits to attend school part-time (Lleras-Muney, 2002).

Lleras-Muney (2005) extended the typical labor market outcomes analysis using this approach to focus on health outcomes. In particular, using grouped regressions and Census data, Lleras-Muney presented relatively large effects of schooling on mortality. These results have been extended by Mazumder (2008, 2010) who questions the ability to detect effects on

mortality with even large datasets like the Census due to the relatively small size (5%) of the population who seemed to be impacted by the laws. Even with these concerns, Mazumder (2008) uses individual SIPP data and shows effects for self rated health. The SIPP also contains information for individuals between ages 60—83, which suggests positive selection into the sample on education and health, likely biasing the results against finding health improvements.

Although less than a decade old, the use of compulsory schooling laws to examine the effects of education on health has now become very common in the literature. Indeed, there are at least two reviews of the literature available (Oreopoulos and Salvanes, 2011; Mazumder, 2011). Oreopoulos and Salvanes (2011) extend the evidence of the effects of education to a variety of non-pecuniary outcomes not often examined. In their review, the authors find effects of education on income, occupational prestige, welfare participation, teenage childbearing, marital status, voting behavior, and life satisfaction. Mazumder (2011) focuses on the education-health/mortality connections and is more pessimistic in his assessment. Supporting Mazumder's assessment, many examinations of this methodology outside of the US have resulted in clear, null results on many health outcomes, especially mortality.

One of the cleanest is by Clark and Royer (2010) who study compulsory schooling laws in Britain and use a regression discontinuity design. In this case, the school leaving age policies in 1947 and 1972 impacted the attainment of about 15 percent of the population (age 16). The authors show the fraction of individuals who completed fewer than 10 years of education fell by 50% and that these increases in schooling were tied to wages. However, the results for mortality are non-existent and the authors show (through simulations) that the findings are not an issue of limited statistical power. They also find no effects on self-reported health (contrary to Mazumder, 2008 in the US), weight, smoking, vitamin use, or high blood pressure status.

More generally, most studies outside the US have found little or no benefit of schooling using compulsory schooling laws as instruments (Braakmann, 2011 for the UK, Meghir et al., 2011 for Sweden, Albouy and Lequien, 2009 for France). However, van Kippersluis et al. (2011) use an RD design with Dutch data and show evidence of mortality reductions associated with schooling (Mazumder, 2011 provides additional discussion). This heterogeneity in estimates across contexts may be important. They suggest the possibility that differences in the particular aspects of the compulsory schooling laws, the birth cohorts of interest, and differences in the potential effects of education on other life outcomes (e.g. earnings) could produce differing results across countries, including the US. Unfortunately, unlike many of the countries in the Mazumder review, the US does not have large registry data to enlist in the analysis. To this point, most relevant datasets, such as the NHIS or NHANES, are likely far too small for precise estimates. This paper focuses on the US context but overcomes this data limitation by utilizing a novel epidemiological dataset of several hundred thousand individuals from the 1990s and the birth cohorts 1925—1945. Results from these data are suggestive of educational impacts on a range of health outcomes in old age for those most likely affected by the compulsory schooling laws.

3. Data and empirical methods

This paper uses novel data from the NIH/AARP Diet and Health Study (Schatzkin et al., 2001). In 1995–96, food frequency questionnaires were mailed to over 3.5 million members of the American Association of Retired Persons (AARP) who were aged 50–69 and resided in one of eight states: California, Florida, Pennsylvania, New Jersey, North Carolina, and Louisiana as well as Atlanta, Georgia and Detroit, Michigan. While all states are represented in the analysis, some states are over-represented. For example, respondents born in Pennsylvania are over 23% of the analysis sample. This feature of the data is a potential limitation with the current study, though most alternative datasets are also not state-representative (NHANES, etc). These areas were selected based on their having cancer registries certified to as having at least 90–95% completeness of case ascertainment as well as having large AARP membership for cost minimization.

The baseline questionnaire focused on diet questions, including 124 food items with portion sizes and 21 questions on nutrient intake, health questions, such as family history of illness, physical activity and medical conditions, and also contained information on sociodemographic characteristics, including race, sex, marital status and date of birth. The mailings resulted in nearly 620,000 responses (17.6%), nearly 570,000 provided useable data. I show below that the useable data is quite similar to the NHIS, both in summary statistics and in baseline regression relationships among variables.

Fortunately, the survey asked respondents for their social security number, allowing matches to state of birth. This allows merging information by state and year for compulsory schooling laws. Because the birth cohorts in the survey range between 1925 and 1945 and thus the individuals were age 15 between 1940 and 1956, this paper uses the laws recently examined in Oreopoulos and Salvanes (2011) that includes more recent changes than other studies (e.g. Lleras-Muney, 2005). Although individuals resided in one of eight states in 1995/6 the state-of-birth data covers all 50 states and DC.

A potential issue with using state of birth rather than state of residence (at age 14) is misclassifying exposure to compulsory schooling laws. This misclassification would reduce the estimates in the first stage (though the F-statistic is still strong in the results presented in the paper). If the migration is random with respect to the compulsory schooling laws, then this measurement error should not have much of an impact on the results. If families move states in response to compulsory schooling laws changes, the results could still be biased. This data limitation should be considered when reviewing the results.

Health outcomes include mortality (linked to the National Death Index), self rated health status (excellent to poor), high blood pressure, diabetes, high cholesterol, heart attack, stroke, emphysema, and weight (BMI, obese, overweight) outcomes. Health outcomes are asked at baseline, except for cholesterol and high blood pressure, which are asked at a one year follow up survey. The key endogenous variable measure is self reported years of schooling. The categories are coded as: less than 8 years (8), 8–11 years (11), 12 years or completed high school (12), post high school training other than college (13), some college (14), college graduate (16), and postgraduate (18). The lack of more precise information on

educational attainment is a limitation of the paper. Because only individuals with low education would be affected by the compulsory schooling laws, I also focus attention on the sample of the nearly 130,000 individuals with fewer than 14 years of schooling (Glymour et al., 2008). Table 1 provides descriptive statistics for the sample with social security numbers and the low education sample.

Two issues with the sample deserve further attention—whether the sample who gave social security number information is a selected sample and whether the sample is similar to nationally representative samples, such as the National Health Interview Survey. As discussed in Schatzkin et al. (2001), the NIH/AARP sample is predominantly white and more educated than the general population. However, other than race and education, the sample differences in health status are relatively small (See Table 1A). The self rated health status means are nearly identical, as are the rates of high blood pressure, diabetes, emphysema, stroke, and weight outcomes between the NIH/AARP and the NHIS. The rate of heart attacks in the AARP sample is higher than NHIS. A second potential issue with the AARP sample is that not all individuals provided their social security number (nearly 370,000 are available). Appendix Table 2 shows the correlates of provision of SSN, which suggests relatively small associations between education, health and provision. Columns 2—4 in Table 2A shows that a basic regression predicting mortality has very similar results whether the full sample or the SSN sample is used. While the coefficient differences are statistically significant between samples, this is not surprising given the very large sample sizes.

The empirical analysis will follow a relatively standard design from the literature, where health outcomes, including mortality, are determined by years of schooling, individual covariates (X), and controls for state of birth (λ) and year of birth (η):

$$\text{health}_{isa} = \beta_0 + \beta_1 \text{schooling}_{isa} + \beta_2 X_{isa} + \lambda_s + \eta_a + \varepsilon_{isa} \quad (1)$$

because schooling is endogenous, compulsory schooling laws that vary by birth cohort and state of birth are used as instruments:

$$\text{schooling}_{isa} = \delta_0 + \delta_1 \text{laws}_{sa} + \delta_2 X_{isa} + \lambda_s + \eta_a + \varepsilon_{isa} \quad (2)$$

where i indexes individuals, a refers to the individuals age (birth cohort), s indicates state of birth, and the errors are clustered at the state of birth level. In particular, state drop out ages and state exceptions of these drop-out age requirements are used as instruments. β_1 is the coefficient of interest, indicating the effect of an additional year of schooling on health outcomes, such as mortality, self reported health status, and health conditions.

4. Results

Although the AARP sample is larger than any individual-level data available for this question in the US, because it sampled higher educated populations (see Table 2A) the

instruments may still be somewhat weak. Table 2 examines the impacts of education on self reported health at baseline (ages 50—69). The baseline specification in Column 1 shows that an additional year of schooling is associated with a 0.09 increase in the 5-point self rated health scale. The estimate for education from a similar regression using the NHIS 1997 wave is 0.10 (available upon request). This estimate is nearly identical when state of birth fixed effects are included (Column 2). In Column 3 and 4 the instrumental variable analysis is presented, showing that the estimated effect on health is doubled but the standard errors do not allow statistical significance and the F-statistics is only 1.3, suggesting that the instrument only weakly predicts schooling for this sample of mostly highly educated individuals (Staiger and Stock, 1997). Columns 5—8 restricts the sample to those who completed less than 14 years of schooling and who arguably would be more likely to be affected by the compulsory schooling laws. Indeed the results suggest that, among these low education individuals, education substantially increases self reported health status. The baseline results (Columns 5 and 6) are nearly 30% higher than the results for the full sample, and the two-stage estimates increase the baseline effects by a factor of 3 as well as increase the F-statistic to nearly 19.

These results motivate the majority of the analysis to be conducted on the sub-set of low educated individuals who are more likely to be affected by the instruments. Table 3 examines three overall health outcomes, including repeating the results for self rated health status, examining a poor health indicator, and examining mortality. Columns 4—6 estimates the effects on poor self reported health (1 = poor/fair, 0 = good, very good, excellent). The OLS results suggested a 4 percentage point reduction in the probability of reporting poor health for each year of schooling, and the effect is slightly reduced with the addition of state of birth fixed effects in Column 5. Like the self rated health scale, the instrumented effects of education on poor health increase by a factor of 4, to a 20 percentage point reduction. Columns 7—9 report the effects of education on the likelihood of death over a 10 year period. The OLS results indicate a nearly 1.9 percentage point reduction in mortality for each year of schooling increase, which is nearly identical to the OLS results in Lleras-Muney (2005) of 1.7 percentage points. The effects are identical with state of birth fixed effects. The IV results in Column 9 suggest a 6.9 percentage point reduction per year of additional schooling, which is slightly higher than the 6.1 percentage point reduction in Lleras-Muney (2005), even using different birth cohorts and compulsory schooling laws. Although the F-statistic is over 16, the large standard errors do not allow rejection of zero effects. Overall, these results are suggestive that for those induced to complete more schooling due to the compulsory laws, the later health benefits could be quite large.

One limitation with many large datasets, such as the Census, is the small number of health related outcomes available. The AARP data includes a range of health conditions and behaviors. Table 4 presents results for a set of health conditions available in the data. In Panel 1 in Columns 1—3, the OLS estimates suggest a nearly 1 percentage point reduction in the likelihood of ever having experienced a heart attack. These estimates increase to nearly 9 percentage points in the IV analysis, which is a very large effect given the 16% prevalence rate in the sample. Similar results are found for stroke and diabetes. The effect for stroke increases from 0.5 percentage points to over 4 percentage points (p -value < 0.14), which is also very large, as the prevalence of stroke in the sample is 3%. For diabetes, the

effect increases from 1.3 percentage points to 2.3 percentage points with instruments, but the larger standard errors do not allow rejections of the OLS or zero effects. In panel two, three additional conditions are examined, including emphysema, high cholesterol, and high blood pressure. OLS estimates actually suggest increases in emphysema and cholesterol reports, but the IV sign flips with cholesterol and becomes larger but statistically insignificant with emphysema. For high blood pressure, the OLS estimates suggest a 1 percentage point reduction while the IV estimates suggest a 1 percentage point increase, so that the overall effects are uninformative. A final limitation is the lack of a clear summary measure of health status in the data. Self reported health status is subjective and is not straightforward to interpret; mortality is objective but may miss the totality of health effects from education. Appendix Table 3 presents results that use factor analysis on the outcomes in the paper as a first step toward a summary measure.

Table 5 examines health behavior outcomes associated with educational attainments. The top panel examines alcohol intake, vitamin intake and daily caloric intake. The results for alcohol intake are uninformative. Vitamin use appears to be positively related to education; the OLS estimates suggest a 6 percentage point increase and the IV results are increased to a 31 percentage point increase (not statistically significant). Daily caloric intake is reduced by approximately 65 calories in the OLS results and 87 calories in the IV analysis (not statistically significant). Panel 2 presents suggestive evidence of reduction in BMI and rates of obesity. Individual's BMI is lower in highly educated individuals in OLS, which is doubled with IV and the rate of obesity is nearly 2 percentage points lower for each year of additional schooling in the OLS, which increases to 4 percentage points with IV (not statistically significant) (see also Fletcher and Frisvold, 2011).

Finally, as a falsification check, Table 6 predicts the “effects” of education on height. Because height is generally fixed in late adolescence and also related to cognitive skills (Case and Paxson, 2008), any positive relationship between schooling and height likely reflects reverse causality. Columns 1 and 2 show a positive relationship between education and height in the sample, even controlling for state of birth fixed effects. However, Column 3 shows that the IV analysis flips the sign and renders the coefficient not statistically different from zero. This is suggestive evidence that the compulsory schooling laws may be breaking the reverse causality links between education and health in the sample and allowing the effects of education on health to be examined.

5. Conclusion

Although there is now a large and growing literature examining the effects of education on health and mortality, the results have been mixed. A key insight by Lleras-Muney (2005), who extended work by Angrist and Krueger (1991), was to use state and year-level variation in compulsory schooling laws to instrument for education in order to circumvent issues of reverse causality and omitted factors that may affect estimates of the education—health relationship. However, subsequent studies using these laws in other countries have found few if any effects of education on health in Europe. These new findings could be due to flaws in the data or methods from the US applications or heterogeneous effects of education that varies by context and time period. Unfortunately, unlike European countries, the US has

few datasets that would allow the precision necessary to replicate and extend prior US findings and provide additional evidence of whether context or methods may be behind the differential results.

This paper revisits this question in the US context by leveraging novel epidemiological data not used in the economics of education literature. A large set of several hundred thousand AARP members born between 1925 and 1945 were mapped to state-compulsory schooling laws in the mid 20th century. Instead of the economic variables typical of many large US surveys (e.g. Census), this survey was focused on health and diet, allowing several health outcomes to be examined for adults above age 50.

The results suggest some support for a causal effect of educational attainment on several health outcomes in the survey, although a second key result is that precision issues are found even in a study of more than a hundred thousand individuals in the US. This latter result suggests that without registry data mimicking those available in Europe, this design may not be able to uncover some relationships in US populations. An alternative view is that compulsory schooling laws were less binding for cohorts in this study (birth cohorts 1925—1945) such that data using earlier birth cohorts could potentially be used to estimate more precise effects, even with similarly sized (or smaller) samples. It is not clear whether data that includes health outcomes is available over this time period in the US.

Some results in the current paper are consistent with other US work as well as that in Holland (i.e. van Kippersluis et al., 2011) that education may be linked to several morbidities and mortality. The effects are often large, suggesting that individuals in the mid 20th century who completed additional schooling due to the law have greater health returns. However, there are also important limitations with the data used in this analysis. The data are not nationally representative, and are more highly educated than nationally representative samples. While the analysis is performed on a sub-sample of individuals who completed fewer than 14 years of schooling, it could be the case that these individuals are positively selected within the birth cohort who survive until data collection. It is less clear whether the impacts of compulsory schooling laws would lead to local average treatment effect (LATE) that is larger or smaller than a more representative population followed prospectively. This issue, among many others discussed in the paper, suggests that the results using this sample are not definitive and would profit from additional data and potentially different research designs. On the other hand, if these results are replicated, a next important research question is to focus on the sources of the potential heterogeneity in estimated effects across countries and time periods.

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Appendix Tables

Table 1A

Comparison between NIA/AARP Sample and NHIS 1997 sample. Adults aged 50—69.

Variable	Obs	Mean	Std Dev	Min	Max	Obs	Mean	Std Dev	Min	Max
Years of Schooling	367,359	14.59	2.34	8	18	8455	12.78	3.16	0	21
Self Rated Health Status	371,550	3.55	0.96	1	5	17,411	3.53	1.12	1	5
Poor Health	376,415	0.13	0.33	0	1	17,411	0.17	0.50	0	1
High Blood Pressure	212,684	0.43	0.50	0	1	8502	0.38	0.49	0	1
Diabetes	376,415	0.09	0.29	0	1	8506	0.12	0.33	0	1
Heart Attack	376,415	0.14	0.35	0	1	8501	0.06	0.23	0	1
Emphysema	376,415	0.03	0.17	0	1	8506	0.03	0.18	0	1
Stroke	376,415	0.02	0.15	0	1	8505	0.03	0.18	0	1
BMI	369,935	27.19	5.04	0	189	8255	27.18	5.28	10.17	78.11
Obese	369,935	0.22	0.42	0	1	8533	0.26	0.44	0	1
Overweight/Obese	369,935	0.65	0.48	0	1	8533	0.65	0.48	0	1
Ever Smoker	364,152	0.65	0.48	0	1	8453	0.58	0.49	0	1
Male	376,415	0.38	0.49	0	1	8533	0.48	0.50	0	1
Non White	376,415	0.07	0.26	0	1	8533	0.14	0.35	0	1
Married	376,415	0.70	0.46	0	1	8533	0.71	0.46	0	1

Appendix Table 2

The predictors of provision of social security number and comparison of results by sampled.

Outcome	State sample	Mortality	Mortality	Mortality
Sample	Full	Full	State sample	No State sample
Male	−0.048*** (0.001)	−0.069*** (0.001)	−0.071*** (0.001)	−0.062*** (0.002)
Non White	−0.086*** (0.002)	−0.024*** (0.002)	−0.020*** (0.002)	−0.024*** (0.003)
Married	−0.008*** (0.002)	−0.041*** (0.001)	−0.043*** (0.001)	−0.036*** (0.002)
Education	0.015*** (0.000)	−0.005*** (0.000)	−0.006*** (0.000)	−0.005*** (0.000)
Health Status	0.007*** (0.001)	−0.075*** (0.001)	−0.080*** (0.001)	−0.065*** (0.001)
Constant	0.467*** (0.005)	0.540*** (0.004)	0.584*** (0.004)	0.470*** (0.005)
Observations	541,654	541,654	362,815	178,839
R-squared	0.012	0.056	0.060	0.047

Appendix Table 3

Effects of schooling on health summary measure.

<u>Outcome</u>	<u>Health Factor</u>	<u>Health Factor</u>
<u>Sample</u>	<u>Low Education</u>	<u>Low Education</u>
<u>Specification</u>	<u>OLS</u>	<u>IV</u>
<u>Fixed Effects</u>	<u>YOB/State</u>	<u>YOB/State</u>
Schooling	-0.079 *** (0.004)	-0.224 (0.206)
Observations	122,328	122,095
R-squared	0.029	0.005
Number of States	49	49
F-Statistic		15.542

Robust standard errors clustered at the state-of-birth level. ***1%, **5%, *10%.

Controls: Gender, Race, Marital Status. YOB: Year of Birth Dummies.

Health Factor is the first principle component of the set of health outcomes in early tables.

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Table 1
NIA/AARP Diet and Health Study. Descriptive statistics for sample with social security numbers and low education.

Variable	Sample with SSNs					Sample with low education				
	Obs	Mean	Std Dev	Min	Max	Obs	Mean	Std Dev	Min	Max
Years of Schooling	367,359	14.59	2.34	8	18	127,792	12.07	0.86	8	13
Self Rated Health Status	371,550	3.55	0.96	1	5	126,074	3.33	0.95	1	5
Poor Health	376,415	0.13	0.33			127,792	0.18	0.38		
Mortality	376,415	0.15	0.36			127,792	0.18	0.38		
High Blood Pressure	212,684	0.43	0.50			68,016	0.47	0.50		
Cholesterol	207,272	0.50	0.50			65,833	0.48	0.50		
Diabetes	376,415	0.09	0.29			127,792	0.11	0.31		
Heart Attack	376,415	0.14	0.35			127,792	0.16	0.36		
Emphysema	376,415	0.03	0.17			127,792	0.04	0.20		
Stroke	376,415	0.02	0.15			127,792	0.03	0.17		
Physical Activity	373,808	3.11	1.45	0	5	126,704	2.89	1.54	0	5
Vitamins	232,641	2.54	1.78	0	4	76,258	2.53	1.80	0	4
Alcohol	376,415	1.02	2.89	0	78.61	127,792	0.97	3.30	0	78.61
Calories (Daily)	376,415	1886.86	961.98	27	65,823	127,792	1935.77	1076.23	67	62,810
BMI	369,935	27.19	5.04	0	189	124,945	27.64	5.28	0	187
Obese	369,935	0.22	0.42			124,945	0.26	0.44		
Overweight/Obese	369,935	0.65	0.48			124,945	0.68	0.46		
Ever Smoker	364,152	0.65	0.48			123,458	0.68	0.47		
Height	373,636	1.73	0.10	1.2192	2.413	126,707	1.71	0.10	1.2192	2.3876
State Law—Drop Out Age	375,459	16.29	0.83	14	18	127,550	16.35	0.90	14	18
State Law—Drop Out Age Exemption	375,459	14.80	0.88	13	16	127,550	14.83	0.91	13	16
Male	376,415	0.38	0.49			127,792	0.45	0.50		
Non White	376,415	0.07	0.26			127,792	0.07	0.26		
Married	376,415	0.70	0.46			127,792	0.68	0.47		

Table 2

The effects of years of schooling on Self Reported Health Status (SRHS). Baseline and IV estimates.

Outcome	SRHS		SRHS		SRHS		SRHS		SRHS		SRHS	
	OLS	YOB	OLS	YOB	OLS	YOB	OLS	YOB	OLS	YOB	OLS	YOB
Specification	OLS	YOB	OLS	YOB	OLS	YOB	OLS	YOB	OLS	YOB	OLS	YOB
Sample	SSN	SSN	SSN	SSN	SSN	SSN	SSN	SSN	SSN	SSN	SSN	SSN
Fixed Effects	YOB	YOB	YOB	YOB	YOB	YOB	YOB	YOB	YOB	YOB	YOB	YOB
Schooling	0.087 *** (0.002)	0.084 *** (0.001)	0.158 (0.229)	0.113 *** (0.005)	0.110 *** (0.004)	0.393 *** (0.145)	0.073 *** (0.011)	0.061 *** (0.014)	0.073 *** (0.011)	0.061 *** (0.014)	0.073 *** (0.011)	0.061 *** (0.014)
Male	0.083 *** (0.007)	0.081 *** (0.007)	0.128 (0.141)	0.092 *** (0.008)	0.091 *** (0.008)	0.073 *** (0.011)	0.073 *** (0.011)	0.061 *** (0.014)	0.073 *** (0.011)	0.061 *** (0.014)	0.073 *** (0.011)	0.061 *** (0.014)
Non white	-0.200 *** (0.015)	-0.200 *** (0.012)	-0.183 *** (0.062)	-0.157 *** (0.019)	-0.157 *** (0.016)	-0.088 *** (0.044)	-0.088 *** (0.044)	-0.246 *** (0.028)	-0.088 *** (0.044)	-0.246 *** (0.028)	-0.088 *** (0.044)	-0.246 *** (0.028)
Married	0.108 *** (0.005)	0.109 *** (0.005)	0.113 *** (0.015)	0.095 *** (0.006)	0.096 *** (0.006)	0.094 *** (0.006)	0.094 *** (0.006)	0.008 * (0.004)	0.094 *** (0.006)	0.008 * (0.004)	0.094 *** (0.006)	0.008 * (0.004)
Dropage												
Dropage exemptions												
Constant	2.076 *** (0.029)	2.294 *** (0.033)	15.448 *** (0.304)	1.913 *** (0.069)	1.961 *** (0.054)	11.191 *** (0.129)	125.835	125.835	11.191 *** (0.129)	125.835	125.835	125.835
Observations	362,815	362,815	361,895	126,074	126,074	125,835	125,835	125,835	126,074	125,835	125,835	125,835
R-squared	0.055	0.052	0.021	0.018	0.018	-0.048	-0.048	0.015	0.018	0.015	-0.048	0.015
fstat			1.266			18.347	18.347	18.347			18.347	18.347
Number of stateid		51	49			49	51	49			49	49

Notes: Robust standard errors clustered at the state-of-birth level. *** 1%, ** 5%, * 10%.

Table 3

The effects of education on health outcomes.

Outcome	SRHS		SRHS		SRHS		Poor Health		Poor Health		Mortality		Mortality		Mortality	
	Low Education	OLS	Low Education	OLS	Low Education	OLS	Low Education	OLS	Low Education	OLS	Low Education	OLS	Low Education	OLS	Low Education	OLS
Sample																
Specification																
Fixed Effects	YOB	YOB	YOB/State	YOB	YOB/State	YOB	YOB/State	YOB	YOB/State	YOB	YOB/State	YOB	YOB/State	YOB	YOB/State	YOB/State
Schooling	0.113 *** (0.005)		0.110 *** (0.004)		0.393 *** (0.145)		-0.044 *** (0.002)		-0.043 *** (0.002)		-0.019 *** (0.002)		-0.019 *** (0.002)		-0.019 *** (0.002)	
Observations	125,835		125,835		125,835		127,550		127,550		127,550		127,550		127,550	
R-squared	0.018		0.018		-0.048		0.015		0.014		0.042		0.042		0.030	
Number of States		49		49		49		49		49		49		49		49
F-Statistic					18.347										16.362	

Notes: Robust standard errors clustered at the state-of-birth level. ***1%, **5%, *10%.

Controls: Gender, Race, Marital Status, YOB: Year of Birth Dummies.

Table 4

The effects of education on health conditions.

Outcome	Heart Attack		Stroke		Diabetes	
	Low Education		Low Education		Low Education	
	OLS	YOB/State	OLS	YOB/State	OLS	YOB/State
Fixed Effects						
Schooling	-0.009 *** (0.001)	-0.009 *** (0.001)	-0.006 *** (0.001)	-0.006 *** (0.001)	-0.013 *** (0.002)	-0.013 *** (0.002)
Observations	127,550	127,550	127,550	127,550	127,550	127,550
R-squared	0.031	0.030	0.005	0.005	0.009	0.008
Number of stateid	49	49	49	49	49	49
fstat		16.362		16.362		16.362

Outcome	Emphysema		Cholesterol		HBP	
	Low Education		Low Education		Low Education	
	OLS	YOB/State	OLS	YOB/State	OLS	YOB/State
Fixed Effects						
Schooling	-0.010 *** (0.001)	-0.010 *** (0.001)	0.005 *** (0.002)	0.005 *** (0.002)	-0.043 (0.090)	-0.043 (0.090)
Observations	127,550	127,550	65,715	65,715	67,900	67,900
R-squared	0.007	0.007	0.004	0.005	-0.001	0.016
Number of stated	49	49	49	49	49	49
fstat		16.362		9.213		6.650

Notes: Robust standard errors clustered at the state-of-birth level. ***1%, **5%, *10%.

Controls: Gender, Race, Marital Status, YOB; Year of Birth Dummies.

Table 5

The effects of education on health behaviors.

Outcome	Alcohol		Alcohol		Vitamins		Vitamins		Calories	
	Low Education		Low Education		Low Education		Low Education		Low Education	
	OLS	YOB	OLS	YOB/State	OLS	YOB	OLS	YOB	OLS	YOB/State
Fixed Effects										
Schooling	-0.003 (0.019)	-0.015 (0.018)	0.819 (0.887)	0.063 *** (0.009)	0.058 *** (0.009)	0.310 (0.427)	-64.401 *** (6.014)	-63.241 *** (6.023)	-87.737 (252.286)	
Observations	127,550	127,550	-0.021	76,101	76,101	76,101	127,550	127,550	127,550	
R-squared	0.025	0.026	-0.021	0.002	0.002	-0.012	0.075	0.074	0.074	
Number of stateid	49	49	49	49	49	49	49	49	49	
fstat			16.362			20.996			16.362	

Outcome	BMI		BMI		Obesity		Obesity	
	Low Education		Low Education		Low Education		Low Education	
	OLS	YOB	OLS	YOB/State	OLS	YOB	OLS	YOB/State
Fixed Effects								
Schooling	-0.224 *** (0.029)	-0.222 *** (0.027)	-0.404 (0.655)	-0.018 *** (0.002)	-0.018 *** (0.002)	-0.042 (0.044)		
Observations	124,706	124,706	124,706	124,706	124,706	124,706		
R-squared	0.010	0.010	0.009	0.009	0.009	0.007		
Number of stateid	49	49	49	49	49	49		
fstat			14.423			14.423		

Notes: Robust standard errors clustered at the state-of-birth level. ***1%, **5%, *10%.

Controls: Gender, Race, Marital Status. YOB: Year of Birth Dummies.

Table 6

Falsification test: The effects of education on height.

<u>Outcome</u>	<u>Height</u>	<u>Height</u>	<u>Height</u>
<u>Sample</u>	<u>Low Education</u>	<u>Low Education</u>	<u>Low Education</u>
<u>Specification</u>	<u>OLS</u>	<u>OLS</u>	<u>IV</u>
<u>Fixed Effects</u>	<u>YOB</u>	<u>YOB/State</u>	<u>YOB/State</u>
Schooling	0.005 *** (0.001)	0.005 *** (0.001)	−0.004 (0.007)
Observations	126,467	126,467	126,467
R-squared	0.524	0.526	0.521
Number of States		49	49
F-Statistic			15.314

Notes: Robust standard errors clustered at the state-of-birth level. ***1%, **5%, *10%.

Controls: Gender, Race, Marital Status. YOB: Year of Birth Dummies.