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The Effect of Education on Adult Mortality and Health: Evidence from Britain†

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There is a strong, positive, and well-documented correlation between education and health outcomes. In this paper, we attempt to understand to what extent this relationship is causal. Our approach exploits two changes to British compulsory schooling laws that generated sharp across-cohort differences in educational attainment. Using regression discontinuity methods, we find the reforms did not affect health although the reforms impacted educational attainment and wages. Our results suggest caution as to the likely health returns to educational interventions focused on increasing educational attainment among those at risk of dropping out of high school, a target of recent health policy efforts. (*JEL* H52, I12, I21, I28)

The causal effect of education on health is a key parameter. It is central to models of the demand for health capital (Grossman 1972) and models of the influence of childhood development on adult outcomes (Heckman 2007; Conti, Heckman, and Urzua 2010). It is also relevant to macroeconomic growth models that incorporate mortality and human capital accumulation (Acemoglu and Johnson 2007; Cervellati and Sunde 2005; Galor and Weil 2000; Soares 2005). Moreover, if the health effects of education are large enough, then education policies might be powerful tools for improving health, especially in comparison to additional health care spending, the returns to which are uncertain (Weinstein and Skinner 2010).

Regardless of the education level, time period, and country being studied, a strong positive correlation emerges between education and health.[1] A recent quasi-experimental literature suggests that the causal effect of education on health is at least as large as the partial correlation (e.g., Lleras-Muney 2005). Some studies even suggest that these causal health effects could outweigh the well-documented

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[1] See Goldman (2001) and references cited therein for a good summary of this literature. There are many correlational studies that look at several different dimensions of the health-education gradient: over time (Pappas et al. 1993); over the life cycle (Beckett 2000; Lynch 2003); across sexes (Christenson and Johnson 1995; McDonough et al. 1999); and across races (Williams and Collins 1995). The first seminal study of this relationship was that of Kitagawa and Hauser (1968).

causal effect of education on earnings (Cutler and Lleras-Muney 2008; Oreopoulos and Salvanes 2011). However, despite this body of evidence, our knowledge of the causal effect of education on health is limited. First, as Fuchs (1982) has emphasized, even in the absence of any causal effect of education on health, a strong education-health correlation would be a natural by-product of the theory of human capital (Becker 1962). That is because the determinants of educational attainment (e.g., discount rates and ability) are also likely important determinants of health. Thus, establishing a causal relationship between education and health is extremely difficult. Second, while the quasi-experimental education-health literature directly addresses this concern, it has proven difficult to find instruments that meet the requirements for instrumental variable estimation.[2] For example, Lleras-Muney (2005), the most widely-cited study in this literature, examines the effects of schooling on mortality using as instruments US compulsory schooling changes across states. Yet Mazumder (2008) shows that her estimates are not robust to the inclusion of state-specific time trends.

In this paper, we present arguably the best evidence to date on the causal effects of education on health. We exploit quasi-experimental variation in education driven by two changes to British compulsory schooling laws. The first, introduced in 1947, increased the minimum school leaving age from 14 to 15. The second, introduced in 1972, increased it from 15 to 16. Four features of these changes allow us to break new ground in understanding the effects of education on health. First, these changes affected a large share of the relevant cohorts—50 percent for the first change and 25 percent for the second change. To put this in perspective, compulsory schooling changes in the United States affected 5 percent of the relevant cohorts (Lleras-Muney 2005).[3] Therefore, our estimates should be closer to the population-average effects of education on health (Oreopoulos 2006) than to the effects for smaller subpopulations that may be of limited interest (Heckman 2010). Moreover, because these compulsory schooling changes had large impacts on educational attainment, we avoid the weak instrument problems that often plague this literature. Second, these changes increased the wages of affected cohorts (Devereux and Hart 2010; Grenet 2013; Oreopoulos 2006), thereby ensuring the existence of an important channel through which education might impact health. Third, given that the relevant compulsory schooling changes depended on month-year of birth, we can identify their effects using a regression discontinuity approach (Lee and Lemieux 2010). Short of random assignment, this is arguably the most credible approach to identifying causal effects (DiNardo and Lee 2011). Our approach rests on especially weak identification assumptions since it effectively compares individuals born one month apart. These assumptions are even weaker than those used in previous analyses of these compulsory schooling changes (Devereux and Hart 2010; Harmon and Walker 1995; Oreopoulos 2006). This matters, since we show that estimates based on

[2] These quasi-experimental studies include: Adams (2002); Albouy and Lequien (2009); Arendt (2005); Berger and Leigh (1989); de Walque (2007); Grimard and Parent (2007); Kenkel, Lillard, and Mathios (2006); Lleras-Muney (2005); van Kippersluis, O'Donnell, and van Doorslaer (2011). They use a variety of instruments including changes in cohort size, parental education, local unemployment rates, and education exemptions for the Vietnam draft.

[3] The effects in other countries—e.g., Norway, Canada, and France—are similarly small (Albouy and Lequien 2009; Black, Devereux, and Salvanes 2008; Lleras-Muney 2005; Oreopoulos 2006).

stronger identification assumptions can generate misleading results. Fourth, we have many sources of health data, including administrative mortality records, census data on self-reported health and survey data on health behaviors, self-reported health and objective health measures. Our use of multiple outcomes and both school leaving changes ensures that our estimates are not particular to one outcome at one point in time. Furthermore, these data allow us to examine education effects on several channels through which health effects may operate. For example, we can examine education impacts on smoking, a health behavior that is strongly correlated with health outcomes and often found to be affected by education (Currie and Moretti 2003; de Walque 2007; Kenkel, Lillard, and Mathios 2006; Grimard and Parent 2007).

Our results suggest that despite their strong effects on educational attainment and wages, both of these compulsory schooling changes had, at best, very small impacts on health. For instance, we can reject that the 1947 change reduced monthly mortality by 0.4 percent. Along other health dimensions such smoking and self-reported health, our estimates also point to small effects. These estimates stand in sharp contrast to previous estimates of the effects of schooling on health,[4] and are even more surprising given that the education-health correlations among our studied population are stronger than those found for the United States (Cutler and Lleras-Muney 2008).

We argue that these results cannot be explained by specific features of the British setting, such as the existence of universal health insurance, the quality of the additional years of education generated by these compulsory schooling changes or the wider circumstances facing the cohort affected by the 1947 reform. The remaining possibility is that our regression discontinuity (RD) research design provides a unique opportunity to generate estimates free of the omitted variables bias problems of the prior literature. As such, we conclude that the causal effect of an additional year of secondary schooling is small. We stress that this does not imply that the health returns to education are negligible throughout the education distribution. In particular, our paper cannot speak to the health returns to college education. Nevertheless, given both the size of the affected population and the policy relevance of the education margin that we study, we think that our results have important implications. They suggest that economic models that assume a strong causal effect of education on health ought to be carefully reconsidered. They also cast doubt on the health returns to education policies, such as the US Healthy People 2010 goal of increasing the high school completion rate to 90 percent and the UK goal of further increasing the minimum age at which children can leave education (Seager 2009).

I. The Relationship between Education and Health

In this section we discuss the mechanisms that might generate a causal relationship between education and health. This sets the scene for our empirical analysis and informs the discussion of our estimates. These mechanisms can be categorized into the direct and indirect effects of education on health. Note that we restrict attention to the relationship between education and own health. There is a large related

[4] See, for instance, Adams (2002); Arendt (2005); Berger and Leigh (1989); de Walque (2007); Grimard and Parent (2007); Kenkel, Lillard, and Mathios (2006); Lleras-Muney (2005).

literature on the impact of education on infant health (e.g., Currie and Moretti 2003; McCrary and Royer 2011), but that is not the focus of this paper.

Education might have a direct effect on health and health behaviors via its influence on productive and allocative efficiency (Grossman 2005). That is, education may impart direct knowledge about health and health behaviors, thereby shifting the health production function. In addition, education could change the allocation of health inputs.

The proposed indirect effects are broad. The most frequently mentioned is the effect of education on labor market opportunities—higher rates of employment and increased earnings (Card 1999). The labor market returns could influence health by increasing the affordability of health-improving goods (e.g., gym membership), by increasing access to medical care (via increased income or employer-based health insurance) or by reducing income volatility and hence stress. But the health effects of income need not always be positive. For example, higher income could increase the consumption of cigarettes and alcohol. The complexity of the income-health relationship has been emphasized by Evans and Moore (2011, 2012) and Cutler and Lleras-Muney (2012). There are many other indirect mechanisms through which the education-health link could run. For example, more-educated people could work in safer environments (Cutler and Lleras-Muney 2006), they could be more patient and hence more likely to engage in healthier behaviors (Fuchs 1982; Becker and Mulligan 1997), they could have a higher rank in society (Rose and Marmot 1981) or they could be exposed to healthier peers (Duncan et al. 2005; Gaviria and Raphael 2001; Powell, Tauras, and Ross 2005; Trogdon, Nonnemaker, and Pais 2008). Note that empirical evidence distinguishing the relative importance of these mechanisms is limited, due partly to a lack of data and partly to the identification problems associated with estimating the causal effect of education on these various mechanisms.[5]

This discussion has several important implications for quasi-experimental studies of the relationship between education and health. First, it implies that manipulations involving different levels of education could generate different health effects. Hence, while our focus is on the health impacts of additional high school education, and while the additional education that we study was received by a large fraction (around 50 percent) of the affected population, the health impacts of college attendance could differ from these.[6] Second, because education could operate through many channels, and because quasi-experimental estimates will capture the combined effect of these, it is important where possible to investigate education effects on these various mechanisms. For example, a small estimated health effect could reflect positive effects on behaviors such as smoking and negative effects operating

[5] Using US data, Cutler and Lleras-Muney (2006) provide a thorough analysis of the cross-sectional relationship between education, health, and these intervening mechanisms. Although they acknowledge that it is hard to draw firm conclusions, they speculate that efficiency effects are especially important, perhaps even more important than income effects. In support of that conclusion, Lleras-Muney (2005) estimates a strong relationship between education and mortality even after controlling for income. In an analysis of British data, Cutler and Lleras-Muney (2010) find that at least in the cross section, cognitive ability and social integration can explain 44 percent and 15 percent, respectively of the effect of education on health behaviors. To the extent that there exists a causal relationship between education and health in Britain, this suggests that some of it might run through cognitive ability and social integration.

[6] For instance, Currie and Moretti (2003); de Walque (2007); and Grimard and Parent (2007) find that college attendance reduces smoking rates. If these effects operate through college impacts on peer composition, and if additional high school education has different impacts on peer composition, then the smoking effects of additional high school education could be different.

through higher income (Snyder and Evans 2006). Whereas the prior literature has focused on a limited number of intervening outcomes, an advantage of our study is that we focus on several of these. For example, we can assess whether there are education impacts on health behaviors by examining education effects on smoking.

II. Compulsory Schooling in Britain

The laws governing the length of compulsory education in Britain are national. This is an important contrast to US compulsory school laws, which are typically state-regulated. The British compulsory schooling laws specify the maximum age by which children must start school and the minimum age at which children can leave school. The maximum age by which children must start school is currently five.[7] We focus on two changes to the minimum age at which children can leave school. See Clark and Royer (2010) for a more detailed description of these compulsory schooling changes.

The 1944 Education Act raised the minimum school leaving age from 14 to 15 and a Ministerial order later in 1944 specified that it would be raised on April 1, 1947. The 1944 Act also gave the Minister of Education the power to raise the age to 16, when conditions allowed. The Minister did this in March 1972 (Statutory Instrument No. 444)[8] and the age was raised to 16 on September 1, 1972. The 1947 change meant that students could not leave school until part way through grade nine. The 1972 change meant they could not leave school until part way through grade ten.

It is useful to study two compulsory schooling changes, particularly because of the circumstances surrounding the first change (e.g., the Great Depression and World War II). While these events are unlikely to bias our estimates (cohorts on either side of the relevant threshold had similar levels of exposure), the second change occurred during less turbulent times, and hence can shed light on the generalizability of the estimates produced by the first. The obvious caveat is that there are other reasons why the two compulsory schooling changes might yield different estimates.

Figure 1 illustrates the impacts of these changes in the context of the wider trends in educational attainment in Britain.[9] This figure shows the fraction of individuals with completed years of education in each of four categories: 9 years or less, 10 years or less, 11 years or less and no university degree. This figure presents data at the quarter-of-birth level using Health Survey for England data, described in more detail below. The two vertical bars denote the first cohorts subject to the 1947 and 1972 compulsory schooling changes. These were, respectively, the April 1933 cohort (born 14 years before the change from age 14 to age 15) and September 1957 cohort (born 15 years before the change from age 15 to age 16). The 1947 change reduced the fraction that completed nine years or less by roughly one-half; the 1972 change decreased the fraction that completed ten years by roughly one quarter. As

[7] As discussed by Woodhead (1989), there has been a recent trend for students to start school before five, with practice varying across local authorities. Crawford, Dearden, and Meghir (2007) provide a thorough analysis of the impacts of British school start policies.

[8] www.legislation.gov.uk/uksi/1972/444/pdfs/uksi_19720444_en.pdf.

[9] Deaton and Paxson (2004) use this cross-cohort variation in schooling to understand the relationship between schooling and mortality in Britain. They estimate that an additional year of schooling reduces the odds of mortality by 2 percent.

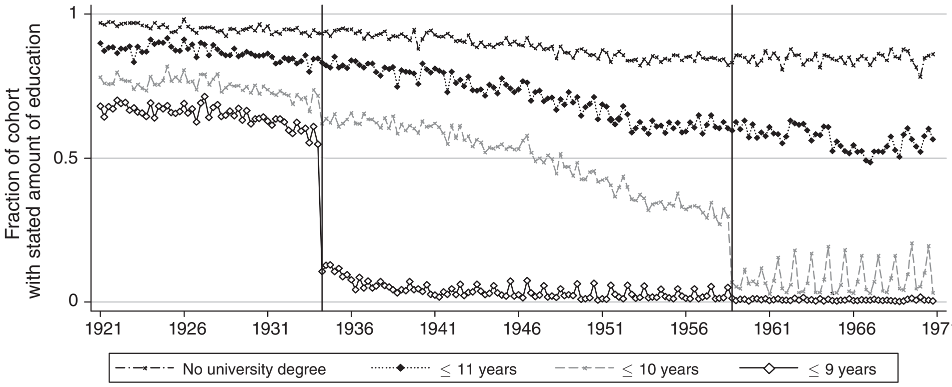

Figure 1. Years of Full-Time Education by Quarter of Birth

Notes: Sample includes individuals in the Health Survey for England: 1991–2004. Points represent means among people in each quarter-year of birth cell (all later graphs present data by month-year of birth). The vertical lines are cutoffs corresponding to the first cohorts subject to the new compulsory schooling laws. The first of these took effect on April 1, 1947 and the second took effect on September 1, 1972. Thus, since the two compulsory schooling reforms affected 14 (first reform) and 15 (second reform) year olds, the first cohorts impacted are those born in April 1933 for the first reform and September 1957 for the second reform.

shown below, finer month-year of birth comparisons and regression-based analyses point to similar conclusions.

Both compulsory schooling changes were secured by an extensive program of school building and the key elements of the school system did not change between 1947 and 1972. The extra year of schooling created by the 1972 change kept students in high school courses for another year and meant that many more finished these and received formal qualifications.[10] Ministry of Education reports and commentary from the time suggest that the extra year created by the 1947 change was used to introduce some students to more advanced materials and help other students master more basic material.[11] Both changes have been found to increase the earnings of affected cohorts. In particular, Harmon and Walker (1995); Oreopoulos (2006); and Devereux and Hart (2010) all find that the 1947 change had statistically significant effects on male earnings; only Devereux and Hart (2010) fail to find statistically significant returns for women. Our own analysis of the earnings effects of the 1947 change (based on our RD model using month-year of birth) confirms this picture (see online Appendix C for analysis). Grenet (2013) finds that the 1972 reform had statistically significant effects on the earnings of men and women. Our analysis of

[10] By the 1970s, high schools in England offered a series of two-year courses that ran through grades nine and ten and required students to sit formal examinations at the end of grade ten ("O" levels and Certificate of Secondary Education). Hence, by compelling students to stay in school until part way through grade ten, the 1972 change gave students an incentive to complete these courses.

[11] According to a 1947 Ministry of Education report, "the main value…of the lengthened school course lies in the fact that the schools will now be able to do more effectively in four years what they previously had to do in three. Even more important, it gives the schools a better chance of exercising a permanent influence for good on those who pass through them" (HMSO 1948, p.13). Based on various school district reports of the implementation of the new law, a leader commented in the Times Educational Supplement on April 5, 1947 that "Teacher supply, accommodation and curricula are not reckoned by those directly responsible to be the insuperable problems they seem to some despondent outside observers" (p.156).

the effects of the 1972 reform are consistent with these findings, although our estimates are based on smaller samples, and hence are less precise.

One difference between these two changes is that the month-year of birth threshold relevant to the 1972 change (September 1957) is also a threshold that matters for school start age and, therefore, age-in-grade. This is not true of the age threshold relevant to the 1947 change (April 1933). This means that the estimated effects associated with the second compulsory schooling change could, in principle, include a component driven by school start age or age-in-grade effects. There are two reasons why this is unlikely to be empirically important. First, our results are robust to the inclusion of month-of-birth dummies, which should capture these types of effects. Second, when we perform placebo analyses at other September age thresholds (e.g., September 1956 versus August 1956) we find no significant or systematic health differences between the cohorts born on either side of these.

III. Empirical Strategy

The sharp changes in educational attainment generated by these changes to compulsory schooling laws provide us with an excellent opportunity to analyze the health effects of education. Our analysis proceeds in three steps. First, we estimate the education effects of these compulsory law changes. Second, we estimate the mortality effects of these changes. Third, we estimate the health effects of these changes and the health effects of the additional years of completed education that they generated. In the remainder of this section, we discuss these steps in more detail.

A. *The Impacts of the Compulsory Schooling Changes on Education*

We use a regression discontinuity framework (Lee and Lemieux 2010; Imbens and Lemieux 2008) to estimate the effects of the compulsory law changes on educational attainment, our "first stage" relationship. Specifically, we estimate the following equation separately for each law change:

$$E_{ict} = \gamma_0 + \gamma_1 D_{ic} + f(R_{ic}) + \mathbf{X}'_{ict} \gamma_2 + u_{ict}, \tag{1}$$

where the dependent variable is a measure of educational attainment for individual i in birth cohort c at time t, D is a dummy variable indicating whether an individual belongs to a post-change cohort, R is an individual's birth cohort (measured in months) relative to the relevant cutoff (April 1933 for the 1947 change, September 1957 for the 1972 change) and $\mathbf{X}$ includes predetermined characteristics. R is positive for cohorts born after the reform and negative for cohorts born before. The predetermined characteristics include the year of the survey and month-of-birth dummies (i.e., dummies for January, February, and so on) to control for seasonality.[12] Hence while the month-of-birth patterns could differ across pre- and post-change cohorts (e.g., as seen later in the patterns for 1972 reform for summer-borns in Figure 2),

[12] For example, a prior literature (Buckles and Hungerman forthcoming) provides evidence of seasonality in motherhood selection. Since this seasonality appears to be quite systematic, much of this variation can be adequately controlled for via month-of-birth dummies.

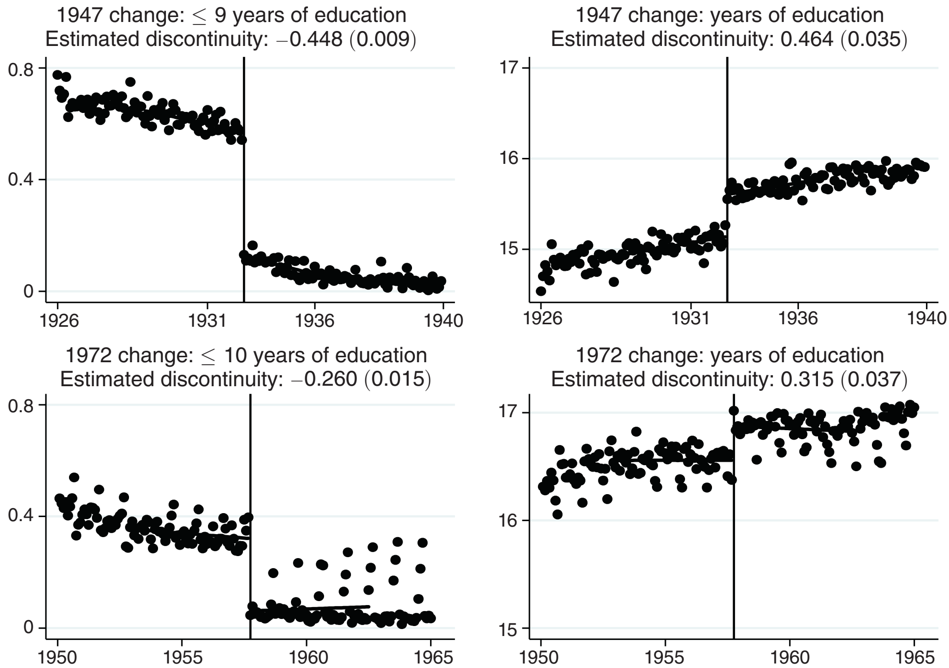

Figure 2. The Impact of the Compulsory Schooling Changes on Educational Attainment

Notes: Samples are based on pooled General Household Survey and Health Survey for England data. Points represent means among people in each month-year of birth cell. The estimated discontinuities are based on local linear regressions; standard errors are in parentheses. The fitted values of these local linear regressions are also plotted.

we present estimates for the 1972 reform that allow for different seasonal patterns (i.e., different month-of-birth fixed effects) on either side of the reform threshold. For the 1947 reform, we do not interact the month-of-birth dummies with the reform dummy as the patterns do not seem to change with the reform.

The inclusion of the vector **X** can, potentially, reduce the residual variation in the outcome, thereby improving the precision of the estimates. Its inclusion should not, however, affect our estimates of γ_1, since for individuals born near the cutoff (i.e., April 1933 or September 1957), the elements of **X** should be uncorrelated with being born on one side of the cutoff or the other. The function $f(\cdot)$ captures the underlying relationship between birth cohort and educational attainment. The term u is the error term representing unobservable factors affecting educational attainment. The parameter of ultimate interest is γ_1, the effect of the law changes on educational attainment.

B. *The Impacts of the Compulsory Schooling Changes on Mortality*

We take a two-step approach to analyzing the mortality effects of these changes.[13] In the first step, we exploit the panel nature of the data (i.e., we observe birth cohorts

[13] We thank a referee for suggesting this two-step approach.

over time) to perform a hazard analysis (Efron 1988). This is the approach adopted by Sullivan and von Wachter (2007) and Lee and McCrary (2009). Specifically, we estimate the following panel logit model of the probability of dying in month t conditional on being alive at the start of month t:

$$P(Y_{ict} = 1 \mid \theta_c, \delta_a) = F(\theta_c + \delta_a), \tag{2}$$

where Y equals 1 if individual i in cohort c dies in month t and 0 otherwise, F is the logit cumulative distribution function, the vector θ represents a full set of month-year of birth dummy variables (e.g., January 1933, February 1933, etc.), and the vector δ represents a full set of age-in-months dummy variables. Each person contributes n observations to this regression where n is the number of time periods from 1970 to 2007 during which person i is alive.[14] Our standard errors are clustered at the month-year of birth level.

The quantities of interest in this first step are month-year of birth fixed effects (i.e., $\widehat{\theta_c}$). These can be interpreted as the relative effect of being born in cohort c on the log-odds of dying (relative to the log-odds for the omitted cohort). In the second step of our two-step approach, we use these quantities as the dependent variable in a local linear regression model similar to that described by equation (1):

$$\hat{\theta}_c = \pi_0 + \pi_1 D_c + f(R_c) + \gamma_m + \epsilon_c, \tag{3}$$

where γ_m are month-of-birth dummies. Since the 1972 change is associated with different month-of-birth patterns in education, we interact month-of-birth dummies with the post-change cohort dummy (i.e., allow for different month-of-birth patterns among cohorts born before and after the relevant threshold).[15] We estimate these models using weighted least squares, where the weights are the inverse of the standard errors on the estimated step one coefficients. The estimated π_1 can now be interpreted as the discontinuous change in the log-odds of dying. Since the probability of dying is small, we will interpret this as the percentage change in the probability of dying for the just-affected cohort relative to the just-unaffected cohort.

We use equations similar to equation (3) to estimate the effects of the compulsory schooling changes on health as reported in the 2001 census. Since these data are aggregated to the month-year of birth cohort level (i.e., by sex, we have the fraction born in a particular month self-reporting being in bad health in the 2001 census), we use weighted least squares to estimate these models, where the weights are the cohort sizes.

We interpret our estimates of parameter π_1 in equation (3) as the reduced-form effects of the compulsory schooling changes on mortality and health reported in the census. Since neither dataset contains education information (the census collects qualification information only), we cannot use them to estimate the effects of education on these outcomes. Since we find that the compulsory schooling changes

[14] Note that while our mortality data are counts of deaths by month-year of birth (see below), we can use this information to construct the individual-level dataset used to estimate (2).

[15] Since the 1947 change is not associated with any change in month-of-birth patterns in education, and since the inclusion of such interactions reduces degrees of freedom, we do not include these interactions when analyzing the 1947 change.

have very small effects of these outcomes, this is not a major concern, particularly in view of the large first-stage effects that we estimate. As such, we report reduced-form estimates rather than split-sample instrumental variables estimates (Angrist and Krueger 1992).[16]

C. *The Impacts of Education on Other Health Outcomes*

We use a "fuzzy" regression discontinuity framework to estimate the impacts of the additional education induced by the compulsory law changes on other measures of health and health behaviors. In particular, we treat equation (1) as a first-stage equation and add an outcome equation describing the relationship between these outcomes and education:

$$H_{ict} = \beta_0 + \beta_1 E_{ic} + h(R_{ic}) + \mathbf{X}'_{ict}\beta_2 + w_{ict}, \tag{4}$$

where H is a health outcome (e.g., self-reported health) for individual i belonging to cohort c at time t. In this equation, the relationship between the dependent variable and birth cohort is captured by the function $h(\cdot)$. The parameter β_1 is the effect of an additional year of education. We combine equations (1) and (4) and estimate β_1 via two-stage least squares, using D_{ic} as the excluded instrument to derive an estimate of the health return to additional schooling.

D. *Estimation, Identification, and Interpretation*

As discussed by Lee and Lemieux (2010), there are two ways to estimate the discontinuity parameters in equations (1) and (3). First, one can capture the cohort trends using a parametric function (e.g., a quadratic polynomial in R) and use all of the available data to estimate these equations via ordinary least squares, typically referred to as the global polynomial approach. Second, one can capture the cohort trends via a linear function of R (potentially fully interacted with D—such that the cohort trends can have different slopes on either side of the threshold) and estimate the equation over a narrower range of data, typically referred to as the local linear approach.

The local linear approach can be viewed as generating estimates that are more local to the threshold (Lee and Lemieux 2010; Imbens and Lemieux 2008). It will be especially useful if a parametric function estimated over all of the data cannot adequately capture the relationship between birth cohort and completed education. We adopt the local linear approach and choose the bandwidth using the cross-validation procedure suggested by Imbens and Lemieux (2008).[17] To demonstrate

[16] Note that even if the mortality data had education information, our instrumental variables estimates of the effect of education on mortality would possibly be inconsistent. Specifically, our first-stage estimates would be based on a selected sample—the population of those not dying. In order to calculate valid instrumental variables estimates of the effect of education on mortality, one needs to estimate the first-stage regressions before cohorts have experienced any health effects of the additional education. These selection biases have been largely ignored in prior instrumental variables calculations. They do not affect our analysis as we find no effects of education on mortality.

[17] Note we use the prediction errors within two years of the relevant threshold to calculate the mean squared error for a given bandwidth.

that our estimates are not sensitive to the chosen bandwidth, we produce estimates for other bandwidths in online Appendix Table A1.[18]

We estimate equation (4) using two-stage least squares (2SLS). Since our 2SLS estimator is the ratio of the reduced-form estimate of the effect of the policy change on an outcome of interest and the first-stage estimate of the effect of the policy change on education, we use the optimal bandwidths for the reduced-form estimate and the first-stage estimate, obtained separately. Standard errors are then calculated via the delta method.

Whenever we estimate individual-level models (equations (1), (2), and (4)), we calculate standard errors clustered at the month-year of birth level (i.e., the level of the running variable). When we estimate cohort-level models (equation (3)), we report robust standard errors.

The key assumption underlying these procedures is that the conditional expectations of the potential outcomes (completed education, mortality, self-reported health) with respect to birth cohort are smooth through the $R = 0$ threshold. In that case, we can attribute any discontinuities at these thresholds to the causal effects of the compulsory schooling changes. Although we cannot test this assumption directly, an implication is that there should be no discontinuities in predetermined outcomes. Since education can impact many outcomes and we mainly observe the impacted cohorts as adults, we focus on predetermined characteristics at birth: cohort size and the fraction of stillborn births. These are outcomes for which we have data at the month-year of birth level and for which we can generate precise estimates. In online Appendix Figure A1, we present averages for these predetermined characteristics by year-month of birth. These figures include local linear fits to these averages using 24 months of data on either side of the law change. There are no obvious discontinuities in any of these outcomes. This is not surprising, since these laws were changed after the affected children were born, limiting the scope for behavior that would generate discontinuities.

We interpret our estimates of parameter β_1 in equation (4) as local average treatment effects, the effects of the additional education for those who would not have received this education in the absence of the compulsory schooling changes (Imbens and Angrist 1994). Since these changes appear to keep students in education for only one additional year, we interpret these estimates as the effects of an extra year of education for students compelled to stay an extra year. These effects may be very different from the effects of an additional year of education at other points of the education distribution. They may also be very different from the effects of an additional year of education for students that would have remained in school regardless of the law changes. As argued by Oreopoulos (2006), however, since these changes affected large fractions of the relevant birth cohorts, these estimates may be close to the average causal effect of this extra year.

[18] Note for the 1972 reform, we exclude the estimates using a bandwidth of 12 months because the associated regressions include calendar month-of-birth effects that are allowed to differ on either side of the threshold.

Table 1—Description of Datasets Used

Data source	Years	Sample	Primary variables	Notes
Office of National Statistics Death Records	1970–2007	All deaths in England and Wales	Death counts	100 percent counts by month-year of birth × month of death × sex
Census data	2001	Resident population in England and Wales who were born in England or Wales	Self-reported health, population counts	100 percent counts by month-year of birth
Health Survey for England	1991–2004	Sample of individuals in England in private households	Age left full-time education, self-reported health, health behaviors (smoking, drinking), clinical health measures	Individual-level survey data
General Household Survey	1986–1996	Sample of individuals living in private households in Great Britain	Age at left full-time education, self-reported health, health behaviors	Individual-level survey data

IV. Data

In this section we describe the datasets used in our analyses. We summarize these datasets in Table 1. We begin by discussing the data used in the mortality analysis. We then discuss the data used in the first-stage analysis and the health analysis.

Data Used in the Mortality Analyses.—To estimate the mortality effects of the compulsory schooling law changes, we use mortality data obtained from the Office for National Statistics. These include counts of deaths among all residents of England and Wales by month-year of birth, month of death, and sex from January 1970 to December 2007. As such, we can observe mortality among cohorts affected by the first change between the ages of 36 and 74 and mortality among cohorts affected by the second change between the ages of 12 and 50.[19]

For each birth cohort, our analysis requires a count of the population at risk of dying in each month. For months before April 1991, we infer this by taking the population of those born in England and Wales and resident in England and Wales at the time of the 1991 census (the earliest available, enumerated April 1991) and adding deaths occurring between the month of interest and April 1991. For months after April 1991, we subtract deaths occurring between April 1991 and the month of interest.

There are two reasons why mortality measured with these data might differ from true mortality. First, our measure of the April 1991 population will differ from the true population born in England and Wales and alive in April 1991. That is because there may be under-counting in the census, errors in reported age and migration out of England and Wales. Second, our measure of deaths in a particular month will also differ from the true number of those born in England and Wales and dying in that month. That is because of errors in recorded age at death (i.e., on the death certificate),

[19] While ideally we would observe mortality beyond age 74, we note that the prior literature has found that the largest effects of education on mortality occur between the ages of 35 and 64 (Lleras-Muney 2005).

because our death counts exclude deaths to individuals who were born in England and Wales but died outside of England and Wales and because they include deaths to individuals who were born outside of England and Wales but died in England and Wales.[20] We discuss the implications of these measurement issues in more detail in online Appendix B. We summarize the main points of this discussion below.

The nonlinear nature of our mortality analyses means that we cannot provide analytical results for the possible biases generated by these measurement issues. Instead, we use simulation methods to assess the likely size of any biases. To do this, we proceed as follows. First, we assume a true reduced-form mortality effect of the ROSLA; our simulations consider a range of values for this parameter. We use these assumed true effects to simulate the probability of death at each age. Then, using counts of those born in England and Wales in each month from given data along with these probabilities, we generate "true" mortality data (i.e., mortality data absent our measurement issues). We then adjust these data to account for the various sources of measurement error (e.g., deleting some observations to mimic census under-counting of a certain stated percent). This gives us "measured" mortality data. We then compare our estimates using the generated "true" mortality data and the generated mismeasured mortality data. As described in online Appendix B, these estimates are similar. Moreover, to the extent that there are differences, our estimated effects using the mismeasured mortality data point to even larger protective effects of education. For example, when we assume a true ROSLA mortality-reducing effect of 0.20 (a 20 percent reduction in mortality), the estimates obtained under our most pessimistic assumptions average −0.23.[21]

It is not surprising that the simulations generate these types of results. First, most sources of measurement error are quantitatively small, particularly the census under-counting and the age misreporting in the census and death records (less than 2 percent in each case). Second, we would not expect emigration to bias our estimates, since emigrants will contribute to neither the death counts nor the population counts. Only if the ROSLA affected the composition of emigrants would we expect emigration to drive a wedge between the true effect and our estimated effect. Although it is difficult to determine whether this was the case, in an attempt to shed some light on this possibility we estimate ROSLA impacts on the emigration rate. Although the ROSLA could impact emigrant composition without impacting the emigration rate, the small estimates we obtain for ROSLA effects on the emigration rate are at least consistent with small, if any, impacts on composition (see online Appendix D for more details).

Third, the one factor that our simulations suggest will drive a wedge between our estimates and any true effect—the immigration rate—is around 0.1 (1991 census data). An analytical analysis based on a modified version of the two-step hazard approach suggests an upper bound on the overall bias that is roughly proportional to this immigrant rate (see online Appendix B for more details). As such, we would not expect our estimates to be biased by more than 10–15 percent. Consistent with this expectation, the largest estimate produced by our simulations assuming a true effect of −0.20 is −0.23.

[20] There is virtually no undercounting in the death records (Charlton and Murphy 1997).

[21] This is the average effect across 100 replications. The size of this effect is in the lower range of the estimates produced by Lleras-Muney (2005).

In addition to these measurement error issues, another limitation of these mortality data is that they start in 1970. Our analysis of the 1947 change will, therefore, miss mortality between 1947 and 1970—roughly when these cohorts were aged 15 to 40. While this is not ideal, there are three reasons why we believe that this limitation does not lessen the value of our analysis. First, this problem does not affect our analysis of the 1972 change. That is, for the cohorts affected by the 1972 compulsory school change, we can observe mortality from 1972 onwards. Second, this problem is common to other analyses of the mortality effects of education, most of which focus on interventions that occurred many years ago (to analyze mortality at later ages) but must use mortality data that are only available for more recent years (e.g., Albouy and Lequien 2009; Lleras-Muney 2005). Third, we can test for selective mortality. We do this in two ways. First, we estimate effects on survival until 1970. To measure survival to 1970 we use birth counts, 1991 census data and deaths recorded between 1970 and 1991. Second, we estimate effects on survival until 1991. While this will identify a combination of pre- and post-1970 mortality effects, survival to 1991 is easily measured using birth counts and 1991 census data.

Neither of these tests is suggestive of selective mortality. For example, for men, the effect of the compulsory schooling change on the probability of survival to 1970 is small (0.01 off of a base survival probability of 0.82) and statistically insignificant (standard error of 0.01). The effect on survival to 1991 is also small (0.01 off of a base of 0.78) and statistically insignificant (standard error of 0.01), consistent with no effects on either pre- or post-1970 mortality. The estimates are similar for women.[22]

Data for First-Stage and Health Analyses.—Our first-stage and health analyses use Health Survey for England (HSE) data. Begun in 1991, the HSE is an annual survey that combines a questionnaire-based component with objective information (such as measured blood pressure) obtained from a nurse visit. We pool all waves of these data from 1991 through 2004 to give us large samples of roughly 20,000 adults born in a 15-year interval around each compulsory school change.

The HSE contains basic demographic information such as gender and age, information on completed education and detailed information on health and health behaviors. We use age left full-time education as our education outcome. This is not the same as completed years of education, but for our purposes we can view these as equivalent (see online Appendix B for a discussion of this point).[23] We use two types of health outcomes: measures of health, including both subjective and objective measures, and measures of health behaviors, including self-reports of smoking, drinking, diet, and exercise. Some of these health measures are strong predictors of mortality (Idler and Benyamini 1997), and some of these health behaviors (e.g., smoking) are known causes of morbidity and mortality. Some may worry that

[22] For example, for survival to 1991, the estimate is 0.01, off of a base probability of 0.73 (standard error of 0.01).

[23] Because we estimate these models using data obtained over the period 1991–2004, any mortality effects of these laws could, potentially, bias these first-stage estimates. This concern applies to our estimates of both the 1947 and 1972 compulsory school changes. Since our mortality analysis suggests that these changes had, at best, small mortality effects, we would not expect any education-based selection into the HSE data. Indeed, we find that the fraction of the birth cohort observed in the HSE is smooth through the April 1, 1933 and the September 1, 1957 thresholds (not reported).

reporting errors in the subjective measures are related to education. However, Johnston, Propper, and Shields (2009) use the Health Survey for England to compare various gradients (including education) in subjective and objective measures of the same condition (hypertension). They find no statistically significant differences in rates of false reporting across education levels.

In some of our analyses and all of our first-stage analyses, we combine data from the HSE with data from the General Household Survey (GHS). The GHS, also used by Oreopoulos (2006), is an annual survey of over 13,000 households in Great Britain. Among other things, the GHS includes information on demographics (including month-year of birth from 1986 to 1996, the survey waves that we use), education, and health. Since the age left full-time education survey question is similar across the GHS and HSE, we combine these two datasets to estimate the first-stage relationship between the compulsory schooling laws and education. The GHS health outcomes are a subset of the HSE health outcomes, and hence we combine HSE and GHS data when analyzing health outcomes.

We also use self-reported health information from the 2001 census. Although this census asked only a small number of health questions, it covers the entire population of England and Wales, and hence generates extremely precise estimates. Since the census does not have information on completed education, we report reduced-form estimates of the effects of the compulsory schooling changes on these health outcomes.

V. Results

We report our results in three subsections. These correspond to the three steps of our empirical strategy, described above. We begin with the effects of the compulsory schooling law changes on educational attainment. We then report our estimates of the effects of the law changes on mortality. Finally, we report our estimates of the effects of the additional education induced by the law changes on other measures of health and health behaviors.

A. *The Impacts of the Compulsory Schooling Changes on Education*

To examine the effect of the law changes on educational attainment, we begin by graphing in Figure 2 the relationship between birth cohort and the probability of completing less than nine (for the 1947 reform) and less than ten years of education (for the 1972 reform). Additionally we include figures for the evolution of average years of education across birth cohorts for the two reforms. All panels of this graph present averages by month-year of birth. The vertical bars denote the appropriate birth cohort cutoff for the relevant compulsory schooling change. Superimposed onto these graphs are the fitted linear trends and estimated discontinuities obtained from estimation of equation (1) without covariates (i.e., with $f(R)$ but without X). The fitted lines cover the bandwidths used in our local linear regressions.[24]

As seen in Figure 2, the 1947 change reduced the fraction of individuals completing nine or fewer years of education by around 0.5; the 1972 change reduced

[24] For the 1947 reform, the bandwidth is 46 months for both outcomes and for the 1972 reform, the bandwidth is 57 months.

the fraction completing ten or fewer years of education by around 0.25. This is consistent with the broader picture painted by Figure 1. Table 2 quantifies these relationships. The table reports estimates from regression models that control for month-of-birth dummies, year- and month-of-survey dummies and a third-order polynomial in age.[25] We would not expect educational outcomes to display an age profile (conditional on month-year of birth) because for most respondents, completed education should be fixed from the early twenties onwards. Nevertheless, the age profile adjustment potentially removes idiosyncratic noise. It is more important to remove the age profile when analyzing health outcomes.

There are five main points to note about these estimates. First, they suggest that a large fraction of the affected cohorts "complied" with these compulsory schooling changes (0.45 in 1947, 0.26 in 1972). Second, the effects of the law changes are precisely estimated. The *t*-statistics for the years of education regressions are in excess of 12 for the 1947 change and 5 for the 1972 change. For the dichotomous outcomes, the *t*-statistics are even higher. The magnitudes of the *t*-statistics suggest that these changes are powerful instruments for educational attainment. Third, as expected, regression-adjustment for age and survey year/month does not affect the point estimates. Fourth, the law changes generated only weak spillovers to higher levels of educational attainment: the 1947 change generated a relatively small increase in the fraction of girls that completed 10 or fewer years of education but had little impact on the corresponding fraction for boys; the 1972 change had small, at best, effects on the fractions completing 11 or fewer years, although these estimates are less precise. To a first approximation therefore, one can view these law changes as forcing students that would previously have left at the earliest opportunity to stay in school for one more year. Fifth, the estimates are not very sensitive to the chosen bandwidth (see online Appendix Table A1a).

One final point concerning Figure 2 and Table 2 relates to the small fraction of those subject to the new laws that appear to complete less education than these new laws required. For the 1972 law change, this is particularly marked among those born in June, July, and August. This can be explained by O-level exams, which were taken in May and June, and which typically marked the completion of compulsory schooling. As such, the summer-borns could finish schooling before reaching the age of 16. We discuss this issue in more detail in online Appendix B. To summarize, we conclude that the pattern should have little impact on our estimates since it can be controlled for via the inclusion of post-change month-of-birth controls.

B. *The Impacts of the Compulsory Schooling Changes on Mortality*

Having shown that the compulsory school law changes affected educational attainment, we turn to their impact on mortality. We begin with an analysis of the impact of the 1947 change. We then analyze the impact of the 1972 change.

Evidence from the 1947 Change.—As noted above, we perform our mortality analysis in two steps. First, we estimate a panel logit model that models the

[25] As such, the point estimates are slightly different from those in the figures. In estimating the 1972 reform effects, we allow the month-of-birth dummies to be different on either side of the reform threshold.

Table 2—Impacts of the Compulsory Schooling Changes on Education

	Years of education	$\leq$ 9 Years	$\leq$ 10 Years	$\leq$ 11 Years	$\leq$ 12 Years	$\leq$ 13 Years
Panel A. Impact of 1947 change						
	All (bandwidth = 46 months, N = 31,345)					
Estimate	0.450	−0.445	−0.040	0.009	0.011	0.015
	(0.035)	(0.009)	(0.009)	(0.008)	(0.008)	(0.007)
Outcome mean	15.11	0.58	0.70	0.83	0.88	0.90
	Men (bandwidth = 105 months, N = 33,337)					
	0.443	−0.478	−0.019	0.021	0.019	0.014
	(0.035)	(0.011)	(0.010)	(0.008)	(0.007)	(0.006)
	15.14	0.57	0.70	0.82	0.86	0.89
	Women (bandwidth = 69 months, N = 24,613)					
Estimate	0.524	−0.472	−0.064	0.004	0.003	0.005
	(0.036)	(0.010)	(0.010)	(0.009)	(0.009)	(0.007)
Outcome mean	15.07	0.59	0.71	0.84	0.89	0.91
Panel B. Impact of 1972 change						
	All (bandwidth = 57 months, N = 51,606)					
Estimate	0.353	NA	−0.261	−0.044	−0.034	−0.009
	(0.060)		(0.016)	(0.022)	(0.016)	(0.011)
Outcome mean	16.55	NA	0.33	0.61	0.70	0.79
	Men (bandwidth = 43 months, N = 18,169)					
	0.362	NA	−0.254	−0.038	−0.059	−0.009
	(0.144)		(0.039)	(0.050)	(0.037)	(0.020)
	16.59	NA	0.32	0.61	0.70	0.77
	Women (bandwidth = 53 months, N = 25,705)					
Estimate	0.314		−0.268	−0.037	−0.006	0.000
	(0.069)	NA	(0.016)	(0.025)	(0.021)	(0.017)
Outcome mean	16.53		0.33	0.61	0.71	0.80

Notes: Table gives the estimated effect of compulsory schooling law change on various outcomes. All regressions estimated using pooled waves of the Health Survey for England and General Household Survey. All estimates based on regressions that include a linear function of month-year of birth, a linear interaction of month-year of birth and the relevant reform dummy, a third-order polynomial in age (measured in months) and dummy variables for survey, sex, year of survey, month of survey, and month of birth. Estimates for the 1972 change also include month-of-birth dummies interacted with being born after the relevant threshold. The dependent variable mean is the mean among those born in the 12 months before the relevant cutoff. Robust standard errors clustered by month-year of birth are presented in parentheses.

probability of dying as a function of age and cohort fixed effects. Second, we use a local linear regression to estimate the discontinuity in the estimated cohort fixed effects. In Figure 3, we plot the cohort log-odds of mortality from the first step (i.e., the estimated cohort fixed effects).[26] The local linear fit on this figure covers a bandwidth of 29 months. Mortality risk during the 1970–2007 period studied is rather high; the size of the 1933 cohort shrinks by nearly 25 percent during the 1970–2007 period.[27] However, the downward trend in log-odds does not change discretely with the April 1933 cohort.

[26] In the top panel, the log-odds are measured relative to the January 1926 birth cohort and are measured on a monthly basis.

[27] Calculations based on the Human Mortality Database.

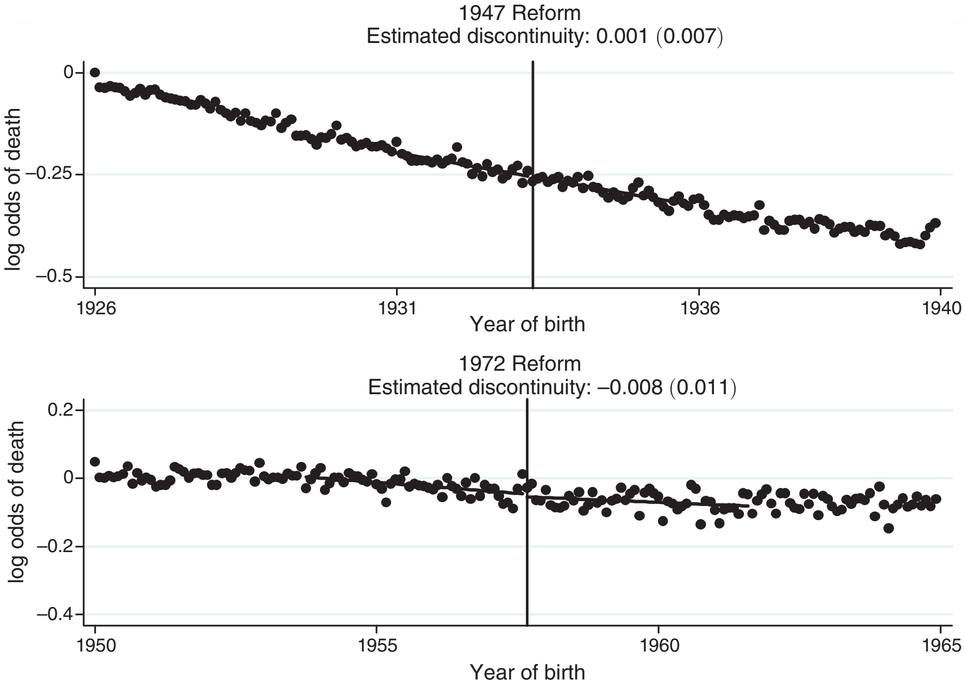

Figure 3. The Impact of the Compulsory Schooling Changes on Mortality

Notes: The log odds ratio is defined as the logarithm of the odds of dying for the relevant cohort relative to the January 1926 cohort for the 1947 reform and relative to the March 1950 cohort for the 1972 reform. Points represent the log odds death ratio for each month-year of birth cell. The estimated discontinuities are based on local linear regressions; the standard errors of the estimates are presented in parentheses. The fitted values of these local linear regressions are also plotted.

Panel A of Table 3 presents the corresponding regression discontinuity estimates—overall and by age group.[28] In the presence of a mortality-reducing effect of education, we would expect these estimates to be negative. In fact, our estimate suggests that the 1947 change increased mortality by 0.5 percent, although this estimate is not statistically significant. The standard errors of our estimates are also small enough to rule out moderately-sized effects. In particular, the 95 percent confidence interval for the overall estimate spans −0.4 to 1.4 percent.

One way to gauge the magnitude of these estimates is to compare them to ordinary least squares estimates. While we cannot compute OLS estimates using these data (since they do not contain education information), we can compute them using the Longitudinal Study, a longitudinal dataset based on a one percent sample of 1971 census records matched to subsequent censuses and other vital statistics (including death records). Although these data measure educational attainment in terms of qualifications rather than age left full-time education, educational attainment

[28] The presented regression discontinuity estimates do not necessarily correspond with those in the analogous figures. The estimates in the figures are derived from the same specification as that for the estimates reported in the table, including the bandwidth, but the estimates in the figures do not control for month-of-birth.

Table 3—The Impact of the Compulsory Schooling Changes on Mortality

Panel A. Impact of 1947 change on log odds of death	Overall	Ages 45–49	Ages 50–54	Ages 55–59	Ages 60–64	Ages 65–69
Reduced-form estimate	0.005	0.030	0.017	−0.008	−0.003	0.017
	(0.005)	(0.014)	(0.011)	(0.012)	(0.007)	(0.011)
Bandwidth in months	29	63	54	31	71	29
Panel B. Impact of 1972 change on log odds of death	Overall	Ages 20–24	Ages 25–29	Ages 30–34	Ages 35–39	Ages 40–44
Reduced-form estimate	−0.004	−0.045	−0.017	−0.010	0.008	−0.053
	(0.026)	(0.065)	(0.035)	(0.046)	(0.028)	(0.030)
Bandwidth in months	47	41	72	82	48	74

Notes: The estimates report the log-odds ratio for the probability of dying for those just to the right of the birth cohort threshold for the 1947 (1972) change versus those just to the left of the threshold. The estimates are derived from a two-step procedure described in the text. All regressions use data by month-year of birth cohort from the Office of National Statistics along with census population counts. Regressions include calendar month-of-birth fixed effects; in the case of the 1972 change, these are allowed to vary on either side of the threshold. Chosen bandwidths are based on a cross-validation procedure described in the text. Robust standard errors are presented in parentheses.

appears to be a very strong predictor of mortality.[29] For example, the cross-sectional "effect" of moving from no qualifications to "some O levels or equivalent" is to reduce mortality by 19 percent, an estimate invariant to the inclusion of controls.[30] Seen in this light, our discontinuity-based estimate of the causal effect of the compulsory schooling change is very small.

We generally reach the same conclusions when examining the estimates broken down by age group. The exception is the estimate for the 45–49 age group, which is positive and on the margin of statistical significance at the 5 percent level. Such an effect is not immediately apparent from the relevant regression discontinuity picture (online Appendix Figure A2), and thus, we view our rejection of the null hypothesis that this effect is zero as the consequence of a type I error.[31] For none of the other age group estimates is there a large and/or statistically significant effect of the reform. Thus, we are assured that our relatively small effects are not due to the age group studied.

To explore the possibility of heterogenous effects across sexes, online Appendix Table A2 displays the sex-specific estimates. Only one of these is statistically significant (i.e., women ages 45–49).[32] This effect is positive and marginally significant. There do not seem to be consistent differences in effects across gender.

Education may have different effects on mortality associated with different diseases. For example, Cutler, Deaton, and Lleras-Muney (2006) find that the education

[29] Educational qualifications are measured as (i) Level 1 which includes some O levels or equivalent; (ii) Level 2 which includes 5 or more O levels or equivalent; (iii) Level 3 which includes 2 or more A Levels or equivalent; and (iv) Level 4 which is a degree or higher.

[30] This thought experiment—moving from no qualifications to "some O levels or equivalent"— is more relevant for the 1972 reform which pushed people from below O levels to O levels. The lowest level of qualification identifiable in the data is O levels.

[31] The bandwidths for the local linear regressions are 63, 54, 31, 71, and 29 months for the 45–49, 50–54, 55–59, 60–64 age groups, respectively.

[32] The estimated effect for males 60–64 has a p-value with 0.057.

gradient in health is steeper for knowledge-intensive, technology-intensive diseases (e.g., diabetes). In results not reported, we estimate whether death rates from three different disease classes (i.e., respiratory, circulatory, and deaths from other causes) are impacted by the 1947 reform. We find no evidence that they are.

To test whether our estimates are sensitive to the dataset used, we employ an alternative, census-based estimate of mortality analogous to that used by Lleras-Muney (2005). Specifically, we measure mortality as the change in month-year of birth cohort size between the 1991 and 2001 censuses.[33] We estimate the reduced-form effect of the 1947 reform on cohort size to be 0.2 percentage points. Since the base cohort shrank by 15.1 percent, this represents a 1.2 percent reduction in 10-year mortality. This is within the confidence interval implied by the hazard estimates.[34]

Overall, we find that the additional education induced by the 1947 compulsory school law change had little impact on mortality. Yet based on this evidence alone, we would be cautious about drawing more general conclusions about the relationship between education and mortality. That is because these effects may be specific to one cohort, and this cohort experienced the Great Depression and the Second World War, factors potentially affecting the life trajectories of its members. We therefore turn next to the 1972 change in compulsory schooling laws.

Evidence from the 1972 Change.—The 1972 change generates another education quasi-experiment that can be used to identify the causal effect of education on health. Estimates based on the second reform might be different from those based on the first reform. First, the later change affected educational attainment at a slightly higher point in the education distribution. The effects of education on health could be non-linear.[35] Second, we observe the cohorts affected by the 1972 change at different ages than those affected by the 1947 change. This is relevant if the health effects of education vary over the life cycle. Nevertheless, since both changes extended the period of compulsory schooling, we might expect their effects to work through similar channels and to have broadly similar impacts.

The bottom panel of Figure 3 plots the log-odds ratios of mortality relevant for the second reform; the reference group is the March 1950 cohort.[36] In this figure, there is little evidence of any discontinuity in mortality risk beginning with the September 1957 cohort.

Panel B of Table 3 displays the regression discontinuity estimates of these mortality effects overall and by age group. The second reform is less powerful for

[33] Cohort size changes across these two years, in principle, could be due to mortality, emigration, and sampling error. Because emigration is likely small for the ages studied (the 1933 cohort would be between the ages of 58 and 68) and because our calculations suggest sampling error is small (see online Appendix B), we expect that mortality accounts for the bulk of the change in cohort size.

[34] These are weighted least squares estimates from a regression of 10-year population decline on a dummy for being born after April 1933, month-year of birth (i.e., the running variable) interacted with this dummy and month-of-birth fixed effects (i.e., dummies for January, February, etc). The weights are population size in 1991. The robust standard error of this estimate is 0.266. A bandwidth of 14 months on either side of April 1933 is used. Estimates are not especially sensitive to bandwidth. For example, estimates (robust standard errors) based on bandwidths half and twice as large (i.e., 7 and 28 months) are 0.43 (0.37) and 0.04 (0.21).

[35] For the United States, Cutler and Lleras-Muney (2006) note that for education levels below ten years of schooling, some outcomes (e.g., mortality) appear to be linearly related to years of schooling while others do not. For education beyond 10 years of schooling, the health returns to education appear to be constant, at least in the cross section.

[36] The local linear fit covers a bandwidth of 47 months.

estimating the effect of education on health given that the first-stage relationship is weaker and mortality rates are much smaller for the impacted cohorts. The 1957 cohort declines in size by roughly 3 percent over the sample period.[37] But overall we can rule out reduced-form effects more negative than 5 percent. The effects by age group are all statistically insignificant. Online Appendix Figure A3 shows the corresponding graphs.[38]

In online Appendix Table A3, we examine these effects more closely by calculating separate estimates by sex. Only one of these 12 estimates that is statistically significant—that for the 35–39 age group for women. This effect is positive, suggesting a mortality-increasing effect on education, although the effect is smaller and statistically insignificant for smaller bandwidths. The direction of the effect suggests a mortality-increasing effect of education. None of these estimates, however, imply a large mortality protective impact of an extra year of compulsory schooling.

In sum, the first reform led to small at best impacts on mortality. The second reform results are consistent with those of the first reform albeit less precise due to the lower observed mortality rates for these groups. Overall, both sets of estimates point to much smaller impacts of education on mortality than the prior literature suggests.

C. *The Impacts of Education on Other Health Outcomes*

Since the mortality effects of these compulsory schooling changes are small, an analysis of their impacts on survivor health outcomes should be free of mortality-driven sample selection biases. We begin this analysis with an assessment of the effects of these changes on health outcomes reported in the 2001 census. As with the mortality analyses, the census data cover almost the entire population of interest and hence the estimates are extremely precise. Also like the mortality analyses, the census data do not include education information and hence we report only the reduced-form effects of the compulsory schooling laws on these outcomes.

We then use HSE data to consider a wider range of outcomes. These include self-reported health status, self-reported health behaviors such as smoking and drinking and clinical health measures collected by a nurse. Because the HSE contains information on education, we report reduced-form (RF) estimates of the effects of the compulsory schooling changes and ordinary least squares (OLS) and instrumental variables (IV) estimates of the effects of education. The OLS estimates are generated using only individuals in pre-reform cohorts whose completed education is less than or equal to the post-reform minimum completed education level (i.e., they are based on the same education difference manipulated by the compulsory schooling change). Although we expect OLS estimates to be biased, they provide a useful benchmark against which the IV estimates can be viewed. The difference between the OLS and IV estimates sheds light on the extent to which the education-health correlation can differ from the causal effect of education on health.

[37] Calculations are based on the Human Mortality Database.

[38] The bandwidths for the local linear regressions in online Appendix Figure A3 are 41, 72, 82, 48, and 74 months for the 20–24, 25–29, 30–34, 35–39, and 40–44 age groups, respectively.

The main limitation of the HSE data is that they include only a small sample of the population of interest (less than 1 percent). As such, they yield estimates which are not as precise as the mortality estimates. We take three steps to increase the precision of these estimates. First, we pool men and women.[39] Second, for the subset of outcomes available in both the GHS and HSE (e.g., education and health outcomes), we pool HSE and GHS data. Third, for the remaining outcomes only in the HSE, we combine the 1947 and 1972 changes.

Evidence from the 1947 Change.—Panel A of Table 4 presents reduced-form (RF) estimates of the impacts of the 1947 compulsory schooling change on three health outcomes derived from the 2001 census. These estimates point to small effects. For example, we estimate that the 1947 change reduced self-reporting having "fair or bad" health and self-reporting having "bad" health by roughly 0.2 percentage points. Since the 1947 change was associated with a completed schooling increase of 0.45 years, this implies that the extra year of education reduced these probabilities by around 0.4 percentage points, off base rates of 58 percent and 18 percent. The effect on the probability of self-reporting a limiting long-term illness is similarly small. All of these estimates are robust to the inclusion of month-of-birth dummies. The standard errors are small enough to rule out economically-meaningful effects. The first row of graphs in Figure 4 provide a graphical representation of these estimates.[40] The smooth and continuous nature of these graphs is striking.

We turn next to several outcomes available in both the HSE and GHS. These include self-reported health measures similar to those available in the censuses and self-reported measures of smoking behavior. The OLS estimates reported in panel A of Table 5A suggest that education has large effects on each of these outcomes. For example, they suggest that an additional year of education reduces the probability of being in fair or worse health by roughly 8 percentage points and reduces the probability of smoking by 6 percentage points (one quarter of the pre-change mean in each case). These are at least as large as the equivalent US estimates.[41]

In contrast, the reduced-form estimates (the column labeled "RF") are smaller, although not typically as small as the census-based estimates. The key difference is that these estimates are less precisely estimated. Thus, while they are typically statistically indistinguishable from zero, we can no longer rule out the types of effects ruled out by the census-based estimates. The same point is evident from comparisons of the OLS estimates and the IV estimates (the column labeled "IV"). Two of the IV's estimates are statistically distinguishable from zero—long illness and ever smoking but the signs of the effects are perverse, suggesting a health-reducing effect of education. These effects are also somewhat sensitive to the chosen bandwidth as seen in online Appendix Table A1. The IV estimates do not rule out the

[39] We began by estimating separate models for men and women but did not reject the hypothesis that effects were the same for men and women.

[40] The bandwidths for the local linear regressions are 15 months.

[41] In US data, an additional year of education is associated with a 0.015 reduction in reporting being in fair or poor health and a 0.02 reduction in reporting currently smoking (Cutler and Lleras-Muney 2006).

Table 4—Impacts of Compulsory Schooling Changes on Self-Reported Health

	Health fair or bad	Health bad	Limiting long-term illness
Panel A. Impact of 1947 change			
All			
Reduced-form estimate	−0.0018	−0.0018	0.00219
	(0.0022)	(0.0015)	(0.0020)
Dependent variable mean	0.58	0.18	0.40
Bandwidth in months	15	15	16
Men			
Reduced-form estimate	−0.0013	−0.0033	0.0005
	(0.0032)	(0.0030)	(0.0029)
Dependent variable mean	0.56	0.18	0.41
Bandwidth in months	15	14	15
Women			
Reduced-form estimate	−0.0042	−0.0006	0.0022
	(0.0019)	(0.0015)	(0.0017)
Dependent variable mean	0.59	0.18	0.38
Bandwidth in months	33	32	30
Panel B. Impact of 1972 change			
All			
Reduced-form estimate	0.0008	−0.0018	−0.0016
	(0.0032)	(0.0013)	(0.0014)
Dependent variable mean	0.31	0.08	0.14
Bandwidth in months	20	55	55
Men			
Reduced-form estimate	−0.0003	−0.0021	−0.0025
	(0.0029)	(0.0016)	(0.0025)
Dependent variable mean	0.30	0.08	0.14
Bandwidth in months	44	55	29
Women			
Reduced-form estimate	−0.0027	−0.0015	0.0009
	(0.0019)	(0.0016)	(0.0019)
Dependent variable mean	0.33	0.09	0.14
Bandwidth in months	45	55	35

Notes: These estimates based on 2001 census data aggregated to the month-year of birth level. All estimates based on regression models that include controls for month of birth. For the 1972 reform, the month-of-birth dummies are allowed to differ on either side of the reform threshold. Regressions weighted by cohort size. Robust standard errors in parentheses. Outcome mean is based on the 12 month-of-birth cohorts preceding the relevant threshold.

possibility that education has large effects on health, although in all but one case we can rule out estimates of the size of the OLS estimates.[42]

In a further attempt to obtain more precise estimates, we estimated education impacts on summary measures of the three self-reported health outcomes and the two smoking outcomes in Table 5A (results reported in panel A of online Appendix Table A4).[43] Although IV estimates of the effects of education on these summary

[42] Here we are treating the least squares estimates as fixed quantities rather than random variables (i.e., we are not conducting Hausman tests).

[43] To generate a summary measure, individual items are standardized (via their sample standard deviation) and then averaged. To enable the appropriate calculation of the variance of the effect on this summary measure, the individual item effects are estimated in a seemingly unrelated regression (SUR) system. Kling, Liebman, and Katz (2007) also use this method.

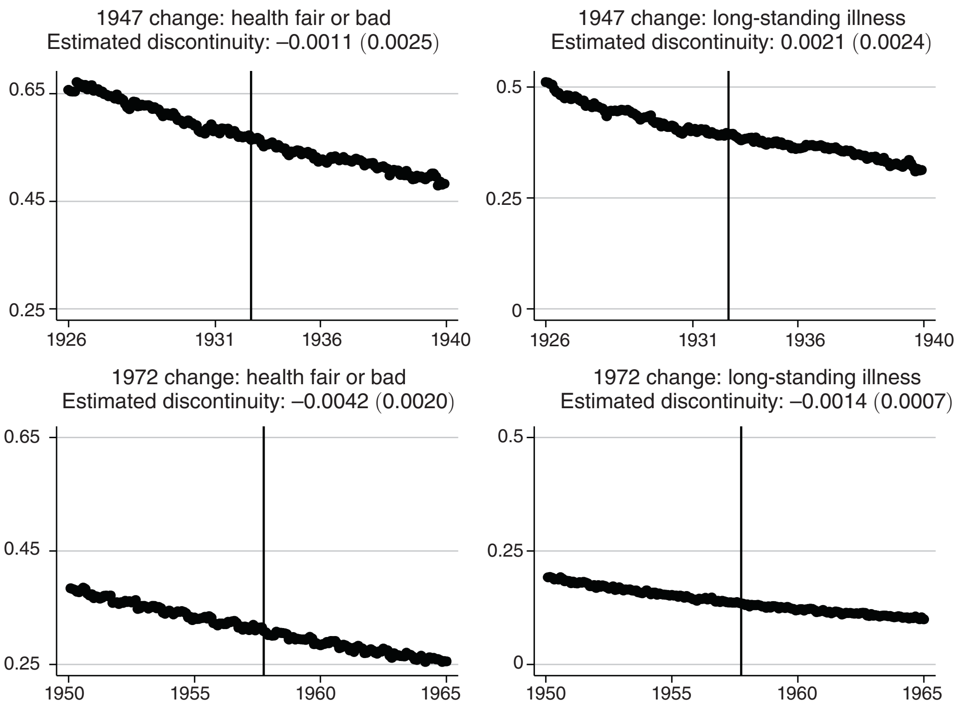

Figure 4. The Impact on Reporting Being in Fair or Worse Health in the 2001 Census

Notes: Samples are based on 2001 census data. Points represent means among people in each month-year of birth cell. The estimated discontinuities are based on local linear regressions; standard errors are in parentheses. The fitted values of these local linear regressions are also plotted.

outcomes are more precise than those for the individual outcomes, and although they are all statistically indistinguishable from zero, they sometimes cannot rule out important health effects of education. For example, for smoking, the IV estimates do not reject the OLS estimates.

Evidence from the 1972 Change.—The estimates obtained for the 1972 change are broadly similar to those obtained for the 1947 change. First, the census-based estimates reported in panel B of Table 4 suggest that the 1972 change had very small impacts on health. For example, the RF estimates suggest that the 1972 change decreased the probability of being in "fair or bad" health by around 0.08 percentage points. Scaled up by a factor of 3.3 (the 1972 change increased years of education by around 0.35, see Table 2), this suggests that an additional year of education reduces this probability by around 0.23 percentage points off of a base of 31 percent. Again, the estimates do not differ systematically between men and women and the bottom graphs in Figure 4 suggest that health outcomes are trending smoothly across cohorts.[44]

[44] The bandwidths for the local linear regressions are 22 months for fair or bad health and 55 months for long illness.

Table 5A—Education Effects on Health and Health Behaviors: Pooled Datasets

Outcome	Mean	OLS	RF	IV	Bandwidth	N
Panel A. 1947 ROSLA						
Health fair or bad	0.25	−0.080	0.001	0.003	134	89,547
		(0.007)	(0.005)	(0.012)		
Long illness	0.54	−0.049	0.021	0.046	60	40,855
		(0.013)	(0.009)	(0.020)		
Reduced activity	0.17	−0.023	0.010	0.022	135	90,284
		(0.006)	(0.005)	(0.012)		
Currently smoke	0.23	−0.060	−0.010	−0.022	102	49,421
		(0.009)	(0.008)	(0.018)		
Ever smoke	0.74	−0.030	0.019	0.042	79	38,677
		(0.010)	(0.009)	(0.020)		
Currently drink	0.81	0.065	0.007	0.015	123	59,642
		(0.007)	(0.006)	(0.013)		
Panel B. 1972 ROSLA						
Health fair or bad	0.12	−0.043	0.002	0.005	43	39,106
		(0.008)	(0.017)	(0.049)		
Long illness	0.30	−0.043	0.000	0.000	91	80,649
		(0.007)	(0.013)	(0.038)		
Reduced activity	0.14	−0.019	−0.006	−0.018	133	114,324
		(0.004)	(0.008)	(0.022)		
Currently smoke	0.30	−0.140	0.000	−0.001	72	47,177
		(0.008)	(0.013)	(0.038)		
Ever smoke	0.67	−0.088	−0.002	−0.006	74	48,450
		(0.009)	(0.017)	(0.050)		
Currently drink	0.92	0.024	0.003	0.010	138	87,552
		(0.004)	(0.006)	(0.016)		

Notes: These estimates are based on models that pool GHS data for 1986–1996 and HSE data for 1991–2004. The column labeled OLS presents the ordinary least squares estimates. The OLS models include only cohorts born before the relevant reforms and are restricted to those that report having the minimum level of schooling or one year more. The column labeled "RF" presents reduced-form estimates including a linear function of month-year of birth, a linear interaction of month-year of birth and the relevant reform dummy, a third-order polynomial in age (measured in months) and dummy variables for sex, survey, year of survey, month of survey, and month of birth. For the 1972 reform, the month-of-birth dummies are allowed to differ on either side of the reform threshold. The column labeled IV presents the instrumental variables estimates, and the column labeled N is the number of observations in the reduced-form and IV models (there are fewer observations in the OLS models because of the sample restrictions (see above)). Robust standard errors clustered at the month-year of birth level are presented in parentheses. The reported mean is among the twelve cohorts preceding the relevant threshold. The IV estimates are the ratio of the reduced-form estimate (column RF) and the first-stage estimate (based on the optimal bandwidths—see Table 2). The standard errors on these estimates are calculated using the delta method.

Second, the OLS estimates in panel B of Table 5A (for individual outcomes) and panel B of online Appendix Table A4 (for summary outcomes) suggest that education has very large impacts on health and health behaviors, reducing the probability of reporting fair or bad health by 4 percentage points and reducing the probability of smoking by 14 percentage points (between one-third and one-half of the pre-change means). The IV estimates are much smaller, although sometimes imprecise. Hence while the IV estimates are statistically indistinguishable from zero, in only one case (currently smoking) can we rule out the effect size implied by the OLS estimate.

Evidence from the 1947 and 1972 Changes.—To increase the power of our analysis, we consider the effects of the two reforms together rather than separately. To begin, we pool all HSE and GHS respondents born between 1920 and 1969. We capture age trends with a flexible age profile, a third-degree polynomial in age. As with our earlier specifications, we control for cohort trends via the inclusion of different order polynomials in birth month (second-, third- and fourth-order polynomials in birth month). Because we now have one endogenous regressor (i.e., years of education) and two instruments (i.e., each reform), we report over-identification test statistics of instrument validity. To generate an OLS estimate that can be compared to this IV estimate, we use the OLS estimate generated by the two-stage least squares weights, as proposed by Lochner and Moretti (2011). We also report the statistics of the Lochner and Moretti (2011) test of the difference between the "same-LATE" OLS and IV estimates. We report these estimates in Table 5B.

Both the conventional and the "same LATE" OLS estimates (columns "OLS" and "OLS (LM)") suggest that education has beneficial effects on all outcomes except drinking, although effect sizes are generally smaller than they were for the outcomes considered in Table 5A. The IV estimates generally suggest no such pattern. An exception is ever smoke, which for specification IV-4 is statistically significant. However, this estimate is sensitive to the choice of the degree of the polynomial for the running variable (c.f., IV-2 and IV-3). We are able to reject the hypothesis that the Lochner-Moretti OLS effect is equal to the IV effect for all outcomes except for currently smoking.

In online Appendix Table A5, we consider the effects on objective health measures obtained from a nurse visit to consenting respondents. Again we combine the effects of the two reforms. The objective health measures include an indicator for being obese (Body Mass Index (BMI) > 30), an indicator for being overweight (BMI > 25), BMI, an indicator for hypertension and blood pressure.[45] These may be more precise indicators of underlying health status than the self-reported health measures analyzed so far. In the cross section, we observe strong health-improving effects of education, but these strong relationships fade once the endogeneity of schooling is accounted for. None of the IV estimates is statistically significant at the 5 percent level. In the cases of BMI and obese, we are able to distinguish statistically between the OLS and IV estimates.

VI. Discussion

Across a wide set of outcomes and for two major compulsory schooling changes, our results suggest that sharp increases in educational attainment in Britain led to small and statistically insignificant changes in health and health behaviors. While these results are intriguing given the prior literature on education and health, they also have provocative implications for the income-health link. In particular, since there is evidence that the reforms increased income, our results suggest that the income effects of health are small, or that they are offset by other factors that negatively

[45] We base our blood pressure measure on dialostic blood pressure, but systolic blood pressure generates similar results.

Table 5B—Education Effects on Health and Health Behaviors: Pooled ROSLAs

Outcome	OLS	IV-2	IV-3	IV-4	OLS(LM)	*N*
Health fair or bad	−0.032	−0.001	−0.003	0.000	−0.066	223,440
(Depvar mean = 0.177)	(0.000)	(0.009)	(0.009)	(0.008)	(0.003)	
p-value (overid/LM)		0.36	0.45	0.85	0.00	
Long illness	−0.018	0.010	0.002	0.014	−0.037	223,460
(Depvar mean = 0.409)	(0.001)	(0.012)	(0.011)	(0.011)	(0.004)	
p-value (overid/LM)		0.23	0.49	0.21	0.00	
Reduced activity	−0.003	0.005	0.005	0.009	−0.023	223,462
(Depvar mean = 0.148)	(0.001)	(0.009)	(0.008)	(0.008)	(0.003)	
p-value (overid/LM)		0.72	0.76	0.24	0.00	
Currently smoke	−0.052	0.002	−0.004	−0.022	−0.032	164,782
(Depvar mean = 0.285)	(0.001)	(0.013)	(0.012)	(0.012)	(0.004)	
p-value (overid/LM)		0.16	0.08	0.30	0.33	
Ever smoke	−0.023	0.006	−0.002	−0.038	−0.008	164,766
(Depvar mean = 0.726)	(0.001)	(0.015)	(0.014)	(0.013)	(0.003)	
p-value (overid/LM)		0.00	0.00	0.46	0.00	
Currently drink	0.021	0.000	0.002	0.002	0.056	164,768
(Depvar mean = 0.873)	(0.000)	(0.009)	(0.009)	(0.008)	(0.003)	
p-value (overid/LM)		0.36	0.44	0.36	0.00	

Notes: This table presents estimates of the effects of years of education on various outcomes using pooled data from the GHS (1986–1996) and HSE (1991–2004) for cohorts born between January 1920 and December 1969. The columns labeled "IV2," "IV3," and "IV4" present instrumental variables estimates. In each case, the instruments are a dummy variable equal to 1 if subject to the 1947 ROSLA and equal to 0 otherwise and a dummy variable equal to 1 if subject to the 1972 ROSLA and equal to 0 otherwise. The IV2 (IV3, IV4) models include a second-order (third-order, fourth-order) polynomial in month-year of birth. The column labeled "OLS(LM)" presents same-LATE least squares estimates corresponding to the "IV4" estimates (see text for more details). In the row labeled "*p*-value (overid/LM)," we present over-identification test statistics (*p*-values) in the IV columns and the Lochner and Moretti (2011) endogeneity test statistic (for the equivalence of the IV and "same-LATE" least squares estimates) in the OLS(LM) column. All models also include a male dummy, month-of-birth dummies, year- and month-of-survey dummies, and a third-order polynomial in age (measured in months). Robust standard errors are clustered by month-year of birth.

impact health. The first possibility is in line with a series of recent papers (e.g., Evans and Moore 2011, 2012) that find negative effects of income on health.[46]

Although we cannot rule out small protective or adverse effects of education on health, these results stand in contrast to those of Lleras-Muney (2005), the most closely related study to ours. She exploits state-level changes in compulsory schooling laws in the United States during the early twentieth century to examine the effects of education on mortality. Her instrumental variables mortality estimates suggest that an additional year of education reduces 10-year mortality rates by roughly 3 percentage points, an effect exceeding ours. More specifically, taking the Lleras-Muney (2005) estimates and converting them into estimates analogous to our own, effects of the size of Lleras-Muney (2005) would imply that our reduced-form estimates would range from 15 to 30 percent.[47] The lower bound of the 95 percent

[46] We note that the variation in income they use is very short-term whereas we think of our examined interventions as impacting long-run incomes. Identifying the effect of permanent income changes on health have proven to be more difficult to identify.

[47] This conversion involves translating the effects on 10-year mortality rates of Lleras-Muney (2005) into 1-month mortality rates (essentially our dependent variable). If the monthly death rate is constant across a 10-year span, then $(1-p) = (1-\overline{p})^{120}$ where p is the 10-year death rate and $\overline{p}$ is the 1-month death rate.

confidence interval for our reduced-form estimate for the 1947 reform is a mortality reduction of 0.4 percent.

This approach has, however, been criticized on two grounds. First, Mazumder (2008) re-analyzes the findings of Lleras-Muney (2005) and shows that her estimates are not robust to the inclusion of state-specific linear trends. Second, Black, Devereux, and Salvanes (2008) report that estimates from difference-in-difference studies using changes in compulsory schooling laws as instruments for education, including Lleras-Muney (2005), often calculate standard errors incorrectly. Specifically, while these studies frequently calculate standard errors under the assumption that errors could be correlated within state × year units (i.e., clustered at the state × year level), Bertrand, Duflo, and Mullainathan (2004) argue that these standard error estimates could be severely downwards-biased. If, instead, standard errors are estimated under the assumption that errors might be correlated within states (i.e., clustered at the state level), as Bertrand, Duflo, and Mullainathan (2004) suggest they should be, then estimates of the first stage relationship between US compulsory schooling laws and education are much less precise.

Of course, in addition to these differences in empirical approach, there were also differences in the education and labor market environments facing individuals affected by the US and UK school leaving changes. To take one example, the US compulsory schooling changes of focus for Lleras-Muney (2005) occurred in the early twentieth century. These reforms might have reduced mortality by reducing exposure to dangerous factory jobs. Since the UK changes were introduced much later, when deaths from industrial accidents had already declined rapidly, we would expect these types of effects to be much weaker.[48]

Our approach builds on Oreopoulos (2006), who exploited the 1947 compulsory change using GHS data. While the main focus of his study was the effect of education on wages, he also considered other outcomes, including self-reported health. A key difference between this paper and ours (in addition to the different outcomes considered and our additional analysis of the 1972 compulsory schooling change) is that our data contain much larger samples and contain information at the month-year of birth level. There are two main advantages to month-year of birth data over the year-of-birth data used by Oreopoulos (2006). First, they allow us to assign treatment status correctly (the 1947 cutoff falls in April). Second, we can focus on a set of cohorts born close to the relevant cutoffs, for whom cohort trends are well captured by our local linear specification. This local approach is difficult to implement with year-of-birth data.[49] As such, Oreopoulos (2006) adopts an alternative

[48] The evidence on this comes from the Annual Abstract of Statistics 1970, Table 200.

[49] Since we began work on this project, Jürges, Kruk, and Reinhold (2013) have also used month-of-birth contrasts to estimate the impacts of the 1947 and 1972 compulsory schooling changes on self-reported health and two biomarkers (blood fibrinogen and C-reactive protein levels). Consistent with our estimates of the impacts of these changes on other outcomes available from the HSE nurse visits (e.g., blood pressure), they find no evidence for causal impacts on these outcomes. They find some evidence for a causal effect on women's self-reported health, although these estimates are imprecise. In one part of our analysis, we use census data based on much larger samples to show that the compulsory schooling changes had very small effects on the self-reported health of both men and women. In another paper published since this project began, Powdthavee (2010) uses a year-of-birth approach to examine education impacts on hypertension. He finds that a year of education induced by the 1947 change reduced hypertension by 7–10 percentage points while a year of education induced by the 1972 change had no statistically significant impacts. These estimates are, however, fairly imprecise. They are also subject to the concerns that we have about estimates based on the year-of-birth approach.

approach, the global polynomial approach that controls for cohort trends via the inclusion of a low order polynomial in birth cohort. Such an approach implicitly uses outcomes for cohorts born a long time before or after 1933 to predict outcomes for cohorts born around 1933. As Lee and Lemieux (2010) note in a more general discussion of regression discontinuity methods, this type of approach is not intuitively appealing. Also, in our case, since the period studied included events as the Great Depression and the Second World War, estimates using the global polynomial approach are far less robust.[50]

On balance, we think that econometric issues provide the leading explanation for the difference between our estimates and those in earlier studies. We now discuss why the alternative explanation, that our results reflect specific features of these compulsory changes and the UK setting, is not consistent with the evidence. The three features that we consider are the National Health Service (NHS), the social context facing the cohorts affected by the 1947 change and the large scale of these compulsory schooling changes.

First, one might wonder whether the universal health insurance guaranteed by the National Health Service (NHS) reduces or eliminates the education-health gradient. The NHS is, however, thought to be inequitable and open to manipulation by more-educated patients.[51] Consistent with this claim, there are well-documented socio-economic differences in both access to care and quality of care in Britain (the Black Report (Department of Health and Social Security 1980); the Whitehead Report (Whitehead 1987); and the Acheson Report (Acheson et al. 1998)). Moreover, irrespective of any NHS effects, we would still expect more-educated patients to engage in less risky behaviors (e.g., smoking) and more efficient self-management (Goldman and Smith 2002).[52] These points are consistent with the similarity of the education-health gradients estimated for the United Kingdom and the United States (Banks et al. 2009) and the similarity of the factors thought to influence these gradients (Cutler and Lleras-Muney 2010).

Second, one might wonder whether the social context facing cohorts affected by these compulsory schooling changes reduced their health returns to education. This seems especially relevant for the cohorts affected by the 1947 change, since these were born during the Great Depression and had their education disrupted by war.[53]

[50] For example, while some of our estimates suggest large effects of education on health, these are typically sensitive to the low-order polynomial chosen. Goodness-of-fit tests that compare the fit of these models with the fit of less restrictive alternatives nearly always reject the use of the global polynomial models. For example, for the outcomes studied in Table 4 (“health bad,” “health fair,” and “long-standing illness”), a set of outcomes similar to the health outcomes considered by Oreopoulos (2006), a year-of-birth specification that pools men and women yields estimates that are quite sensitive to the order of the polynomial chosen. For instance, for “long-standing illness,” they range between 0.008 and 0.034; for “health bad,” they range between −0.005 and 0.020.

[51] While the combination of gatekeepers and resource constraints should in theory limit the extent to which more-educated patients can shop for specialists and treatments, more-educated patients can work this system by, for example, persuading their general practitioner (GP) to refer them for treatment or, if that fails, switching GPs or going directly to a specialist (Lyall 2008). Until recently, to benefit from new treatments, patients first had to be referred to a specialist by a GP. The specialist then decided whether to treat the patient and patients requiring treatment were placed on a waiting list (patients could avoid waiting by opting for private treatment). See Aaron and Schwartz (1984) for a comparison of the UK and US health systems.

[52] Consistent with this hypothesis, some evidence suggests that highly-educated parents in the UK are more responsive to changes in medical knowledge concerning the perceived safety of childhood vaccines (Anderberg, Chevalier, and Wadsworth 2011).

[53] Evacuation of school-aged children (aged 5 to 15) went into effect in September 1939. School children from “evacuation” areas thought at risk were sent to “reception” areas; “neutral” areas were not affected. Around

Although the men affected by the change were too young to fight in the Second World War, they were required to serve two years of military service from ages 18 to 20.[54] Both men and women were affected by rationing, introduced in 1940 and removed in 1954 (Zweiniger-Bargielowska 2000). Yet the cohorts affected by the 1972 reform, born 25 years later, were exposed to a completely different and arguably less turbulent environment. The consistency of our findings across the two compulsory schooling changes therefore points away from this possibility.

Third, while the scale of these changes helps us to generate precise estimates, and ensures that these are applicable to a large share of the population, these changes might have generated smaller health returns than compulsory schooling changes that affected fewer people. One possibility is that the large scale of these changes lowered the quality of the additional education that they generated. This is not supported by anecdotal evidence from the time or by the finding that these changes led to significant earnings gains. Another possibility is that they had smaller impacts on peer composition—and therefore health—than smaller compulsory schooling changes would have had. This explanation does not ring true either. While teenage peers could affect health outcomes (Gaviria and Raphael 2001; Powell, Tauras, and Ross 2005; Trogdon, Nonnemaker, and Pais 2008; Duncan et al. 2005), adult peers—especially workplace peers—could also affect health outcomes (Cutler and Glaeser 2007). Since the compulsory schooling changes affected earnings, they likely affected workplace peers. Similar considerations apply to a similar story—that these large compulsory schooling changes had unusually small impacts on relative well-being and therefore health. If relative well-being is especially important, the affected cohorts may not have improved their rank relative to their month-year of birth cohort (all subject to the same compulsory schooling law) but should have improved it relative to, for example, their year-of-birth cohort (not all subject to the same law).[55] There seems little reason to suppose that the month-year of birth cohort is the relevant reference group.

VII. Conclusion

Does education have a causal effect on health? The answer to this question has implications for our understanding of economic phenomena ranging from the demand to health capital to economic growth. It also has important policy implications now that education policies are being crafted with health goals in mind. For example, in the United States, there is an explicit directive within the US health goals (i.e., US Healthy People) to raise the percent of students graduating from high school to 90 percent. In Britain, positive health effects have been cited among the advantages of a further raising of the school-leaving age (Seager 2009).

Our paper answers this question using a research design that is, arguably, the most compelling used so far. Specifically, we exploit the fact that compulsory school

40 percent of school children in London were evacuated; figures for other evacuation areas are smaller. Some of these children were away for up to two years, although many returned sooner and all attended school in the reception areas (Titmuss 1950).

[54] See Scott (1993) for details of post-war conscription policies and Buonanno (2006) for an analysis of the labor market impacts of military service based on its phasing out in 1960.

[55] See the series of articles on social status and rank by Sir Michael Marmot, including Marmot (1994).

leaving changes led to dramatically different educational experiences for individuals born just days apart. We find that the health returns to the extra education generated by these compulsory schooling changes were small. We believe there is little reason to suppose that these findings reflect specific features of the British compulsory schooling changes or the broader British setting. Instead, the more plausible explanation is that these types of interventions have generally small causal effects on health. If that is correct, our findings imply that economic models that assume a strong causal connection between education and health may need to be carefully rethought. It also casts doubt on the health returns to policies designed to increase educational attainment.

We stress that our findings do not imply that there are small health returns to all types of education interventions. For example, there could be health returns to college education. Although this is a possibility that our study cannot address, de Walque (2007), and Grimard and Parent (2007), find that the Vietnam draft, which induced some individuals to go to college, reduced their probability of smoking. This caveat aside, we think it is interesting and important that we find small health returns to compulsory schooling changes that affected up to one-half of the relevant population.

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