The Effect of Early-Life Education on Later-Life Mortality

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Abstract

Many studies link cross-state policy variation in compulsory schooling laws to early-life educational attainment of individuals born in the early twentieth century U.S., thus providing a plausible way to investigate the causal impact of education on various lifetime outcomes. We use this strategy to estimate the effect of education on older-age mortality. Our key innovation is to combine U.S. Census data and the complete Vital Statistics records to form precise mortality estimates by sex, birth cohort, and birth state. In turn we find that virtually all of the variation in these mortality rates is captured by cohort effects and state effects alone, making it impossible to reliably tease out any additional impact due to changing educational attainment induced by state-level changes in compulsory schooling.

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1 Introduction

A large literature documents a strong correlation between education and health outcomes.[1] Many scholars have suggested that the relationship is likely causal. Cutler and Lleras-Muney (2010), for example, provide persuasive evidence about a number of pathways by which education might improve health: highly educated people have higher income and better insurance, higher cognitive ability, and stronger social networks, all of which contribute to better health behaviors.

Even so, it is difficult to cleanly demonstrate a causal relationship between education and mortality. One possible way to proceed—exemplified by the pioneering work of Lleras-Muney (2005)—is to exploit changes in compulsory schooling laws as a means of finding plausibly exogenous variation for the purpose of identifying the impact of education on subsequent mortality. Using an IV strategy based on changes in compulsory schooling laws in the early twentieth century U.S., Lleras-Muney (2005) estimates a large causal impact of education on mortality; she finds that among whites "one additional year of schooling reduces ten-year mortality rates by about 60 percent."

Several additional studies have followed Lleras-Muney's lead. Among recent examples, Albouy and Lequien (2009) use French data to examine two successive reforms in compulsory education laws. Analyses using regression discontinuity (RD) and instrumental variables (IV) lead the authors to conclude that "survival rates at 50 and 80 years old do not seem to be affected by years of schooling between 13 and 16." Similarly, Clark and Royer (forthcoming) examine the relationship between education and health in the United Kingdom, using 1947 and 1972 British compulsory school laws reforms. Also implementing RD and IV

[1] As Grossman (2000) states, many studies show that "years of formal schooling completed is the most important correlate of good health. This finding emerges whether health levels are measured by mortality rates, morbidity rates, self-evaluation of health status, or physiological indicators of health, and whether the units of observation are individuals or groups. The studies also suggest that schooling is a more important correlate of health than occupation or income, the two other components of socioeconomic status."

strategies, they find no sizable effect of education on mortality. Finally, Meghir, Palme, and Simeonova's (2012) study of compulsory education reform in Sweden shows that increased education appears to have contributed to limited gains in mortality among women from such preventable causes as lung cancer and liver cirrhosis, and some delay in mortality among men from their forties to their fifties. Overall, though, they find that "the reform had very small effects on overall health up to age 60" (which is the oldest age they could evaluate with their data).[2]

The literature cited above suggests that the causal effects of education on mortality rates are sufficiently small that they are difficult to estimate in many settings. What is required, first of all, is a policy shift substantial enough to affect meaningful changes in completed schooling among a relatively large number of people. Beyond that, though, there are two common problems for researchers: First, for some populations under study, mortality is sufficiently rare that it is difficult to tease out a small impact of education on mortality even with large samples and accurate data. Second, mortality is often measured with imprecision. The following examples are illustrative.

Consider Meghir, Palme, and Simeonova's (2012) study of mortality in Sweden. These authors evaluate a policy change—school reforms implemented at the municipal level in Sweden between 1949 and 1962—that induces a reasonably large response in completed education. But while they have impressive and accurate data, based on population registers of more than 745,000 men and 715,000 women, their data allow analysis only at ages 60 and younger, and in the Swedish setting this means their data include only approximately 35,000 deaths for men and 21,000 deaths among women.

At the opposite end of the spectrum, consider Lleras-Muney's (2005) analysis. Her work also is based on important changes in compulsory schooling laws (in her case, at the state

[2] Lochner (2011) provides thoughtful overview of recent papers on the causal impact of education on mortality and on health more generally.

level), which have meaningful impacts on the attainment of education.[3] By analyzing mortality from 1960 through 1980 for white men and women born in the U.S. between 1901 and 1925, she has millions of deaths to account for, over a wide span of ages. The problem is that her research design requires death rates for cohort $\times$ gender $\times$ state of birth cells, which she estimates using one percent public use samples from the Census. Thus, while the population under study is much larger than in Meghir, *et al.* (2012), the key variable of interest—the mortality rate—is measured with massively less precision.

The imprecision of the mortality estimates used as the dependent variable in Lleras-Muney (2005) means that inferences drawn may also be somewhat tenuous. Thus, for example, despite the fact that her estimated effect size is large, statistical power is sufficiently low that in the key regressions, results are not statistically significant for men and women separately.

Our innovation here is to incorporate far more data than has been employed in previous work. In particular, like Lleras-Muney, we examine cohorts of white men and women born 1901–1925 in the U.S., but our primary focus is on mortality during the years 1980 to 1990, during which we can construct mortality estimates using the complete death counts from Vital Statistics records, as those data include state of birth on an on-going basis from 1978 forward. Mortality is reasonably high for our study cohorts, who are aged approximately 55–89. Thus we use several million death records in constructing our mortality estimates. A key challenge is appropriately estimating denominators for calculating death *rates* at the cohort by state of birth level. We use a simple generalized method of moments (GMM) procedure that optimally combines Census and Vital Statistics data for this purpose.[4]

[3] For more on compulsory schooling laws in the U.S., see, for example, Angrist and Krueger (1991), Acemoglu and Angrist (2000), Goldin and Katz (2008), and Stephens and Yang (2012).

[4] Ideally we would like to use our GMM methodology to look at mortality at younger ages as well. While state of birth is not generally available for public records prior to 1978, we fortunately did find data for the years 1960–1963. Thus we can conduct our analyses for those years also (at which time individuals in our study cohorts are aged 35 to 62). The results are consistent with the analysis reported in this paper (although generally lacking power) and are available upon request.

Having estimated mortality rates quite precisely for the population under study, we proceed to estimate the impact of schooling on mortality using compulsory schooling law changes as instruments. Our most important finding is that when we use our highly-precise mortality estimates, a simple OLS regression, with state fixed effects and region by cohort fixed effects, gives an R^2 greater than 0.995 for both men and women.[5] Thus there is very little variation to be explained; only an extraordinary quasi-experiment can be expected to allow us to detect an effect of schooling changes on mortality against the existing trends.

Nonetheless we attempt IV analyses using conventional methods. Inferences are somewhat sensitive to specification, and also they depend on the way schooling laws are coded, i.e., whether we use variables constructed by Lleras-Muney (2005), Goldin and Katz (2008), or Stephens and Yang (2012). In all cases, however, the estimated coefficients are very close to zero. In short, we find no persuasive evidence of a substantial causal impact of schooling on mortality.

2 GMM Estimates of Mortality Using Census and Vital Statistics Data

In the empirical work that follows we are relying on year-to-year state-level educational policy variation as a means of generating variation in educational attainment, which we then hope to map into older-age mortality. So the first step is to find a way to estimate mortality by birth state and birth cohort. Also, because mortality rates differ substantially by sex, we need to estimate these separately for men and women. We restrict attention to whites.

As noted above, Lleras-Muney (2005) adopts a clever and straightforward approach. The idea is to calculate *ten-year mortality rates,* using U.S. Decennial Census samples. These data have all the required elements. For example, suppose we want to estimate mortality for

[5]Indeed, the R^2 statistics are well over 0.99 with the cohort effects and state effects alone.

men born in Georgia in 1912 (denoted here as group i) over the period 1980 through 1990. Let N_i^0 be the number of individuals in this cohort in 1980, a Census year, and N_i^1 be the comparable number from the following Decennial Census, taken in 1990. Then if we assume that there is no net migration of this group,

$$\frac{(N_i^0 - N_i^1)}{N_i^0} \tag{1}$$

is the ten-year mortality rate (from, roughly, age 68 to 78). We do not observe the required elements in (1); instead we can use as our estimator

$$\frac{(\omega^0 S_i^0 - \omega^1 S_i^1)}{\omega^0 S_i^0}, \tag{2}$$

where S_i^0 and S_i^1 are respective *sample* counts and ω^0 and ω^1 are the appropriate *inflation factors.* Thus, for example, if we are using a 1-in-20 sample in both periods, these factors are 20.

Estimator (2) is likely to be quite noisy when samples are small. Here we suggest that improvements are possible if we have a count from Vital Statistics data of the number of individuals in group i who have died between time 0 and time 1. Let D_i be such a count, and suppose that it has been accurately recorded. Then mortality is D_i/N_i^0. Our goal is to use available data efficiently for the purpose of estimating N_i^0, which we can then use as a base to form this mortality estimate. Black, *et al.* (2011) derive a simple generalized method of moments (GMM) procedure for handling the problem. We outline the basic idea here.

Given our data structure we have two moment restrictions:[6]

$$
\begin{aligned}
\mathrm{E}\left\{\omega^0 S_i^0 - N_i^0\right\} &= 0, \\
\mathrm{E}\{\omega^1 S_i^1 + D_i - N_i^0\} &= 0.
\end{aligned}
\tag{3}
$$

The hope is to find an estimator that fits these restrictions well.

A starting point is to minimize

$$
\begin{bmatrix} N_i^0 - \omega^0 S_i^0 & N_i^0 - \omega^1 S_i^1 - D_i \end{bmatrix}
\begin{bmatrix} 1 & 0 \\ 0 & 1 \end{bmatrix}
\begin{bmatrix} N_i^0 - \omega^0 S_i^0 \\ N_i^0 - \omega^1 S_i^1 - D_i \end{bmatrix},
\tag{4}
$$

which leads to a minimum distance estimator,

$$
\hat{N_i^0} = \frac{1}{2}\left(\omega^0 S_i^0\right) + \frac{1}{2}\left(\omega^1 S_i^1 + D_i\right).
\tag{5}
$$

This in turn can be used as a first step to form a GMM estimator. The next step is to undertake a minimization exercise, such as the one given in (4), but in which the matrix in the interior of (4) replaces the identity matrix with W^{-1}, the inverse of the covariance matrix from the vector of "moment restrictions," which in our case is

$$
\begin{aligned}
W &= \mathrm{E}\left\{ \begin{bmatrix} N_i^0 - \omega^0 S_i^0 \\ N_i^0 - \omega^1 S_i^1 - D_i \end{bmatrix} \begin{bmatrix} N_i^0 - \omega^0 S_i^0 & N_i^0 - \omega^1 S_i^1 - D_i \end{bmatrix} \right\} \\
&= \begin{bmatrix} \left(\omega^0\right)^2 S^0 p_i^0 (1 - p_i^0) & 0 \\ 0 & \left(\omega^1\right)^2 S^1 p_i^1 (1 - p_i^1) \end{bmatrix},
\end{aligned}
\tag{6}
$$

where p_i^0 and p_i^1 are, respectively, the probability in period 0 that an observation from the

[6] Our restrictions assume that D_i, the death counts for individuals in group i, have been accurately recorded in Vital Statistics records. If this number is thought to be recorded with error, and the error process can be modeled, we would instead have three moment restrictions.

complete sample S^0 is a member of group i, and the analogous probability in period 1.[7] The terms in W are easy to find as our particular problem entails draws from two independent binomial processes.[8] With a bit of algebra we can show that the resulting estimator is

$$\begin{aligned}\hat{\hat{N}}_i^0 = &\left[\frac{\left((\omega^0)^2 S^0 \hat{p}_i^0(1-\hat{p}_i^0)\right)^{-1}}{\left((\omega^0)^2 S^0 \hat{p}_i^0(1-\hat{p}_i^0)\right)^{-1} + \left((\omega^1)^2 S^1 \hat{p}_i^1(1-\hat{p}_i^1)\right)^{-1}}\right]\omega^0 S_i^0 \\ &+ \left[\frac{\left((\omega^1)^2 S^1 \hat{p}_i^1(1-\hat{p}_i^1)\right)^{-1}}{\left((\omega^0)^2 S^0 \hat{p}_i^0(1-\hat{p}_i^0)\right)^{-1} + \left((\omega^1)^2 S^1 \hat{p}_i^1(1-\hat{p}_i^1)\right)^{-1}}\right]\left(\omega^1 S_i^1 + D_i\right), \qquad (7)\end{aligned}$$

where, as a practical matter, p_i are replaced with $\hat{p}_i$—estimates derived in the first stage. As in (5), we are using a weighted sum of two consistent estimates of N_i^0 for our estimator, but in the GMM case we use asymptotically optimal weights.

Finally, having found the GMM estimate of N_i^0, our estimate of the mortality rate for group i from time 0 to time 1 is

$$d_i = \frac{D_i}{\hat{\hat{N}}_i^0}. \qquad (8)$$

We estimate these objects for several thousand birth state $\times$ birth cohort $\times$ sex cells. They form the basis for the analysis that follows. As we have noted, state of birth is a recorded element for Vital Statistics death records on an on-going basis only after 1978. So in the work that follows, we use estimates of ten-year mortality for 1980 to 1990.

Tables 1 is intended to give some sense of our GMM mortality estimates, which combine Census data and Vital Statistics records, and can be expected to compare to the Census-only estimates. First, from Panel A we notice that some of the cells we are working with are quite small—just a few hundred observations (estimated with far fewer than 100 observations,

[7]That is, $p_i^0 = \frac{N_i^0}{N^0}$ and $p_i^1 = \frac{N_i^1}{N^1}$. We of course don't directly observe p_i^0 or p_i^1, since N_i^0 and N_i^1 are unknown.

[8]Conceptually, the Census finds the entire population, and samples a fraction of these individuals for public use releases. Then, for example, in period 0 each of these individuals has a p_i^0 probability of belonging to group i and a $1-p_i^0$ probability of being in some other group. Estimates of the first moment have variance $S^0 p_i^0(1-p_i^0)$.

given our 5 percent sample). Thus we expect the estimates of mortality to be rather noisy using the Census-only approach, particularly for the small cells. Panel B shows that in general the correlation between the Census-only and GMM estimates is very high overall—greater than 0.95. As expected, however, the correlation is much lower for smaller cells, i.e., is only approximately 0.70 for cells below the 10th percentile. This means that the Census-only approach is introducing a great deal of noise, relative to the efficient GMM approach, for smaller cells.

Panel C gives summary statistics for our mortality rates using our GMM estimates for the denominator. For comparison sake we also give Census-only estimates (where possible). The standard deviation of the Census-only estimates is slightly higher than the estimates that combine Census and Vital Statistics data, as is expected given that the former estimates are noisier.

3 The Impact of Schooling on Older-Age Mortality

Our approach closely parallels Lleras-Muney's (2005). Her work uses the 1960, 1970, and 1980 1 percent U.S. Census samples to evaluate mortality of white individuals who were born in 1901–1925 in the U.S. In that work, state-level measures of the expansions of compulsory schooling laws over the 1915–1939 period are used as instruments for education. Her results indicate that an additional year of schooling lowers ten-year mortality rates by about 60 percent, but results were not statistically significant for men and women separately, and subsequent analysis has given researchers some pause about the robustness of that finding.[9] Our hope here is that with our GMM mortality estimates derived from Census and Vital Statistics data—estimates that are more precise than the Census-only estimates used in previous work—we can sharpen inference.

[9] See Lleras-Muney (2006) and Mazumder (2008, 2010). Lochner (2011) also expresses some skepticism about the magnitude of Lleras-Muney's (2005) findings.

3.1 Data

For our primary analysis, mortality estimates are based on data from the 1980 and 1990 5 percent public use samples of the U.S. Census and annual 1980–1990 detailed mortality files from the U.S. Vital Statistics.

We include cases where age, state of birth, and education are imputed by the Census. Like Lleras-Muney, we topcode the years of completed education at 18 years for consistency. We use the weighted samples with the personal weights assigned by the Census. Summary statistics for completed education are given in Table 2. Educational attainment is higher when measured in 1980 than in 1960, which is reported for comparison purposes. This might reflect that a larger fraction of the earliest cohorts have died, and that they generally had lower levels of education. It might also reflect imprecision in recall about one's educational attainment, and that such imprecision is increasing in age and is predominantly one-sided (as in Black, Sanders, and Taylor, 2003).

As for measures of the compulsory schooling laws, we use data provided by Lleras-Muney in our initial analysis. The same is true for data on other state-level characteristics.

3.2 Results

The basic research design is very simple. We begin by forming mortality estimates, as described above, for birth state $\times$ birth cohort for 48 states for the 1901–1925 cohorts of white men and women. The resulting estimates are used as the dependent variable in a regression that includes as an explanatory variable the average level of completed education for the corresponding group.

Let $\ln(d_{cs})$ be the log mortality rate for the period under study (the ten-year rate for 1980–1990) for cohort c (single-year birth cohorts 1901 through 1925) in state s, and let E_{cs}

be the corresponding average level of completed education for that same cell.[10] Then in a baseline differences-in-differences specification we would have

$$\ln (d_{cs}) = \omega_s + \delta_c + \alpha E_{cs} + \varepsilon_{cs}, \tag{9}$$

i.e., the log mortality rate for a demographic group is estimated to be a function of average education in a regression that includes cohort fixed effects and state fixed effects. We estimate such regressions using weighted least squares, where weights are constructed using cell sizes. Regressions are estimated separately for men and women. As for standard error calculations, we cluster at the birth cohort by state level.

As a starting point, to our baseline regression we also include state-level covariates used by Lleras-Muney (2005). These include variables (measured at age 14): percent urban population, percent foreign born, percent black, percent of people employed in manufacturing, annual manufacturing wage in real terms, average value of farm per acre, number of doctors per capita, educational expenditures per capita, and number of school buildings per square mile. In short, we begin by replicating the Lleras-Muney approach as closely as possible, so as to facilitate comparison. Results are given in Table 3.

In the first column of Table 3, we report results when we use the Census-only mortality estimates. We find that the IV estimate of the impact of education on later-life mortality is negative for both men and women, but is statistically significant only for women (and only at the 0.10 in this latter case). Of course, the Census-only estimates are not our preferred mortality estimates; we do not use them further in the analysis that follows.

Moving to the second column of Table 3, we report results when we use the mortality estimates derived by combining Census data and Vital Statistics data. There are two important differences we wish to highlight. First, the standard errors on our "years of education"

[10] Where d_{cs} is defined in (8) for the 1980–1990 data.

coefficients drop substantially—for men, from 0.126 to 0.019, and for women, from 0.210 to 0.023. Second, R^2 statistics increase substantially—for men, from 0.889 to 0.996, and for women, from 0.837 to 0.997. As we have only changed the dependent variable, these changes reflect only the noise reduction from our more precise measures of the relevant populations.

In fact, even if we omit the explanatory variable "years of education" from our regression, the R^2 statistics are extremely high, 0.996 for men and 0.997 for women. Put another way, virtually all of the variation in mortality can be accounted for by state effects and region by cohort effects. In this light, it is quite remarkable that in column (2) of Table 3 we also find a precisely estimated, reasonable effect of education on mortality. For both men and women, on the basis of our IV estimates, we would infer that one extra year of education reduces mortality by approximately five percent.[11]

The inclusion of cohort and state effects is natural in our regression. After all, mortality rates clearly increase with age, necessitating the cohort effects. Also, the instruments for completed education—compulsory schooling laws—are implemented at the state level, so inclusion of state effects is standard. There is a somewhat *ad hoc* feel to the inclusion of region $\times$ cohort effects, though it is hard to see why such variables would, on their own, cause problems beyond reducing the power of the analysis.[12]

In looking at the state-level covariates that Lleras-Muney used, we saw that many were clearly imputed. For instance, Figure 1 plots the measure of the percent black for the state of New York, which shows the characteristic pattern of linear interpolation. Treating the imputed data as if it was the actual data, as we did in Table 3, of course, provides overly optimistic standard errors. While in principal we could adjust the standard errors to reflect the imputation, we believe it more prudent simply to exclude these covariates from the

[11] In the first stage, education seems to be only weakly explained by the state-level compulsory school laws (as coded by Lleras-Muney, 2005). The F statistics are in the vicinity of 7.

[12] We also tried using Census division $\times$ cohort effects as an alternative, and the results are similar to those using the Census region $\times$ cohort fixed effects specification.

regression given the number of fixed effects in the model.

In Table 4, we explore the consequence of dropping the state-level demographic covariates and region $\times$ cohort variables. It is somewhat disconcerting to see that results for men, in Panel A, seem to be quite sensitive to our change in specification, in particular to omitting the region by cohort fixed effects. Indeed, our IV estimates changes sign (going from -0.06 to 0.08). For women, results don't change much when we drop state-level covariates and region by cohort fixed effects, but the estimated coefficients are no longer statistically significant.

With all this in mind, we turn to another important issue: As we have noted, many papers use compulsory schooling laws as instruments in various analyses. Over time, scholars have updated the coding of these laws. We are grateful to authors of two such studies—Goldin and Katz (2008) and Stephens and Yang (2012)—for providing us with their coding of compulsory schooling laws. It is important to know if results are sensitive to these updates. This is particularly true given results from Stephens and Yang (2012) indicating that estimates of the return to schooling vary substantially depending on the coding of compulsory schooling laws.

Table 5 summarizes correlations between the instruments and the observed years of education, and also the correlations of the instruments among themselves. The Stephens-Yang coding provides the highest correlation with years of education. Of course, Stephens and Yang (2012) have the benefit of being able to build on the work of the previous authors. Also, we note that in undertaking this analysis we are now using only cohorts 1905–1925, rather than starting with 1901, as Stephens-Yang only provide data for those cohorts.[13]

With three sets of instruments now in place, we now repeat our IV analyses. Table 6 reports results when we include region by cohort fixed effects. Results are disquieting. We find that some estimates are consistent with the expected negative impact—increased

[13]See also Stephens and Yang (2012), who show that in analysis extending over more cohorts their coding of schooling laws increases first-stage fit substantially, in comparison to previous efforts.

schooling reduces mortality—but that others are unexpectedly positive.[14] It is likely that part of the problem is due to weak instruments (see the relatively low F statistics).

It is tempting to conclude that at least we have shown that the causal impact of education on mortality is *small.* In our regressions, however, the fixed effects "explain" a great deal of the variation in morality, and some of that variation might be due to state- and regional-level differences in educational policies. We *can* say that the local average treatment effects (LATE)—due to changes in the compulsory schooling laws—are likely very small. Even our conclusion that the LATE is very small comes with important caveats. Our LATE estimates hinge crucially on an exclusion restriction with regard to compulsory schooling law changes, and it is easy to construct plausible scenarios in which violations might occur. For example, school reform within states might have been more likely during periods of prosperity, and that prosperity itself might have been associated with better health among children.

3.3 Are the Standard Errors too Small?

To this point in the paper, we have followed Lleras-Muney (2005) in using standard errors that are "clustered" at the birth state $\times$ cohort level, which is the unit of analysis. This is equivalent to using robust standard errors on observation at the birth state $\times$ cohort level. Of course, it is well understood that correlation of the errors may result in substantial understatement of the magnitude of the standard errors in a regression. The anomalous findings in Table 6—significant positive and negative impact of education on the mortality rate—could be the result of ignoring possible correlations within state.

To investigate this issue, in Table 7 we replicate the analysis in Table 6, but cluster our standard errors at the state of birth level. The standard errors increase in every case. With these larger standard errors, we cannot reject the hypothesis that the estimated coefficient

[14]Similarly, if we omit the region by cohort fixed effects we continue to find some statistically significant positive coefficients and some statistically significant negative coefficients. Inclusion of the state-level demographic covariates makes little difference.

is no different than zero in any specification (using the five percent confidence level).

Given these results, it is tempting to conclude that there is no causal impact of education on later-life mortality, but we think that conclusion premature. Rather, we interpret this null result as an indication about a fundamental problem with the research design—an issue to which we now turn.

3.4 The "Curse" of Better Data

Thus far we have seen modest negative and modest positive estimated impacts of education on later-life mortality. In this section, we argue that the reason for these peculiar results is a flawed research design: the use of state and cohort $\times$ region (or even just cohort) fixed effects removes such a massive portion of the variation both in the dependent and independent variable of interest that identification becomes problematic.

The most important contribution of our analysis, we believe, is to drastically improve the quality of the dependent variable—the mortality rate. We now show that with this improved measure of mortality, the research design used by Lleras-Muney removes essentially all of the variation in both the dependent variable and the instrumented independent variable. To see the issue at hand, consider the 2SLS equation we have used to estimate the impact of education on mortality in Tables 6 and 7. The 2SLS estimator is equivalent to running OLS on the equation

$$\ln(d_{cs}) = \omega_s + \delta_{cr} + \alpha \widehat{E}_{cs} + \epsilon_{cs} \tag{10}$$

where again d_{cs} is the state $\times$ cohort death rate, ω_s is the state fixed effect, δ_{cr} is the cohort $\times$ region fixed effect, ϵ_{cs} is the regression error, α is the parameter of interest, and $\widehat{E}_{cs}$ is predicted value of the education variable from the first-stage equations. Because we apply OLS to equation (10), we may take advantage of the partial regression theorem (Yule, 1907).

The partial regression theorem allows us to decompose $\ln(d_{cs})$ and $\widehat{E}_{cs}$ as

$$\ln(d_{cs}) = \widehat{\omega}_{s,d} + \widehat{\delta}_{cr,d} + d^y_{cs}, \text{ and} \tag{11a}$$

$$\widehat{E}_{cs} = \widehat{\omega}_{s,E} + \widehat{\delta}_{cr,E} + \widehat{E}^y_{cs}, \tag{11b}$$

where d^y_{cs} is what Black and Smith (2006) call the "Yulized residual" from the regression of $\ln(d_{cs})$ on the fixed effects, and $\widehat{E}^y_{cs}$ is the Yulized residual of the regression of $\widehat{E}_{cs}$ on the fixed effects. By the partial regression theorem, the application of OLS to the equation

$$d^y_{cs} = \alpha \widehat{E}^y_{cs} + e_{cs} \tag{12}$$

yields identical estimates of α to the 2SLS estimate from equation (10).

This remarkable theorem can be used to summarize the variation left for identification of α. In Table 8, we report the R^2 from equations (11a) and (11b). In terms of the logarithm of the death rates, the fixed effects explain all but 4 to 5 parts per thousand of the variation in the death rates. This feature of the data was hidden from Lleras-Muney (2005) and Mazumder (2008) because of their noisier measure of the death rate. The results for any of the three instrumented values of education are even more stark: less than one part per thousand of the variation in predicted education remains for identification of α after condition on the fixed effects.

In Table 9, we compare the correlations of the education variable and three instrument predicted values and then compare the Yulized residuals of each of these variables. Examining the relationship in levels is completely uninformative about the underlying variation that will be used for identification. What *is* important is correlation after conditioning on the fixed effects; here correlation among our various instrumented values of education is quite weak, considering that they attempt to measure precisely the same theoretical construct. Moreover, as emphasized by Black and Smith (2006), to the extent that any measurement error and noise in the predicted values of education are uncorrelated with the fixed effect, removing 999 parts in a thousand of the variation those predicted values dramatically increases the noise-to-signal measure in those variables. In short, then, it is hardly remarkable that we do not have credible findings to report about the impact of education on mortality with our empirical strategy.

Having said all that, it is useful to find that the LATE is likely very small. This result echoes findings in Clark and Royer's (forthcoming) study of mortality based on British compulsory school laws. It also calls into question Lleras-Muney's (2005) finding of a large impact of education on mortality.

4 Conclusion

Schooling appears to be a key correlate of older-age mortality. In this paper we seek to provide evidence about the extent to which this important relationship is causal. In particular, we exploit the fact that changes in state-level mandatory schooling laws had important effects on completed schooling for white men and women born in the early twentieth century. While changes in schooling laws—as coded in three important papers (Lleras-Muney, 2005; Goldin and Katz, 2008; and Stephens and Yang, 2012)—do influence educational attainment for individuals born in the early twentieth century U.S., we do not find that such variation appears to have a large effect on mortality. Despite the fact that we have assembled very precise mortality estimates for our regressions, our findings do not even allow us to make a definitive statement about the direction of the impact of education on mortality.

A great deal about links between education and mortality is not well understood. Our research underscores the difficulties researchers are likely to encounter if they focus on compulsory schooling law changes as a way of learning about these links. At least in the case of U.S. mortality, other research designs will be needed to make further headway.

Table 1: Data

Panel A: Estimated Cell Sizes (Sex by Age by State), 1980

percentile	min	10th	25th	50th	75th	90th	max
S_i^{1980}	240	1,970	4,500	10,130	19,370	30,990	96,060

Panel B: Correlations Between the Census-Only Mortality Estimates and GMM Estimates Using Census and Vital Statistics Data, 1980–1990, by 1980 Cell Size

S_i^{1980}	Correlation Between Estimates	n
$\leq$ 10th percentile	0.703	240
$\leq$ 25th percentile	0.854	603
$\leq$ 50th percentile	0.928	1200
$\leq$ 75th percentile	0.946	1800
$\leq$ 90th percentile	0.955	2160
All	0.959	2400

Panel C: Summary Statistics, Mortality Rates for 1901–1925 Cohorts, Estimates for 1980–1990

	Men			Women		
	Mean	Std. Dev.	n	Mean	Std. Dev.	n
d_{cs}, Census and Vital Statistics	0.340	0.158	1200	0.227	0.131	1200
$\ln(d_{cs})$, Census and Vital Statistics	-1.185	0.459	1200	-1.639	0.552	1200
d_{cs}, Census Data Only	0.368	0.177	1200	0.253	0.151	1200
$\ln(d_{cs})$, Census Data Only	-1.126	0.531	1197	-1.558	0.673	1184

Note: Panel A: Data are for white men and women, born 1901–1925, in the 5 percent public use sample of the 1980 Census.
Panel B: Data are from the 1980 and 1990 public use samples of the U.S. Census and Vital Statistics. In all cases correlations are weighted by S_i^{1980}. There are 2400 cells in total: 2 sexes by 25 cohorts (1901–1925) by 48 states.
Panel C: The estimates that use Census and Vital Statistics data are constructed using GMM (see text). Census-only estimates are as in Lleras-Muney (2005). For a small number of observations estimates are non-positive. These are dropped when we report log mortality rates. All statistics are weighted using personal weights provided by IPUMS.

Table 2: Summary Statistics, Years of Education, 1901–1925 Cohorts

A. Measured in 1960

	Men			Women		
	Mean	Std. Dev.	n	Mean	Std. Dev.	n
Years of Completed Education	10.47	1.10	1200	10.57	0.84	1199

B. Measured in 1980

	Men			Women		
	Mean	Std. Dev.	n	Mean	Std. Dev.	n
Years of Completed Education	10.94	1.11	1200	10.89	0.83	1200

Note: Census data, 1960 and 1980. Weighted statistics are calculated using U.S. Census samples with personal weights provided by IPUMS. Data for white men and women born in 1901–1925 in 48 U.S. states (n = 2400). For 1960, we miss white women born in 1904 in Nevada.

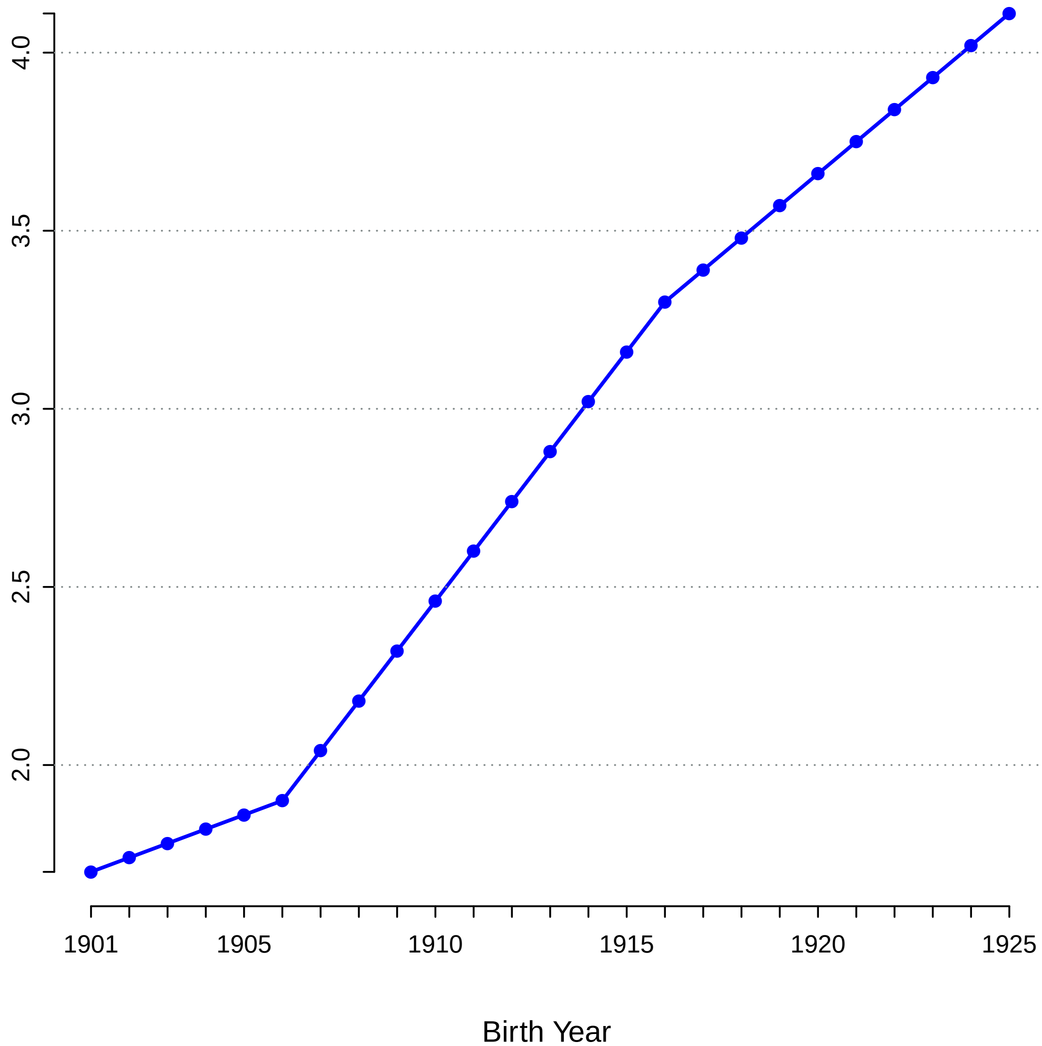
Table 3: The Effect of Education on Mortality with the Lleras-Muney Specification

A. Men		
	Census Only 1980–1990	Census/Vital Stat. 1980–1990
Years of education (OLS)	-0.026	-0.005
	(0.033)	(0.006)
Years of education (IV)	-0.065	-0.056***
	(0.126)	(0.019)
State fixed effects	Yes	Yes
Region by cohort fixed effects	Yes	Yes
Nine state-level covariates	Yes	Yes
R^2 for OLS regression	0.889	0.996
First-stage F-stat for IV	7.72***	7.75***
n	1,197	1,200

B. Women		
	Census Only 1980–1990	Census/Vital Stat. 1980–1990
Years of education (OLS)	-0.027	-0.012**
	(0.063)	(0.006)
Years of education (IV)	-0.373*	-0.052**
	(0.210)	(0.023)
State fixed effects	Yes	Yes
Region by cohort fixed effects	Yes	Yes
Nine state-level covariates	Yes	Yes
R^2 for OLS regression	0.837	0.997
First-stage F-stat for IV	7.13***	6.76***
n	1,184	1,200

Notes: Birth cohorts, 1901–1925. The dependent variable is the log mortality rate. Regressions use Lleras-Muney's instruments for compulsory schooling laws. Clustered standard errors in parentheses (clustered at the birth cohort by state level). Statistical significance: ***$p < 0.01$, **$p < 0.05$, *$p < 0.10$.

Figure 1: Percent Black by Birth Year for the State of New York



Source: Data from Lleras-Muney (2005).

Table 4: The Effect of Education on Mortality, 1980–1990, Alternative Specifications

A. Men	(1)	(2)	(3)
Years of education (OLS)	-0.005	-0.009	0.018***
	(0.006)	(0.006)	(0.006)
Years of education (IV)	-0.056***	-0.062***	0.076***
	(0.019)	(0.020)	(0.022)
Cohort fixed effects	No	No	Yes
State fixed effects	Yes	Yes	Yes
Region by cohort fixed effects	Yes	Yes	No
Nine state-level covariates	Yes	No	No
R^2 for OLS regression	0.996	0.996	0.994
First-stage F-stat for IV	7.75***	7.55***	8.10***
n	1,200	1,200	1,200

B. Women	(1)	(2)	(3)
Years of education (OLS)	-0.012**	-0.007	-0.006
	(0.006)	(0.006)	(0.006)
Years of education (IV)	-0.052**	-0.034	-0.029
	(0.023)	(0.024)	(0.019)
Cohort fixed effects	No	No	Yes
State fixed effects	Yes	Yes	Yes
Region by cohort fixed effects	Yes	Yes	No
Nine state-level covariates	Yes	No	No
R^2 for OLS regression	0.997	0.997	0.996
First-stage F-stat for IV	6.76***	6.40***	12.69***
n	1,200	1,200	1,200

Notes: Birth cohorts, 1901–1925. The dependent variable is the log of the 10-year mortality rate, 1980–1990, calculated using GMM. Regressions use Lleras-Muney's instrument. Clustered standard errors in parentheses (clustered at the birth cohort by state level). Statistical significance: $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.10$.

Table 5: Correlations Between Completed Education and Instruments for Compulsory Schooling Laws

A. Men

	Years of Education	Lleras-Muney	Goldin-Katz	Stephens-Yang
Years of Education	1.0000	—	—	—
Lleras-Muney	0.3473	1.0000	—	—
Goldin-Katz	0.3203	0.9084	1.0000	—
Stephens-Yang	0.5476	0.5998	0.6073	1.0000

B. Women

	Years of Education	Lleras-Muney	Goldin-Katz	Stephens-Yang
Years of Education	1.0000	—	—	—
Lleras-Muney	0.3498	1.0000	—	—
Goldin-Katz	0.3244	0.8991	1.0000	—
Stephens-Yang	0.5707	0.5981	0.6030	1.0000

Notes: Years of education are calculated using the 1980 Census data. Instruments are from Lleras-Muney (2005), Goldin and Katz (2008), and Stephens and Yang (2012). Correlations weighted by S_i^{1980} (cell size in the Census). $n = 2016$ (2 sexes, 21 cohorts, and 48 states).

Table 6: The Effect of Education on Mortality, IV Analyses Using Different Instruments

A. Men			
Instruments	Lleras-Muney	Goldin-Katz	Stephens-Yang
Years of education (IV)	-0.035*	-0.004	0.056**
	(0.019)	(0.031)	(0.027)
State fixed effects	Yes	Yes	Yes
Region by cohort fixed effects	Yes	Yes	Yes
First-stage F-statistic	15.22***	3.94***	3.48***
n	1,008	1,008	1,008
B. Women			
Instruments	Lleras-Muney	Goldin-Katz	Stephens-Yang
Years of education (IV)	-0.042	-0.070**	0.022
	(0.028)	(0.033)	(0.044)
State fixed effects	Yes	Yes	Yes
Region by cohort fixed effects	Yes	Yes	Yes
First-stage F-statistic	5.81***	4.91***	1.63*
n	1,008	1,008	1,008

Notes: Birth cohorts from 1905–1925. Regressions with log 10-year GMM death rate, 1980–1990 as the dependent variable. Clustered standard errors in parentheses (clustered at the birth cohort by state level). Statistical significance: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table 7: The Effect of Education on Mortality, IV Analyses Using Different Clustered Standard Errors

A. Men			
Instruments	Lleras-Muney	Goldin-Katz	Stephens-Yang
Years of education (IV)	-0.035	-0.004	0.056
	(0.025)	(0.040)	(0.039)
State fixed effects	Yes	Yes	Yes
Region by cohort fixed effects	Yes	Yes	Yes
First-stage F-statistic	20.53***	3.98***	2.64**
n	1,008	1,008	1,008
B. Women			
Instruments	Lleras-Muney	Goldin-Katz	Stephens-Yang
Years of education (IV)	-0.042	-0.070*	0.022
	(0.033)	(0.036)	(0.057)
State fixed effects	Yes	Yes	Yes
Region by cohort fixed effects	Yes	Yes	Yes
First-stage F-statistic	6.07***	2.91**	1.07
n	1,008	1,008	1,008

Notes: Birth cohorts from 1905–1925. Regressions with log 10-year GMM death rate, 1980–1990 as the dependent variable. Clustered standard errors in parentheses (clustered at the birth state level). Statistical significance: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table 8: The Impact of Fixed Effects and Other Controls

	Men	Women
State fixed effects	Yes	Yes
Region by cohort fixed effects	Yes	Yes
Nine state-level covariates	No	No
R^2 for death rate regression	0.995	0.996
R^2 for education measure	0.981	0.970
R^2 for predicted education (Lleras-Muney instruments)	0.999	0.998
R^2 for predicted education (Goldin-Katz instruments)	0.999	0.998
R^2 for predicted education (Stephens-Yang instruments)	0.999	0.999

Notes: Birth cohorts, 1905–1925. The death rate is 10-year GMM death rate, 1980–1990. The education measure uses 1980 data.

Table 9: Nominal versus Actual Correlations among Variables of Interest

A. Men				
Levels	Years of education	Lleras-Muney edu.	Goldin-Katz edu.	Stephens-Yang edu.
Years of education	1.0000	—	—	—
Lleras-Muney edu.	0.9910	1.0000	—	—
Goldin-Katz edu.	0.9907	0.9996	1.0000	—
Stephens-Yang edu.	0.9908	0.9993	0.9994	1.0000
Yulized residuals	Years of education	Lleras-Muney edu.	Goldin-Katz edu.	Stephens-Yang edu.
Years of education	1.0000	—	—	—
Lleras-Muney edu.	0.2480	1.0000	—	—
Goldin-Katz edu.	0.1815	0.5936	1.0000	—
Stephens-Yang edu.	0.2178	0.3450	0.2859	1.0000
B. Women				
Levels	Years of education	Lleras-Muney edu.	Goldin-Katz edu.	Stephens-Yang edu.
Years of education	1.0000	—	—	—
Lleras-Muney edu.	0.9860	1.0000	—	—
Goldin-Katz edu.	0.9859	0.9994	1.0000	—
Stephens-Yang edu.	0.9854	0.9991	0.9991	1.0000
Yulized residuals	Years of education	Lleras-Muney edu.	Goldin-Katz edu.	Stephens-Yang edu.
Years of education	1.0000	—	—	—
Lleras-Muney edu.	0.2526	1.0000	—	—
Goldin-Katz edu.	0.2364	0.6572	1.0000	—
Stephens-Yang edu.	0.1536	0.3456	0.3224	1.0000

Notes: Yulized regressions with state fixed effects and region by cohort fixed effects. Years of education are calculated using the 1980 Census data. Instruments are from Lleras-Muney (2005), Goldin and Katz (2008), and Stephens and Yang (2012). Correlations weighted by S_i^{1980}(cell size in the Census). $n = 2016$ (2 sexes, 21 cohorts, and 48 states).

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