Finance and Economics Discussion Series

Federal Reserve Board, Washington, D.C.
ISSN 1936-2854 (Print)
ISSN 2767-3898 (Online)

Revisiting the Effect of Education on Later Life Health

Avinash Moorthy, Theodore F. Figinski, and Alicia Lloro

2022-007

Please cite this paper as:
Moorthy, Avinash, Theodore F. Figinski, and Alicia Lloro (2022). "Revisiting the Effect of Education on Later Life Health," Finance and Economics Discussion Series 2022-007. Washington: Board of Governors of the Federal Reserve System, https://doi.org/10.17016/FEDS.2022.007.

REVISITING THE EFFECT OF EDUCATION ON LATER LIFE HEALTH*

Avinash Moorthy
Harvard Kennedy School

Theodore F. Figinski
Department of the Treasury

Alicia Lloro
Federal Reserve Board of Governors

Abstract

We provide new evidence on the effect of education on later life health. Using variation in state compulsory schooling laws, we examine education's effect on a range of outcomes encompassing physical health, decision-making, and life expectancy. We employ under-utilized Health and Retirement Study data linked to restricted geographic identifiers, allowing us to match individuals more accurately to compulsory schooling laws. While positively related to educational attainment, compulsory schooling laws have no significant effect on later life health outcomes. Our results suggest that increased educational attainment has no significant causal effect on health.

Keywords: Compulsory school attendance laws, returns to education, human capital, health, education policy

JEL Classifications: H75, I12, I26, I28, J24

* The findings, conclusions, views, and opinions are solely those of the authors and do not necessarily represent the views of the U.S. Department of the Treasury, the Federal Reserve Board of Governors or its research staff, the United States government, or the Federal Reserve Bank of Chicago, where Moorthy previously worked on this project. We thank Marianne Bitler, Jeff Larrimore, Bhash Mazumder, and Marianne Wanamaker for their helpful comments. This research represents our independent research efforts; the U.S. Department of the Treasury, the Federal Reserve Board of Governors, and the Federal Reserve Bank of Chicago had no influence on the content, direction, or results of this research. All results using the restricted geographic data from the Health and Retirement Study (HRS) have been cleared by HRS staff. Any and all errors are our own. This version was completed on February 4, 2022.

Avinash Moorthy: amoorthy@g.harvard.edu. 79 John F. Kennedy Street, Cambridge, MA 02138.
Theodore F. Figinski: Theodore.Figinski@treasury.gov. 1500 Pennsylvania Ave NW, Washington, DC 20220.
Alicia Lloro: Alicia.Lloro@frb.gov. 20th Street and Constitution Avenue NW, Washington, DC 20551.

1. Introduction

Individuals with higher levels of education tend to have better health outcomes and live longer. For example, in 2005 a twenty-five year old male high school graduate could expect to live seven years longer than a twenty-five year old male non-high school graduate (Hummer and Hernandez 2013). Individuals with lower levels of educational attainment are also more likely to be at risk for heart attacks and strokes (Qureshi et al. 2003).

The strong relationship between educational attainment and health suggests that investing in education could be one way of improving health outcomes. Proposals to increase the high school dropout age to eighteen, and thereby reduce the number of individuals without a high school degree, cite improvements in health outcomes and life expectancy as a rationale (Messacar and Oreopoulos 2012). This rationale, however, requires higher educational attainment to cause better health.

An extensive literature uses the variation created by changes in compulsory schooling laws to investigate the returns to education. Compulsory schooling laws dictate the amount of time a student must remain in school before leaving. The age at which students can drop out of school, which typically ranges between sixteen and eighteen with substantial variation across states and over time, traditionally receives the most attention. More recent work focuses on the minimum dropout age in conjunction with the entry age to determine the number of years of required schooling (Stephens and Yang 2014). Generally, the economics literature has treated a state's decision to raise or lower the compulsory schooling age as an exogenous shock to an individual's educational attainment, especially affecting those who were on the periphery of dropping out.

The literature examining the effect of compulsory schooling laws on health remains largely inconclusive. Some studies find that compulsory schooling laws decrease mortality rates (Lleras-Muney 2005; van Kippersluis et al. 2009), while others find that they have no effect on mortality (Albouy and Lequin 2009; Clark and Royer 2013; Mazumder 2012; Meghir et al. 2018). Similarly, some studies suggest that compulsory schooling laws may improve health outcomes such as self-reported health, weight, long-term illness, memory, infant health, and teenage pregnancy, whereas others find no significant effect on fertility, infant health, or risky behaviors such as smoking (Adams 2002; Black et al. 2011; Braakmann 2011; Brunello et al. 2016; Crespo et al. 2014; Cygan-Rehm and Maeder 2013; Dilmaghani 2020; Fletcher 2015; Grabner 2009; Glymour et al. 2008; Kemptner et al. 2011; Oreopoulos 2007). Studies find conflicting results regarding infant health and mental health (Avendano et al. 2020; Crespo et al. 2014; Günes 2015; McCrary and Royer 2011).

We contribute to this literature by providing new evidence on the causal effect of education on health outcomes. To do so, we follow past studies by relying on variation in compulsory schooling laws, and use data from the Health and Retirement Study (HRS) linked to restricted geographic identifiers. Surprisingly, given the detail of the questions asked in the HRS, the HRS has been relatively under-utilized for analyses of education on health.[1] Using the HRS allows us to explore more granular health outcomes than mortality or self-rated overall health, which have been a focus of much of the previous literature. This gives us a clearer perspective on an individual's health and healthcare utilization, whereas focusing solely on the effect on mortality potentially conceals unobservable yet nontrivial improvements in an individual's quality of life. In addition, the HRS allows us to improve on other studies, such as Lleras-Muney

[1] The HRS data, which can be matched to administrative earnings records, have also been under-utilized to investigate the effect of compulsory schooling laws on earnings. See Figinski, Lloro, and Li (2019).

(2005) and Black, Hsu, and Taylor (2015), because we are able to follow individuals rather than cohorts or synthetic cohorts within or across data sources.

The HRS asks respondents an extensive battery of questions related to health. We focus on the effect of the number of years of required schooling on an individual's body mass index (BMI) and ability to perform activities of daily living (ADLs), as well as their likelihood of getting a flu shot, visiting a doctor regularly, ever smoking, or having diabetes or cancer. For consistency with past studies, we also look at the effect of compulsory schooling laws on mortality and self-rated health. We find that while an increase in the number of years of compulsory schooling significantly increases educational attainment, this increased education fails to have a statistically significant effect on later-life health outcomes.

We improve upon the most closely related study to ours, Fletcher (2015), by utilizing the HRS, which is designed to provide a nationally representative sample of individuals older than age 50. Fletcher (2015) uses the NIH/AARP Diet and Health Study data to examine education's effect on a wide range of health outcomes including mortality, self-rated measures, and cardiovascular and weight outcomes. Fletcher (2015) finds a positive relationship of education on health for some outcomes. The Diet and Health Study data, however, are restricted to respondents living in eight states and consist predominantly of individuals with above-average levels of education. Further, whereas Fletcher (2015) matches individuals to compulsory schooling laws in their state of birth, we are able to more accurately match to state of residence at age 10.

To our knowledge, only two studies have used the HRS to analyze education's effect on health: Glymour et al. (2008) and Adams (2002). Glymour et al. (2008) focuses on mental health and uses the 5 percent sample of the 1980 census for the first stage of compulsory schooling laws

on education. However, Zhao et al. (2019) caution that a two-sample instrumental variable approach will be biased if the two samples come from heterogenous populations with different distributions of the instrument. Adams (2002) looks at self-reported health status among the initial HRS cohort when they first entered the panel in 1992, using quarter of birth as an instrument for educational attainment. The validity of quarter of birth has been questioned by Bound and Jaeger (2000), as well as by Buckles and Hungerman (2013), who document large differences in family background by quarter of birth, which can account for differences later in life. We improve upon these studies by estimating the first stage with data from the HRS and using compulsory schooling laws as an instrument for education. Moreover, by using data from 1992 to 2016, we can analyze more recent cohorts and better observe later-life health and mortality.

2. Data: Health and Retirement Study

To estimate the effect of compulsory schooling laws on later life health, we use data from the University of Michigan's HRS. The HRS is a longitudinal survey administered every two years, interviewing a nationally representative sample of approximately 20,000 individuals older than age 50. RAND provides a processed version of the raw HRS data files to facilitate research analysis, referred to as RAND HRS. The HRS began collecting data in 1992, and the most recent available data (at the time we began this project) in the RAND HRS are from the 2016 survey wave. From the RAND HRS, we utilize data on self-reported years of education, which is capped at 17 years, demographics such as year of birth, gender, and race, and responses to health outcomes. The HRS includes extensive information on a variety of health outcomes. We focus on whether an individual: (1) survived to age 70, (2) ever experienced "some difficulty" with two or more activities of daily living (ADLs) defined as being able to bathe, dress, eat, get in and out

of bed, and walk across a room, (3) ever reported being in fair or poor health, (4) ever reported a BMI greater than or equal to 25 or (5) 30, (6) ever smoked, (7) was ever diagnosed with diabetes or (8) cancer, (9) ever reported getting a flu shot during the sample period, and (10) visited a doctor at least once between each survey wave.[2]

These outcomes give us a comprehensive picture of an individual's health and health care utilization. For example, an individual's ability to perform ADLs has significant implications on their wellbeing but may have less of an effect on their life expectancy. Further, ADLs are an important determinant of health expenditures, as tax-qualified long-term care insurance policies are required to include a trigger that pays long-term care benefits when an individual experiences at least two ADL limitations, and Medicaid uses ADL limitations to determine eligibility for long-term care assistance (Administration for Community Living 2017; National Association of Insurance Commissioners 2019).[3]

Moreover, our outcomes allow us to implicitly test potential mechanisms for how compulsory schooling laws affect health. Explanations range from improved decision-making to better information about health to greater awareness of new medical technologies (Grossman 1972; Kenkel 1991; Rosenzweig and Schultz 1991; Nelson and Phelps 1966). We examine the effect of compulsory schooling laws on whether an individual ever smoked or was ever diagnosed with diabetes or cancer, or if they ever got a preventive flu shot or visited a doctor regularly. In doing so, we can speak to whether increased education causes individuals to take

[2] In results not shown, we find similar results if we examine whether an individual ever smoked during the HRS sampling period. Finally, the HRS also contains data on whether an individual (1) has ever had high-blood pressure or hypertension, (2) has ever had arthritis, (3) has ever had psychological problems, and (4) has ever reported binge drinking. While our findings for these outcomes are consistent with the ones presented in this study, for brevity, we exclude the results on these outcomes from our discussion. Results for these outcomes are available upon request.
[3] We note that the Census defines disability as having difficulty with at least one ADL. Given the age of our sample and the importance of at least two ADLs for long-term care benefits, we chose to focus on the presence of at least two ADL limitations. Using at least one ADL versus at least two ADLs has no effect on our results (available upon request).

part in less risky behaviors, access potentially more innovative tests to screen for diseases, and utilize health care.

Our data on compulsory schooling laws are drawn from Stephens and Yang (2014). Compulsory schooling laws dictate the number of years of required schooling. We follow Stephens and Yang (2014) and create a set of indicator variables corresponding to being required to attend seven, eight, and nine or more years of schooling.[4] We assign the appropriate compulsory schooling laws to an individual in the RAND HRS survey data by matching them to restricted HRS geographic data. Differing from past literature, we assign compulsory schooling laws based on the individual's state of residence at age 10, rather than state of birth. This provides a more accurate measure of the required number of years of compulsory attendance and reduces measurement error.[5]

Table 1 shows summary statistics, weighted by the HRS provided person-level weight from the first wave in which the individual responded to the HRS. Overall averages are shown, as well as averages by ten-year birth cohort. Our sample is 50 percent male, 83 percent white, 11 percent black, and 5 percent Hispanic, roughly in line with American averages for individuals over age 50 (U.S. Census Bureau 2019). The average educational attainment of our sample is 12.9 years and increases over time following the national trend (Schmidt 2018). Figure 1 shows a

[4] We also define compulsory schooling according to the methodologies of Acemoglu and Angrist (2000) and Oreopoulos (2009). Acemoglu and Angrist (2000) define years of compulsory attendance as the larger of the number of years of schooling required before dropping out and the difference between the minimum dropout age and the maximum enrollment age. This summary measure is coded as a series of indicator variables: (1) less than or equal to eight years of required attendance, (2) nine years of required attendance, (3) ten years of required attendance, and (4) eleven or more years of required attendance. Oreopoulos (2009) focuses on the minimum school leaving age, creating an indicator variable for if the dropout age is greater than age 16. Results shown in Tables Table A1 and Table A2A2. We note, however, that the majority of first stage Olea-Pflueger (2013) F-statistics using categorical measures of compulsory schooling are substantially less than the conventional measure of 10 (statistics available upon request). Nevertheless, we report the results as a comparison to prior studies.

[5] As a robustness check, we also match based on state of birth. Results shown in Table A3. The F-statistic for the first stage is 7.18.

corresponding increase in years of compulsory schooling over time. For cohorts born prior to the mid-1940s, the average number of years of compulsory attendance was increasing at a similar rate as the number of years of schooling. This is suggestive that increasing compulsory attendance increases the number of years of schooling individuals attained.

Regarding our health outcomes, we find 78 percent of our sample during their time in the sample had a BMI over 25, and 40 percent ever had a BMI exceeding 30. Around 58 percent of individuals reported smoking at least once, and 22 percent and 18 percent had diabetes and cancer respectively. Further, 73 percent have received a flu shot, 77 percent visited a doctor regularly, and 22 percent reported difficulty with two or more ADLs. With regard to self-reported measures, 14 percent ever stated that they were in fair or poor health.

Population estimates based on the HRS data are comparable to those based on the decennial census data typically used by previous research, such as Lleras-Muney (2005). The HRS sample is slightly more educated, male, and white, although these differences diminish for more recent cohorts.[6] The comparability of the HRS to the decennial census data suggests that any difference between our results and the results of previous research would not necessarily be driven by different samples.

3. Econometric Model

We employ two approaches to measure the effect of compulsory schooling laws on health outcomes: a two-stage least squares (2SLS) approach using variation in compulsory schooling laws as an instrument for years of education and a reduced form approach. The 2SLS approach is standard in the returns to schooling literature (Acemoglu and Angrist 2000; Angrist and Krueger 1991; Stephens and Yang 2014) to address endogeneity present in the education-health

[6] Summary statistics using the decennial censuses are available upon request.

relationship. We also estimate a reduced form, looking at the direct effect of compulsory schooling laws on health outcomes.

The first stage of our 2SLS model estimates the effect of the years of required schooling on years of education, E_i. We follow Stephens and Yang (2014) and code the years of required schooling as a set of indicator variables corresponding to being required to attend seven ($RS7_i$), eight ($RS8_i$), and nine or more ($RS9_i$) years of schooling. The second stage then uses the predicted years of education to measure education's effect on various health outcomes, represented by Y_i. Since all of our health outcomes are binary indicators, we use a linear probability model for the second stage. We include controls for year of birth, quarter of birth, state of birth, state of residence at age 10, and a vector of demographic controls, X_i consisting of race, Hispanic, and gender indicators. Our base specification is:

$$E_i = \alpha + \beta_1 RS7_i + \beta_2 RS8_i + \beta_3 RS9_i + \beta_4 BirthYear_i + \beta_5 BirthQuarter_i + \beta_6 State{:}\,Birth_i + \beta_7 State{:}\,Age10_i + \beta_8 X_i \quad (1)$$

$$Y_i = \theta + \gamma_1 \hat{E}_i + \gamma_2 BirthYear_i + \gamma_3 BirthQuarter_i + \gamma_4 State{:}\,Birth_i + \gamma_5 State{:}\,Age10_i + \gamma_6 X_i \quad (2)$$

Recent evidence has shown the importance of controlling for region-by-birth-year fixed effects and variations of school quality across the country (Stephens and Yang 2014). Thus, we also run specifications with (1) region of residence at age 10 by year of birth fixed effects and (2), per Card and Krueger (1992), include pupil-to-teacher ratios, term length, and relative teacher wages as measures of school quality. Lastly, we experiment with restricting the analysis to individuals born between 1906 and 1955 to create a sample consistent with Stephens and Yang (2014).

Finally, we also employ a reduced form model because, as we discuss in the following section, while we find compulsory schooling laws increase years of schooling, with first-stage coefficients very similar to those using the decennial census data employed in the previous

literature, the effective F-statistics are lower than the conventional level of 10 (Table 2). Our finding of effective F-statistics below 10 is surprising and interesting given that these instruments generally produce F-statistics well above 10, and the Adams (2002) study, which also uses the HRS data, finds F-statistics well above 10.

To provide evidence that our results are not driven by weak instruments, we run a reduced form model, which offers valid inference and is robust to weak instruments (Chernozhukov and Hansen 2008). In our reduced form model, we replace years of education, E_i, in equation (1) with our various health outcomes, represented by Y_i. We estimate the reduced form model using a probit specification and report the average marginal effect.[7]

4. Results

Overall, our results suggest that while compulsory schooling laws have an effect on the number of years of schooling, they have no statistically significant effect on health outcomes. In many cases, the estimates are wrong signed.

If compulsory schooling laws are to improve health outcomes, we would expect this effect to operate through increased educational attainment. We verify that compulsory schooling laws increase years of schooling (Table 2). Specifically, we find that higher levels of required schooling increase the years of education an individual receives by approximately 0.4 to 0.5 years. These findings are consistent with those of Acemoglu and Angrist (2000), Angrist and Krueger (1991), Oreopoulos (2007), and Stephens and Yang (2014), all of whom use the

[7] Under the reduced form probit specification, equation (1) takes the form of: $\text{Prob}(Y_i = 1) = \Phi(\alpha + \beta_1 RS7_i + \beta_2 RS8_i + \beta_3 RS9_i + \beta_4 BirthYear_i + \beta_5 BirthQuarter_i + \beta_6 State{:}\,Birth_i + \beta_7 State{:}\,Age10_i + \beta_8 X_i)$. The average marginal effect for the j[th] covariate, which is binary, is calculated as: $\frac{1}{N}\sum_{i=1}^{N}\left[\Phi\left(x_i'\hat{\beta}|x_{ij}=1\right) - \Phi\left(x_i'\hat{\beta}|x_{ij}=0\right)\right]$.

decennial census data. We present first-stage estimates using both the HRS and the decennial census data for comparison (Table 2).

Although we find compulsory schooling laws increase years of schooling, the effective F-statistics are lower than the conventional level of 10 (Table 2).[8] To provide evidence that our results are not driven by weak instruments, we consider several additional specifications: (1) we plug the first-stage estimates from the decennial census data into the second-stage using the HRS; (2) we restrict the sample to cohorts exhibiting a higher F-statistic for the first-stage, which includes only those born between 1906 and 1940 (F-statistic of 10). These results, available upon request, are consistent with the findings we present below. Finally, we estimate the reduced form, which offers valid inference and is robust to weak instruments (Chernozhukov and Hansen 2008). In all cases, our findings are consistent with our primary specification based on 2SLS.

Results from the 2SLS analysis show statistically insignificant and, in several cases, wrong signed effects across our range of outcomes (Table 3, Figure 2). For example, our 2SLS estimates suggest that individuals who are exposed to longer periods of compulsory attendance are approximately 7.0 percentage points, or about 8 percent, less likely to survive to age 70 (Table 3, Figure 2). However, the corresponding reduced form estimates for the effects on survival to age 70 are statistically insignificant (Table 3).[9]

Our other health outcomes with statistically significant effects are whether an individual was ever diagnosed with diabetes and whether during the sample period an individual ever reported being overweight (BMI greater than or equal to 25) or obese (BMI greater than or equal to 30). Again, the coefficients are wrong signed, and in the case of BMI only marginally significant, indicating that individuals exposed to longer periods of compulsory attendance,

[8] Effective F-statistics computed according to Olea and Pflueger (2003).
[9] In results not shown, we find no effect of compulsory schooling laws on survival to age 80.

which increases their years of schooling, are 11.3 percentage points or 45 percent more likely to have diabetes, 7 percentage points or 9 percent more likely to be overweight, and 8.6 percentage points or 22 percent more likely to be obese (Table 3, Figure 2). One explanation for the diabetes result could be increased health care utilization, in other words, increases in compulsory schooling laws cause an individual to seek out screening for diabetes, leading to higher detection. However, we find no statistically or economically significant effects for our other utilization outcomes – whether the individual received a flu shot or visited a doctor regularly.[10] Another possible explanation for the BMI and diabetes results is a survivorship bias, where individuals who live longer are more likely to experience health issues. However, as we note earlier, we find no evidence that compulsory schooling laws increase life expectancy (rather, we observe the opposite). The rest of our outcomes are both statistically and for the most part economically insignificant, suggesting there is no effect of compulsory schooling laws on later life health.[11]

For completeness, we include estimates restricted to individuals born between 1906 and 1955, creating a sample consistent with Stephens and Yang (2014). This restriction has little effect on our analysis (Table A4). Further, we include estimates after controlling for school quality using Card and Krueger (1992) definitions and including region of residence at age 10 by year of birth fixed effects (Tables A5 and A6). In both cases, the few point estimates that were

[10] In results not shown, we also find that compulsory schooling laws had no effect on whether the individual wears hearing aids. Results available upon request.

[11] We also construct robust Anderson-Rubin confidence intervals, which ensure correct uniform asymptotic coverage in cases with potentially weak instruments, as recommended by Andrews et al. (2019). Consistent with our main results, all but two confidence sets include zero, and the exceptions, diabetes, and survival to age 70, are both wrong signed. However, almost all of them have either an infinite upper or lower bound, limiting the conclusions that can be drawn from the intervals. Davidson and MacKinnon (2014) highlight this undesirable property of AR confidence sets, noting that correct coverage need not imply an informative test.

statistically significant become statistically insignificant, and the results become less economically significant.[12]

Our results add to the growing literature that suggests compulsory schooling laws have no effect on later life health. Our results are consistent with the findings of Stephens and Yang (2014), who find no effect of compulsory schooling laws on earnings, unemployment, and other welfare outcomes upon inclusion of region-by-birth-year fixed effects. Stephens and Yang (2014) argue that the effects of compulsory schooling are driven by differences across regions rather than across states. Our results, however, show that even absent accounting for region-by-cohort fixed effects, compulsory schooling laws likely have no statistically or economically significant effect on health outcomes.

Moreover, our results are unlikely to be explained by limited variation after accounting for cohort and state of birth fixed effects. Black, Hsu, and Taylor (2015) examine the effects of compulsory schooling laws on mortality using Vital Statistics and decennial census data and show that there is little variation remaining in years of schooling after accounting for cohort and state of birth fixed effects. However, in our HRS sample, a significant amount of variation remains when regressing our outcome variables on years of schooling and the controls from our primary specification: year of birth, quarter of birth, state of birth, state of residence at age 10, race, Hispanic, and gender indicators (Table A7). The adjusted R^2 values range from 0.03 in the case of diabetes to 0.17 for mortality. Moreover, while mortality is a coarse outcome, we continue to find null effects across our more granular health outcomes. As a result, we view the lack of statistically or economically significant effects as evidence that compulsory schooling laws have no significant causal effect on health.

[12] First stage F-statistics fall short of the conventional 10. However, reduced form results similarly show no effect (Tables A5 and A6).

5. Conclusion

We provide new evidence on the effect of compulsory schooling laws on later life health outcomes. Using Health and Retirement Study data linked to restricted geographic identifiers, we improve upon past studies by more accurately matching individuals to compulsory schooling laws. Further, by focusing on granular health outcomes encompassing physical strength, decision-making, and pervasiveness of severe illnesses, we are able to provide a more holistic assessment of an individual's health. We find that while compulsory schooling laws significantly increase educational attainment, the effects fail to carry over to later life health. Our results provide evidence that education has no significant causal effect on a wide range of later-life health outcomes.

References

Acemoglu, D., & Angrist, J. (2000). How large are human-capital externalities? Evidence from compulsory schooling laws. *NBER Macroeconomic Annual* 15, 9-59.

Adams, S. J. (2002). Educational attainment and health: evidence from a sample of older adults. *Education Economics* 10(1), 97-109.

Administration for Community Living. (2017). Glossary. *U.S. Department of Health and Human Services.* Retrieved from https://longtermcare.acl.gov/the-basics/glossary.html

Albouy, V., & Lequien, L. (2009). Does compulsory education lower mortality? *Journal of Health Economics* 28(1), 155-168.

Andrews, Isaiah, James H. Stock, and Liyang Sun. Weak instruments in instrumental variables regression: Theory and practice." *Annual Review of Economics* 11 (2019): 727-753.

Angrist, J. D., & Krueger, A. B. (1991). Does compulsory school attendance affect schooling and earnings? *Quarterly Journal of Economics* 106(4), 979-1014.

Avendano, M., De Coulon, A., & Nafilyan, V. (2020). Does longer compulsory schooling affect mental health? Evidence from a British reform. *Journal of Public Economics*, *183*, 104137.

Black, D.A., Hsu, Y.C., & Taylor, L.J. (2015) The effect of early-life education on later-life mortality. *Journal of Health Economics*, 44, 1-9.

Black, S. E., Devereux, P. J., & Salvanes, K. G. (2011). Too young to leave the nest? The effects of school starting age. *The Review of Economics and Statistics*, *93*(2), 455-467.

Bound, J., & Jaeger, D. A. (2000). Do compulsory school attendance laws alone explain the association between quarter of birth and earnings? *Research in Labor Economics* 19(4), 83-108.

Braakmann, N. (2011). The causal relationship between education, health and health related behavior: evidence from a natural experiment in England. *Journal of Health Economics* 30(4), 753-763.

Brunello, G., Fort, M., Schneeweis, N., & Winter-Ebmer, R. (2016). The causal effect of education on health: What is the role of health behaviors?. *Health Economics*, *25*(3), 314-336.

Buckles, K. S., & Hungerman, D. M. (2013). Season of birth and later outcomes: old questions, new answers. *Review of Economics and Statistics*, 95(3), 711-724.

Card, D., & Krueger, A. B. (1992). Does school quality matter? Returns to education and the characteristics of public schools in the United States. *Journal of Political Economy* 100(1), 1-40.

Chernozhukov, V., & Hansen, C. (2008). The reduced form: a simple approach to inference with weak instruments. *Economic Letters* 100(1), 68-71.

Clark, D., & Royer, H. (2013). The effect of education on adult mortality and health: evidence from Britain. *American Economic Review* 103(6), 2087-2120

Crespo, L., López-Noval, B., & Mira, P. (2014). Compulsory schooling, education, depression and memory: New evidence from SHARELIFE. *Economics of Education Review*, *43*, 36-46.

Cygan-Rehm, K., & Maeder, M. (2013). The effect of education on fertility: Evidence from a compulsory schooling reform. *Labour Economics*, *25*, 35-48.

Davidson, R., & MacKinnon, J. G. (2014). Confidence sets based on inverting Anderson–Rubin tests. *The Econometrics Journal*, 17(2), S39-S58.

Dilmaghani, M. (2020). The cause effects of education on health over the life course: evidence from Canada. *Public Health* 186, 170-177.

Figinski, T.F., Lloro, A., & Li, P. (2019). New evidence on the effect of compulsory schooling laws. *Advances in Econometrics* 40A, 293-318.

Fletcher, J. M. (2015). New evidence of the effects of education on health in the US: Compulsory schooling laws revisited. *Social Science & Medicine*, *127*, 101-107.

Glymour, M. M., Kawachi, I., Jencks, C. S., & Berkman, L. (2008). Does childhood schooling affect old age memory or mental status? Using state schooling laws as natural experiments. *Journal of Epidemiology and Community Health* 62(6), 532-537.

Grabner, M. (2009). The causal effect of education on obesity: Evidence from compulsory schooling laws. *Available at SSRN 1505075*.

Grossman, M. (1972). On the concept of health capital and the demand for health. *Journal of Political Economy* 80(2), 223-255.

Güneş, P. M. (2015). The role of maternal education in child health: Evidence from a compulsory schooling law. *Economics of Education Review*, *47*, 1-16.

Hummer, Robert A., & Hernandez, E.M. (2013). The effect of educational attainment on adult mortality in the United States. *Population Reference Bureau Population Bulletin* 68(1).

Kemptner, D., Jürges, H., & Reinhold, S. (2011). Changes in compulsory schooling and the causal effect of education on health: Evidence from Germany. *Journal of Health Economics, 30*(2), 340-354.

Kenkel, D. (1991). Health behavior, health knowledge, and schooling. *Journal of Political Economy* 99(2), 287-305.

Lleras-Muney, A. (2005). The relationship between education and adult mortality in the United States. *Review of Economic Studies* 72(1), 189-221.

Mazumder, B. (2012). The effects of education on health and mortality. *Nordic Economic Policy Review, 1*(2012), 261-301.

McCrary, J., & Royer, H. (2011). The effect of female education on fertility and infant health: evidence from school entry policies using exact date of birth. *American Economic Review*, *101*(1), 158-95.

Meghir, C., Palme, M., & Simeonova, E. (2018). Education and mortality: Evidence from a social experiment. *American Economic Journal: Applied Economics, 10*(2), 234-56.

Messacar, D., and Oreopoulos, P. (2012). *Staying in School: A Proposal to Raise High School Graduation Rates* (Discussion Paper 2012-07). Washington, DC: The Hamilton Project, Brookings Institute. Retrieved from https://www.hamiltonproject.org/assets/files/a_proposal_to_raise_high_school_graduation_rates.pdf.

National Association of Insurance Commissioners. (2019). A shopper's guide to long-term care insurance." https://www.naic.org/documents/prod_serv_consumer_ltc_lp.pdf

Nelso, R.R., & Phelps, E.S. (1966). Investment in humans, technological diffusion, and economic growth. *American Economic Review* 56(1/2): 69-75.

Olea, J. L. M., & Pflueger, C. (2013). A robust test for weak instruments. *Journal of Business & Economic Statistics* 31(3), 358-369.

Oreopoulos, P. (2007). Do dropouts drop out too soon? Wealth, health and happiness from compulsory schooling. *Journal of Public Economics* 91, 2213-2229.

Oreopoulos, P. (2009). Would more compulsory schooling help disadvantaged youth? Evidence from recent changes to school-leaving laws. In J. Gruber (Ed.) *The problems of disadvantaged youth: An economic perspective*, 85-112. Chicago, IL: University of Chicago Press.

Qureshi, A. I., Suri, M. F. K., Saad, M., and Hopkins, L.N. (2003). Educational attainment and risk of stroke and myocardial Infarction. *Medical Science Monitor* 9(11): CR466-473.

Rosenzweig, M. R., & Schultz, T. P. (1983). Estimating a household production function: heterogeneity, the demand for health inputs, and their effects on birth weights. *Journal of Political Economy* 91(5): 723-746.

Ruggles, S., Flood, S., Goeken, R., Grover, J., Meyer, E., Pacas, J., & Sobek, M. (2020). IPUMS USA: Version 10.0 [dataset]. Minneapolis, MN: IPUMS, 2020. https://doi.org/10.18128/D010.V10.0.

Schmidt, E. (2018). U.S. population more educated than ever before." *U.S. Census Bureau.*

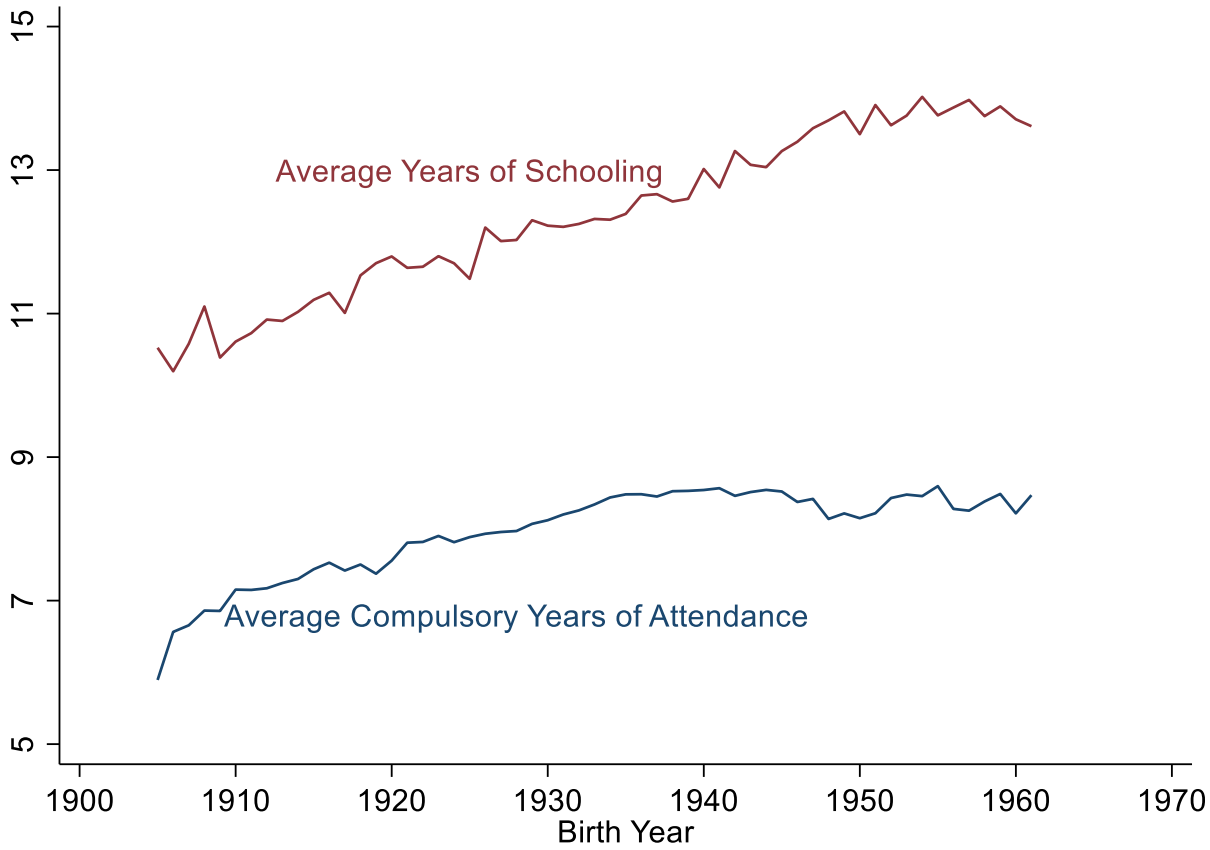
Stephens, Jr., M., & Yang, D. Y. (2014). Compulsory education and the benefits of schooling. *American Economic Review* 104(6), 1777-1792.

U.S. Census Bureau. (2019). Annual estimates of the resident population by sex, age, race, and Hispanic origin for the United States and States: April 1, 2010 to July 1, 2018.

van Kippersluis, H., O'Donnell, O., & Van Doorslaer, E. (2009). Long run returns to education: does schooling lead to an extended old age? *The Journal of Human Resources*, *4*, 1.

Zhao, Q., Wang, J., Spiller, W., Bowden, J., & Small, D. S. (2019). Two-sample instrumental variable analyses using heterogeneous samples. *Statistical Science* 34(2), 317-333.

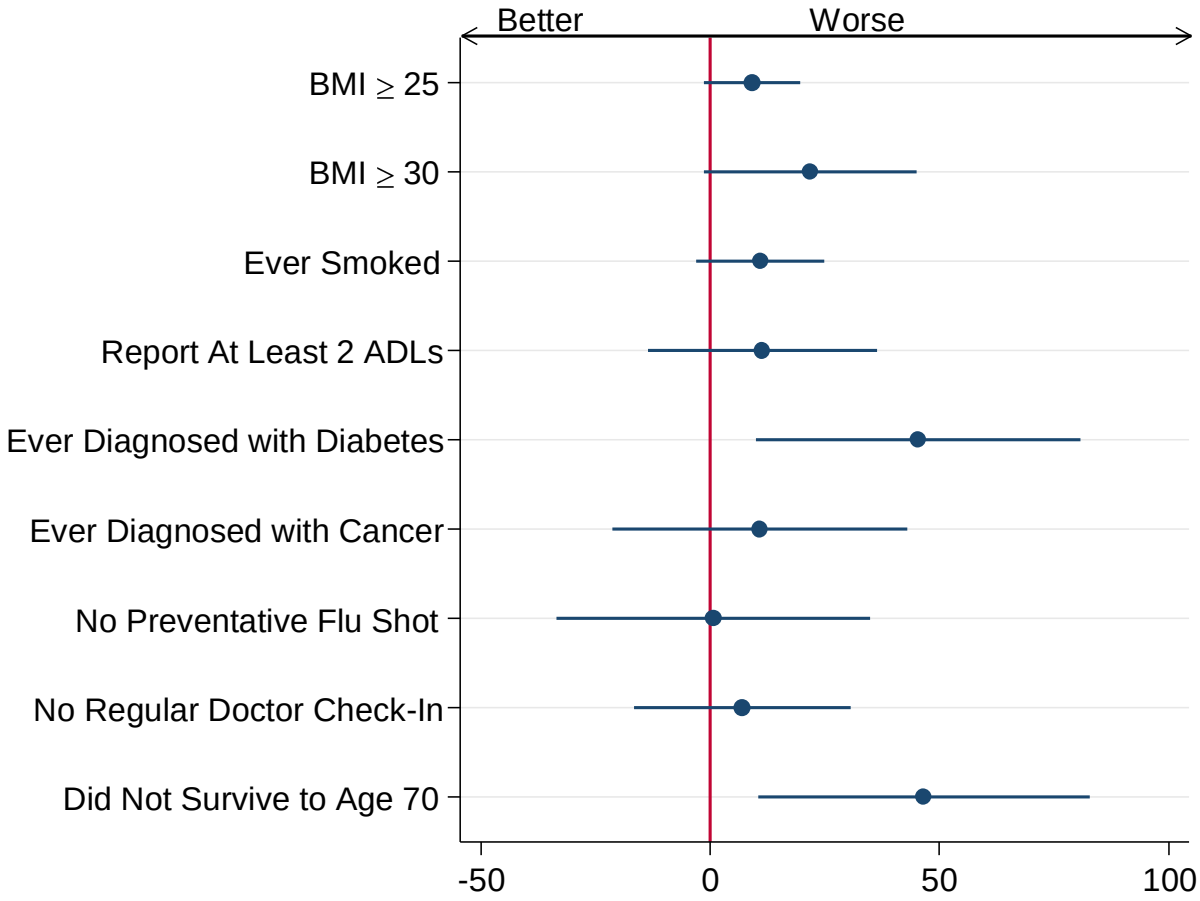
Figure 1: Average Years of Schooling and Average Compulsory Years of Attendance by Birth Year



Notes: Observations are weighted by the Health and Retirement Study (HRS) provided person weight from the first wave the individual entered the sample. Years of schooling is self-reported and is capped at 17 years. Years of required schooling are defined according to Stephens and Yang (2014).

Source: Authors' calculations using data from the RAND Health and Retirement Study matched with restricted geographic identifiers.

Figure 2: 2SLS Estimates of the Effect of Compulsory Schooling Laws on Health Outcomes as Percent Changes Relative to the Mean



Notes: For presentation purposes, we display the inverse of the effects for preventative flu shot, regular doctor check-in, and survive to age 70. Observations are weighted by the Health and Retirement Study (HRS) provided person weight from the first wave the individual entered the sample. The dependent variable is listed in each row. Circles represent percent changes relative to the mean (coefficient divided by mean), with the 95 percent confidence interval calculated using robust standard errors, clustered at the state of residence at age 10/year of birth cell. The 2SLS estimates are from a specification that includes the compulsory schooling attendance laws as defined according to Stephens and Yang (2014) on years of schooling in the first stage. The reference category in the first stage of the 2SLS is less than seven years of required schooling. Control variables include: quarter of birth, year of birth, state of birth, state of residence at age 10, race, Hispanic, and gender.

Source: Authors' calculations using data from the RAND Health and Retirement Study matched with restricted geographic identifiers.

Table 1: Summary Statistics, Overall and by 10-Year Birth Cohort, Health and Retirement Study

	All	Born						
		1900-1909	1910-1919	1920-1929	1930-1939	1940-1949	1950-1959	1960-1969
Years of Schooling	12.9	10.5	11.2	11.8	12.4	13.3	13.8	13.6
Male	0.501	0.320	0.400	0.431	0.476	0.551	0.525	0.564
White	0.833	0.910	0.907	0.888	0.876	0.838	0.806	0.713
Black	0.114	0.080	0.079	0.090	0.104	0.119	0.133	0.131
Hispanic	0.050	0.012	0.018	0.023	0.035	0.056	0.061	0.088
BMI $\geq$ 25	0.775	0.401	0.581	0.706	0.807	0.845	0.818	0.792
BMI $\geq$ 30	0.400	0.095	0.179	0.294	0.416	0.481	0.461	0.417
Report At Least 2 ADLs	0.219	0.555	0.485	0.383	0.255	0.176	0.117	0.072
Fair or Poor Health	0.142	0.247	0.230	0.160	0.087	0.091	0.111	0.270
Ever Smoked	0.580	0.384	0.533	0.599	0.649	0.626	0.566	0.505
Ever Diagnosed with Diabetes	0.225	0.113	0.178	0.244	0.265	0.269	0.207	0.177
Ever Diagnosed with Cancer	0.177	0.191	0.246	0.264	0.254	0.186	0.116	0.076
Ever Received a Flu Shot	0.728	0.727	0.827	0.859	0.804	0.748	0.666	0.561
Regular Doctor Check-In	0.767	0.841	0.822	0.809	0.648	0.731	0.769	0.882
Number of Individuals	32,666	991	3,486	5,297	7,120	6,151	6,500	3,121

Notes: Observations are weighted by the Health and Retirement Study (HRS) provided person weight from the first wave the individual entered the sample. Years of schooling is self-reported and is capped at 17 years.

Source: Authors' calculations using data from the RAND Health and Retirement Study matched with restricted geographic identifiers.

Table 2: The Effect of Compulsory Schooling Laws on Years of Schooling

	HRS		Decennial Census	
	(1)	(2)	(3)	(4)
7 Years of Required Schooling	0.230	0.191	0.173***	0.153***
	(0.151)	(0.158)	(0.030)	(0.030)
8 Years of Required Schooling	0.392***	0.391***	0.391***	0.379***
	(0.134)	(0.135)	(0.029)	(0.029)
9+ Years of Required Schooling	0.504***	0.517***	0.585***	0.573***
	(0.148)	(0.151)	(0.033)	(0.034)
Number of Observations	26,278	23,065	7,835,216	7,740,190
Effective F-statistic	4.46	4.76	127.20	125.30
Mean Years of Schooling	12.93	12.71	11.35	11.38
Sample: Born 1906 to 1955	No	Yes	No	Yes

Notes: Compulsory schooling attendance laws are defined according to Stephens and Yang (2014). The reference category is less than seven years of required schooling. The Health and Retirement Study (HRS) specification matches to compulsory schooling laws in an individual's state of residence at age 14; the census specifications use state of birth. Robust standard errors are reported in parentheses. The HRS standard errors are clustered at the state of residence at age 10/year of birth cell; the census standard errors are clustered at the state of birth/year of birth cell. Observations are weighted by the HRS provided person weight from the first wave the individual entered the sample, and the IPUMS decennial census provided person weights respectively. We follow Stephens and Yang (2014) in converting IPUMS decennial census highest grade of schooling reported by the individual to years of schooling. The decennial census data are also limited to individuals who are age 25 or older. The outcome variable is years of schooling, which is capped at 17 years. In limiting the sample to those born between 1906 and 1955, we create a sample that is consistent with Stephens and Yang (2014). Control variables include: quarter of birth, year of birth, state of birth, race, Hispanic, and gender. HRS specifications additionally control for state of residence at age 14. Effective F-statistics computed according to Olea and Pflueger (2003).

Source: Authors' calculations using data from the RAND Health and Retirement Study matched with restricted geographic identifiers, and the following decennial census data acquired from IPUMS – 1960 1% sample, 1970 1% form 1 and form 2 state samples, 1980 5% state sample, 1990 5% state sample, and the 2000 5% sample (Ruggles et. al., 2020).

Table 3: The Effect of Compulsory Schooling Laws on Health Outcomes

	2SLS	Reduced Form		
	Years of Schooling	7 Years of Required Schooling	8 Years of Required Schooling	9+ Years of Required Schooling
	(1)	(2)	(3)	(4)
Survived to age 70	-0.070**	-0.103	-0.047	-0.032
(mean = 0.850; N = 17,976)	(0.028)	(0.080)	(0.072)	(0.069)
Report At Least 2 ADLs	0.032	-0.012	0.011	0.014
(mean = 0.277; N = 26,264)	(0.035)	(0.014)	(0.013)	(0.014)
Fair or Poor Health	-0.030	-0.012	0.001	0.002
(mean = 0.145; N = 26,275)	(0.027)	(0.011)	(0.011)	(0.013)
BMI ≥ 25	0.070*	0.011	0.025*	0.029*
(mean = 0.768; N = 26,222)	(0.041)	(0.016)	(0.014)	(0.017)
BMI ≥ 30	0.086*	-0.009	0.026	0.031
(mean = 0.393; N = 26,222)	(0.047)	(0.021)	(0.018)	(0.021)
Ever Smoked	0.065	-0.008	0.014	0.027
(mean = 0.598; N = 26,163)	(0.043)	(0.020)	(0.017)	(0.020)
Ever Diagnosed with Diabetes	0.113**	0.036*	0.055***	0.055***
(mean = 0.249; N = 26,276)	(0.045)	(0.018)	(0.016)	(0.017)
Ever Diagnosed with Cancer	0.022	0.011	0.003	0.016
(mean = 0.204; N = 26,277)	(0.034)	(0.017)	(0.014)	(0.016)
Ever Received a Flu Shot	-0.002	0.046**	0.028*	0.008
(mean = 0.760; N = 25,526)	(0.042)	(0.019)	(0.017)	(0.020)
Regular Doctor Check-In	-0.018	-0.024	-0.011	-0.016
(mean = 0.739; N = 26,204)	(0.031)	(0.018)	(0.014)	(0.016)

Notes: The dependent variable is listed in each row. The 2SLS estimates are from a specification that includes the compulsory schooling attendance laws as defined according to Stephens and Yang (2014) on years of schooling in the first stage. The reference category in both the reduced form and the first stage of the 2SLS is less than seven years of required schooling. Robust standard errors, clustered at the state of residence at age 10/year of birth cell, are reported in parentheses. Control variables include: quarter of birth, year of birth, state of birth, state of residence at age 10, race, Hispanic, and gender. The mean and sample sizes are reported for the first column.

Source: Authors' calculations using data from the RAND Health and Retirement Study matched with restricted geographic identifiers.

Table A1: The Effect of Acemoglu and Angrist (2000) Compulsory Schooling Laws on Health Outcomes

	2SLS	Reduced Form		
	Years of Schooling	9 Years of Compulsory Attendance	10 Years of Compulsory Attendance	11+ Years of Compulsory Attendance
	(1)	(2)	(3)	(4)
Survived to age 70	-0.049**	-0.021	0.040	0.005
(mean = 0.852; N = 18,213)	(0.022)	(0.027)	(0.033)	(0.026)
Report At Least 2 ADLs	-0.014	0.005	0.004	-0.001
(mean = 0.269; N = 27,998)	(0.030)	(0.009)	(0.014)	(0.012)
Fair or Poor Health	-0.017	0.006	0.018	-0.011
(mean = 0.155; N = 28,019)	(0.026)	(0.009)	(0.014)	(0.001)
BMI ≥ 25	0.005	0.006	-0.015	-0.002
(mean = 0.767; N = 27,944)	(0.033)	(0.011)	(0.016)	(0.013)
BMI ≥ 30	0.023	0.005	-0.002	0.014
(mean = 0.394; N = 27,944)	(0.034)	(0.013)	(0.018)	(0.016)
Ever Smoked	0.092**	0.025*	0.058***	0.031*
(mean = 0.592; N = 27,903)	(0.043)	(0.013)	(0.019)	(0.016)
Ever Diagnosed with Diabetes	0.050	0.021*	-0.004	0.013
(mean = 0.245; N = 28,018)	(0.031)	(0.011)	(0.014)	(0.013)
Ever Diagnosed with Cancer	0.041	0.012	0.007	0.020
(mean = 0.196; N = 28,020)	(0.030)	(0.010)	(0.015)	(0.013)
Ever Received a Flu Shot	0.022	0.003	-0.011	0.011
(mean = 0.747; N = 27,210)	(0.037)	(0.013)	(0.017)	(0.015)
Regular Doctor Check-In	0.004	0.010	-0.004	-0.002
(mean = 0.747; N = 25,298)	(0.028)	(0.011)	(0.015)	(0.013)

Notes: The dependent variable is listed in each row. The 2SLS estimates are from a specification that includes the compulsory schooling attendance laws as defined according to Acemoglu and Angrist (2000) on years of schooling in the first stage. The reference category in both the reduced form and the first stage of the 2SLS is eight years or less of required schooling. Robust standard errors, clustered at the state of residence at age 10/year of birth cell, are reported in parentheses. Control variables include: quarter of birth, year of birth, state of birth, state of residence at age 10, race, Hispanic, and gender. The mean and sample sizes are reported for the first column.

Source: Authors' calculations using data from the RAND Health and Retirement Study matched with restricted geographic identifiers.

Table A2: The Effect of Oreopoulos (2009) Compulsory Schooling Laws on Health Outcomes

	2SLS		Reduced Form	
	Years of Schooling	Years of Schooling	Minimum Dropout Age > 16	Minimum Dropout Age > 16
	(1)	(2)	(3)	(4)
Survived to age 70	0.059	0.063	0.076***	0.076***
(mean = 0.852; N = 18,213)	(0.073)	(0.075)	(0.024)	(0.024)
Report At Least 2 ADLs	-0.040	0.085	-0.002	0.022
(mean = 0.269; N = 27,998)	(0.053)	(0.111)	(0.013)	(0.017)
Fair or Poor Health	0.109	0.137	0.023*	0.028*
(mean = 0.155; N = 28,019)	(0.090)	(0.126)	(0.014)	(0.017)
BMI ≥ 25	0.061	0.063	0.013	0.009
(mean = 0.767; N = 27,944)	(0.068)	(0.087)	(0.014)	(0.014)
BMI ≥ 30	0.226*	0.204	0.054***	0.043**
(mean = 0.394; N = 27,944)	(0.125)	(0.163)	(0.018)	(0.021)
Ever Smoked	0.142	0.050	0.035*	0.011
(mean = 0.592; N = 27,903)	(0.106)	(0.097)	(0.019)	(0.018)
Ever Diagnosed with Diabetes	0.027	-0.022	0.011	-0.001
(mean = 0.245; N = 28,018)	(0.062)	(0.081)	(0.014)	(0.016)
Ever Diagnosed with Cancer	0.075	0.053	0.019	0.009
(mean = 0.196; N = 28,020)	(0.064)	(0.084)	(0.017)	(0.017)
Ever Received a Flu Shot	0.102	0.103	0.020	0.015
(mean = 0.747; N = 27,210)	(0.086)	(0.136)	(0.015)	(0.016)
Regular Doctor Check-In	-0.076	-0.071	-0.019	-0.012
(mean = 0.747; N = 27,916)	(0.075)	(0.100)	(0.014)	(0.014)
Sample: Born 1906 to 1955	No	Yes	No	Yes

Notes: The dependent variable is listed in each row. The 2SLS estimates are from a specification that includes an indicator for if the minimum dropout age was > 16 as defined according to Oreopoulos (2009) on years of schooling in the first stage. Robust standard errors, clustered at the state of residence at age 10/year of birth cell, are reported in parentheses. Control variables include: quarter of birth, year of birth, state of birth, state of residence at age 10, race, Hispanic, and gender. The mean and sample sizes are reported for the first column.

Source: Authors' calculations using data from the RAND Health and Retirement Study matched with restricted geographic identifiers.

Table A3: The Effect of Compulsory Schooling Laws in State of Birth on Health Outcomes

	2SLS	Reduced Form		
	Years of Schooling	7 Years of Required Schooling	8 Years of Required Schooling	9+ Years of Required Schooling
	(1)	(2)	(3)	(4)
Survived to age 70	-0.085***	-0.050	-0.069	-0.046
(mean = 0.819; N = 19,313)	(0.027)	(0.072)	(0.065)	(0.062)
Report At Least 2 ADLs	0.022	-0.006	0.008	0.016
(mean = 0.274; N = 27,485)	(0.030)	(0.014)	(0.013)	(0.015)
Fair or Poor Health	-0.018	-0.006	-0.001	0.009
(mean = 0.151; N = 27,494)	(0.025)	(0.012)	(0.011)	(0.012)
BMI ≥ 25	0.024	0.018	0.016	0.026*
(mean = 0.763; N = 27,439)	(0.031)	(0.015)	(0.012)	(0.015)
BMI ≥ 30	0.057	-0.012	0.012	0.014
(mean = 0.389; N = 27,439)	(0.036)	(0.021)	(0.017)	(0.020)
Ever Smoked	0.041	0.010	0.012	0.031
(mean = 0.602; N = 27,380)	(0.034)	(0.021)	(0.017)	(0.019)
Ever Diagnosed with Diabetes	0.075**	0.019	0.036**	0.041**
(mean = 0.247; N = 27,495)	(0.034)	(0.018)	(0.015)	(0.016)
Ever Diagnosed with Cancer	0.011	0.011	0.003	0.018
(mean = 0.201; N = 27,496)	(0.027)	(0.016)	(0.013)	(0.015)
Ever Received a Flu Shot	-0.054	0.034*	0.004	-0.008
(mean = 0.761; N = 25,783)	(0.038)	(0.019)	(0.017)	(0.019)
Regular Doctor Check-In	0.052*	-0.029*	0.005	0.008
(mean = 0.741; N = 27,418)	(0.027)	(0.017)	(0.014)	(0.016)

Notes: The dependent variable is listed in each row. The 2SLS estimates are from a specification that includes the compulsory schooling attendance laws as defined according to Stephens and Yang (2014) on years of schooling in the first stage. The reference category in both the reduced form and the first stage of the 2SLS is less than seven years of required schooling. Robust standard errors, clustered at the state of birth/year of birth cell, are reported in parentheses. Control variables include: quarter of birth, year of birth, state of birth, race, Hispanic, and gender. The mean and sample sizes are reported for the first column.

Source: Authors' calculations using data from the RAND Health and Retirement Study matched with restricted geographic identifiers.

Table A4: The Effect of Compulsory Schooling Laws on Health Outcomes for 1906-1955 Birth Cohorts

	2SLS	Reduced Form		
	Years of Schooling	7 Years of Required Schooling	8 Years of Required Schooling	9+ Years of Required Schooling
	(1)	(2)	(3)	(4)
Survived to age 70	-0.072**	-0.103	-0.047	-0.032
(mean = 0.849; N = 17,886)	(0.029)	(0.080)	(0.072)	(0.069)
Report At Least 2 ADLs	0.026	-0.015	0.008	0.011
(mean = 0.297; N = 23,056)	(0.035)	(0.016)	(0.014)	(0.016)
Fair or Poor Health	-0.057*	-0.008	-0.006	-0.005
(mean = 0.138; N = 23,062)	(0.029)	(0.011)	(0.011)	(0.013)
BMI ≥ 25	0.044	0.008	0.021	0.018
(mean = 0.761; N = 23,017)	(0.038)	(0.017)	(0.015)	(0.017)
BMI ≥ 30	0.064	-0.013	0.018	0.021
(mean = 0.382; N = 23,017)	(0.043)	(0.022)	(0.019)	(0.022)
Ever Smoked	0.055	0.001	0.006	0.029
(mean = 0.601; N = 22,952)	(0.042)	(0.021)	(0.018)	(0.021)
Ever Diagnosed with Diabetes	0.082**	0.040**	0.044***	0.048***
(mean = 0.254; N = 23,064)	(0.039)	(0.019)	(0.016)	(0.018)
Ever Diagnosed with Cancer	0.024	0.010	0.001	0.015
(mean = 0.219; N = 23,065)	(0.033)	(0.019)	(0.016)	(0.018)
Ever Received a Flu Shot	-0.002	0.058***	0.027	0.022
(mean = 0.780; N = 22,333)	(0.042)	(0.020)	(0.017)	(0.020)
Regular Doctor Check-In	-0.011	-0.024	-0.009	-0.015
(mean = 0.733; N = 23,011)	(0.032)	(0.019)	(0.015)	(0.018)

Notes: The dependent variable is listed in each row. The sample is limited to those born between 1906 and 1955 to be consistent with Stephens and Yang (2014). The 2SLS estimates are from a specification that includes the compulsory schooling attendance laws as defined according to Stephens and Yang (2014) on years of schooling in the first stage. The reference category in both the reduced form and the first stage of the 2SLS is less than seven years of required schooling. Robust standard errors, clustered at the state of residence at age 10/year of birth cell, are reported in parentheses. Control variables include: quarter of birth, year of birth, state of birth, state of residence at age 10, race, Hispanic, and gender. The mean and sample sizes are reported for the first column.

Source: Authors' calculations using data from the RAND Health and Retirement Study matched with restricted geographic identifiers.

Table A5: The Effect of Compulsory Schooling Laws on Health Outcomes including School Quality Controls

	2SLS	Reduced Form		
	Years of Schooling	7 Years of Required Schooling	8 Years of Required Schooling	9+ Years of Required Schooling
	(1)	(2)	(3)	(4)
Survived to age 70 (mean = 0.850; N = 17,976)	-0.104 (0.091)	-0.114 (0.081)	-0.059 (0.074)	-0.034 (0.069)
Report At Least 2 ADLs (mean = 0.283; N = 25,351)	-0.196 (0.628)	-0.020 (0.015)	-0.002 (0.014)	0.005 (0.016)
Fair or Poor Health (mean = 0.138; N = 25,360)	-0.084 (0.313)	-0.013 (0.011)	-0.005 (0.012)	-0.006 (0.014)
BMI ≥ 25 (mean = 0.767; N = 25,312)	-0.583 (1.615)	0.011 (0.016)	0.026* (0.015)	0.024 (0.018)
BMI ≥ 30 (mean = 0.392; N = 25,312)	-0.618 (1.733)	-0.012 (0.022)	0.020 (0.019)	0.022 (0.022)
Ever Smoked (mean = 0.600; N = 25,248)	0.007 (0.044)	-0.012 (0.021)	-0.001 (0.019)	0.007 (0.022)
Ever Diagnosed with Diabetes (mean = 0.250; N = 25,362)	-0.262 (0.711)	0.026 (0.018)	0.038** (0.017)	0.039** (0.019)
Ever Diagnosed with Cancer (mean = 0.208; N = 25,363)	0.401 (1.075)	0.006 (0.018)	-0.008 (0.016)	-0.001 (0.018)
Ever Received a Flu Shot (mean = 0.767; N = 24,612)	0.697 (1.760)	0.040** (0.020)	0.011 (0.018)	-0.004 (0.021)
Regular Doctor Check-In (mean = 0.735; N = 25,298)	-0.496 (1.544)	-0.026 (0.018)	-0.006 (0.015)	-0.017 (0.018)

Notes: The dependent variable is listed in each row. The 2SLS estimates are from a specification that includes the compulsory schooling attendance laws as defined according to Stephens and Yang (2014) on years of schooling in the first stage. The reference category in both the reduced form and the first stage of the 2SLS is less than seven years of required schooling. Robust standard errors, clustered at the state of residence at age 10/year of birth cell, are reported in parentheses. Control variables include: quarter of birth, year of birth, state of birth, state of residence at age 10, race, Hispanic, gender, and school quality as defined by Card and Krueger (1992). The mean and sample sizes are reported for the first column.

Source: Authors' calculations using data from the RAND Health and Retirement Study matched with restricted geographic identifiers.

Table A6: The Effect of Compulsory Schooling Laws on Health Outcomes including Region of Residence at Age 10 by Year of Birth Fixed Effects

	2SLS	Reduced Form		
	Years of Schooling	7 Years of Required Schooling	8 Years of Required Schooling	9+ Years of Required Schooling
	(1)	(2)	(3)	(4)
Survived to age 70	-0.10	-0.118	-0.051	-0.041
(mean = 0.850; N = 17,976)	(0.081)	(0.083)	(0.076)	(0.071)
Report At Least 2 ADLs	0.002	-0.019	-0.005	-0.006
(mean = 0.277; N = 26,264)	(0.109)	(0.014)	(0.014)	(0.016)
Fair or Poor Health	-0.002	-0.011	-0.005	-0.010
(mean = 0.145; N = 26,275)	(0.093)	(0.012)	(0.011)	(0.014)
BMI ≥ 25	0.197	0.008	0.032**	0.031*
(mean = 0.768; N = 26,222)	(0.197)	(0.015)	(0.014)	(0.018)
BMI ≥ 30	0.134	0.006	0.046**	0.050**
(mean = 0.393; N = 26,222)	(0.165)	(0.022)	(0.019)	(0.022)
Ever Smoked	0.230	-0.018	0.025	0.013
(mean = 0.598; N = 26,163)	(0.228)	(0.021)	(0.019)	(0.022)
Ever Diagnosed with Diabetes	0.214	0.038**	0.048***	0.042**
(mean = 0.249; N = 26,276)	(0.206)	(0.019)	(0.017)	(0.019)
Ever Diagnosed with Cancer	-0.094	0.010	-0.008	-0.001
(mean = 0.24; N = 26,277)	(0.134)	(0.017)	(0.016)	(0.018)
Ever Received a Flu Shot	0.268	0.029	0.026	-0.004
(mean = 0.760; N = 25,526)	(0.214)	(0.019)	(0.019)	(0.022)
Regular Doctor Check-In	-0.022	-0.025	-0.009	-0.009
(mean = 0.739; N = 26,204)	(0.106)	(0.018)	(0.015)	(0.018)

Notes: The dependent variable is listed in each row. The 2SLS estimates are from a specification that includes the compulsory schooling attendance laws as defined according to Stephens and Yang (2014) on years of schooling inthe first stage. The reference category in both the reduced form and the first stage of the 2SLS is less than seven years of required schooling. Robust standard errors, clustered at the state of residence at age 10/year of birth cell, are reported in parentheses. Control variables include: quarter of birth, year of birth, state of birth, state of residence at age 10, race, Hispanic, gender, and region-by-year of birth fixed effects. The mean and sample sizes are reported for the first column.

Source: Authors' calculations using data from the RAND Health and Retirement Study matched with restricted geographic identifiers.

Table A7: Coefficients of Determination between Years of Schooling and Health Outcomes

	(1)	(2)
Survived to age 70 (mean = 0.846; N = 18,213)	0.172	0.170
Report At Least 2 ADLs (mean = 0.219; N = 27,998)	0.143	0.117
Fair or Poor Health (mean = 0.142; N = 28,019)	0.092	0.073
BMI $\geq$ 25 (mean = 0.775; N = 27,944)	0.074	0.076
BMI $\geq$ 30 (mean = 0.401; N = 27,944)	0.066	0.074
Ever Smoked (mean = 0.580; N = 27,903)	0.062	0.071
Ever Diagnosed with Diabetes (mean = 0.225; N = 28,018)	0.040	0.034
Ever Diagnosed with Cancer (mean = 0.177; N = 28,020)	0.035	0.022
Ever Received a Flu Shot (mean = 0.729; N = 27,210)	0.059	0.034
Regular Doctor Check-In (mean = 0.767; N = 27,916)	0.061	0.054
Sample: Born 1906 to 1955	No	Yes

Notes: The dependent variable is listed in each row. Adjusted R^2 values are obtained from regressing the dependent variable on years of schooling. Control variables include: quarter of birth, year of birth, state of birth, state of residence at age 10, race, Hispanic, and gender. The mean and sample sizes are reported for the first column.

Source: Authors' calculations using data from the RAND Health and Retirement Study matched with restricted geographic identifiers.