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Does childhood schooling affect old age memory or mental status? Using state schooling laws as natural experiments

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Abstract

Background—The association between schooling and old age cognitive outcomes such as memory disorders is well documented but, because of the threat of reverse causation, controversy persists over whether education affects old age cognition. Changes in state compulsory schooling laws (CSL) are treated as natural experiments (instruments) for estimating the effect of education on memory and mental status among the elderly. Changes in CSL predict changes in average years of schooling completed by children who are affected by the new laws. These educational differences are presumably independent of innate individual characteristics such as IQ.

Methods—CSL-induced changes in education were used to obtain instrumental variable (IV) estimates of education's effect on memory (n = 10 694) and mental status (n = 9751) for white, non-Hispanic US-born Health and Retirement Survey participants born between 1900 and 1947 who did not attend college.

Results—After adjustment for sex, birth year, state of birth and state characteristics, IV estimates of education's effect on memory were large and statistically significant. IV estimates for mental status had very wide confidence intervals, so it was not possible to draw meaningful conclusions about the effect of education on this outcome.

Conclusions—Increases in mandatory schooling lead to improvements in performance on memory tests many decades after school completion. These analyses condition on individual states, so differences in memory outcomes associated with CSL changes cannot be attributed to differences between states. Although unmeasured state characteristics that changed contemporaneously with CSL might account for these results, unobserved genetic variation is unlikely to do so.

Memory and other cognitive domains strongly influence morbidity and mortality in the elderly, but few exposures have been conclusively demonstrated to improve cognitive outcomes.[1–3] Although educational attainment is associated with cognition, memory impairment and dementia in the elderly, it has been difficult to demonstrate causality.[4 5] Childhood IQ and other personal characteristics are potential confounders and the threat of confounding is nearly unavoidable in conventional observational data analyses.[6 7] The hypothesis that education

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affects memory function in old age is extremely appealing, however, because there are numerous plausible mediating pathways.[8] Schooling may confer direct neurological benefits, enhance occupational opportunities, material conditions, social network resources, health knowledge, behavioural norms and medical access, reducing the risk of disease or ameliorating the neurological consequences of disease.[9]

This paper exploits natural experiments to circumvent confounding by unmeasured common causes of schooling and cognitive outcomes. We use changes in state compulsory schooling laws (CSL) and school to work laws (CSL-w) to identify changes in educational attainment that are not attributable to innate characteristics (henceforth, we refer to CSL and CSL-w collectively as “CSL”). We use these changes in education to estimate the effect of additional schooling on old age memory and mental status. US state laws set the minimum years children must attend school. During the early 20th century, requirements varied between states and states changed their CSL repeatedly.[10] States extended mandatory schooling by lowering enrollment ages or raising ages at which children could drop out or obtain work permits. We treat CSL changes as “natural policy experiments” for extra education.[11] Changes in education resulting from CSL changes can, in principal, be used as instrumental variables (IV) to estimate the causal effect of education on cognitive outcomes, even if attained education is strongly influenced by innate characteristics such as IQ.[12,13] The strength of IV analyses is that violations of the assumptions for conventional observational studies (eg, unmeasured common causes of education and memory) do not compromise the analysis (hypothesised causal model as in fig 1).[14–16] A difficulty in IV analyses is identifying instruments that sufficiently influence the exposure (education) to provide informative confidence bounds. We therefore focus our analyses on the population in which CSL probably had the largest effects on educational attainment: US-born, non-Hispanic white individuals who did not attend college.

A crucial assumption is that, conditional on other covariates, state CSL changes were independent of other factors that influence old age cognitive outcomes (eg, innate intelligence of children born in that state or their parents’ education). Under this assumption, the only credible explanation for significant cognitive differences between children born in a given state before and after CSL changes would be the effect of education. Appendix 1 (available online only) reviews assumptions for IV analyses.

MATERIALS AND METHODS

The IV analyses estimate the effect of education on memory (or mental status) by combining information on the association between CSL and education and that between CSL and memory (fig 1). Our analyses use data from three sources: the Health and Retirement Study (HRS; for individual education and outcome assessments); historical federal reports (for CSL) and the 1980 census 5% sample (to estimate the effects of changing CSL on education).

Individual-level outcome data

Individual-level data on education, mental status and covariates are from HRS, an ongoing national probability sample of US residents born before 1948 and their spouses. We have up to six assessments, depending on the enrollment cohort. We excluded non-white and Hispanic participants because school segregation and tepid enforcement probably rendered CSL irrelevant to the educational experiences of minority groups during this period.[11] Non-US-born participants were also excluded because over 80% of these individuals immigrated after school age. CSL constrain drop out from primary and secondary school and are unlikely to have affected the educational attainment of college attendees. We therefore restricted primary analyses to individuals reporting high school education or less (results including the college educated are very similar). Detailed documentation on HRS sample design and validation of cognitive measures is available elsewhere.[17–19]

HRS participant data were linked to state characteristics using self-reported birth state, which largely coincides with states where children attended school but entails some misclassification.[20] The detailed residential history information necessary to classify more precisely was unavailable. Barring selective migration, IV estimates are consistent despite this misclassification, because the attenuation affects both stages of the IV calculations.

From 27 109 sample members enrolled in HRS, we made the following exclusions: non-US born (n = 2545); unknown birth state (n = 752); not born between 1901 and 1947 (n = 1048); born in states without applicable CSL (n = 14); non-white (n = 3522); Hispanic (n = 837) and years of schooling unknown (n = 29) or greater than 12 (n = 6935). This left 11 427 potentially eligible respondents (or 18 362 for supplementary analyses including all education levels). Additional sample members were excluded from specific analyses when outcome measures were missing.

HRS participants took repeated word list recall (memory) and mental status (mental status) tests. Immediate and delayed word list recall (n = 10 694) scores were summed and standardised by subtracting the mean and dividing by the standard deviation and were averaged over all waves in which the participant took the assessment (average of 3.9 waves). Memory tests in 1992 and 1994 used lists of 20 common nouns; 10-word lists were used in all other waves. To reduce outliers' influence and account for cross-wave range differences, scores above 1.96 were recoded to 1.96 before averaging; a similar floor was imposed at −1.96. This recoding affected only a small fraction of individuals.

Mental status (n = 9751) was assessed using a modified telephone interview for cognitive status (TICS), which included serial-7 subtractions.[21] We omitted items for "naming scissors" (because of low correlation with other items) and the second attempt at counting backwards from 20 (because of apparent inconsistencies in administration). TICS scores ranged from 0 to 13; in each wave many (21–30%) respondents scored 13. As with memory, mental status scores were standardised, with a floor imposed at −1.96 (no scores exceeded 1.96) and averaged over all years in which participants took assessments (average of 2.3 waves). Mental status was not assessed in 1992 or 1994 and most individuals younger than 65 years took the TICS only once. These outcomes predicted memory diseases, institutionalisation and mortality in HRS (results available from the authors).

Compulsory schooling laws

State schooling law data covering 1915–1939, compiled by Lleras-Muney[11] and Angrist and Acemoglu[22] are available online.[23] We extended this data series to 1907–1961 using federal education reports, typically published biennially.[24] A few states recorded different laws across state regions and we used the lowest documented value. For years without federal reports available, we carried forward the previous year's data. For each state and year of birth, years of compulsory schooling were calculated by subtracting the mandatory enrollment age from the minimum drop out age (CSL) or minimum work permit age (CSL-w). For example, states that required enrollment at age six years and permitted drop out at 16 years had CSL of 10. Some states regulated minimum years of schooling before receiving work permits. When laws were inconsistent, we used the shorter requirements. For each birth year/birth state cohort, we calculated applicable laws using minimum enrollment ages that applied in states six years after the birth year and drop out or work ages applicable 14 years after the birth year. If a state first enacted CSL while the child was aged six to 14 years, we applied this CSL. Birth cohorts for years with no CSL were excluded. In all analyses, three variables were used as instruments for predicting education: two continuous variables measuring CSL and CSL-w and an indicator for CSL-w not restricted. Results coding the instruments as a set of dichotomous variables for each CSL level did not differ substantively.

Statistical analyses

We used a separate-sample instrumental variable (SSIV) estimator,[14 25] summarised in equation 1 and equation 2.

$$\text{Predicted}_{\text{education}} = \beta_0 + \boldsymbol{\beta_j}\,\mathbf{instrument_j} + \boldsymbol{\beta_k}\mathbf{X_k} \tag{1}$$

$$\text{Memory} = \gamma_0 + \gamma_1{}^{*}\text{predicted}_{\text{education}} + \boldsymbol{\gamma_k}\mathbf{X_k} + \gamma \tag{2}$$

where $\mathbf{X_k}$ is a vector of control variables, all of which are included in both equations.

SSIV is similar to the more common two-stage least squares method, except that each stage is calculated using separate data, allowing a more precise estimation of equation 1. The first stage used CSL for each state and year of birth to predict education in the 1980 US census 5% sample.[26] We restricted the census and HRS samples to match on race, ethnicity, nativity, birth years and education (n = 2 721 255). The 1980 census education questions were comparable to the HRS education questions. The second stage used the regression predictions of years of education from the first stage as independent variables predicting memory and mental status scores of HRS respondents. The education predictions were linked to HRS data by sex, birth state and birth year. The regression coefficient for the predicted value of education (γ_1 in equation 2) is the IV estimate for the effect of one year of education on the outcome.[12 27]

We present models with increasingly comprehensive covariate adjustment to rule out common causes of CSL and cognitive outcomes. Model 1 includes no covariates. Model 2 includes sex plus dichotomous indicators for birth years 1901–1947 (1934 omitted; results differed little using continuous birth year). Model 3 adds 48 birth state indicators (New York omitted). Model 4 adds birth state characteristics when respondents were age six years (percentage black, urban and foreign born, from the Statistical Abstracts of the United States) or 14 years (manufacturing jobs per capita and manufacturing wages per manufacturing job, from federal manufacturing employment data reported every two to eight years).[28] These state variables were chosen because of their likely relationship with CSL adoption and the expansion of public education.[10 29] State characteristics for years between federal reports were linearly interpolated.

The first and second stage regressions for each model use identical independent variables except for the CSL instruments, which are included only in the first stage. Although uncertainty in estimates from either stage contributes to standard errors, the census sample is approximately 200 times larger than HRS, so first stage imprecision is negligible compared with second stage imprecision. We therefore present 95% confidence interval (CI) estimates based on robust (Huber–White) variance estimates adjusted for birth state clustering but ignoring imprecision in the first stage. We also compare effect estimates for education from IV to ordinary least squares (OLS) estimates. The complex sample design weights of HRS are not applicable when combining interviews across waves. In supplemental analyses, we apply synthetic weights based on the US census and explore the sensitivity of results to missing data (see Appendix 2, available online only). Primary analyses were conducted using Stata version 8.0 (Stata Corp, College Station, Texas, USA).

RESULTS

Compared with HRS participants with complete data, those missing outcome scores were more likely to be men with low education and low or missing parental education (table 1). Applicable CSL ranged from four to 12 years and CSL-w ranged from four to nine years. There were 42

CSL combinations, with an average of nearly four different compulsory schooling regimes per state (although some states never changed laws, whereas others had seven different regimes).

In the 5% US census sample, state CSL predicted years of education completed after full covariate adjustment (table 2). Each additional year of compulsory schooling was associated with 0.037 years extra schooling completed for participants born in the state (95% CI 0.034 to 0.040). Each year increase in CSL-w was associated with 0.044 years additional schooling (95% CI 0.040 to 0.48). The small magnitude of these coefficients indicates that only a fraction of students remain in school specifically because of state mandatory minimums.[11]

Table 3 compares IV estimates of the effect of education on memory and mental status, instrumented with CSL, to OLS regression estimates. The IV estimate of the effect of education on memory remained statistically significant after adjustment for sex, birth year and state of birth indicators (model 3) and birth state characteristics (model 4). The model 3 IV estimate suggests that, for individuals constrained by CSL, each additional year of schooling improved memory scores by 0.18 standard deviations. The IV parameter estimate is larger than the OLS parameter estimate.

In contrast, after birth state adjustment, the IV point estimate for mental status approaches zero. With additional adjustment for state characteristics, the IV estimate of education's effect on mental status is slightly negative with a large standard error. The 95% CI includes both the null and OLS estimate. These results are consistent across a range of model specifications (see Appendix 2, available online only).

As a sensitivity analysis, we examined individuals with more than 13 years of school. CSL influence primary and secondary schooling and the IV coefficients estimate the effect of additional schooling on people whose schooling was constrained by CSL. We therefore expect the IV estimates for the entire sample (including participants with more than 12 years of schooling) to equal IV estimates for those who completed less than 13 years of school. Furthermore, we expect IV estimates calculated only using individuals with more than 12 years of schooling to be uninformative (the CSL had no effect on their schooling). These predictions were supported. For example, the model 3 IV estimate for the effect of education on memory (adjusting for sex, birth year and birth state) calculated including all sample members is 0.15 (95% CI −0.01 to 0.31), nearly the same as the estimate for individuals with fewer than 13 years of school (0.18). In contrast, the IV estimate based only on individuals who completed 13 or more years of schooling is −1.04 (95% CI −3.70 to 1.62). The wide confidence bounds are because, as expected, among those with more than 13 years of education, CSL are nearly independent of attained schooling. The instruments also fail to predict memory scores for HRS sample members with more than 13 years of schooling when adjusted for model 3 ($F_{3.48}$ = 1.39, p=0.26) or model 4 ($F_{3.48}$ = 0.89, p=0.45) covariates.

DISCUSSION

We used CSL as natural experiments for the effect of education on old age memory and mental status. The IV estimate for the effect of education on memory was large, in the expected direction and was statistically significant even after adjustment for demographics, state of birth and state social and economic characteristics. IV estimates for the effect of education on mental status had wide 95% CI and were not statistically distinguishable from either the null or OLS estimates.

Under the IV assumptions,[12] the IV coefficient consistently estimates schooling effects for the subgroup of individuals who received extra schooling because of their state's CSL. A troubling assumption is that schooling is the only pathway linking CSL and cognition. State laws are imperfect natural experiments. The instruments correlate with other measured state characteristics and may also correlate with unmeasured variables affecting memory. IV analyses statistically inflate such biases even if unmeasured relationships are weak.

States with greater wealth, equality, or other desirable characteristics may have increased CSL earlier.[10] We found no effect of CSL on schooling or memory beyond high school, so plausible unmeasured confounders of the instruments must not affect individuals with more than 13 years of school. State characteristics would only confound our instrument if CSL changes were associated with changes in unmeasured state characteristics. Although states with high CSL also differed on measured demographic characteristics, CSL changes did not correlate with changes in these state characteristics (results not shown). These variables should not introduce bias after adjusting for the state of birth. Improvements in educational quality that occurred in synchrony with CSL increases may be the most plausible potential confounders, because the same political movements may have influenced both quality and required quantity.

Although structural confounders of mandatory schooling regulations and cognition may bias IV estimates, confounding by innate characteristics, eg, IQ, seems unlikely. Such confounding could occur only if genetic pools within states changed in tandem with CSL, for example if conditions that led states to require more schooling also attracted especially able migrants from other states.

As a result of the small effective sample size and the small difference in education induced by the instruments, our effect estimates have wide 95% CI. We can only examine outcomes strongly associated with education. HRS is among the largest panel studies of US elderly. CSL might be used to identify the effect of education on additional outcomes in countries such as the United Kingdom[30] and Sweden,[31] where legal changes were better enforced and induced larger schooling increases. With very strong instruments, informative bounds for population average treatment effects could be calculated, instead of just the effects on compliers.[32]

Confidence bounds for IV and OLS effect estimates overlap substantially for most models. Differences in estimates could be caused by random fluctuation, larger effects of education in lifecourse periods when CSL increase schooling, or larger education benefits for individuals affected by CSL. School dropouts typically have test scores well below average and the effects of additional schooling for these children may exceed the average effects of extra schooling on children. We would require additional data to disentangle these possibilities.

Given that IV estimates for memory were significant with identical instruments and similar sample sizes, the non-significant findings for mental status may indicate that education does not affect the construct measured by the TICS, the effect is obscured by limitations in our measure, eg, a low measurement ceiling and substantial measurement error, or the variance of the mental status IV estimate is inflated because unmeasured state-level conditions have large effects on mental status.

If extra schooling improves memory in old age, this is substantively important because memory impairments influence quality of life, functional independence and mortality risk.[1–3 33] Robust relationships between education and dementia provide impetus for the “use-it-or-lose-it” hypothesis of cognitive aging.[34–36] Investments in increasing schooling access may have large health payoffs as populations age in the United States and internationally. We did not find evidence that changes in required years of primary schooling, which may have indirectly affected classroom environments or labour market competition,[31] influenced cognitive outcomes for individuals who completed more than high school. Our analyses were restricted to US-born non-Hispanic white individuals, a population for which we can overcome some of the methodological difficulties in observational studies. Our results provide evidence that observational studies linking education and memory function in this elderly population are not spurious reflections of uncontrolled confounding. We cannot be sure our results generalise to other populations or time periods, but they suggest that confounders often thought to threaten observational evidence, eg, intelligence or innate personality characteristics, did not introduce substantial bias in this population.

In conclusion, we present new evidence linking education and old age cognitive function and memory. Our results provide evidence that the association between education and memory is the result of a causal effect of education, but we cannot draw conclusions regarding the association of education and mental status because of wide 95% CI. Plausible alternative explanations of our results might focus on educational quality or socioeconomic changes in the community of birth. These results are an important complement to existing observational studies linking education and old age cognitive outcomes. Nearly all previous studies face potential confounding by innate characteristics, but such confounding is an unlikely source of bias in the current analysis.

What this study adds

- Previous research shows that those with more schooling have better memory and cognitive function in old age and less risk of memory-related diseases. This association may not be causal, however, if individuals who pursue more schooling are protected from cognitive decline for other reasons.
- We used changes in state CSL as natural experiments to estimate the effect of extra education on memory and mental status in old age. Because individual characteristics such as IQ do not affect state laws, this allows us to avoid confounding by such innate personal traits.
- Changes in state CSL in childhood predicted improved performance on memory tests in old age. This suggests that extra education beneficially influenced memory function.

Policy implications

The substantial investments in expanding schooling made by US states in the early 20th century may now be paying off in the form of improved memory performance and therefore potentially a reduced risk of dementing diseases in elderly cohorts who benefited from these investments. When considering the potential value of future investments in educational opportunities, the long-term health and cognitive benefits should be considered. Important questions remain about how the timing of extra educational investments modifies the benefits of the education and how school content influences the pay off.

Supplementary Material

Refer to Web version on PubMed Central for supplementary material.

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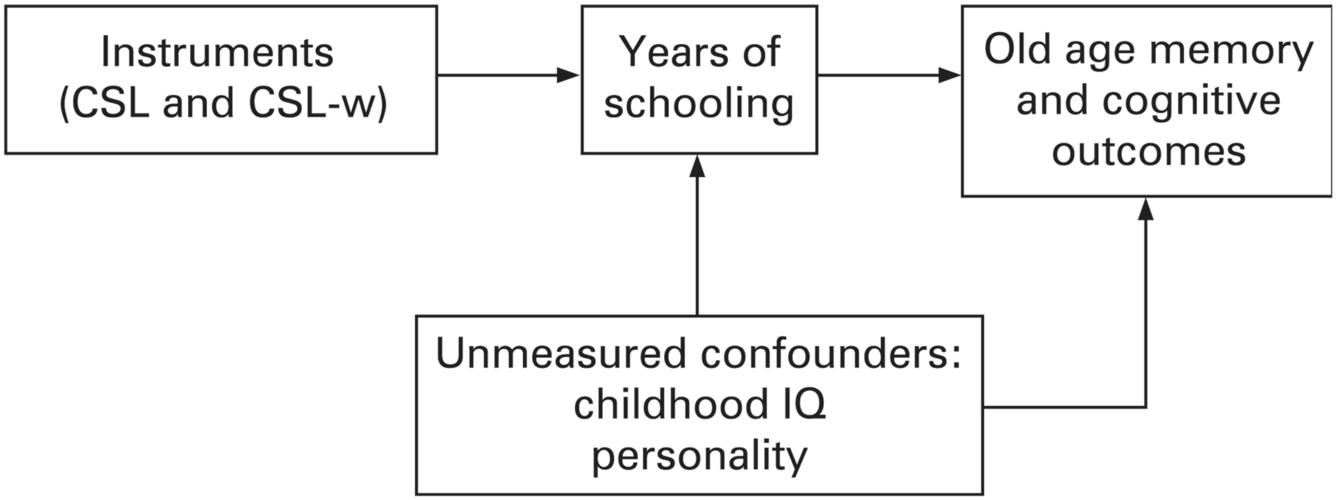

Figure 1.
Hypothesised structural relations among instruments, education and outcomes. CSL, Compulsory schooling laws; CSL-w, compulsory school to work laws.

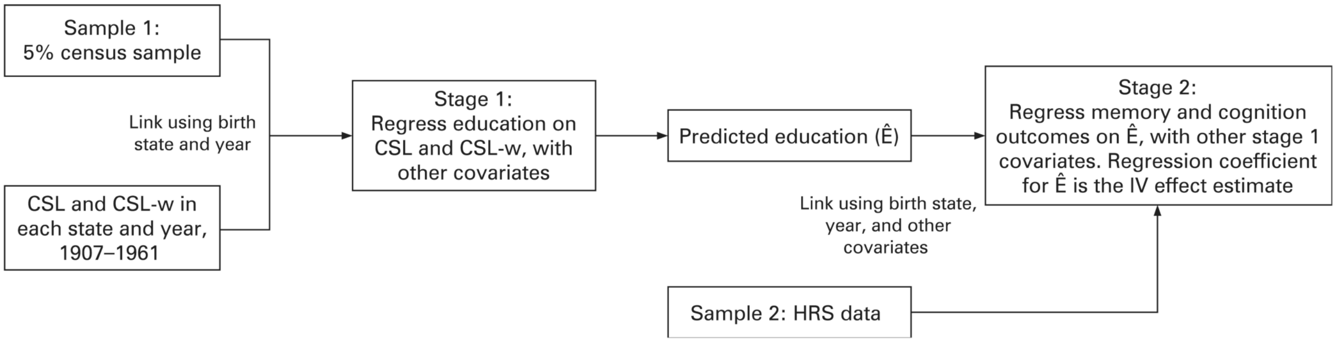

Figure 2.

Separate-sample two-stage least squares analysis approach.

CSL, Compulsory schooling laws; CSL-w, compulsory school to work laws; HRS, Health and Retirement Study; IV, instrument variable.

Table 1

Demographic characteristics of age, birthplace and education-eligible non-Hispanic white Health and Retirement Study participants, comparing sample members with complete data with sample members missing either outcome measure

	Eligible sample members with complete data*	Eligible sample members* missing memory or mental status
	n (%)	n (%)
n	11 427 (100)	1712 (100)
Male	4862 (43)	976 (57)
Birth year		
<1914	1278 (11)	272 (16)
1914–1921	2078 (18)	208 (12)
1922–1930	2656 (23)	208 (12)
1931–1941	4229 (37)	864 (50)
1942–1947	1186 (10)	160 (9)
Years of schooling		
<6	308 (3)	122 (7)
6–8	1706 (15)	351 (21)
9–11	2677 (23)	419 (24)
12	6719 (59)	810 (47)
Mother's education, years		
<8	3264 (29)	528 (31)
8+	6613 (58)	846 (49)
Unknown	1550 (14)	338 (20)
Father's education, years		
<8	3814 (33)	610 (36)
8+	5734 (50)	717 (42)
Unknown	1879 (16)	385 (22)

* Eligible sample members include white non-Hispanic individuals born 1901–47, with education of less than 13 years, known US state of birth with compulsory schooling laws.

Table 2

Linear regression coefficients for the effect of schooling policies in birth state on years of education among age, birthplace and education-eligible non-Hispanic white respondents in the US 5% 1980 census sample

	1. Unadjusted model	2. Birth year* and sex	3. Model 2 + state of birth indicators	4. Model 3 + state characteristics†
	β (95% CI)	β (95% CI)	β (95% CI)	β (95% CI)
CSL	0.238 (0.236 to 0.240)	0.110 (0.108 to 0.112)	0.062 (0.059 to 0.064)	0.037 (0.034 to 0.040)
CSL-w	0.143 (0.146 to 0.141)	−0.032 (−0.034 to −0.029)	0.063 (0.060 to 0.066)	0.044 (0.040 to 0.048)
CSL-w unrestricted	−1.397 (−1.429 to −1.365)	−0.282 (−0.315 to −0.249)	−0.204 (−0.238 to −0.17)	0.034 (0.000 to 0.069)

Data are from the Integrated Public Use Microdata sample, US census 5% sample. Parameter estimates are for models adjusted simultaneously for compulsory schooling laws (CSL), compulsory school to work laws (CSL-w) and an indicator for CSL-w unrestricted.

*Birth year includes an indicator variable for every year of birth, 1901–1947, with 1934 as the reference category.

†State characteristics: percentage black, percentage urban and percentage foreign born at age 6 years; manufacturing jobs per capita and wages per manufacturing job at age 14 years.

Table 3

Estimates for the effect of one year of schooling on memory and mental status comparing separate sample instrumental variables estimates with ordinary least squares estimates

	Memory		Mental status	
Model covariates	**β_{SSIV}* (95% CI)†**	**β_{OLS}* (95% CI)†**	**β_{SSIV}* (95% CI)†**	**β_{OLS}* (95% CI)†**
1. Unadjusted	0.33 (0.27 to 0.39)	0.13 (0.12 to 0.14)	0.19 (0.12 to 0.26)	0.16 (0.15 to 0.18)
2. Birth year and sex	0.30 (0.14 to 0.46)	0.10 (0.09 to 0.10)	0.34 (0.05 to 0.63)	0.16 (0.15 to 0.17)
3. Model 2 + state of birth indicators	0.18 (0.02 to 0.33)	0.09 (0.08 to 0.10)	0.03 (−0.22 to 0.27)	0.15 (0.14 to 0.16)
4. Model 3 + state characteristics‡	0.34 (0.11 to 0.57)	0.09 (0.08 to 0.10)	−0.06 (−0.37 to 0.26)	0.15 (0.14 to 0.16)

*Separate sample instrumental variables (SSIV) estimates based on first stage calculations in the US census 5% sample and second stage calculations based on Health and Retirement Survey (HRS) participants. Ordinary least squares (OLS) estimates were calculated in the HRS sample.

†Confidence intervals based on robust variance estimates, with adjustment for clustering on state of birth.

‡State characteristics: age six years percentage black, percentage urban and percentage foreign born; age 14 years manufacturing jobs per capita and age 14 years manufacturing wages per manufacturing job.