Live and Learn or Learn and Live:

Does Education Lead to Longer Lives?

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Abstract

This paper revisits the question of whether people live longer if they get more education or if people who get more education have unobservable traits and habits that cause them to live longer. We use better instruments and Panel Study of Income Dynamics data that follow individuals who were either alive in 1968 or who were born in a subsequent year. We confirm that there is a causal relationship – people who get more education live longer and we show that the relationship is stronger when one estimates it with individual data. We also propose and test a new hypothesis about the mechanism through which education matters. In particular, we suggest that education affects mortality across generations because educated parents are more likely to instill lasting habits in their children that contribute to long term health than less educated parents are. We specify and estimate an IV model to show that the data support this hypothesis.

Section 1 - Introduction

Numerous studies establish a large and positive correlation between health and education but few convincingly show whether the relationship is causal. Among the many factors correlated with health, social scientists and policy makers often focus on education because the production of human capital is publicly subsidized and because education is more strongly correlated with health than with either income or occupation (Grossman and Kaestner, 1997). While education certainly plays a role in determining both income and occupation, it is also independently correlated with health. Further, education is positively correlated with health measured in several ways: mortality rates, morbidity rates, self-evaluated health, and psychological health indicators (Grossman and Kaestner, 1997).

The literature has advanced three mechanisms that have emerged as leading candidates to explain the correlation between education and health. The first mechanism is that a person who gets more education learns to how to produce health more efficiently. The second is that if a person is healthy he pays less (in effort and time) to invest in schooling. The third explanation is that unobserved factors affect both schooling and health. However, these explanations are not mutually exclusive.

Researchers have long recognized that it is difficult to statistically identify which direction causality runs between education and health. This difficulty arises because people choose their ultimate educational attainment and they also choose whether or not to engage in activities that affect their health.

Because health is so strongly correlated with education, social scientists and policy makers are keenly interested in understanding whether the two are causally related, what direction the causality runs, and how strongly one predicts the other. The keen policy interest arises in part because governments separately target both health and education with so many public policies. Policy makers could better design policies related to both education and health if they had more and better evidence on whether education causes people to be in better health, better health causes people to get more education, or another “third-variable” explains both.

In what follows, we contribute theory and evidence to the literature on the causal effect of education on health. In Section 2 we review the literature that links education and health and the literature that empirically tries to identify whether there is a causal relationship in either direction. In Section 3 we contribute a new hypothesis to the theoretical literature. We describe a structure that plausibly links the education of multiple generations with the health of a given generation and that yields testable implications about that generation’s health over the life-course and at the end of life (which is our focus here). Section 4 describes our data, instruments, and highlights how they improve over existing instruments. Like much of the recent literature, we use compulsory schooling laws as instruments to predict educational attainment. However, our data are more comprehensive and measured with less error. In Section 5 we describe the methods we use and specify our models. We also discuss and specify a robustness check both to put our results into the context of other published findings and to demonstrate the advantages of the data we use. We present results in Section 6. In section 7 we present a robustness check by using the PSID to replicate previous results in the literature. In Section 7 we discuss our results and conclude with observations about directions future research might take.

Section 2 - Background

-Theoretical framework

To build a causal role for education, Grossman's (1972) model of health capital assumes that when a person gets more education his health production function changes and that he also may get more information about the relationship between inputs and outputs in the production of health. This first assumption says that education creates a new production function that yields more health for a given level of inputs than the old production function yielded. Stated differently, education teaches a person how to combine a given set of inputs differently so they produce more health. Grossman (1972) labels the effect of education as productive efficiency. For example, suppose two individuals both exercise on a treadmill for the same amount of time. A more educated individual might exercise more and less intensively during the workout in a way that produces better cardiovascular health. A study of Roman Catholic nuns found that although all of the sisters had essentially the same health inputs those with at least a bachelor's degree had half the mortality rate of nuns with less education (Snowdon, Ostwald, & Kane, 1989).

Second, because education is partly conveying information, when a person gets more education he might also learn new information about the relationship between inputs and health.[1] Thus, a more educated person will better allocate his fixed resources across health and non-health related inputs. Grossman (1972) labels the effect of education as allocative efficiency.

If a less educated person understands less about how health is affected when he consumes a good or set of goods (or engages in activities) then he will choose inputs inefficiently. For example, suppose two individuals spend the same amount on inputs used to produce health. Under the hypothesis of allocative efficiency, a more educated person will choose from the full (or a fuller) set of health inputs until the marginal product of the last unit of each input used is equal across all inputs. By contrast, a less educated person will simply ignore some inputs

[1] The failure to invest in education might occur because of capital market constraints.

(whose marginal product yields greater value than they cost). As a result, the value of the marginal product of all inputs he chooses (for the last unit of each chosen) will be greater than the marginal cost of another input that he ignored (i.e. he will choose an inefficient combination of health inputs). Kenkel (1991) establishes a positive relationship between schooling, health knowledge and health behavior. He shows that the more educated smoke less, drink to excess less, and exercise more.

The literature has also recognized that causality could also run from health to education – people in better health might produce more education. For example, it is clear that students miss fewer days of school when they are healthier. As a result, they probably produce human capital more efficiently at a point in time. And, because they complete a given level of schooling in shorter time, are more likely to progress to each subsequent level of education earlier in their life-cycle (Edwards and Grossman 1979).

Finally, the literature recognizes that there might be no causal relationship between education and health (in either direction). Schooling will be positively correlated with health if increases in unobserved variables (independently) increase both schooling and health. Fuchs (1982) studied whether time preference might be one such unobserved third variable.

-Empirical literature on causal nature of relationship

Empirical researchers have long tried to establish whether education causes health (or visa versa). Most studies have used one of two empirical strategies. Most studies use the method of instrumental variables (IV). A smaller and more recent set of studies uses regression discontinuities to identify whether changes in education cause health to change.

Both methods pose statistical challenges. The IV method requires that one find variables (instruments) that predict variation in educational attainment but that are also uncorrelated with health. This requirement, the “exclusion restriction,” represents a statistical challenge because it is difficult to find variables that both predict education and are arguably orthogonal to health. Some of the first IV studies relied on instruments that invoked questionable exclusion restrictions. Berger and Leigh (1989) predict education with state of birth, per capita income, and state education expenditures as instruments. More recent studies use instruments that are easier to defend such as the geographic availability of colleges (Currie and Moretti 2003), state high school graduation requirements and state policies used to award certificates of General Educational Development (GED) (Kenkel, Lillard, and Mathios 2006), and compulsory school entry laws (Adams 2002; Lleras-Muney 2005; Chevalier 2004; Black, Devereux, and Salvanes 2004; and Arendt 2005).[2]

More recently studies have employed regression discontinuity (RD) designs. The design of these studies exploits a threshold, usually created by policy, that puts observationally similar individuals into different educational categories because of small differences in a continuously distributed variable. For example, individuals graduate from high school if they score above some level on a high school graduation test and fail to graduate if they score less than the required amount. A set of studies uses school exit laws – laws that specify the age (or grade) an individual must complete before he may leave school – to compare health of the cohort that was first required to meet the (usually) higher age (grade) to the cohort just before the change was implemented (Grossman et al 2009; McCrary and Royer 2006). These studies face two main challenges. First, the estimated effects may be specific to the cohorts who were affected by the changed policy in a narrow window around the years just after the policy change. Second, when the RD design exploits some rule that assigns different levels of education based on a continuously distributed variable (such as a test score threshold), the estimated effects are local

[2] Angrist and Krueger (1991) first used compulsory schooling laws as instruments to study the returns to education.

treatment effects and may not generalize to other levels of education. For this reason, RD provides insights but is also limited.

Our focus on mortality and our use of variation in compulsory school entry laws is most similar to Lleras-Muney (2005). She uses US census data to estimate whether education affects adult mortality and uses variation across states and time in compulsory school entry laws as instruments. Since the U.S. census data do not contain information about the death of respondents, the author imputes a mortality rate. To do so she aggregates the data at the count of the population in a state that was born in a given year, creates a pseudo-panel of the state population of each cohort across decennial censuses, and assumes that, from one decennial census to the next, there is no net migration into a state for a given cohort. She then constructs the mortality rate as change in the population of a given birth cohort between censuses divided by the base year population. To predict education, Lleras-Muney uses variation across states and over time in the compulsory school entry laws that applied to each cohort. She finds that increases in education result in statistically and economically meaningful reductions in the probability of mortality in ten-year age groups. The evidence suggests that education does indeed cause mortality to decline but, because mortality rates are imputed, this conclusion is open to interpretation. The imputed mortality rate is potentially biased because it cannot differentiate whether an individual with less education is missing in later censuses because he died or because he is simply less likely to be sampled.

Our study improves advances the literature in several ways. We use instruments (on compulsory school entry laws) that are richer and better measured than those used in recent studies. We do not rely on imputed mortality but use data on individual mortality that is drawn from the National Death Index. Those data are linked to respondents to the Panel Study of

Income Dynamics and so we are able to model an individual's mortality risk over his whole life – controlling for *individual* covariates. These data also improve over existing literature because they span a broad range of birth cohorts. Our analysis also improves the first-stage prediction because we develop an algorithm that assigns each PSID respondent to a state of residence in each year of life. That algorithm reduces the measurement error in the assignment of the instrument we use to predict educational attainment. Finally, we also take advantage of the inter-generational design of the PSID to link parents with children. This survey design allows us to estimate models to test the new hypothesis we bring to the literature – that the causal effect of education runs across generations through the healthy habits educated parents engender in their children (regardless of the education the children have).

Section 3 Theoretical framework – an intergenerational linkage

In addition to testing the relationships implied by the theoretical framework of Grossman (1972), we propose and test a new theoretical mechanism. In particular, we adapt the Grossman model to incorporate what has long been recognized in the demographics literature – that observed old age morbidity and mortality is linked to variation in health conditions (and possibly inputs) a person experienced at birth (Elo and Preston 1992). The demographic literature observes that a person's risk of mortality in old age is greater if the infant mortality rate was higher in his region in the year he was born.

Using this stylized fact as our point of departure, we propose a health production function that includes very long lags between inputs and observed health. That is, observed health at any point in time, like good wine, flows from investments and inputs made many years in the past. To build a role for this relationship, we posit that a person not only chooses his health production functions when he chooses to acquire more education (Grossman's productive efficiency) but that health production functions are also endogenously produced by a person's parents. Parents invest time, effort, and resources to engender in their children habits that govern what and how much they eat, whether they regularly exercise, and whether and how much they consume goods like alcohol, recreational drugs, or tobacco. When successfully engendered, these habits cause a person to choose to consume inputs that produce more rather than less health. Of course, as every parent knows, children do not always take advantage of such good works. And so the habits parents engender are not deterministic.

In Grossman's framework, gross investment in health each period is a function of medical care and time, and is conditioned on the amount of education a person has. Formally he writes this as:

$$I_t = g\left(M_t, TH_t; E_t\right)$$

where M_t is medical care, TH_t is time spent in production of health, and E_t is a person's level of education at time t. We modify the above to explicitly include a role for habits that parents produce in their child. To keep things simple we assume that a parent engenders habits in her child using the parent's human capital or knowledge (E_{t-1}) and parental time ($ParTime_{t-1}$).

$$Habits_t = h\left(E_{t-1}, ParTime_{t-1}\right)$$

We also assume that a child's education varies directly with his parent's education. Incorporating both modifications into Grossman's health investment function yields:

$$I_t = g\left(M_t, TH_t; E_t(E_{t-1}), Habits_t(E_{t-1})\right)$$

Now the education of two generations affects health in a causal way. A person's own education affects his health as before. But here we allow a role for a parent's education to affect a child's health in two indirect ways. First, it affects the habits the parent has been able to engender in her child. Second, parental education affects the child's education. Taking the partial derivative of gross health investment with respect to parental education shows each of these effects:

$$\frac{\partial I_t}{\partial E_{t-1}} = \frac{\partial h}{\partial E_t}\frac{\partial E_t}{\partial E_{t-1}} + \frac{\partial h}{\partial Habits_t}\frac{\partial Habits_t}{\partial E_{t-1}}$$

Section 4 Data

We use data from the Panel Study of Income Dynamics and the PSID Death File 1968-2005. The PSID began in 1968 with a sample of 4,802 families containing 17,807 individual respondents. The survey was administered annually until 1997, when the survey became biannual. Respondents who moved away from one of the original families were followed to their new residence and their new coresidents were added to the survey, which expanded the survey to over 7,000 families in 2001. The 1968-2005 Death File contains information about all known deaths of PSID respondents. When possible, deceased PSID respondents were matched to the National Death Index archived by the National Center for Health Statistics to obtain up to five causes of each death. The 1968-2005 Death File contains date of death information for 4,807 decedents and the cause of death for 3,105 of these respondents.

We include controls for gender, race (black, Hispanic, and other), father's education (less than high school, high school graduate, some college, and college graduate or higher), and year of birth. We measure education by the highest grade level each respondent reports completing. Due to survey limitations, our measure of highest grade completed is top-coded at grade 17 (at least a year of postgraduate work). To ensure most respondents have finished their education we restrict our sample to individuals who responded to a wave of the survey at age 25 or older.

We use the age of death for decedent respondents and the age of last survey response for living respondents to create a variable indicating whether the respondent died between 1968 and 1978 (i.e. a measure of mortality over ten years). We exclude all individuals that last responded to the survey before 1978 and do not have a verified year of death, since we do not know if or when they died. Our main analysis is restricted 13,026 adult PSID respondents with mortality information. We also created five-year mortality rates from age 25 to age 75. In Table 2 we compare these rates to equivalent figures from the National Vital Statistics compiled by the Centers for Disease Control.

To effectively use compulsory schooling laws as instrumental variables one must identify what state a person lived in when she was affected by those laws. Angrist and Krueger (1991) and Lleras-Muney (2005) match the schooling laws of each individual's birth state in the year they are 14. This approach matches the wrong compulsory schooling laws to individuals who moved from one state to another state between the year they entered school and the year they turned 14. We more accurately match a person to the state in which she resided when the laws applied. Our place of residence matching algorithm uses all information available in the PSID. We describe our state residence assignment algorithm in Appendix 1

We draw our policy data on compulsory education laws from the Compulsory Schooling Law (CoSLAW) database. This database covers the complete history of compulsory schooling laws for each state. These data were compiled at Cornell University using one of the most complete collections of state statutes in the United States. While other compulsory schooling law databases exist, they are typically cross-sectional snapshots taken every 5 years or so. The CoSLAW data measure the exact dates specific laws changed, which specific sections changed, and the date the laws took effect. The CoSLAW data are more accurate relative to other compilations. The database includes the earliest age at which parents may enroll their children in public school, the age by which they must enroll their children in school, and the youngest age a student may drop out of school.

In previous studies, respondents are assigned all the compulsory schooling laws in effect in their state of birth the year they turn age 14. We improve on the instruments used in previous research by using a more accurate residence history, a more complete compulsory schooling law database, and better rules for matching these laws to individuals. We use our state residence history to match respondents to the compulsory schooling laws that potentially influenced their education decisions. We first merge our schooling law data by state and year to each respondent's state residence history. We then determine which minimum permitted entry age, maximum permitted entry age, and minimum permitted dropout age affected each respondent. Minimum permitted entry age and maximum permitted entry age laws typically specify a date (often the first day of classes) by which a child must be a certain age to be allowed or forced to attend school. We use the respondent's date of birth to determine the age he is on the date specified in law. We use this age to select the minimum permitted entry law in effect the first year a respondent is allowed to enter school and the maximum permitted entry law in effect the first year the respondent must enter school. Minimum permitted dropout age laws typically only specify an age at which a student may leave school. We assign individuals the minimum dropout age law from the year they were first old enough to leave school. Since some states did not have one or more of these age requirements in effect over our sample period, we created three dummy variables indicating individuals who were not subject to each of these compulsory laws.

We use two sets of instruments from the CoSLAW database. The first set (1) is the minimum entry age, the maximum entry age, the minimum exit age, and three dummy variables indicating whether states mandated each of these three ages. The second set (2) is the number of years of compulsory required by each state (the mandated exit age minus the mandated entry age) and a dummy variable indicating the absence of a state mandate (no mandated entry age, exit age, or both).

Section 5 Methods and Models

Grossman's model of health capital can be written in the general form:

$$\text{Heath Stock}_{it} = f(\text{Individual Characteristics}_{it}, \text{Education}_{i}, \text{Environmental Factors}_{it}, \varepsilon_{it})$$

(Grossman 1972). Grossman assumes that an individual dies when his health stock falls below a specified value. Of course we do not observe a person's health stock directly. However, because a person's health stock is negatively related to the probability of death we can use the standard latent variables approach. We assume health stock is a linear function of individual characteristics, education, and state factors. We estimate the linear probability model (OLS):

$$D_{it} = E_i\alpha + X_{it}\beta + W_{it}\gamma + \varepsilon_{it},$$

where D_{it} indicates the death of individual i at time t, E_i is individuals i's education measured in the highest grade she has completed, X_{it} is a vector of individual characteristics, W_{it} is a vector of state characteristics for individual i's state of birth at age 14, and ε_{it} is the error term.

We then estimate comparable IV two-stage least square models of the form:

$$D_{it} = E_i\alpha + X_{it}\beta + W_{it}\gamma + \varepsilon_{it}$$

$$E_{it} = C_i\delta + X_{it}\beta + W_{it}\gamma + \eta_{it},$$

where C_i is the set of compulsory schooling laws faced by individual i, which serve as an instrument for education, and η_{it} is the error term in the first stage. We include state of birth and year of birth fixed effects in all models. We estimate a linear probability model and two two-stage least square models. In both the linear probability model (OLS) and the two two-stage least square models the dependent variable is a dichotomous indicator of death between 1968 and 1978 conditional on surviving to 1968.

Section 6 - Results

We first present the results of our main analysis of 13,026 PSID respondents. We report our first stage estimates of our set of compulsory schooling instruments on education in Table 3. The first column shows the results of using the minimum entry age, the maximum entry age, the minimum exit age, and indicator dummies as instruments. Our estimates imply that requiring children to enter school a year later is associated with 0.51 years less of education on average. Requiring students to stay in school an extra year is associated with an additional 0.13 years of schooling. Allowing children to enter school a year earlier is associated with an additional 0.22 years of education. The absence of a maximum compulsory entry law is associated with an average 4.6 years of education. The absence of minimum entry law and a minimum exit law are both associated with more years of education. The joint significance of these instruments is high (F=14.58). The second column shows the first stage results using the number of compulsory schooling years and an indicator of a compulsory education length as instruments. The estimates imply that an extra year of compulsory education is associated with an additional 0.24 years of average education. Respondents in states with no compulsory education length have an average 0.98 additional years of education. This instrument set is also strong (F=34.80).

We report the second stage of these models in Table 4. The first column reports OLS estimates, assuming education is exogenous. In this sample we find that an extra year of education is associated with a 0.3 percentage point reduction in the probability of death in the next ten years from a baseline of 5 percent. The second column reports the results of a model that uses our broader set of CoSLAW variables (2) as instruments. The coefficient estimate of the effect of schooling on mortality is negative and significant; implying that an extra year of compulsory schooling reduces one's ten-year probability of death by 2.1 percentage points. The third column reports the results of the model using the number of years of compulsory school from CoSLAW and a compulsory indicator. The coefficient estimate of the effect of schooling on mortality is again negative and significant; implying an extra year of compulsory schooling causes a 3.0% drop in the probability of dying in the next ten years.

Section 7 - Robustness checks - comparison with other sample definitions

As a robustness check and comparison to previous findings we replicate Lleras-Muney (2005) from the PSID data. We draw a similar sample of white respondents born in the United States (Hawaii, Alaska, and Washington D.C. excluded) between 1900 and 1925 (N=2,836). We match state specific characteristics and compulsory schooling laws from Lleras-Muney (2005) to each individual's state of birth in the year they are 14. We also use mortality between 1968 and 1978 as our dependent variable. See Table 5 for summary statistics of this sample.

In our replication we use two additional sets of compulsory schooling instruments. The first two sets of instruments are our previously mentioned CoSLAW instrument sets. The third and fourth sets are identical to those from Lleras-Muney (2005). We obtained the publicly available compulsory schooling law data used in Lleras-Muney (2005) and assigned each respondent the laws in effect in their state of birth in the year they turned 14. The third set of instruments (3) is seven dummy variables indicating the number of years a child must attend school before the state allows him to work and a dummy variable indicating whether the state has school continuation laws. The number of school years required before working is calculated by subtracting the maximum school entry age from the minimum age a child could leave school and get a work permit. We omit the indicator of 0 years of required schooling and include indicator variables for 4, 5, 6, 7, 8, 9, and 10 years. The fourth set of instruments (4) is the number of years of compulsory schooling required by each state. Similar to set (2), this is calculated by subtracting the maximum school entry age from the minimum school exit age.

We present the first-stage estimates of the effect of compulsory schooling laws on years of completed education results in Table 6. The first column shows the results from using dummy variables indicating the number of years of education necessary to receive a work permit. The second column estimates the effect of the number of years of compulsory education (as specified and assigned in Lleras-Muney (2005)) on education. The third column shows the results from the broadest set of instruments: the minimum and maximum entry ages, the minimum exit age, and dummies indicating whether these policies existed. The fourth column shows the results from the estimation using the number of years of compulsory schooling (as specified in CoSLAW and assigned using our algorithm) and a dummy for no mandated years of education.

Our results show that all of these are weak instruments for education (F-statistic <10). A small sample size (N=3,116) may partially explain why all four instruments set are so weak. However, our specification of the number of compulsory years (4) has the highest F-statistic of joint significance. Our broad specification (3) has this highest partial R^2 of all instrument sets. These first stage results may suggest that the CoSLAW data coupled with our assignment algorithm are better instruments than those used in Lleras-Muney (2005).

We report the OLS and 2SLS second stage results of our Lleras-Muney (2005) replication in Table 7. The first column reports the estimates of an OLS model that assumes education is exogenous. We find that an extra year of education is related to a 0.3 percentage point reduction in the probability of dying in the next ten years. Columns 2-5 report the second stage 2SLS results using the four sets of instruments as previously specified. We find a marginally significant relationship between education and mortality using our first set of instruments (1). However, given the weak instruments, one should interpret these results cautiously. We find no other significant effects of education on 10-year mortality using any of these IV specifications.

Section 8 Discussion and Conclusion

We find a significant causal impact of compulsory education on mortality. Similar to Lleras-Muney (2005) our 2SLS coefficient estimates (-0.021 and -0.030) of the effect of education are larger than our OLS coefficient estimate (-0.0026). One possible explanation is that the IV models correct for measurement error in our education variable. However, there is little evidence that self-reported education measures have significant error. A second explanation is that while the OLS estimates reflect the average relationship between education and mortality, the IV estimates show the decrease in mortality of the group affected by compulsory schooling laws. If the decreased mortality effect of a year of school is greater for an individual for which the state schooling requirements bind than for an individual that would have attended college regardless of the law, we should expect to see large IV coefficients. Despite these explanations the coefficient estimates seem unusually large. Only 5 percent of our expanded sample dies in our 10-year period. However, our estimates suggest that an extra year of compulsory schooling reduces one's probability of death by 2-3 percentage points. This result suggests mortality rates vary immensely between groups with different education levels.

Our first stage results show that the CoSLAW data and our matching algorithm provide a strong instrument for education. We intend to utilize this instrument to further explore the relationship between education and health.

We find a negative and significant relationship between education and mortality in the OLS results of our robustness check. This result is unsurprising given the extensive literature on the correlation between health and education. However, the coefficient estimate for instrumented education was not significant in any of our IV models. Similarly, Lleras-Muney (2005) does not find a significant effect of education on mortality using individual level NHEFS data. Lleras-Muney (2005) only finds a significant effect by using aggregate Census data that summarizes data from hundreds of thousands of respondents.

Our results imply a causal impact of education on mortality. However, we cannot currently explain why education has this effect on mortality. Education is closely related to income and occupation, both of which can affect health. Education is also correlated with health-related behaviors such as diet and smoking. Education may also provide useful health information that affects how individuals produce health capital (Kenkel 1991). The effect of education may also be non-linear. There may a greater mortality reduction from completing high school than the reduction from completing first grade. In future research we intend to use the richness of the PSID and our detailed CoSLAW database to explore the mechanism by which education affects mortality.

Table 1 – Summary Statistics of Main Sample

	Mean	Std. Dev.
Died 1975-1979	0.05	
Female	0.52	
White	0.66	
Black	0.28	
Hispanic	0.04	
Other Race	0.02	
Year of Birth	1931.33	14.89
Highest Grade Completed	11.12	3.49
Minimum Entry Age	6.87	1.69
Minimum Exit Age	15.58	2.85
Minimum Allowed Entry Age	2.26	2.66
No Minimum Entry Law	0.05	
No Maximum Entry Law	0.03	
No Allowed Entry Law	0.57	
# Compulsory Years (CoSlaw)	8.36	2.21
No Compulsory Ed. Length	0.05	
N	13026	

Table 2 – Comparison of Mortality Rates from the PSID and the National Vital Statistics

	National Vital Statistics		
Mortality Rate	1949-1951*	2002*	PSID Sample
Age 25-30	0.008	0.005	0.003
Age 30-35	0.010	0.006	0.004
Age 35-40	0.014	0.008	0.006
Age 40-45	0.022	0.012	0.009
Age 45-50	0.034	0.018	0.017
Age 50-55	0.051	0.025	0.027
Age 55-60	0.076	0.038	0.042
Age 60-65	0.110	0.058	0.065
Age 65-70	0.156	0.088	0.103
Age 70-75	0.230	0.133	0.158
Age 75-80	0.332	0.201	0.258

*Source: CDC – National Center for Health Statistics: United States Life Tables 2002

Table 3 – Effect of Compulsory Schooling Laws on Years of Education

	(1) All Comp. Ages		(2) Comp. Years	
Minimum Entry Age	-0.509	***		
	(0.103)			
Minimum Exit Age	0.134	***		
	(0.051)			
Minimum Allowed Entry Age	0.221	***		
	(0.083)			
No Minimum Entry Law	-4.614	***		
	(0.792)			
No Minimum Exit Law	1.705	**		
	(0.855)			
No Minimum Allowed Entry Law	0.994	**		
	(0.474)			
# Compulsory Years Laws			0.240	***
			(0.042)	
No Compulsory Years			0.981	**
			(0.392)	
Female	-0.041		-0.040	
	(0.054)		(0.054)	
Black	-0.892	***	-0.897	***
	(0.068)		(0.068)	
Hispanic	-3.258	***	-3.268	***
	(0.170)		(0.170)	
Other Race	-0.714	***	-0.718	***
	(0.263)		(0.264)	
R2	0.246		0.245	
N	13026		13026	
F-Statistic on Instruments	14.58		34.80	
Partial R-Sq. on Instruments	0.0069		0.0052	

Note: both models contain both year of birth and state of birth fixed effects. Robust standard errors are in parentheses.

Table 4 –OLS and 2SLS Estimates of Mortality

	OLS		(1) All Comp. Ages		(2) Comp. Years	
Highest Grade Completed	-0.003	***	-0.021	**	-0.030	**
	(0.001)		(0.010)		(0.012)	
Female	-0.026	***	-0.027	***	-0.027	***
	(0.004)		(0.004)		(0.004)	
Black	0.012	**	-0.005		-0.013	
	(0.005)		(0.011)		(0.012)	
Hispanic						
Other Race	-0.012	*	-0.025	**	-0.032	**
	(0.006)		(0.011)		(0.013)	
R2	0.146					
N	13026		13026		13026	

Table 5 – Summary Statistics of Lleras-Muney (2005) Robustness Test Sample

	Mean	Std. Dev.
Died 1975-1979	0.09	
Female	0.55	
% Pop. Urban	51.46	20.39
% Pop. Foreign	11.91	8.54
% Pop. Black	8.61	11.29
% Emp. In Manufacturing	0.07	0.04
Annual Manufacturing Wage	7114.72	1367.21
Value of Farms Per Acre	552.45	315.72
Per capita Doctors	0.0013	0.0003
Per capita Education Exp.	98.30	40.14
School Buildings Per Sq. Mile	0.16	0.10
Highest Grade Completed	10.39	3.46
Work Age - Entry Age = 4	0.02	
Work Age - Entry Age = 5	0.01	
Work Age - Entry Age = 6	0.23	
Work Age - Entry Age = 7	0.60	
Work Age - Entry Age = 8	0.06	
Work Age - Entry Age = 9	0.05	
Work Age - Entry Age = 10	0.04	
Continuation School Required	0.69	
# Compulsory Years (L-M)	8.57	1.30
Minimum Entry Age	7.23	1.17
Minimum Exit Age	15.90	1.27
Minimum Allowed Entry Age	1.81	2.59
No Minimum Entry Law	0.02	
No Maximum Entry Law	0.00	
No Allowed Entry Law	0.66	
# Compulsory Years (CoSlaw)	8.40	1.71
No Compulsory Length	0.02	
N	3116	

Table 6 – Robustness Test: Effect of Compulsory Schooling Laws on Years of Education

	(1) All Comp. Ages	(2) Comp Yrs CoSLAW	(3) Entry-Work Age	(4) Comp Yrs (L-M)
Work Age - Entry Age = 4			0.103	
			(1.013)	
Work Age - Entry Age = 5			-0.034	
			(1.067)	
Work Age - Entry Age = 6			0.301	
			(0.955)	
Work Age - Entry Age = 7			0.268	
			(0.934)	
Work Age - Entry Age = 8			-0.310	
			(0.950)	
Work Age - Entry Age = 9			0.643	
			(1.018)	
Work Age - Entry Age = 10			-0.645	
			(1.121)	
Continuation School Required			0.183	
			(0.342)	
# Compulsory Years (L-M)				0.008
				(0.102)
Minimum Entry Age	-0.199			
	(0.236)			
Minimum Exit Age	-0.222			
	(0.155)			
Minimum Allowed Entry Age	0.053			
	(0.276)			
No Minimum Entry Law	-2.318			
	(1.863)			
No Maximum Entry Law	-5.497 **			
	(2.739)			
No Allowed Entry Law	0.145			
	(1.510)			
# Compulsory Years (CoSlaw)		-0.085		
		(0.116)		
No Compulsory Length		-1.749 *		
		(1.044)		
Female	0.052	0.051	0.060	0.054
	(0.122)	(0.122)	(0.122)	(0.122)
% Pop. Urban	-0.112 **	-0.119 **	-0.104 **	-0.115 **
	(0.048)	(0.048)	(0.048)	(0.047)
% Pop. Foreign	0.076	0.095	0.094	0.098
	(0.077)	(0.075)	(0.077)	(0.075)
% Pop. Black	0.162	0.153	0.161	0.118
	(0.116)	(0.114)	(0.122)	(0.115)
% Emp. In Manufacturing	11.100	10.959	5.592	9.020
	(9.866)	(9.819)	(10.473)	(9.783)
Annual Manufacturing Wage	0.000	0.000	0.000	0.000
	(0.000)	(0.000)	(0.000)	(0.000)
Value of Farms Per Acre	0.000	0.000	0.000	0.000
	(0.001)	(0.001)	(0.001)	(0.001)
Per capita Doctors	82.372	-42.252	-440.663	5.464
	(1123.386)	(1116.375)	(1210.561)	(1111.818)
Per capita Education Exp.	0.000	0.000	0.000	0.000
	(0.007)	(0.007)	(0.007)	(0.007)
School Buildings Per Sq. Mile	-2.684	-3.691	-4.816	-2.369
	(3.872)	(3.815)	(4.101)	(3.854)
R2	0.124	0.123	0.123	0.122
N	3116	3116	3116	3116
F-Statistic on Instruments	1.75	2.61	0.69	0.01
Partial R-Sq. on Instruments	0.0031	0.0015	0.0016	0.0000

Table 7 – Robustness Test: OLS and 2SLS Results

	OLS		(1) All Ages		(2) Comp Yrs (CoSLAW)		(3) Entry-Work Age		(4) Comp Yrs (L-M)	
Highest Grade Completed	-0.003	**	-0.047		-0.734		-0.079	*	-0.082	
	(0.002)		(0.041)		(8.931)		(0.042)		(0.067)	
Female	-0.074	***	-0.072	***	-0.034		-0.070	***	-0.070	***
	(0.010)		(0.012)		(0.492)		(0.014)		(0.014)	
% Pop. Urban	-0.001		-0.006		-0.084		-0.009		-0.010	
	(0.004)		(0.006)		(1.021)		(0.007)		(0.009)	
% Pop. Foreign	-0.011	*	-0.006		0.061		-0.003		-0.003	
	(0.005)		(0.007)		(0.877)		(0.009)		(0.010)	
% Pop. Black	0.018	*	0.023	*	0.103		0.026	*	0.027	*
	(0.009)		(0.012)		(1.045)		(0.013)		(0.015)	
% Emp. In Manufacturing	0.239		0.639		6.845		0.928		0.953	
	(0.798)		(0.944)		(80.878)		(1.122)		(1.221)	
Annual Manufacturing Wage	0.000		0.000		0.000		0.000		0.000	
	(0.000)		(0.000)		(0.000)		(0.000)		(0.000)	
Value of Farms Per Acre	0.000	***	0.000	***	0.000		0.000	**	0.000	**
	(0.000)		(0.000)		(0.001)		(0.000)		(0.000)	
Per capita Doctors	-49.494		-49.240		-45.296		-49.056		-49.040	
	(91.851)		(104.523)		(805.847)		(124.188)		(126.218)	
Per capita Education Exp.	-0.001		-0.001		-0.001		-0.001		-0.001	
	(0.001)		(0.001)		(0.005)		(0.001)		(0.001)	
School Buildings Per Sq. Mile	-0.289		-0.398		-2.087		-0.476		-0.483	
	(0.317)		(0.446)		(22.145)		(0.495)		(0.523)	
R2	0.094									
N	3116		3116		3116		3116		3116	

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Appendix 1 – State of Residence Matching Algorithm

Our state of residence matching algorithm uses all information available in the PSID. These data include an individual's current state of residence when interviewed, state of birth for some respondents, a self-identified state in which heads and wives grew up, and the PSID Relationship File. Each of these data identifies a state in a given year and establishes temporal gaps during which a person moved across state lines at least one time. To shrink the number of years in those gaps, we assign a respondent's state of birth to anyone who was in the household at the time of her birth. We assume parents and older siblings (if they were younger than 18 at the time of birth) were present in the household. With these data we shrink the gap over which known moves occurred and sometimes identify a previously unidentified state of residence. Our algorithm produces a residential history that runs from each person's year of birth to the last year she participated in a survey.

However gaps remain in this history and we develop another algorithm to fill in the remaining gaps. We adopt the following rules: 1) when a person lived in the same state on both sides of a gap in the series, we assume she did not move; 2) we use all information on dates of moves (these data identify any move - even when it is from one house to another in the same city) and assign a cross-state move to have occurred on the date that falls into a gap where we know a move occurred; 3) for the remaining cases we impute the date of the move. To impute the move date, we use age-specific probabilities of moving across state lines. We first generate the probability an individual moves to another state for every age using the 1960 U.S. Census. Then, for each individual, we identify the ages associated with the endpoints of the years over which a move occurred. Using these individual specific endpoints, we compute the probability a move occurred in that interval. Using the above age-specific probability distribution, we identify the age in the range that is the midpoint of the conditional probability distribution. That is, over the individual specific age range, we identify the midpoint age as that age where she is equally likely to have moved in the years before and in the years after that age. We assume each cross-state move occurs on this probabilistic-midpoint date. This procedure fills all remaining gaps.

By taking into account all available information in the PSID and by using external, nationally representative information to probabilistically identify dates of moves in remaining gaps, we generate a more accurate state of residence history. Because it uses more information, the assignment is less prone to bias that is introduced when one assumes a respondent always attends school in her state of birth.