REAPING WHAT WE SOW: EARLY 20TH CENTURY STATE SCHOOL POLICIES, COGNITIVE SKILLS, AND ELDER HEALTH.

M. Maria Glymour[1,2,3], Christopher C. Weiss[3], and Jennifer J. Manly[4,5]

[1] Department of Society, Human Development, and Health,
Harvard School of Public Health
Boston, Massachusetts

[2] Department of Epidemiology,
Mailman School of Public Health at Columbia University
New York, New York 10032

[3]Institute for Social and Economic Research and Policy (ISERP)
Columbia University
420 West 118th Street
New York, NY 10027

[4]Taub Center for Alzheimer's Disease Research in the City of New York,
Columbia University College of Physicians and Surgeons,
New York, NY 10032

[5]Department of Neurology,
Columbia University College of Physicians and Surgeons,
New York, NY 10032

Corresponding author:
M. Maria Glymour
Department of Epidemiology
Mailman School of Public Health
722 West 168th Street, room 1603
Columbia University
New York, NY 10032
mg2630@columbia.edu
phone: (212) 342-0166
fax: (212) 342-5169

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SHORT ABSTRACT

Although cognitive skills correlate with health, the influence of cognitive skills on health remains controversial, in part because of the dynamic relationship between socioeconomic status and cognitive skills. We examine the influence of cognitive skills on health by estimating long-term health benefits of cognitive skills attained as a consequence of social policies. We treat measures of school quality (teacher-student ratio, teacher salaries, and term length) as instruments for cognitive skills in US-born whites from the 1901-1947 birth cohorts with less than 13 years of school. We use changes in state school quality to generate instrumental variable (IV) estimates of the effects of cognitive skills (vocabulary, mental status, and word list memory) on health (disability, self-rated health, and prevalence of chronic conditions) in Health and Retirement Study participants. We find that improvements in cognitive skills induced by school policies predict reductions in disability, independent of attained education and measured background factors.

Various measures of cognitive function have been repeatedly linked to adult morbidity and mortality (Starr, 2000; Martin, 2004; Batty, 2007), and this association has led some to argue that the socioeconomic gradient in health may primarily reflect confounding by cognitive ability. Cognitive function is frequently conceptualized as a stable, possibly innate, feature of individuals, as in much of the literature on IQ or *g*. As Linda Gottfredson argues, perhaps "*g* theory can explain social class differences in health better than can conventional theories of social inequality."(Gottfredson, 2004) This is a potentially devastating argument for those who advocate policies for improving socioeconomic equality as a technique for improving population health outcomes and reducing health disparities.

Contrary to much work in cognitive epidemiology, we focus on learned cognitive skills instead of more general and abstract constructs related to mental capacity (Deary, 2007). We argue here that cognitive skills are attained in part as a result of policy investments in children, and that the skills so developed influence adult health even into old age. We test this claim by examining how differences in school quality when children attended primary school predict a) cognitive skills in old age, and b) health in old age.

This paper exploits natural experiments to circumvent confounding by unmeasured common causes of schooling and health. The first half of the 20th century was a time of rapid change in the availability and quality of primary and secondary school. We use changes in state compulsory school quality measures (teacher salary, term length, and student-teacher ratios) as "natural policy experiments" to identify changes in cognitive skills that are not due to innate characteristics. The changes in cognitive skills resulting from changes in school quality can, in principal, be used as instrumental variables (IVs) to estimate the causal effect of cognitive skills on health outcomes, even if cognitive skills are strongly influenced by innate characteristics or other unmeasured factors that influence health.(Angrist, 1996; Pearl, 2000) School quality may be higher in states with other health-enhancing policies or characteristics, so our analyses condition on individual states. Thus, we identify effects using *changes* in average school quality within a state. Because educational experiences may influence health through other pathways besides cognitive skills, for example via credentials that improve labor market outcomes, we consider these relationships with and without controlling for attained years of education (figure 1).

Although IV analyses have substantial advantages in research questions where unmeasured confounding (omitted variable bias) is likely, an important difficulty in IV analyses is identifying an instrument with a sufficiently strong influence on the exposure of interest to provide informative confidence bounds. For this reason, we focus our analyses on the subpopulation in which our school quality measures were likely to have the largest effects on educational attainment: US-born, non-Hispanic whites who did not attend college. We exclude non-whites because for most of the 20th century schools have been nearly perfectly segregated by race, so black and white children born in the same place and time are nonetheless exposed to very different school policy environments. For some states and years, this segregation was enforced by law, and separate accounts were kept of the quality measures for schools serving black and white children. For many places, the segregation was de facto, however, and we are not able to link children to the relevant policy information.

MATERIAL AND METHODS

The instrumental variables (IV) analyses estimate the effect of cognitive skills on health by combining information on two associations: the association between the instruments (school quality) and cognitive skills, and the association between the instruments and health

Individual level data

The individual-level data on education, cognitive skills, and covariates are from the Health and Retirement Study (HRS), an ongoing national probability sample of US residents born 1947 or earlier and their spouses. We have up to seven assessments, depending on enrollment cohort. We excluded non-white and Hispanic HRS participants as explained above. HRS participants born outside the US are also excluded because over 80% of these individuals immigrated when they were past school age. Prima facie, school quality in primary and secondary school seems most likely to influence cognitive skills for individuals who did not attend college. College attendees presumably have greater subsequent demands and opportunities to develop cognitive skills compared to those with high-school or less. We therefore restrict our analyses to individuals reporting high school education or less. Detailed documentation on HRS sample design and validation of HRS cognitive measures is available online.(Heeringa, 1995; Wallace, 1995; Ofstedal, 2002)

HRS participant data were linked to state characteristics using self-reported birth state, which largely coincides with the state where children attended school but entails some misclassification.(Card, 1992) The residential history information necessary to classify more precisely using school policies in the state where each child resided during the exact years of school attendance was unavailable. Barring selective migration, IV estimates are consistent despite this misclassification, because the attenuated relationship affects both stages of the IV calculations.

From the 25,570 HRS sample members born 1901-1947, we made the following exclusions: not born in a US state or the District of Columbia (n=2,392); black or Hispanic (n=4411); years of schooling greater than 12 (n=6981); missing information on key covariates (n=361); missing information on one of or more cognitive test scores (n=2,272); and missing one or more of the health measures (n=3). This left 9,150 potentially eligible respondents.

Health Outcomes

We consider four health outcomes: limitations in 5 activities of daily living (ADLs: reporting at least "some difficulty" in bathing, dressing, eating, getting in/out of bed, and walking across a room); limitations in 5 instrumental activities of daily living (IADLs: reporting at least "some difficulty" in using the phone, managing money, taking medications, shopping for groceries and preparing hot meals); self-rated health (rated on a scale from 1 to 5 corresponding to excellent, very good, good, fair, or poor), and a count of 8 possible chronic conditions ("has a doctor ever told you that you have" high blood pressure, diabetes, cancer, lung disease, heart disease, stroke, psychiatric problems, or arthritis).

. Each of these measures were assessed at every interview, and, to improve statistical power, we averaged across all available reports for each respondent.

Cognitive Tests (endogenous variables)

HRS participants were repeatedly administered vocabulary, word list recall (Memory) and mental status (Mental Status) tests. Detailed descriptions and evidence on the reliability and validity of these measures are available elsewhere.(Ofstedal, 2005) We used all available vocabulary assessments through the year 2000 and all available Memory and Mental Status assessments through the year 2002.

Vocabulary was assessed based on definitions of 5 words from the WAIS-R list (randomly chosen from one of two alternating word lists). Respondents were scored 0 for incorrect answers, 1 for partially correct answers, and 2 for completely correct answers. Each participants' scores were summed and converted to a z-score relative to others assigned the same word list.

Immediate and delayed word list recall scores were summed and standardized by subtracting the mean value and dividing by the standard deviation, and averaged over all waves in which the participant took the assessment. Word recall tests in 1992 and 1994 used lists of 20 common nouns; in all other waves, recall was based on 10-word lists. To reduce the influence of outlying values and account for wider ranges of scores achieved in some years than others, 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 was assessed with a modified Telephone Interview for Cognitive Status (TICS), which included a serial-7 subtraction task.(Brandt, 1988) Our coding omitted the items for "naming scissors" (because of low correlation with other items) and second attempt at counting backwards from 20 (because of apparent inconsistencies in the administration). The possible range for the TICS was 0-13, and in each wave many (21-30 percent) eligible respondents scored 13. As with Memory, Mental Status scores were standardized, with a floor imposed at -1.96 (no scores were above 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 were only given the TICS once. Evidence regarding the value of these outcomes for predicting memory diseases, institutionalization, and mortality in HRS sample members is available from the authors.

With our analysis approach, it is impossible to disentangle the effects of one cognitive skill from another. Ideally, we would base the analysis on a comprehensive measure of all cognitive skills potentially influenced by schooling, but no such comprehensive measure is available. Our best substitute is a Combined Cognitive skills score created by averaging the z-scores on the 3 domains.

School quality measures

We used federal education reports, typically published biennially, to collect average teacher salaries, term length (number of days in the school year), and student-teacher ratio for each state and year, 1907-1955. For years with no available federal reports, we used a linear interpolation between the two closest years with data. Teacher salaries were CPI-U adjusted to 1967 dollars, with a linear extrapolation of the trend across subsequent years used to calculate CPI-Us from 1907-1912. For each birth-year/birth-state cohort in the HRS sample, we calculated applicable policies in the years when cohort members were ages 8 to 12 (inclusive). The average school quality was calculated as the average of non-missing values for these ages.

Covariates

The IV estimates are valid under the assumption that, conditional on the covariates, the school quality measures relate to health outcomes only via the cognitive scores we present. However, there are several other plausible causal links between school quality and health, for example states that provide high quality schooling may also be beneficial for health for other reasons. Parental socioeconomic status may tend to be higher in states with high-quality schooling. Perhaps most importantly, school quality probably increases attained education, and the credential of achieving additional years of education may itself directly influence health. We attempt to address each of these threats to the validity of the instruments by adjusting for measured individual level and state level covariates.

We present results adjusted three sets of covariates: Model 1 includes adjustment for sex, year of birth (coded as individual years of birth), and state of birth; model 2 covariates include all of the above plus years of attained education, and model 3 covariates include all model 2 covariates plus measures of mother's education (<8 years, >=8 years, unknown), father's education (<8 years, >=8 years, unknown), father's occupation (0=manager/professional, 1=white collar, 2=skilled labor, 3=unskilled labor, plus indicators for military, farming, and unknown), an indicator for whether the state had a compulsory schooling law and the number of mandatory years of schooling, an indicator for whether the state had a compulsory school-to-work law (schooling requirements in order to receive a work permit) and the number of years of mandatory schooling under this law, state demographic characteristics when the respondent was age 6 (% urban, % black, and % foreign born), and state labor market characteristics when the respondent was age 14 (manufacturing jobs per capita and average wages per manufacturing job).

State characteristics were collected from the Statistical Abstracts of the United States using decennial census data and manufacturing employment data (reported every 2-8 years).(Commerce, 1906-1973) These state level variables were chosen because of their likely relationship with investments in high-quality schooling and the expansion of public education.(Goldin, 1998; Goldin, 1999) Linear interpolation was used to estimate state characteristics for years between federal reports. The compulsory schooling law information is based on data reported by (Angrist 1996) and Lleras Muney (Lleras-Muney, 2002), supplemented with additional years of data collected from federal schooling reports.

Statistical analyses

We first present ordinary least squares (OLS) estimates of the effect of each cognitive score on each health outcome. We then used two-stage least squares (2SLS) to calculate IV effect estimates. Respondents with missing data on school quality measures, cognitive test scores, years of education, or health outcomes were excluded from the analyses. For other variables (specifically, the parental SES measures), unknown values were handled with missing value indicators, in which all missing values are set to the same constant value, and regressions include an indicator variable for whether the variable value is known. Respondents with missing values for parental SES are not excluded because many HRS respondents did not grow up in intact 2-parent families, and unknown values for parental SES may reflect such differences in family structure.

The HRS complex sample design weights are not applicable when combining all interviews for 1901-1947 birth cohorts. As a result, these estimates are probably slightly inconsistent with what would be seen using a nationally representative sample of age-eligible whites at a single point in time. The standard errors may be slightly too narrow, although previous efforts to adjust standard errors for the clustered sample design have not indicated substantial changes. All analyses were conducted in SAS, version 9.1.

RESULTS

Characteristics of eligible HRS sample members included in the analyses and eligible sample members who were excluded due to missing data are compared in Table 1.

In models adjusted for core demographic characteristics, each of the cognitive skills predicted all four health outcomes, ADLs, IADLS, self-rated health, and chronic conditions (Table 2, model 1). Nearly all of the relationships retained statistical significance after adjustment for years of education (model 2) and the full set of measured background variables (model 3). The Combined cognitive skills measure was generally the best predictor of the health outcomes (indicated by the largest t-statistic). Of the four outcomes, the count of chronic conditions had the weakest and least consistent relationships with the various cognitive measures, especially for the vocabulary test.

The first stage of the IV analysis is to estimate the effect of school quality on cognitive test scores. Table 3 (model 3) shows that, in fully adjusted models, longer term lengths predicted better vocabulary, mental status, and combined cognitive scores. Higher student teacher ratios predicted reduced vocabulary scores, but were not significantly associated with mental status or memory in fully adjusted models. Teacher salary did not predict any of the cognitive measures. The bottom rows in each section show that the variance explained by all three instruments (the school quality measures) is very small, but remains statistically significant even after full covariate adjustment for the vocabulary and combined cognitive test score. Because these models are conditioned on reported years of education attained, the influence of school quality on these cognitive test scores is presumably not mediated by additional years of schooling.

We next examined the reduced form estimates, regressing health directly on the school quality measures. In fully adjusted models, we find that the school quality measures predict both ADL ($F_{3,9033}$=3.19, p=0.02) and IADL disability ($F_{3,9033}$=4.34, p<0.01), but neither self-rated health ($F_{3,9033}$=1.18, p=0.31) nor chronic condition count ($F_{3,9033}$=0.17, p=0.91).

The IV effect estimates indicate strong and consistent relationships between cognitive test scores and both ADL and IADL disability (Table 4). Even after full covariate adjustment, vocabulary score and the combined cognitive measure significantly predict ADL and IADL limitations. For example, a 1 standard deviation increased in Combined cognitive score is associated with a 1.5 (95% CI: -2.7, -0.2) point reduction in ADL limitations. However, there is no evidence for an effect of cognitive test scores on self-rated health or chronic conditions.

We conducted two supplementary analyses to assess potential sources of bias in our approach. One concern is that our cognitive and health measures were collected concurrently. Thus, it is possible that disability may have led to reductions in cognitive skills, rather than vice-versa. We conducted a limited sensitivity analysis restricting to participants with no disability at first assessment (n=7,425 for ADLs and n=7,615 for IADLs) and examining worst level of disability reported in any subsequent interview as the outcome. For this assessment, we used only the vocabulary test score at the first assessment as the endogenous variable. Thus, we are using cognitive skill before the onset of any disability to predict subsequent development of I/ADL limitations. The IV estimated effect of baseline vocabulary test was a significant predictor of both maximum subsequent ADLs (β=-1.08, 95% CI: -2.06, -0.10) and IADLs (β=-0.95, 95% CI: -1.81, -0.09), in models adjusted for the full covariate set.

We also examined the IV estimates when individuals with 13 or more years of schooling were included in the analyses. We expect the results when including these highly educated people, among whom school quality is not expected to influence cognitive test scores, to have approximately the same IV estimate. Results for the disability measures were nearly identical to results in the restricted sample. For example, the Combined cognitive score predicted a 1.41 point reduction in ADL limitations (95% CI: -.47, -2.35, n=15,488), and a 1.51 point reduction in IADL limitations (95% CI: -.62, -2.41).

DISCUSSION

Our results indicate that improvements in school quality within a state predict improved cognitive test scores for exposed students decades later, and that these improvements in cognitive skills predict reductions in disability. We find no evidence that cognitive skills improved self-rated health or prevalence of chronic conditions, although the confidence bounds on our estimates are very wide and thus do not preclude substantial effects. Our approach rests on strong assumptions and has important limitations. We first discuss our concerns and then discuss the substantive implications of these findings, if they prove credible.

The IV coefficient consistently estimates the effect of cognitive skills only for the subgroup of individuals whose skills improved specifically because of the higher quality schooling they received, and even then only under strong assumptions. The most potentially controversial assumption is that cognitive skills are the *only* pathway linking the school quality and health. This may not be plausible because state policies are imperfect natural experiments. The instruments may correlate with unmeasured variables that affect health. IV analyses statistically inflate such biases even if the unmeasured relationships are weak.

In models including birth-state indicators, IV estimates are calculated by contrasting outcomes among people born in the same state before and after a change in the state average school quality measure. Investments in improved school quality may reflect improving socioeconomic conditions in the state, and state level SES in early life may affect long term health outcomes via improved nutrition or other pathways, rather than increases in cognitive skills. States with greater wealth and equality may have invested more in improving school quality and access earlier in the century, for example.(Goldin 1999) Because all of our models were conditioned on state of birth, unmeasured state characteristics would only threaten the validity of our instrument if *changes* in school quality measures were associated with *changes* in these unmeasured state characteristics. Addition of a fairly comprehensive list of covariates to the models did little to alter the IV effect estimates. Although Model 3 estimates have larger standard errors (which is to be expected given that many of the covariates were strongly correlated with the instrumental variables), the coefficient estimates themselves are relatively stable, suggesting that confounding by measured state and individual characteristics did not contribute substantially to the estimates.

The most plausible confounders of school quality are structural characteristics of the social or policy environments that changed within states and resulted in differences in health outcomes decades later. Innate individual characteristics are unlikely to account for our results. Confounding by innate characteristics could occur only if the genetic pools within states changed in tandem with school quality, for example if conditions that led states to improve school quality also attracted especially able migrants from other states.

The measures of our instrumental and endogenous variables available to us introduced important limitations to our study. We are not able to measure the quality of the school each individual actually attended, but only the state average school quality. This has the advantage of reducing the possibility that high-capability students would select into high-quality schools. However, it has the substantial disadvantage that the relationship between state average school quality measures and cognitive skills developed by individual children is quite modest. As a result of this, and the small effective sample size, our effect estimates have wide CIs. HRS is among the largest panel studies of US elderly, but without greater geographic detail on place of schooling, the potential statistical power of analyses such as ours is limited. An additional disadvantage of the IV estimates we present is that they cannot be interpreted as the population average effect. Rather, they are the estimated effect for those individuals whose cognitive skills were influenced by school quality. For children with highly educated parents, for example, school quality may be irrelevant to their cognitive development. Very strong instruments can help overcome this problem and provide informative bounds for population average treatment effects, instead of just estimated effects on compliers.(Balke, 1997)

Confidence bounds for IV and OLS effect estimates overlap substantially for most of our models. However, overall, when the IV effect estimates are statistically significant, they are much larger than the OLS effect estimates (compare Table 2 parameters to Table 4 parameters). Divergent OLS and IV effect estimates are conventionally attributed to a) unmeasured common causes of the instrument and the outcome, as discussed above; b) random fluctuation due to the large standard errors for IV effect estimates or c) to differences between the population average effect (estimated by OLS) and the complier-specific effect (estimated by the IV).

Although these explanations are possible, the limited measures of our endogenous variable may play a more important role in inflating the IV estimates in the current analyses. School quality likely affects multiple cognitive skills, each of which is imperfectly correlated with the brief assessments we have here. As evidence for this, we note that the correlations among vocabulary, mental status, and memory measures were modest (in the range of 0.32-0.38). If school quality influences long-term health via other cognitive skills, and these skills are imperfectly correlated with our measured skills, this will serve to inflate the IV effect estimate.

We found significant IV estimates for the effects of cognitive skills on disability, but no significant relationship with self-rated health or prevalence of chronic conditions. The OLS estimates suggest that the relationship between cognition and prevalence of chronic conditions, although quite strong, is substantially weaker than the relationship between cognition and disability. Thus, the null results for prevalence of chronic conditions may largely reflect the inadequate statistical power in this analysis. However, the result is also substantively consistent with a conceptualization of disability that emphasizes a mismatch between biological capabilities (driven in many respects by disease processes) and environmental demands (Verbrugge, 1994). Cognitive skills may be exceptionally powerful in enabling individuals to negotiate that capability-demand gap in ways that ameliorate disability. For example, cognitive skills can help individuals negotiate the medical and rehabilitation care systems, problem solve in their daily lives to introduce environmental modifications, and command greater resources in the world at large to eliminate or circumvent environmental barriers.

The null findings for self-rated health are more puzzling. The OLS relation between cognitive skills and self-rated health is just as strong as the OLS relation between cognitive skills and disability, so it is difficult to attribute the null results to inadequate power. One possibility is that self-rated health is assessed relative to other community members rather than to an absolute reference point for health. Thus, contextual factors that change the level of self-rated health for everyone in a community may fail to improve any individual's self-rated health.

A final important limitation in the current analyses is the restricted sample. Our analyses were limited to US-born non-Hispanic whites, a subpopulation for which the school quality measures we have available are likely most relevant. We cannot be sure that these results generalize to other populations, in the US or elsewhere. Analyses examining whether these associations hold in African American cohorts are crucial.

Given the dramatic differences in school quality between black and white schools in the early 20th century, the potential role that school quality differences may play in contemporary health disparities is also an important area for further research.

Our results are consistent with the hypothesis that IQ or cognitive abilities could be important causes of health. They are not consistent, however, with the (sometimes implicit) premise of this argument that cognitive skills are intrinsic features of individuals. Our evidence suggests instead that cognitive skills manifested by individuals are in part a product of the social policy environment in which those individuals were raised. Substantively, this argument may help reframe our understanding of persistent socioeconomic and racial disparities in health.

The influence of contextual level variables on cognitive skills also presents a methodological challenge in research on IQ that relies on twin or sibling designs to calculate the percent of variance attributable to genetic influence. To the extent that the measurements of IQ are susceptible to macro-level influence, the variability of IQ estimated from any single population exposed to the same policy environment will be underestimated (relative to what the variance would have been without the common policy environment), and the variance contribution of non-genetic factors will be concomitantly underestimated.

In conclusion, our findings indicate that school quality prevailing in childhood influences cognitive skills and ultimately disability decades later. Although there are plausible alternative explanations of our results based on other changeable state characteristics, the findings are not consistent with confounding by "IQ" or any innate characteristic. The associations we present highlight the relevance of macro-level social policies in shaping cognitive skills of children raised in those policy contexts, and the long term influence of policy-induced cognitive skills on health.

Our results have important implications for population health and health disparities. The early 20th century was a time of great social investment in educating white American children. Our findings suggest that we are now very likely reaping the benefits of those investments in reduced rates of disability experienced by the now-elderly cohorts. This process is not mediated merely by the credential of education, but by learned cognitive skills that increase the capability of the children to garner needed resources to prevent disability even as they reach old age. This also suggests that the historical segregation of schools and differences in school curriculum and quality along lines demarcated by race, socioeconomic background, geography, and in some cases gender, is likely to manifest in long-term health disparities.

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TABLES

Table 1. Characteristics of eligible HRS participants, comparing sample members included in the analyses to sample members excluded due to missing data.

	Eligibles Included in Analyses		Eligibles Excluded Due to Missing Values	
	n	%	n	%
n	9,150	100	2,636	100
Male	3,624	40	1,423	54
Birth Year, mean (SD)	1929	(10)	1926	(12)
Birth Regions				
Northeast	1,974	22	415	16
West	804	9	204	8
Midwest	3,145	34	574	22
South	3,227	35	845	32
Uknown	-		327	12
Years of schooling, mean (SD)	10.8	(1.9)	10.0	(2.6)
*Father's Occupation^*				
Unskilled Laborer (3)	2,033	22	187	7
Skilled Labor (2)	2,034	22	199	8
White Collar (1)	602	7	45	2
Managerial/Professional (0)	552	6	54	2
Farming	2,155	24	296	11
Army	39	0	6	0
Unknown	1,735	19	1,849	70
Father's Education				
< 8 Years	2,633	29	908	34
8+ Years	4,782	52	1,003	38
Unknown	1,735	19	725	28
Mother's Education				
< 8 Years	2,492	27	825	31
8+ Years	5,527	60	1,142	43
Unkown	1,131	12	669	25
Term length, ages 8-12, mean (SD)	174	(11)	171	(15)
Teacher salary, ages 8-12, mean (SD)	3,426	(1,267)	3,110	(1,279)
(SD)	29.5	(4.5)	30.8	(5.2)

Eligibles were restricted to white non-Hispanics born in the US from 1901-1947 with education < 13 years.
Percentages do not always sum to 100 due to rounding.

Table 2. OLS estimates of the effect of cognitive skills on health outcomes, non-Hispanic whites born 1901-1947 with less than 13 years education in HRS, n=9,137

	ADL Summary			IADL Summary			Self-Rated Health			Condition Count		
	β	t-stat	95% CI	β	t-stat	95% CI	β	t-stat	95% CI	β	t-stat	95% CI
Model 1[*]												
Vocabulary	-0.09	-8.9	(-0.11, -0.07)	-0.10	-11	(-0.12, -0.08)	-0.15	-12	(-0.17, -0.12)	-0.08	-4.6	(-0.11, -0.04)
Mental Status	-0.17	-17	(-0.19, -0.15)	-0.20	-22	(-0.22, -0.18)	-0.24	-19	(-0.26, -0.21)	-0.18	-11	(-0.21, -0.15)
Memory	-0.19	-15	(-0.22, -0.17)	-0.23	-21	(-0.25, -0.21)	-0.30	-20	(-0.32, -0.27)	-0.22	-11	(-0.26, -0.18)
Combined	-0.26	-18	(-0.29, -0.24)	-0.30	-23	(-0.33, -0.28)	-0.39	-23	(-0.42, -0.35)	-0.27	-12	(-0.32, -0.23)
Model 2[^]												
Vocabulary	-0.06	-5.8	(-0.09, -0.04)	-0.07	-7.6	(-0.09, -0.06)	-0.08	-6.4	(-0.11, -0.06)	-0.02	-1.3	(-0.06, 0.01)
Mental Status	-0.15	-14	(-0.17, -0.13)	-0.18	-18	(-0.2, -0.16)	-0.17	-13	(-0.2, -0.15)	-0.13	-7.6	(-0.17, -0.1)
Memory	-0.17	-13	(-0.19, -0.14)	-0.21	-18	(-0.23, -0.19)	-0.24	-16	(-0.27, -0.21)	-0.18	-8.8	(-0.22, -0.14)
Combined	-0.24	-15	(-0.27, -0.2)	-0.29	-20	(-0.31, -0.26)	-0.30	-16	(-0.34, -0.26)	-0.20	-7.9	(-0.25, -0.15)
Model 3[#]												
Vocabulary	-0.06	-5.6	(-0.08, -0.04)	-0.07	-7.5	(-0.09, -0.05)	-0.08	-5.9	(-0.1, -0.05)	-0.02	-1.3	(-0.06, 0.01)
Mental Status	-0.15	-13	(-0.17, -0.13)	-0.18	-18	(-0.2, -0.16)	-0.17	-13	(-0.19, -0.14)	-0.13	-7.4	(-0.17, -0.1)
Memory	-0.17	-13	(-0.19, -0.14)	-0.21	-18	(-0.23, -0.19)	-0.23	-15	(-0.26, -0.2)	-0.18	-8.7	(-0.22, -0.14)
Combined	-0.23	-15	(-0.27, -0.2)	-0.29	-20	(-0.32, -0.26)	-0.29	-16	(-0.33, -0.25)	-0.20	-7.8	(-0.25, -0.15)

* Model 1 includes adjustment for year of birth (indicators for every year 1902-1947), state of birth (indicators for

^ Model 2 includes Model 1 covariates plus years of education.

compulsory school to work laws, and state characteristics at age 6 (% urban, % black, % foreign-born) and age 14 (manufacturing jobs per capita and average wages per manufacturing job).

Table 3. Schooling policies in state of birth and cognitive test scores, non-Hispanic whites born 1901-1947 with education < 13 years in HRS

	1. Vocabulary			2. Mental Status			3. Memory			4. Combined		
	β	t-stat	95% CI	β	t-stat	95% CI	β	t-stat	95% CI	β	t-stat	95% CI
Model 1[*]												
Term Length	0.71	5.3	(0.45, 0.98)	0.45	3.4	(0.19, 0.72)	0.25	2.2	(0.02, 0.47)	0.10	4.9	(-9.49, 9.68)
Teacher Salary	0.06	0.2	(-0.51, 0.62)	0.12	0.4	(-0.45, 0.69)	0.43	1.8	(-0.05, 0.9)	0.20	1.0	(-0.2, 0.6)
Student-Teacher Ratio	-0.23	-4.7	(-0.32, -0.13)	-0.05	-1.0	(-0.14, 0.05)	0.01	0.4	(-0.06, 0.09)	-0.09	-2.5	(-0.15, -0.02)
r^2 with IVs	0.07			0.08			0.22			0.13		
Variance explained by IVs	0.0036			0.0012			0.0008			0.0024		
$F_{3,9037}$-statistic (p-value)	11.7 <.0001			3.9 (0.01)			3.0 (0.03)			8.2 <.0001		
Model 2[^]												
Term Length	0.53	4.1	(0.28, 0.78)	0.24	1.9	(0, 0.49)	0.12	1.1	(-0.1, 0.33)	0.30	3.4	(0.12, 0.47)
Teacher Salary	-0.19	-0.7	(-0.73, 0.35)	-0.16	-0.6	(-0.69, 0.37)	0.25	1.1	(-0.2, 0.71)	-0.03	-0.2	(-0.4, 0.34)
Student-Teacher Ratio	-0.21	-4.5	(-0.3, -0.12)	-0.02	-0.5	(-0.11, 0.07)	0.03	0.8	(-0.05, 0.11)	-0.07	-2.1	(-0.13, -0.01)
r^2 with IVs	0.15			0.19			0.27			0.27		
Variance explained by IVs	0.0025			0.0004			0.0003			0.0009		
$F_{3,9038}$-statistic (p-value)	8.8 <.0001			1.5 (0.22)			1.3 (0.29)			4.0 (0.01)		
Model 3[#]												
Term Length	0.55	2.7	(0.14, 0.95)	0.42	2.1	(0.02, 0.82)	0.29	1.7	(-0.05, 0.64)	0.42	3.0	(0.14, 0.7)
Teacher Salary	-0.15	-0.5	(-0.75, 0.46)	-0.16	-0.5	(-0.76, 0.44)	0.12	0.5	(-0.4, 0.64)	-0.06	-0.3	(-0.48, 0.35)
Student-Teacher Ratio	-0.16	-3.1	(-0.26, -0.06)	-0.02	-0.4	(-0.12, 0.08)	0.02	0.5	(-0.06, 0.11)	-0.05	-1.5	(-0.12, 0.02)
r^2 with IVs	0.17			0.20			0.28			0.29		
Variance explained by IVs	0.0012			0.0004			0.0003			0.0008		
$F_{3,9020}$-statistic (p-value)	4.6 (0.00)			1.5 (0.22)			1.3 (0.28)			3.2 (0.02)		

* Model 1 includes adjustment for year of birth (indicators for every year 1902-1947), state of birth (indicators for every state), and sex.

^ Model 2 includes Model 1 covariates plus years of education.

Model 3 includes Model 2 covariates plus: father's occupation, father's education, mother's education, state compulsory schooling laws and compulsory school to work laws, and state characteristics at age 6 (% urban, % black, % foreign-born) and age 14 (manufacturing jobs per capita and average wages Instrumental Variables (IVs) include term length (in 10 day increments), teacher salary (in $10,000), and student-teacher ratio (in 10s).

Table 4. 2SLS estimates of the effect of cognitive test scores on health outcomes, non-Hispanic whites born 1901-1947 with education < 13 years in HRS.

	ADL Summary			IADL Summary			Self-Rated Health			Condition Count		
	β	t-stat	95% CI	β	t-stat	95% CI	β	t-stat	95% CI	β	t-stat	95% CI
Model 1[*]												
Vocabulary	-0.73	-3.6	(-1.13, -0.34)	-0.79	-4.1	(-1.16, -0.42)	0.15	0.7	(-0.26, 0.55)	-0.04	-0.1	(-0.56, 0.49)
Mental Status	-1.15	-2.8	(-1.96, -0.35)	-1.30	-3.1	(-2.11, -0.49)	0.00	0.0	(-0.69, 0.69)	-0.24	-0.5	(-1.15, 0.67)
Memory	-1.13	-2.2	(-2.12, -0.14)	-1.33	-2.6	(-2.33, -0.33)	-0.60	-1.3	(-1.54, 0.33)	-0.12	-0.2	(-1.35, 1.12)
Combined	-1.23	-3.6	(-1.9, -0.56)	-1.36	-4.2	(-2.01, -0.72)	0.04	0.1	(-0.63, 0.7)	-0.13	-0.3	(-1.01, 0.75)
Model 2[^]												
Vocabulary	-0.75	-3.1	(-1.23, -0.27)	-0.80	-3.5	(-1.25, -0.35)	0.42	1.6	(-0.09, 0.93)	0.06	0.2	(-0.57, 0.69)
Mental Status	-1.42	-1.8	(-2.97, 0.12)	-1.63	-2.0	(-3.24, -0.02)	0.82	1.1	(-0.66, 2.31)	-0.37	-0.5	(-1.96, 1.21)
Memory	-0.84	-1.2	(-2.26, 0.58)	-1.14	-1.5	(-2.59, 0.31)	-0.53	-0.7	(-2.01, 0.95)	0.21	0.2	(-1.8, 2.22)
Combined	-1.67	-2.8	(-2.85, -0.48)	-1.85	-3.1	(-3.02, -0.68)	0.78	1.3	(-0.4, 1.96)	0.04	0.1	(-1.34, 1.41)
Model 3[#]												
Vocabulary	-0.84	-2.4	(-1.53, -0.14)	-0.81	-2.5	(-1.44, -0.18)	0.01	0.0	(-0.64, 0.67)	0.04	0.1	(-0.84, 0.93)
Mental Status	-1.28	-1.7	(-2.74, 0.17)	-1.26	-1.8	(-2.6, 0.08)	-0.42	-0.7	(-1.6, 0.77)	-0.28	-0.3	(-1.86, 1.31)
Memory	-1.09	-1.4	(-2.62, 0.44)	-1.34	-1.7	(-2.9, 0.22)	-1.15	-1.3	(-2.85, 0.54)	-0.21	-0.2	(-2.15, 1.73)
Combined	-1.48	-2.3	(-2.73, -0.23)	-1.50	-2.6	(-2.64, -0.35)	-0.36	-0.6	(-1.48, 0.77)	-0.09	-0.1	(-1.63, 1.45)

* Model 1 includes adjustment for year of birth (indicators for every year 1902-1947), state of birth (indicators for every state), and sex.

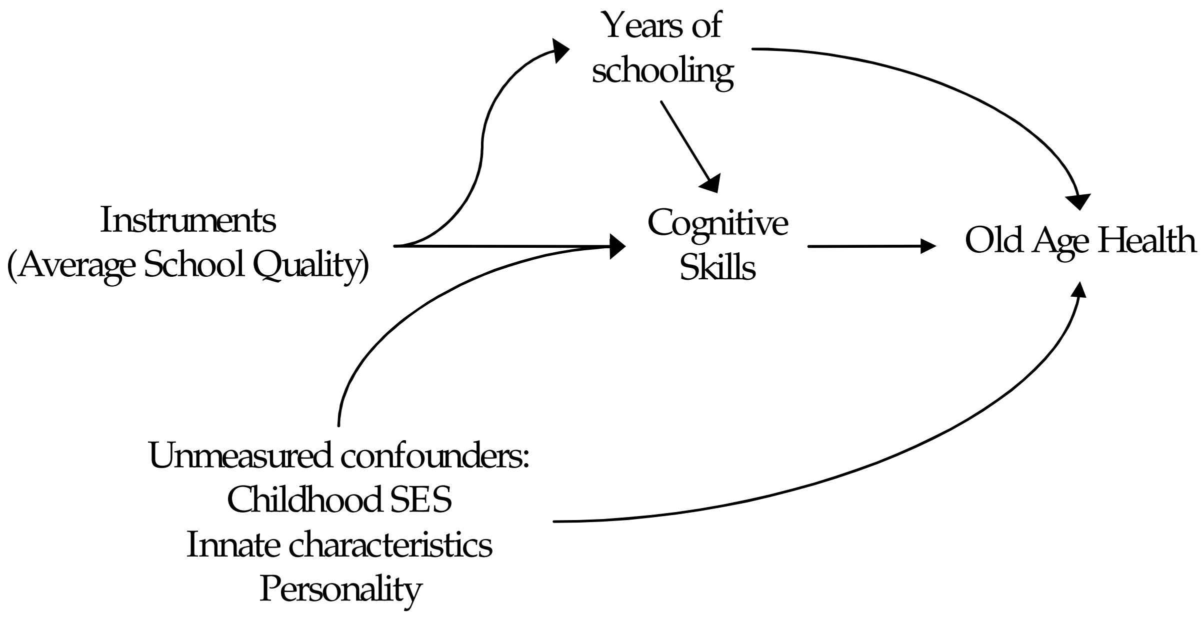
^ Model 2 includes Model 1 covariates plus years of education.

Model 3 includes Model 2 covariates plus: father's occupation, father's education, mother's education, state compulsory schooling laws and compulsory school to work laws, and state characteristics at age 6 (% urban, % black, % foreign-born) and age 14 (manufacturing jobs per capita and average wages per manufacturing job).

Instrumental Variables (IVs) include term length (in 10 day increments), teacher salary (in $10,000), and student-teacher ratio (in 10s).

FIGURE

Figure 1. Hypothesized Structural Relations Among School Quality, Cognitive Skills, and Outcomes



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