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The Effect of Education on Adult Mortality and Health: Evidence from the United Kingdom

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I. Introduction

In many developed countries, health differences across education groups are striking. For instance, among working-age adults in the United States in 1999, the mortality rate among those with exactly 12 years of education was nearly *double* that among those with 13 or more years of education (Hoyert et al., 2001). Deaton and Paxson (2001) also find strong correlations between education and mortality risk in the United Kingdom. Since Grossman (1972) it has been suspected that these correlations could reflect a causal relationship between education and health, and the causal effect of education on health is the subject of much current debate.

In this study, we exploit a major change in UK compulsory schooling laws to evaluate the causal impact of education on adult health behaviors and outcomes, including mortality. The compulsory schooling reform, introduced in 1947, increased the school-leaving age from 14 to 15. By most estimates, this had a huge impact on the educational attainment of affected students. For example, comparing individuals based on year of birth using a regression discontinuity approach, Oreopoulos (2006) shows that the probability of leaving at or before 14 dropped from around 60 percent for those who turned 14 immediately prior to 1947 (not subject to the new law) to around 10 percent for those who turned 14 after immediately following 1947 (subject to the new law). We use similar contrasts to isolate the effect of the reform on adult health to understand the impact of education on health.

Past analyses, including Oreopoulos (2006) and Harmon and Walker (1995), use comparisons based on year of birth, effectively assuming that these birth cohorts born pre- and post-reform are exchangeable (i.e., otherwise identical except for their educational attainment).[1]

[1] Oreopoulos (2006) uses a regression discontinuity approach in which he effectively compares the 1933 birth cohort with the 1932 birth cohort. Harmon and Walker (1995) use a differencing-type approach and contrast cohorts subject to a compulsory schooling age of 14 with cohorts subject to a compulsory schooling age of 15. Hence, the

However, the affected cohorts were born during a time of great social change in the UK (e.g., the Depression, World War II). As such, exchangeability assumptions necessary for the identification of the effect of education may be unlikely to be met in this case.[2] To combat this concern, we take advantage of the exact details of the compulsory schooling legislation and make finer comparisons based on quarter of birth. The validity of type of comparison rests on less restrictive assumptions than Oreopoulos (2006) and Harmon and Walker (1995). The relaxation of these assumptions appear to make a difference as our estimates of the effect of education on self-reported health are different than Oreopoulos (2006).

Since the law was introduced part way through 1947 and our data contain quarter of birth information, we take an even finer approach. Specifically, we compare individuals based on quarter of birth and show that this drop in the probability of leaving before 14 occurs between those turning 14 in the first quarter of 1947 and those turning 14 in the second quarter of 1947. We then compare quarter-of-birth cohorts born on either side of the school leaving law threshold to identify the effect of education on mortality and adult health.

This study builds on a large quasi-experimental literature examining the effects of education on a range of adult health outcomes including mortality (e.g., Adams, 2002; Arendt, 2005; Arkes, 2003; de Walque, 2004; Lleras-Muney, 2005; MacInnis, 2006; Mazumder, 2007). These studies have undoubtedly advanced our knowledge of these effects but they suffer from two key limitations. First, the studies looking at mortality use only imperfect measures of mortality. While Lleras-Muney (2005) measures mortality using changes in cohort size across Census years, cohort size often *increases* across Census years. Second the instrumental variables used to predict education are typically

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identification assumptions invoked by Oreopoulos (2006) (i.e., the 1933 cohort is otherwise identical to the 1932 cohort except for their educational attainment) are less stringent than those used by Harmon and Walker (1995) (i.e., the cohorts born prior to 1933 are otherwise exchangeable with cohorts born after 1933).

[2] Neither Oreopoulos (2006) or Harmon and Walker (1995) test whether these cohorts are similar along non-education-related characteristics.

weak, generating estimates that are sensitive to the specification of the "first-stage" equation (e.g., Arendt, 2005; Lleras-Muney, 2005). While Lleras-Muney estimates large effects of education on mortality using instruments based on compulsory school laws across states in the United States, Mazumder (2007) shows that these estimates are reduced by a factor of four when state-specific cohort trends are included in the first-stage equation. In contrast, our study overcomes each of these limitations. First, we measure mortality directly. Second, we exploit a compulsory schooling law that led to stark, sudden changes in educational attainment across cohorts born just one quarter apart. Since there is no reason to suppose that mortality and adult health among these cohorts would have differed in the absence of the law, any differences can be plausibly attributed to the causal effects of the increased education induced by the law.

Using this regression discontinuity strategy, we show that the increase in completed schooling levels induced by the law had almost no effect on mortality and at best small effects on adult health. A reasonable conclusion is that while affected the cohorts have yet to reach the age at which mortality effects will be felt (the affected cohorts are aged 70 when our data finish in 2003), the estimated health effects are consistent with mortality effects being observed at older ages. Since Oreopoulos (2006) shows that the same law had dramatic impact on the earnings of affected individuals, it is interesting to speculate as to why these health effects are not larger. An obvious explanation is that the affected cohorts enjoyed access to universal health insurance via the National Health Service introduced in 1948, but we have little direct evidence on this point.[3]

II. Background

[3] To evaluate this health insurance hypothesis more thoroughly, we plan to estimate the effect of education on mortality using earlier changes in compulsory schooling laws in the United Kingdom affecting cohorts born before the advent of national health insurance in England (i.e., changes occurring in the early 1900's). We are in the process of obtaining more information about the effectiveness of these policies. Unfortunately, standard UK datasets with information on educational attainment are a relatively recent development, limiting our ability to test the first stage relationship.

In the US and elsewhere, there exists a strong correlation between education and adult health outcomes. In respect to mortality, an early seminal study by Kitagawa and Hauser (1968) matched 1960 death certificates to the 1960 Census and found that an individual's mortality risk declined with his/her educational attainment. Since then, many others have confirmed this pattern and have examined the education gradient in mortality along many dimensions - over time (Pappas et al, 1993), over the life cycle (Beckett, 2000; Lynch, 2003) and across the sexes (McDonough et al, 1999; Christenson and Johnson, 1995) and races (Williams and Collins, 1995).[4] A strong correlation between education and mortality is also seen in the UK (Deaton and Paxson, 2004).

In relation to other health outcomes, the education gradient is similarly strong. Recent evidence from the US National Health Interview Surveys shows that individuals with more education are less likely to report being in fair or poor health, less likely to report having diseases such as hypertension, less likely to smoke and drink and more likely to exercise (Cutler and Lleras-Muney, 2006)). Since the focus of this paper is the UK, it is interesting to note that the education-health gradient in England is of comparable magnitude. Using the Health Survey of England and a similar sample as Cutler and Lleras-Muney (2006), we document a similar relationship between completed schooling and being in poor health and smoking in Figures 1a and 1b.[5] These effects are similar to the effects that Cutler and Lleras-Muney found in the NHIS.[6]

Despite documenting a strong correlation between education and adult health outcomes, prior observational studies do not in general confirm that a causal link exists.[7] As a result, some recent literature uses quasi-experimental evidence to assess the extent of any casual link. In relation

[4] As these literatures are not directly relevant to our proposed study, we do not provide details on their methods and findings.
[5] We only include individuals 25 and older from the 2000 Health Survey of England, which is analogous to Cutler and Lleras-Muney's (2006) sample.
[6] We assume that individuals reporting leaving school at 14, 15, ..19+ have 10, 11, ..15+ years of completed schooling. These estimates are derived from models that include dummies for years of completed school, year of birth and sex. See Figure 2 of Cutler and Lleras-Muney (2006) for the NHIS estimates.
[7] We read the previous literature as aiming mainly to document the observable relationship, not estimate the causal effect.

to mortality, Lleras-Muney (2005) identifies the effect of education on mortality using changes to US compulsory schooling laws (i.e., laws mandating the minimum dropout age). Arguing that these changes are unrelated to factors directly affecting mortality, Lleras-Muney finds that an extra year of schooling leads to a reduction in the probability of dying within a 10-year interval of 3.6 percentage points.[8] Relative to the mean probability of death within 10 years of 11 percent, this is a sizeable effect—larger, for example, than the simple cross-sectional estimate.[9] Various studies have used similar research designs to assess the impact of education on health outcomes and behaviors such as smoking initiation, smoking cessation and obesity (Adams, 2002; Arkes, 2003; de Walque, 2004; Lleras-Muney, 2005, MacInnis, 2006).

These quasi-experimental studies have furthered our understanding of the effect of education on mortality and adult health. However, as noted in the Introduction, they suffer three key limitations. First, none examine mortality directly (Lleras-Muney (2005) infers mortality from the change in cohort size across Censuses). Second, the instruments used to predict education have been shown to be weak (Mazumder, 2007). For example, Lleras-Muney (2005) documents that compulsory schooling changes implemented in the United States at the beginning of the twentieth century only affected educational attainment for five percent of the potentially-treated population. In comparison, the 1947 UK reform affected the school exit age for 50 percent of the cohorts for which the reform was binding. Third, nearly all studies are unable to look at mortality and non-mortality health conditions simultaneously, giving us a limited understanding of the mechanisms by which education may manipulate an individual's health.

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8 Since Lleras-Muney uses Census data to estimate mortality rates and the quasi-experiment affects the educational attainment of different cohorts, her outcome mortality measure is essentially the fraction of the cohort observed in subsequent Censuses.

9 This degree of overstatement may reflect the measurement error bias of the cross-sectional estimate. Extensive education research in several disciplines indicates that the signal-to-total-variance ratio in self-reported education is about 0.8.

III. The 1947 Reform

As a means of disentangling the effect of education from other correlated factors (e.g., tastes, family background), we take advantage of a dramatic educational reform in 1947 that effectively increased the compulsory schooling age, the minimum age at which a student can lawfully dropout of school. In the UK, compulsory school laws are decided at the national level, with the two most recent changes occurring in 1947 and 1974. In this paper we focus on the first of these changes, introduced as part of the 1944 Education Act. Prior to 1947, students attended primary school from grades one to six and secondary school from grade seven to at least the end of the term in which they turned 14 (UK schools have Autumn, Spring and Summer terms that run September-December, January-Easter, Easter-July). After the 1944 Education Act took effect, (effectively applicable from April 1, 1947), students had to stay in secondary school until the end of the term in which they turned 15.

As a result of this law, students born in 1932 (who turned 14 in 1946) were subject to the old regime; they could leave school at the end of the term in which they turned 14. Students born in 1934 (who turned 14 in 1948) were subject to the new regime; they could leave at the end of the term in which they turned 15. Students born in 1933 (who turned 14 in 1947) were subject to the old or the new regime depending when their fourteenth birthday fell. Those whose fourteenth birthday fell before the end of the Easter term of 1947 (roughly January-April) were allowed to leave at the end of the Easter term of 1947 (i.e., aged 14).[10] Those whose fourteenth birthday fell after the end of the Easter term of 1947 were subject to the new law, and could not leave until after the end of the summer term of 1948 (i.e., aged 15). Since Easter fell on April 6 in May 1947, we would expect none of those born in January-March, few of those born in April and all of those born in May-December to be subject to the new regime.

[10]Term dates differed slightly across school districts.

The data shown in Figure 2a (based on "age left full-time education" responses in the Health Survey of England) are consistent with this account, as are Ministry of Education enrollment data from that time.[11] Figure 2a shows the fraction of each birth quarter leaving school before the age of 14. As expected, this fraction jumps sharply from the first to the second quarter of 1933. That only around 60 percent of those born in the first quarter of 1933 left at aged 14 is because many chose to stay in school longer. Compliance with the compulsory schooling age change was not likely perfect. From Figure 2a, we observe a small share of individuals born in the second quarter of 1933 leaving before age 15.[12]

From the perspective of identifying the effects of education on mortality and adult health, the important point is that the law had a large effect on completed education (as seen in Figure 2b). Assuming the reform did not coincide with changes in non-education related factors that might influence health, identification can be achieved via comparisons of mortality and adult health among those on either side of the school leaving threshold. For instance, one potential threat to identification is the Great Depression; cohorts subject to a compulsory schooling age of 14 had slightly more exposure than cohorts subject to a compulsory schooling age of 15. However, since we rely on contrasts of individuals born just a quarter of a year apart from one another, for this concern to be credible, it must be true that a quarter of additional exposure to the Great Depression had a profound effect on health. Another concern might be that individuals born around 1933 had their education interrupted by the Second World War (since they were aged seven or eight at the time that England and Wales was most seriously affected). Yet there is no reason to think that students born in the second quarter of 1933 should have been more or less affected than those born

[11] The data underlying Figures 2a and 2b are discussed in more detail below.
[12] It is also possible that there is some measurement error in the age left full-time education variable reflected in the non-zero fraction of individuals born in the second quarter of 1933 reporting that they completed their schooling before 15.

in the first quarter of 1933. Moreover, we can test the extent to which these cohorts have any "pre-treatment" differences (e.g., infant mortality risk).

In terms of interpretation, an obvious question is what the extra year meant in practice. While the law compelled students to stay in school until age 15 (i.e., into grade nine), students did not officially complete secondary school (and receive a certificate) until age 16 (i.e., at the end of grade ten). Hence the extra year did not result in an extra credential. Only a small fraction of affected students stayed in school until 16 to receive their secondary school credential. Thus, the policy quasi-experiment identifies the effect of an additional year of schooling independent of a credentialing effect. While the Government was aware that the extra year would not ensure that students completing secondary school, they nevertheless anticipated that the extra year would be beneficial, as this excerpt from the 1947 Ministry of Education report (HMSO, 1948) makes clear:

> "The main value, however, 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." (p.13)

The extra year of compulsory schooling imposed a significant cost on the government. Schools had to hire additional teachers and build extra classrooms to accommodate the additional students. To ease this transition, the law went into effect 3 years after passage in 1947. The government trained 13,000 extra teachers via the Emergency Training Scheme - a one-year version of the traditional two-year teacher training course. The government also constructed as part of an extensive school building program also intended to renovate war-damaged schools. With a fixed budget, one might worry that the sudden increase in educational costs would result in fewer resources allocated per grade, and if such resources affect the return to education, the compulsory schooling change could effectively dilute the effect of the additional year of education. Yet after reviewing reports of its implementation from around the country, the Times Educational Supplemented commented on 5 April 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). Additionally, empirically, this extra year of schooling did seem to provide some value in the labor market.[13] Oreopoulos (2006) has shown that the extra year led to a large increase in labor market earnings. The question addressed in this paper is whether there were consequent impacts on mortality and adult health.

IV. Estimation Approach

The quasi-experiment generated by the 1947 reform is a "fuzzy regression discontinuity design" (Angrist and Lavy, 1998; van der Klauw et al , 1999) with a discrete "running variable" (quarterly birth cohort).[14] In this context, a standard procedure for estimating the causal effect of the "treatment" (in this case educational attainment) on the outcomes (mortality and adult health) is to estimate the following pair of equations (see for example Card and Lee, 2006):

(1) $E_{ic} = \beta_0 + \beta_1 D_{ic} + X'_{ic}\pi + f(c) + u_{ic}$

(2) $Y_{ic} = \alpha_0 + \alpha_1 D_{ic} + X'_{ic}\pi + g(c) + v_{ic}$

where E_{ic} denotes completed years of schooling for individual *i* in birth cohort *c* (defined at the quarterly level), Y_{ic} is a measure of health outcomes, D_{ic} is a dummy variable for whether the individual is affected by the 1947 reform (i.e., has a value of one for those born after the second quarter of 1933, zero otherwise), X_{ic} is a vector of control variables and f(c) and g(c) are smooth

[13] We have tried to assess whether the extra year of schooling had effects on more direct measures of schooling – test scores and literacy but we have been unable to acquire such data.

[14] It is a fuzzy design as compliance with the policy seems to be imperfect although the degree of imperfect compliance appears to be small.

functions of birth cohort (low-order polynomials). The random error terms u_{ic} and v_{ic} capture the unobserved determinants of E_c and Y_c respectively. The functions f(c) and g(c) for general cohort trends in the outcome variables of equations (1) and (2).

This approach to estimating the regression discontinuity approach is frequently-referred to as the global polynomial approach (Lee and Card, 2007). An alternative approach would be local linear regression. An advantage of such an approach is that estimates of the difference in outcomes between pre-reform cohorts and post-reform cohorts are more local to the threshold (i.e., quarter 2 of the 1933 birth cohort). However, in the case that f(c) and g(c) are relatively smooth functions of birth cohort, which in our case, they are, the local linear approach will result in nearly identical estimates as the global approach. In this context, another advantage of the global polynomial approach is that it is relatively easy to control for covariates, X, which are smooth functions of birth cohort, for variance reduction. In the case of local linear regression in the context of regression discontinuity, there is no developed procedure to control for covariates X while estimating β_1 and $\alpha_{1,}$ the parameters of interest.

Least squares estimation of equations (1) and (2) generates regression discontinuity estimates of β_1 and α_1 the effects of the reform on education and health outcomes respectively. These estimates represent the "jump" in educational attainment and health outcomes associated with being born in the second quarter of 1933. This is the sense in which the research design exploits the comparison of those just unaffected and just affected by the reform. The ratio of the estimates (α_1/β_1) gives the standard IV Wald estimate. The resulting IV estimate is an estimate of the local average treatment effect (LATE), the effect of education on health for those individuals whose educational achievement was affected by the law (Angrist, Imbens and Rubin, 1994).

As discussed by Card and Lee (2006), this procedure identifies the causal effect of the treatment under relatively weak conditions. As such, it provides a promising route to identifying the causal effects of education on health in the UK. However, as in any study exploiting a regression discontinuity design, a number of practical issues must be addressed. First, an important condition for identification is that the functional forms of f(.) and g(.) are specified correctly.[15] We follow the literature (e.g., Lemieux and Milligan, 2005) in specifying a flexible function for f(.) and g(.). We provide visual checks of these functional forms, employ goodness of fit tests and check the robustness of our results using alternative specifications. Second, for identification to hold, it must also be the case that the underlying relationship between the running variable (in our case, birth cohort measured in quarters) and the outcomes be continuous through the threshold (in our case, quarter 2 of 1933). Since many of the underlying determinants of health are unobservable, we can only partially test this assumption. We check for the continuity of observable "pre-treatment" characteristics through the threshold.

In addition, there are some econometric issues that are specific to this study. First, the data used to estimate the impacts of the reform on mortality do not contain education information. As such, we only present reduced-form estimates of the effect of the reform on mortality risk. However, since these estimates are small, the implied IV estimates are also small, too.[16] Second, when looking at adult health outcomes, we are obviously focusing on the (potentially selected) sample of individuals that are still alive at the time of the HSE. If education reduces mortality, we might expect these to under-estimate the impact of education on adult health. However, since our results provide no evidence for pre-adult mortality effects, we interpret our estimated adult health effects as being free from these types of sample selection concerns.

[15] The functional forms are usually inferred from the data.

[16] Alternatively, we could also present two-sample instrumental variables estimates. However, there is an issue of what would be the relevant first-stage estimate. In particular, our estimates of the first-stage relationship between birth cohort and schooling come from data starting in 1991. By the time, there may have been selective mortality which would bias the estimates of the reform on schooling.

V. Data

Our analysis takes advantage of several data sources: (1) the 1991-2004 Health Surveys of England (HSE) which provide data on educational attainment and health-related outcomes, (2) the 1970-2003 Office of National Statistics vital statistics which provide data on deaths by cause, sex, year of death, and quarter of birth, (3) the Human Mortality Database which furnishes data on deaths by sex, year of birth, and year of death.

Our analysis of adult health outcomes uses data from the Health Survey of England (HSE). Began in 1991, the Health Surveys of England are annual surveys combining a questionnaire-based component and more objective information (such as measured blood pressure) obtained from a nurse visit. These data are quite similar to the National Health Interview Survey and the National Health and Nutrition Examination Survey in the United States. We pool all waves of these data from 1991 through 2004 to give us a large sample of roughly 60,000 adults born roughly 15 years on either side of the 1933 threshold. Since the questionnaire asks respondents at what age they left full-time education and collects information on month of birth, we can use these data to implement the 2SLS methods discussed above.[17]

Our mortality analysis is based on a variety of UK vital statistics data. First, we combine historical data on the number of live births by quarter with the number of deaths by birth quarter to calculate mortality rates at the quarter of birth level from the Office of National Statistics. The mortality data furnish the number of deaths by quarter of birth, sex, cause (i.e., overall, circulatory, respiratory, and other), and year of death for the years 1970-2003. The birth data come from the

[17] Between 1991 and 2003, individuals in this 30-year window range from ages 41 to 83. Focusing on older individuals arguably gives us a better chance of detecting education effects on health, but we might worry that respondents are less capable of recalling the precise age at which they left full-time school. To check this, we assigned respondents to year rather than quarter of birth cohorts and re-estimated the effect of the reform on educational achievement at the year of birth level (i..e, the approach used by Oreopoulos, 2006). Our results were very similar to those reported by Oreopoulos (2006), who used General Household Survey data from the 1970s and early 1980s and suggest that imperfect recall is not a serious concern.

Registrar General's Annual Reports and Quarterly Returns and count all live births that occurred in England and Wales and were registered in a particular quarter.

The individual-level mortality records are only available from 1970. To address the concern that selective mortality before 1970 may affect the interpretation of our estimates, we have also collected mortality data by year of birth, year of death, and sex from birth from the Human Mortality Database (http://www.mortality.org/). These data cover births and civilian deaths as recorded by the UK General Registry Office (GRO) over the period 1901-2001 and military deaths as estimated by Jdanov *et al* (2005).[18]

Another issue with all these data sources is mobility. In particular, in order to be our estimation sample, a person must be living or have died (in the case of the mortality data) in England or Wales. However, there is a strong correlation between educational attainment and mobility (Malamud and Wozniak, 2006) meaning that it is possible that individuals born after 1933 quarter 2 may have left England or Wales. The possibility of education-induced mobility could lead us to underestimate the fraction of the post-reform cohort that is dead. In this case, our estimates of the effect of the educational reform on mortality would be biased upwards. We investigate the mobility issues empirically by using US and Canadian census data to estimate the effect of the reform on the probability of emigrating to North America and by using HSE data to check there are no discontinuities in the fraction of foreign-born residents born in the quarters before and after the reform.

VI. RESULTS

The First-Stage Relationship Between Birth Cohort and Age at School Exit

[18] Live births are those that occurred in England and Wales in that year and were registered in that year or within 42 days of the following year and (the small number of) births that occurred in previous years and were registered in England and Wales in that year. Deaths refer to the total number of deaths registered in England and Wales in that year (occurred in that year from 1993).

Figure 2a displays the fraction of males and females as observed in the Health Survey of England (described below) leaving school at age 14 by year of birth. All cohorts to the right of the vertical lines in these figures were subject to a compulsory schooling age of 15 whereas the cohorts to the left of the vertical lines were subject to a compulsory schooling age of 14. Before the 1947 reform, about 70 percent of individuals dropped out of school at either age 14 or earlier. After the reform, nearly everyone completed schooling after age 14. This change is sudden and stark and appears to coincide with the change in compulsory schooling age. This jump in the fraction exiting school at age 14 suggests that a regression discontinuity regression approach in which we compare individuals who were 14 immediately prior to the reform with individuals who were 14 immediately after the reform may be valid method to estimate the effect of education on mortality. The relationship between quarter of birth and the fraction dropping out at age 14 or earlier is nearly identical to that for men.

Figure 2b presents the mean age at school exit by quarterly birth cohorts. The sudden discontinuous pattern observed in Figure 1 is also apparent here. For both men and women, the age at school exit increases by about 0.5 years after the reform comes into effect.

Tables 1a and 1b quantify the relationships seen in Figures 2a and 2b. In particular, this Table presents the corresponding regression estimates of equation (1) for a variety of outcomes for males and females: fraction dropping out by 14, 15, 16, 17 and 18 and age when left full-time education. These regressions specify f(c) as a third-degree polynomial in quarter of birth interacted with a post-reform dummy variable. To test whether the third-degree polynomial specification is sufficient, we performed the F-tests suggested by Lee and Card (2006).

For each outcome, we present two estimates. The first uses all cohorts born between 1920 and 1950 and the second uses a narrower window of cohorts born between 1929 and 1939. The results suggest the reform reduced the fraction dropping out of school by age 14 by around 50 percentage points, a large decline given a pre-reform base of about 70 percent. There are two reasons why this impact is less than the full year by which the school leaving age was raised. First, even before the law was changed, a fraction of children voluntarily continued in education to age 15 and beyond, hence the law only had bite for those students that previously left at 14. Second, the law's effects would have been diluted by the small number of districts that raised the district-specific leaving age to 15 before 1947 and the small number of districts that failed to comply with the law after 1947, typically those that could not build sufficient school places in time (O'Keefe, 1975). For both males and especially females, there was a slight decline in the fraction dropping out by age 15, suggesting that the reform compelled individuals to stay in school for only one more year. Overall, the reform increased the average age at school exit by about 0.4 of a year.

Although it may appear otherwise, we can use this law to identify the effect of education on mortality even though it was not perfectly complied with. Instead, the key identifying assumption (described more precisely below), is that the law did not coincide with any shocks to mortality. The assumption would be violated if there were specific events that meant that in the absence of the law, the affected cohorts would have experienced higher rates of mortality. We know of no such events: histories of the law suggest it had been on the agenda for many years, with the exact timing driven by budgetary and political concerns (O'Keefe, 1975) and we present evidence consistent with the absence of any pre-treatment differences between affected and unaffected groups. Imperfect compliance does however change the interpretation of our estimates, which must now be interpreted as effects for individuals whose educational attainment was affected by the law. This group of "compliers" contains those that would have left school at 14 in the absence of the law but were compelled to leave at 15 because of it (Imbens and Angrist, 1994).

Although these compliers are only a fraction of the affected cohorts, it should be stressed that this fraction is much larger than the fraction of individuals whose education is typically manipulated by a quasi-experiment.[19] This is a point made by Oreopoulos (2006), who uses the 1947 law to study the causal impact of education on labor market outcomes. Indeed, one could make the argument that this law affects too many people, since no single education policy is likely to affect such a broad range (almost 50%) of the education distribution. Instead, different policies (such as those designed to prevent high school dropout and those designed to encourage college graduates to enroll in postgraduate training) will affect different parts of the distribution. We view the quasi-experiment generated by the 1947 law change as a unique opportunity to assess whether education affects mortality across a broad range of the educational distribution. We believe our results will have implications for a variety of K-12 and other (e.g., pre-school) policies aimed broadly at the bottom half of the education distribution. The large fraction of the UK educational distribution affected by this law can also be contrasted with the relatively small fraction of the US educational distribution affected by US compulsory school laws (typically around 5% according to Lleras-Muney, 2005).

The Reduced-Form Relationship between Birth Cohort and Mortality

To start, we present graphically the relationship between birth cohort and mortality in Figure 4 by sex. In each panel of Figure 4, we display the fraction of the birth cohort dying between 1970 and 2003. About one-quarter of the affected male cohort died between 1970 and 2003. For females, this figure is smaller; only about 15 percent of the females born in the second quarter of 1933 died during this same period. These fractions vary considerably with birth cohort; from nearly 50 percent for male cohorts in the early 1920s to 5 percent for cohorts in the early 1950s. This dramatic difference is not surprising as the risk of death rises sharply with age. Later we will control for these age-related mortality trends by including various controls for age in the regressions.

[19] See for example Card (1999).

The main interest in this figure is whether there is a discernable difference in the mortality risk of cohorts born just after 1933 quarter 2 versus those born just before this time. Apart from the general downward trend in the mortality rate, visually, there appears to be little effect of the compulsory schooling reform on mortality between 1970 and 2003 for both males and females.

This aggregated analysis may cloud effects for particular types of diseases. Cutler, Deaton, and Lleras-Muney (2006) find that the education gradient in health is steeper for knowledge-intensive, technology-intensive diseases (e.g., diabetes). As such, it is plausible that the effect of education may be heterogenous across diseases. In Figures 5a-c, we present evidence on this hypothesis by considering mortality rates for different general causes of death – circulatory, pulmonary, and non-circulatory, non-pulmonary. The most common circulatory diseases resulting in death are heart disease and stroke. The most common pulmonary diseases resulting in death are pneumonia, nearly a half of all cases, cancers and chronic obstructive pulmonary disease, which includes chronic bronchitis and emphysema. Again, for all these causes of death for both males and females, there is no discrete difference in the mortality trend beginning with the 1933 quarter 2 cohort.

The figures presented thus far do not allow us to discern whether the effects of education vary by age. To address this possibility, we plot mortality rates by birth cohort for 3 11-year time periods (1970-1981, 1981-1992, 1992-2003) in Figures 6a-c. Earlier quasi-experimental work on the effect of compulsory schooling on mortality in the United States (Lleras-Muney, 2005 and Mazumdar, 2007) finds that the effects of compulsory schooling on mortality are largest for the 55-64 years old but only slightly larger than those for the 35-54 year olds. Using this research design, interestingly and quite surprisingly, the effect of schooling on mortality is nearly zero for the 65-89 year olds. If these results are generalizable to our study, we should expect that the effects of the reform would be largest for the 1992-2003 time period. For each of the three time periods, the effect of the reform appears to be quite homogenous; there is no striking discontinuity in mortality risk for the cohort born in 1933 quarter 2 relative to the cohort born in 1933 quarter 1.

In Tables 2a and 2b, we provide the reduced-form estimates of equation (2) for males and females, respectively. Specifically, these estimates are regression discontinuity estimates of the effect of the change in compulsory schooling on mortality using the data shown in Figures 4 through 6. We examine the effects for four different time periods – the entire 1970-2003 period, 1970-1981, 1981-1992, and 1992-2003 and several different causes of death. All regressions include age, as measured in years, fixed effects, so effectively the estimates are identified off of within-birth-year variation. We present the estimates with the 1929-1939 birth cohorts, in addition to the main 1921-1951 birth cohort sample, as a robustness check to insure that our estimates isolate an effect local to the compulsory schooling change threshold.

Given the common perception that education reduces mortality risk, we would expect that the post-reform cohorts would have a reduced likelihood of dying. However, this is not what we observe. Nearly all of the coefficients in Tables 2a and 2b are positive. Most of them are small and insignificant. For instance, the results for overall mortality for men using the 1921-1951 cohorts imply that cohorts who were just subject to the compulsory schooling room had 0.7 percentage point or 3 percentage point higher likelihood of dying between 1970 and 2003 relative to those born just before. This estimate is measured with precision, ruling out a negative reduced-form effect. The estimates are relatively impervious to the sample used and the inclusion of age controls suggesting that specification error is not driving the results.[20]

Examining the effects of the compulsory schooling age change on mortality rates between 1970 and 2003 effectively combines the effects of compulsory schooling on both early-life and later-life mortality. It is plausible that the effects of schooling on mortality are not felt until later in life

[20] Estimates without age fixed effects are nearly identical to those in Tables 2a and 2b.

although as mentioned earlier, existing studies (Lleras-Muney, 2005 and Mazumdar, 2007) suggest that the biggest effects of compulsory schooling are experienced between 35 and 64, equivalent to the 1968-1997 time period. To estimate the life-cycle effects of compulsory schooling, we estimate the effects of the compulsory schooling reform separately for the 1970-1981, 1981-1992, and 1992-2003 time periods. These reduced-form effects, if anything, suggest that the effect of education on mortality increases throughout the lifecycle, meaning that an extra year of education increases mortality at an increasing rate throughout one's life.

To ensure that our results are robust to the chosen polynomial, we present analogous estimates to Tables 2a and 2b using birth cohort polynomials of different degrees. Due to space concerns, we only present those estimates using mortality rates between 1970 and 2003 as the dependent variable.[21] In Appendix Tables 1a and 1b, we present these reduced-form estimates.[22] Most of the estimates are stable across the specifications. There is one slight exception – the 5th order polynomial for the 1929-1939 sample which suggests a large negative effect of education on mortality. However, this polynomial appears to overfit the data; such a large effect is not visually transparent in Figures 4 and 5. In sum, Tables 2a and 2b corroborate the visual evidence presented in Figures 4-6 that the reform had little effect on mortality risk. One concern with this type of analysis is sample selection; we only observe the cohorts if they survive to 1970. Later, we will test whether sample selection is problematic.

The Relationship Between Age Left Full-Time Education and Self-Reported Health and Health-Related Behaviors

Since the reform does not appear to have affected mortality, we investigate its effects on other health outcomes without concerning ourselves with sample selection issues. The health outcomes that we look at are a subset of the large number contained in the Health Surveys of

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[21] Estimates are similarly robust using each of the 11-year time period mortality rates as dependent variables.
[22] We present only the estimates for which we control for age.

England and fall into three categories: self-reported health, including reported of general health and specific diseases; self-reported cigarette smoking behavior and measured BMI and systolic blood pressure. The self-reported health measures have been shown to be strongly predictive of mortality (Ilder and Benyamini, 1997) while smoking is a well-known cause of morbidity and mortality. Since Johnstone et al (2007) have shown that the SES gradient in the HSE data differ according to whether one considers self-reported or objectively measured outcomes, the objectively measured BMI and blood pressure outcomes should prove useful addition to these self-reported measures.

Using data from all waves of the HSE from 1991 to 2004, Figure 7a plots the fraction of each quarterly birth cohort that self-reports having "fair", "bad" or "very bad" health (the residual categories are "good" and "very good"). Although this graph is similar to the mortality graphs presented above, an important difference between the mortality and the HSE data is the existence of multiple waves of HSE data. Since these allow us to observe the same outcome more than once (for example, we observe self-reported health of the 1933Q2 cohort in 1991 and 1992), we can compare outcomes among different birth cohorts evaluated at the same age (for example, we observe self-reported health at age 60 among the 1933Q2 cohort in 1993 and self-reported health at age 60 among the 1934Q2 cohort in 1994). This allows us to add age to the set of control variables (X) in models (1) and (2) which implies that we are using age-adjusted outcomes to identify the effects of the reform

Since we will use age-adjusted outcomes to identify the effects of the reform, Figure 7b plots the age-adjusted fraction of each quarterly birth cohort that self-reports having "fair", "bad" or "very bad" health. The fraction reporting fair or bad health might be smaller among later cohorts both because these cohorts are younger at the time of the survey and because later cohorts are, at the same age, healthier (because of advances in medical technology for example). In contrast, the age-adjusted outcomes will only display a downward trend if later cohorts are at the same age healthier, and Figure 7b suggests this is not the case. Moreover, based on the visual evidence in this graph, there does not appear to be any "jump" in age-adjusted health at the school reform threshold for either males or females.

Table 3 reports various estimates relating to the impact of education on this outcome. Although the graphs showed the relationship between birth cohort and this outcome separately for men and women, the Table reports estimates from models that pool males and females (and include a separate dummy variable for sex). Although we began by estimating separate models, we could not reject the hypothesis that the estimated effects of the reform were the same for men and women and so we pooled the data. This generates larger cell sizes and more precise estimates of the effects of the reform. It is worth bearing in mind that while sample sizes in the HSE are relatively large (around 500 individuals per birth cohort), they are tiny in comparison with the mortality analysis samples, which effectively contain all individuals born in each quarter (roughly 100,000). The dependent variable mean reported in Table 3 suggests that roughly one third of those born in the second quarter of 1933 have (self-reported) fair or bad health. This number can be compared with the least squares estimate reported in column (1), which implies that an additional year of education reduces the probability of reporting fair or bad health by 9 percentage points or roughly 25% of this mean.[23]

Since this least squares estimate is likely biased towards finding education effects by the correlation between education and unobserved determinants of health (such as family background), column (2) reports the reduced-form regression discontinuity estimate of the effect of being born after the second quarter of 1933 (and affected by the reform). In contrast to the least squares estimate, this estimate is negative but close to zero. The instrumental variables estimate reported in

[23] Since the "age left full-time education" variable is coded as 14, 15, ..19+, we provide least squares estimates for the subsample of individuals that leave full-time education at sixteen or less. As shown above, all of the effects of the reform were felt by students between the ages of 14 and 16.

column (3) implies that an additional year of education reduces the probability of reporting fair or bad health by only 1%. Although we cannot reject that the effect is zero, we can reject effect sizes of the magnitude suggested by the least squares estimates. Repeating the reduced form and IV estimates on a smaller sample closer to the reform threshold produces similar point estimates, albeit with larger standard errors (columns (4) and (5)). Repeating the analysis for the outcomes "bad or very bad" health, "reduced activity in the last fortnight" and "longstanding illness" points to similar conclusions: large and negative least squares estimates and small and usually negative IV estimates. These IV estimates can never be distinguished from zero and typically allow us to rule out effects comparable to the least squares estimates. Only when we look at specific illnesses (cancer and hypertension) do these patterns cease to hold, and in this case even the least squares estimates are small. As others have noted, it may be that more educated people are more likely to be diagnosed and report having these illnesses, offsetting the effects of education in reducing the probability of contracting them.

Turning to self-reported smoking behavior (Table 4), we classify respondents into three mutually exclusive and exhaustive categories: "started smoking, currently smoking", "started smoking, stopped smoking" and "never smoked".[24] Starting with the probability of reporting being in the first of these categories, the results are consistent with those relating to self-reported general health. The least squares estimates suggest that an additional year of education reduces the probability of currently smoking by almost six percentage points, a very large effect off a mean of only 20% for those born in the second quarter of 1933. In contrast, and consistent with Figure 7, the instrumental variables estimates point to effect sizes around half of this size, becoming even smaller when the narrower interval is used. While the ordinary least squares estimates cannot be

[24] It is possible that some of those classified as "started smoking, stopped smoking" currently smoke pipes or cigars, since the "currently smoking" question relates only to cigarettes while the "ever smoked" question includes pipes and cigars.

ruled out, the evidence is consistent with that reported for general health and points to least squares estimates as being driven largely be selection biases.

The smoking results are complicated by the relatively large fraction (almost 50% that report) having smoked at some time but not currently. The least squares estimate of the effect of an additional year of education on the probability of belonging to this category is positive, suggesting that education increases the likelihood of taking up smoking. Interestingly, the instrumental variables estimates tell a similar story and suggest these positive effects may be larger than the least squares estimates suggest. This kind of positive education effect has been found for the probability of trying soft drugs (Cutler and Lleras-Muney, 2006) and it may be that similar mechanisms are at work.

Turning finally to objectively measured health, Figures 9a and 9b plot average age-adjusted BMI and blood pressure by quarterly birth cohort. The corresponding regression estimates are reported in Table 5. Both the graphs and the reduced-form regressions estimates point against education lowering BMI or blood pressure. Indeed, both effects are positive, although small and statistically indistinguishable from zero. These contrast with the least squares estimates, which again point to education as having large and beneficial effects on health, this time in terms of lower BMI and blood pressure. Transforming these continuous variables into indicators of high BMI (obesity) and high blood pressure (stage 1 hypertension) does not eliminate the negative education gradient associated with BMI, although the associated IV estimates are positive. While these seem implausibly large, they are also relatively imprecise and sensitive to the cutoff used to define obesity. In the case of objectively measured hypertension, as with self-reported hypertension, neither the least squares nor the IV estimates suggest significant effects of education.

To summarize, estimates from the HSE paint a relatively consistent picture. This consists of large least squares estimates pointing towards education having important beneficial effects on self-reported and measured health outcomes and health behaviors and reduced form and IV estimates suggesting that most of these apparent benefits are driven by selection bias. Nevertheless, instrumental variables point estimates typically suggest smaller beneficial education effects, and since sample sizes are not large enough to estimate these effects as precisely as we were able to estimate mortality effects, we cannot rule out important effects of education on health. Whether these will eventually result in mortality effects will only become clear in 10 or 20 years time, but for now we interpret these results as consistent with our mortality analysis. In the next subsection we check that both of these are robust to various robustness checks.

Robustness Checks: Mortality 1947-70

One of the disadvantages of the ONS quarter-of-birth mortality data is that they begin in 1970, over twenty years after the compulsory schooling reform. If education impacts early-life mortality, then the ONS estimates may be subject to sample selection bias. To address this issue, we plot 1947-1970 mortality rates by year of birth using data from the Human Mortality Database in Figure 10. Unfortunately, this data source does not provide data by quarter of birth or cause of death.[25] The first point to note is that mortality rate for the affected group during this time period is rather low; only 3 percent of the cohort died between 1947-1970, so we would suspect that sample selection bias, if truly a concern, would be small. However, for both males and females, the 1947-1970 mortality rates are smooth functions of year of birth that exhibit no break in trend starting with the 1933 cohort. In sum, these figures suggest that sample selection bias in the post-reform era is small.

To test for sample selection bias more formally, we present reduced-form estimates of the effect of the compulsory schooling change on 1947-1970 mortality rates in Tables 6a and 6b for

[25] We note that the first-stage estimates using comparisons by year of birth are similar to those using quarter-of-birth comparisons.

males and females, respectively.[26] Given that the year of birth analyses are imperfect because the 1933 quarter 1 cohort is subject to a compulsory schooling age of 14 whereas the 1933 quarter 2 and later cohorts are subject to a compulsory schooling age of 15, there are three reduced-form estimates in each table. The first estimate treats the entire 1933 cohort as treated, the second estimate excludes the 1933 cohort from the estimation, and the third estimate assigns a treatment "dummy" of ¾ to the 1933 cohort and 1 for later cohorts.[27]

For women, the discontinuity point estimates are small and insignificant across all three specifications. For men, the estimates exhibit more variation, but in two of the three cases are statistically insignificant. However, for males, the mortality rates are not as smooth functions of year of birth as for the females, so it is difficult to fit a global polynomial to the year-of-birth mortality rates with so few data points (i.e., 26 birth cohorts). Some of the variation in mortality rates between 1947 and 1970 may be an after-effect of World War II as some of the earlier cohorts were involved in World War II. Since such a small fraction of the affected cohorts died between 1947 to 1970, none of the estimates appear to be large enough to warrant serious concerns about sample selection.

Robustness Checks: Pre-determined characteristics

The underlying assumption in the regression discontinuity approach is that the cohort just subjected to the compulsory schooling age of 15 (e.g., 1933 quarter 2 birth cohort) is exchangeable with the last cohort subjected to the compulsory schooling age of 14 (e.g., 1933 quarter 1 birth cohort). Ultimately this implies that the cohort just to the left of the compulsory schooling change

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[26] We exclude the 1947-1951 cohorts from this analysis because (1) 1948-1951 cohorts were born after 1947 and (2) members of the 1947 cohort were infants and 4.7 percent of the cohort died in the first year of life, a statistic much higher than the risk of death for most of the cohorts in Figure 10.

[27] We have also estimated the first stage relationship between yearly birth cohort and educational attainment using these three methods. Using the General Household Survey, we find that the first stage using the quarterly birth cohort is roughly the same as those using the yearly birth cohort (1) excluding the 1933 birth cohort or (2) using ¾ treatment dummy correction.

threshold should be similar along pre-determined characteristics as the cohort just to the right of the threshold. Unfortunately as many of the characteristics we observe among these cohorts as adults may be functions of educational attainment, it is rather difficult to test whether the pre-reform and post-reform cohorts are exchangeable. However, one compelling test is to test whether their pre-reform mortality rates, adjusting for age, are similar.

In Figure 11, we plot mortality rates between birth and 1947 for 1921 to 1946 cohorts.[28] The patterns by yearly birth cohort are less smooth than those in Figure 10, especially for men. This is not surprising, as cohorts born up to the late 1920s were at risk of dying in World War II, and World War II also affects infant mortality rates. Given the sparseness of the data (i.e., 26 data points), it is difficult to discern the patterns. However, in neither plot, we observe no sharp discontinuity in mortality rates beginning with the 1933 birth cohort.

In Tables 7a and 7b, we fit regression functions to these data and look at the reduced-form impact of the compulsory schooling reform on mortality between birth and 1947. For men, all of the reduced-form estimates are statistically significant and large. The point estimates imply that cohorts just subject to the reform had roughly a 15 percent lower risk of mortality than those cohorts born slightly earlier and not subjected to the reform. For women, the story is different. The reduced-form effects are much smaller in size. The regression discontinuity estimates suggest that the difference in pre-reform mortality between the just-affected and the just-unaffected cohorts was a negative 2 percent. The obvious conclusion is that based on only 26 observations, we do not have enough degrees of freedom to specify a polynomial that is flexible enough to account for the highly nonlinear mortality patterns driven by World War II.

Ideally, we would like to test the validity of our research design by comparing characteristics based on quarterly birth cohorts rather than yearly birth cohorts. We do this in Figures 12 and 13,

[28] We exclude the 1947 and later cohorts as most of them were not born as of the beginning of 1947.

using administrative data to show that quarterly birth cohort characteristics evolved smoothly during 1933. In particular, it does not appear there were no sharp changes in cohort size, sex ratios, infant health (as measured by fraction stillborn) or illegitimacy rates. Regression-based estimates (available from the authors upon request), also suggest that there were no significant trend breaks associated with the second quarter of 1933.

Robustness Checks: Migration

A final concern might be that our results are clouded by differential rates of emigration across these cohorts. This would occur if the extra year of education affected the propensity to emigrate. The maintained assumption that there were no sharp changes in the fraction of immigrants in the resident population across these birth cohorts seems a lot more robust (it is hard to imagine why the fraction of immigrants would change across these quarters), but is also worth checking.

We examine migration in Figures 14a-c. Figure 14a uses Canadian Census data to examine the fraction of those born in England and Wales in a particular year that appear in the 1996 Canadian Census. There are clear migration trends visible on these graphs, but the overall proportion that migrates to Canada is relatively small (around 2%) and there does not appear to be any trend break around 1933. This is even more apparent in Figure 14b, which takes a similar approach to migration to the US, where migration is this time defined on the basis of quarterly birth cohorts. Finally, Figure 14c uses the HSE to show that there is no trend break in the fraction of foreign-born residents in England and Wales born after 1933. Again, regression-based estimates (available from the authors upon request) suggest that there were no significant trend breaks associated with the second quarter of 1933.

VII. CONCLUSION

The results presented here suggest that the 1947 UK policy reform, which increased the compulsory schooling age from 14 to 15, is a useful tool for estimating the effect of education on adult health outcomes including mortality. In particular, the reform had a huge impact on school completion decisions and lead to stark and dramatic differences in age at school exit by birth cohort. Despite these effects on education, we observe no differences by birth cohort in mortality risk, self-reported health, and smoking and drinking behaviors. These results contrast with the findings of earlier studies.

One possible reason for this difference are the different settings considered. Unlike the setting considered here, treated and untreated US cohorts were not necessarily covered by health insurance. However, it is unlikely that the different results are driven solely due to the availability of insurance. The difference in the cultures of the two countries may also be an explanation. Clearly, this is an interesting and important avenue for future work.

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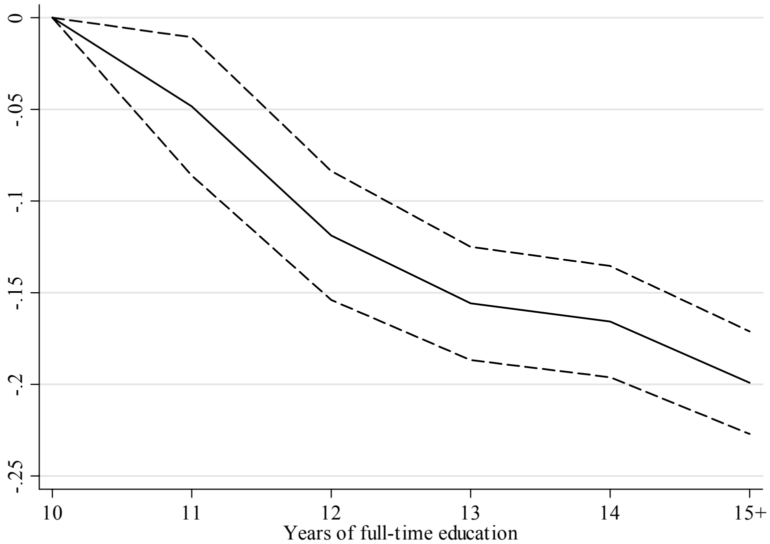
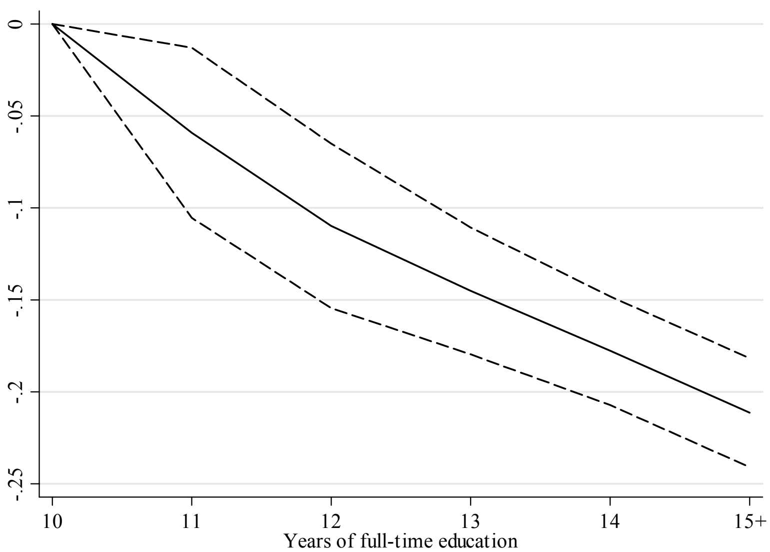
Figure 1a: The education-health gradient in England: Fair or bad health



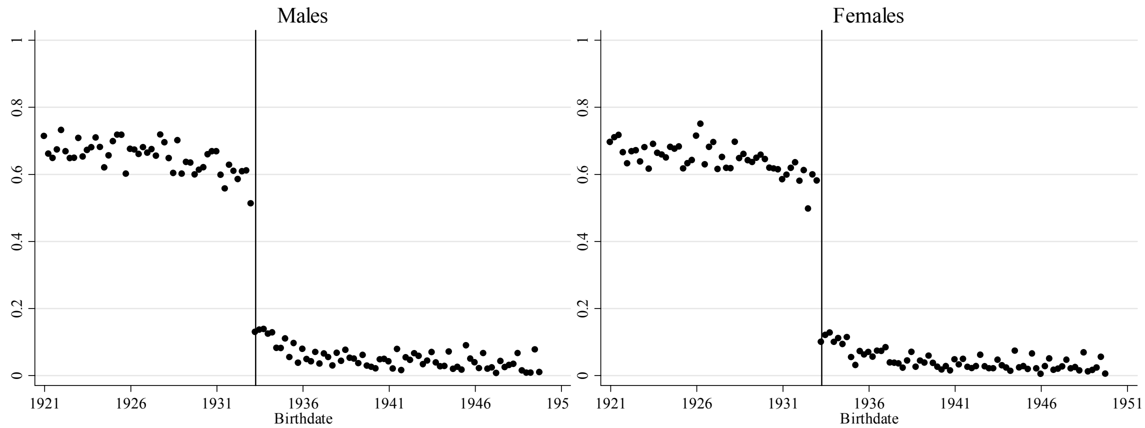
Notes: The graph shows the marginal effects (and 95% confidence interval) of additional years of education derived from a logit model of the probability of reporting being in "fair", "bad" or "very bad" health (the other categories are "good" and "very good"). The model also includes year of birth, year of survey and sex dummy variables. Data on those aged 25 or older from the 2000 Health Survey of England. N=6445. The HSE asks respondents at what age they completed full-time education. The answers have been recoded such that leaving full-time education at age 14, 15, ..19+ corresponds to 10, 11, ..15+ years of completed education.

Figure 1b: The education-health gradient in England: Currently smoking



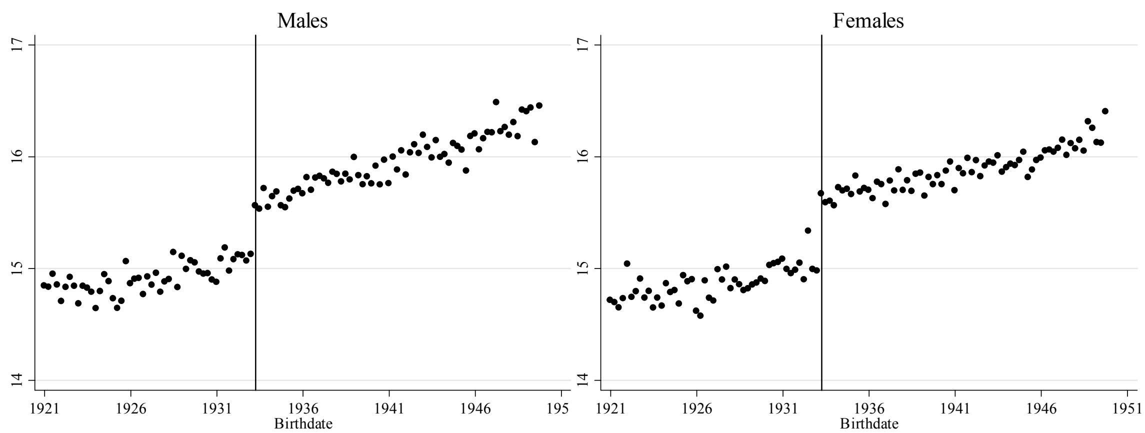
Notes: the graph shows the marginal effects (and 95% confidence interval) of additional years of education derived from a logit model of the probability of reporting "currently smoking". The model also includes year of birth, year of survey and sex dummy variables. Data on those aged 25 or older from pooled waves of the Health Survey of England 1991-2004. N=6414. The HSE asks respondents at what age they completed full-time education. The answers have been recoded such that leaving full-time education at age 14, 15, ..19+ corresponds to 10, 11, ..15+ years of completed education.

Figure 2a: Fraction leave full-time aged 14 or lower



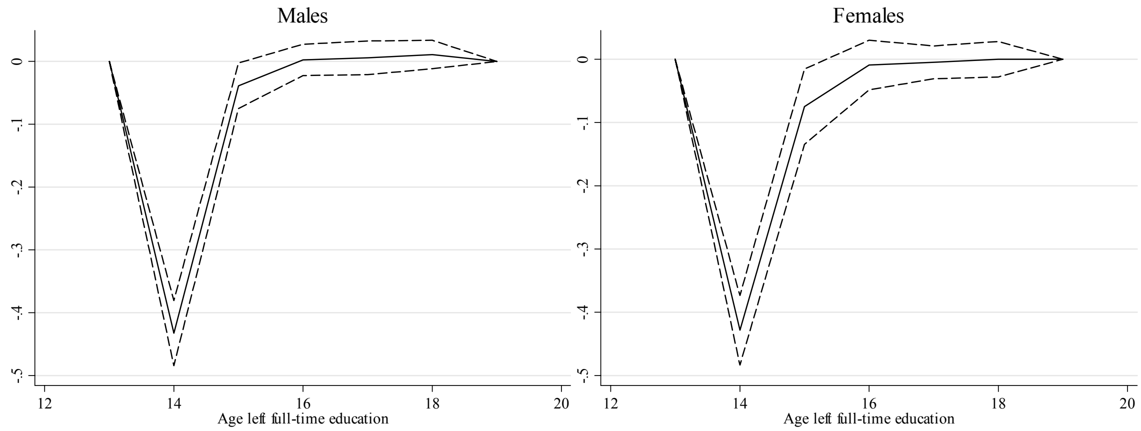
Notes: The dots are the fractions of each quarterly birth cohort that report leaving full-time education aged 14 or lower. Data from pooled waves of the Health Survey of England 1991-2004. N=25,736 (males), 29,956 (females). The vertical lines correspond to the second quarter of 1933.

Figure 2b: Age left full-time education



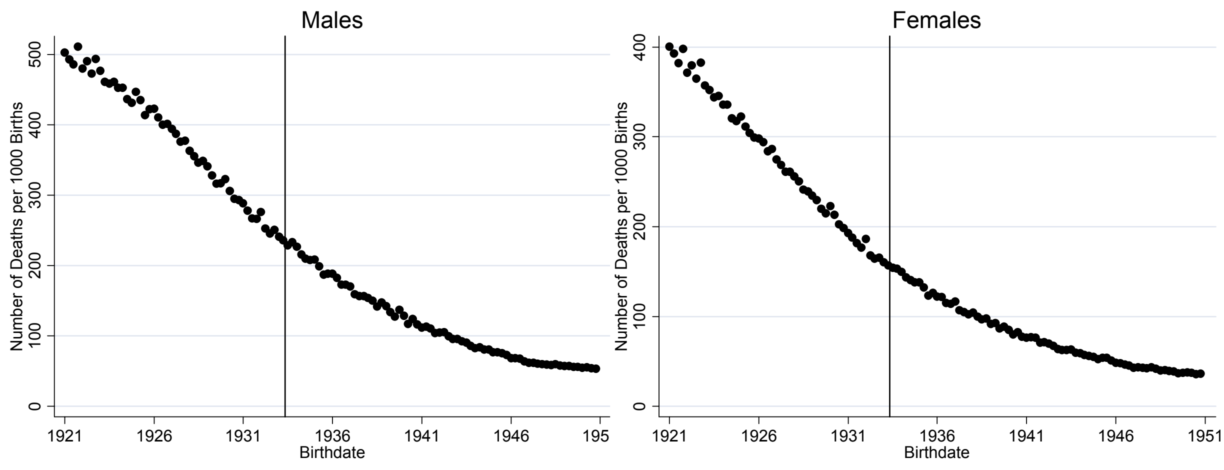
Notes: The dots are the means of "age left full-time education" among respondents in each quarterly birth cohort. Data from pooled waves of the Health Survey of England 1991-2004. N=25,736 (males), 29,956 (females). The vertical lines correspond to the second quarter of 1933.

Figure 3: Distribution of estimated education effects



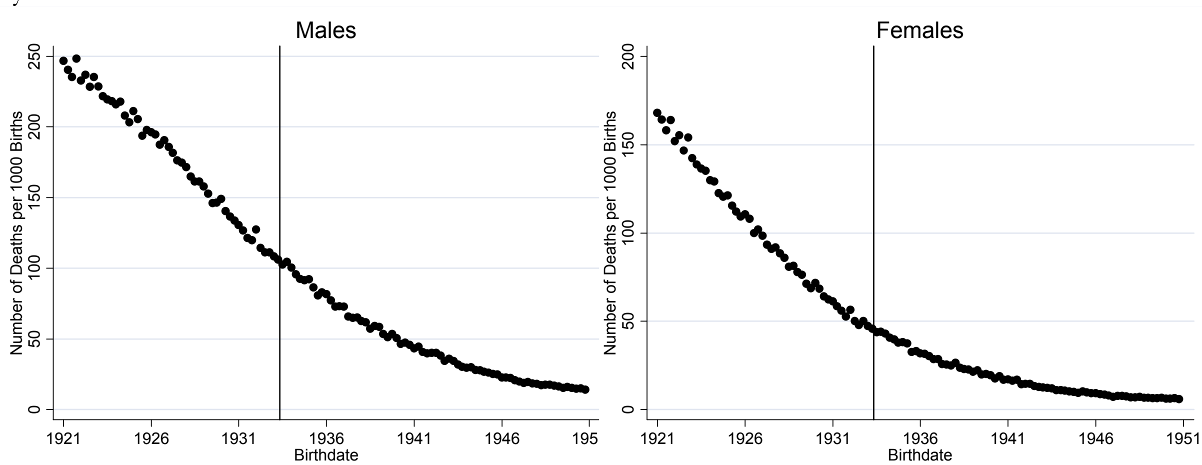
Note: Graphs show the estimated effect (and the 95% confidence interval) of the compulsory school reform on the fraction leaving full-time education at age less than or equal to 13, 14,..19. For example, the reform is estimated to decrease the fraction leaving education at age less than or equal to 14 by slightly more than 0.4. See Tables 1a and 1b for the associated estimates and sample sizes.

Figure 4: Overall Mortality Rates (Deaths/1000 Births) between 1970 and 2003 by Birth Cohort



Notes: The dots are the mortality rates (deaths/1000 births) of each quarterly birth cohort for the years 1970 to 2003. Data are from Office of National Statistics mortality data. The vertical lines correspond to the second quarter of 1933.

Figure 5a: Circulatory Disease Mortality Rates (Deaths/1000 Births) between 1970 and 2003 by Birth Cohort

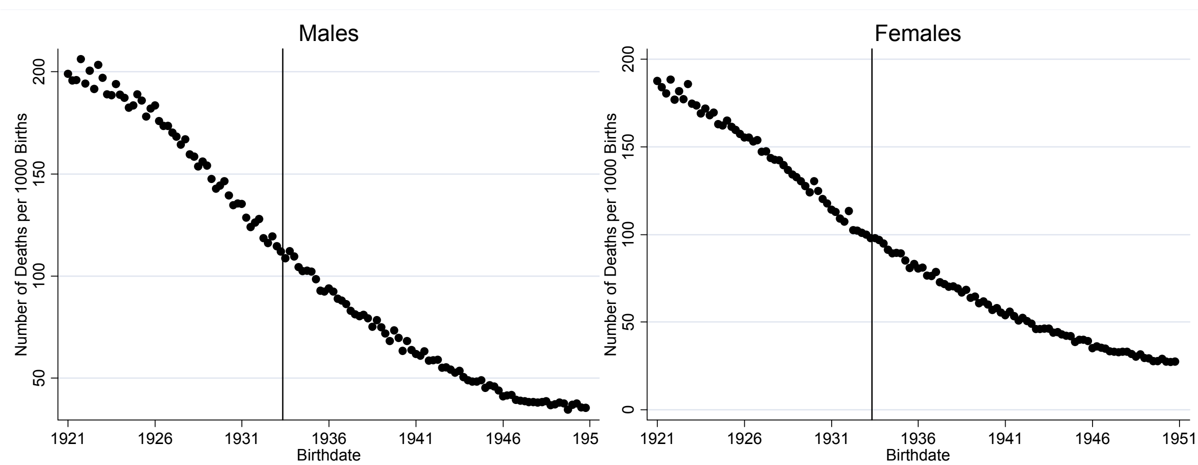


Notes: The dots are the circulatory disease mortality rates (circulatory disease deaths/1000 births) of each quarterly birth cohort for the years 1970 to 2003. Data are from Office of National Statistics mortality data. The vertical lines correspond to the second quarter of 1933.

Figure 5b: Pulmonary Disease Mortality Rates (Deaths/1000 Births) between 1970 and 2003 by Quarterly Birth Cohort

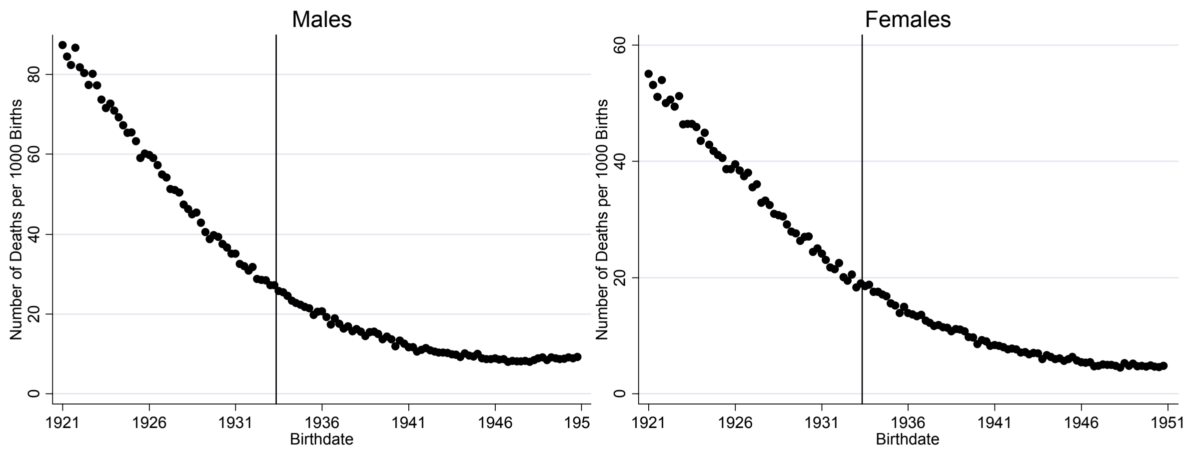
Notes: The dots are the pulmonary disease mortality rates (pulmonary disease deaths/1000 births) of each quarterly birth cohort for the years 1970 to 2003. Data are from Office of National Statistics mortality data. The vertical lines correspond to the second quarter of 1933.

Figure 5c: Non-Circulatory, Non-Pulmonary Mortality Rates (Deaths/1000 Births) between 1970 and 2003 by Quarterly Birth Cohort



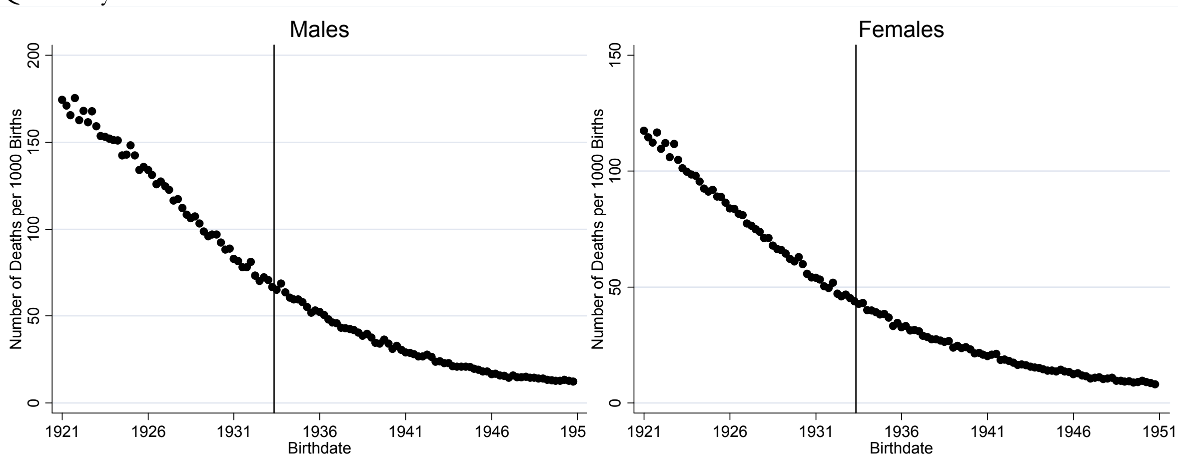
Notes: The dots are the non-circulatory, non-pulmonary disease mortality rates (non-circulatory, non-pulmonary disease deaths/1000 births) of each quarterly birth cohort for the years 1970 to 2003. Data are from Office of National Statistics mortality data. The vertical lines correspond to the second quarter of 1933.

Figure 6a: Overall Mortality Rates (Deaths/1000 Births) between 1970 and 1981 by Quarterly Birth Cohort



Notes: The dots are mortality rates (deaths/1000 births) of each quarterly birth cohort for the years 1970 to 1981. Data are from Office of National Statistics mortality data. The vertical lines correspond to the second quarter of 1933.

Figure 6b: Overall Mortality Rates (Deaths/1000 Births) between 1981 and 1992 by Quarterly Birth Cohort

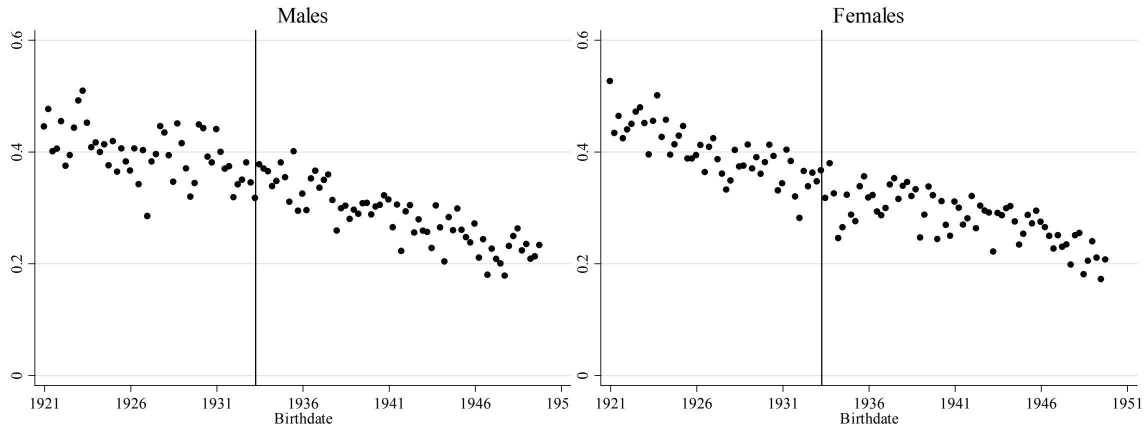


Notes: The dots are mortality rates (deaths/1000 births) of each quarterly birth cohort for the years 1981 to 1992. Data are from Office of National Statistics mortality data. The vertical lines correspond to the second quarter of 1933.

Figure 6c: Overall Mortality Rates (Deaths/1000 Births) between 1992 and 2003 by Quarterly Birth Cohort

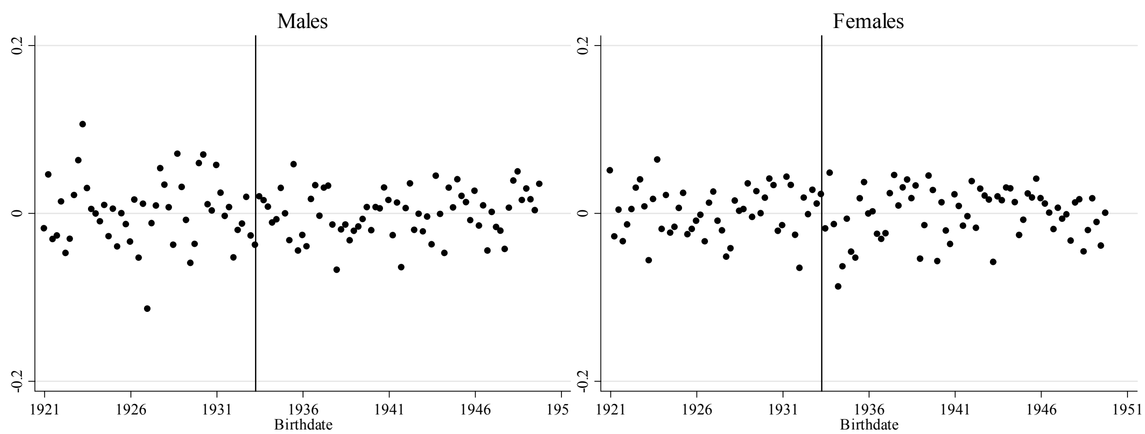
Notes: The dots are mortality rates (deaths/1000 births) of each quarterly birth cohort for the years 1992 to 2003. Data are from Office of National Statistics mortality data. The vertical lines correspond to the second quarter of 1933.

Figure 7a: Fair or bad health



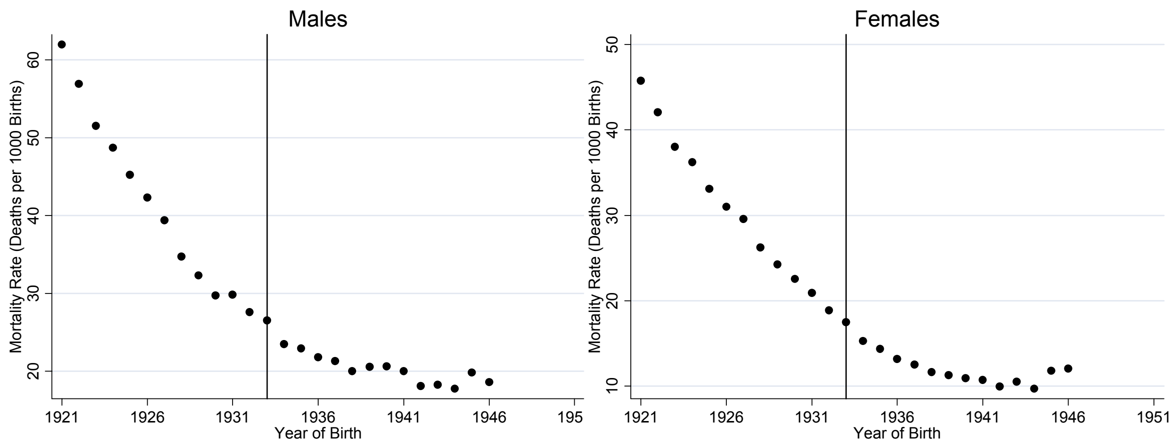
Notes: The dots are the fractions of each quarterly birth cohort that report being in "fair", "bad" or "very bad" health (the other categories are "good" and "very good"). Data from pooled waves of the Health Survey of England 1991-2004. N=25,736 (males), 29,956 (females). The vertical lines correspond to the second quarter of 1933.

Figure 7b: Fair or bad health (age-adjusted)



Notes: The dots are the regression-adjusted fractions of each quarterly birth cohort that report being in "fair", "bad" or "very bad" health (the other categories are "good" and "very good"). The regression adjustment is done for quarter of birth and age at the time of the survey. Age is measured in quarters and in both cases adjustment is done via the inclusion of a full set of dummy variables. Data from pooled waves of the Health Survey of England 1991-2004. N=25,736 (males), 29,956 (females). The vertical lines correspond to the second quarter of 1933.

Figure 10: Overall Mortality Rates (Deaths/1000 Births) between 1947 and 1970 by Yearly Birth Cohort

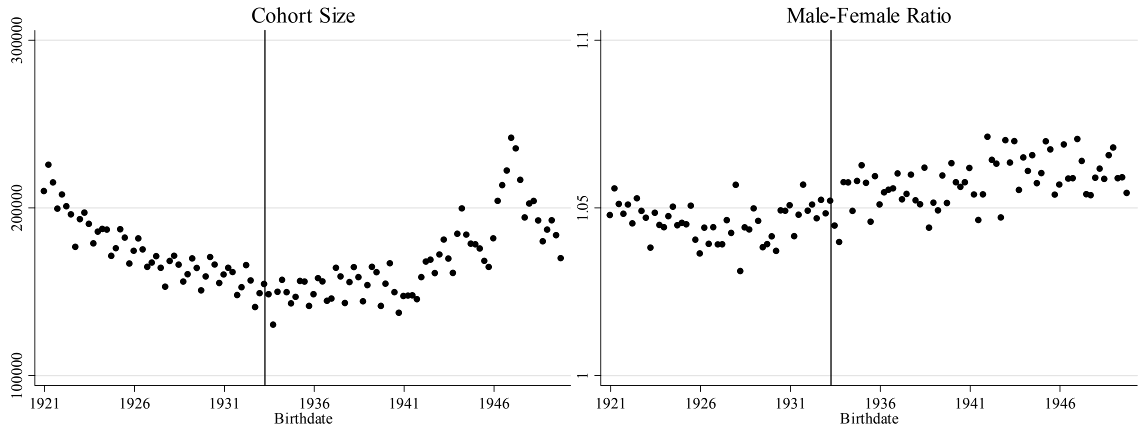


Notes: The dots are mortality rates (deaths/1000 births) of each yearly birth cohort for the years 1947 to 1970. Data are from Human Mortality Database. The vertical lines correspond to the year 1933.

Figure 11: Overall Mortality Rates (Deaths/1000 Births) between Birth and 1947 by Yearly Birth Cohort

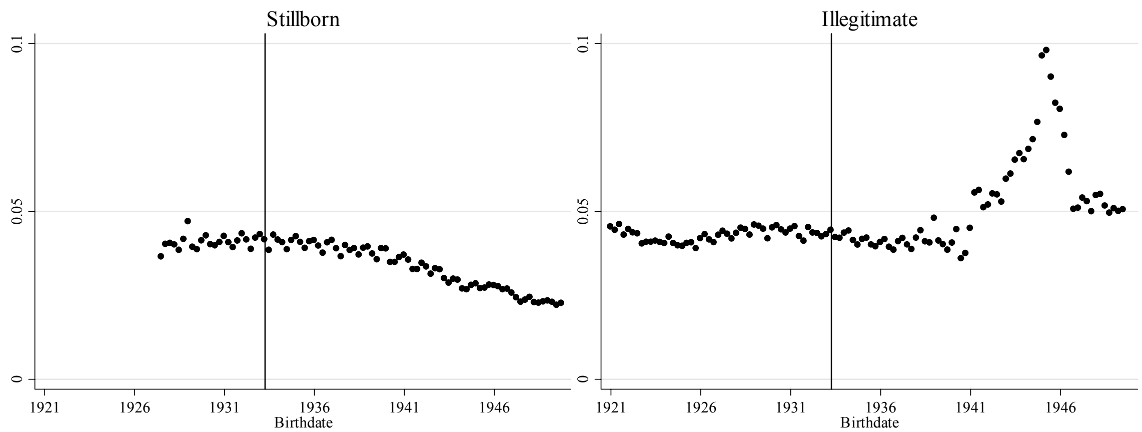
Notes: The dots are mortality rates (deaths/1000 births) of each yearly birth cohort between birth and 1947. Data are from Human Mortality Database. The vertical lines correspond to the year 1933.

Figure 12a: Robustness check: cohort size and male-female ratio



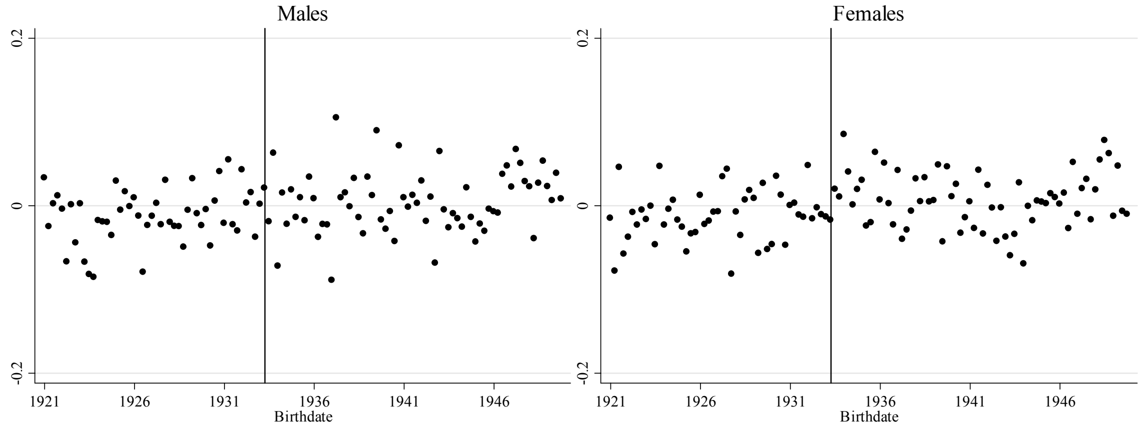
Notes: birth counts by quarter, disaggregated by sex from the Registrar General's Quarterly Return.

Figure 12b: Robustness check: fraction of births stillborn and illegitimate



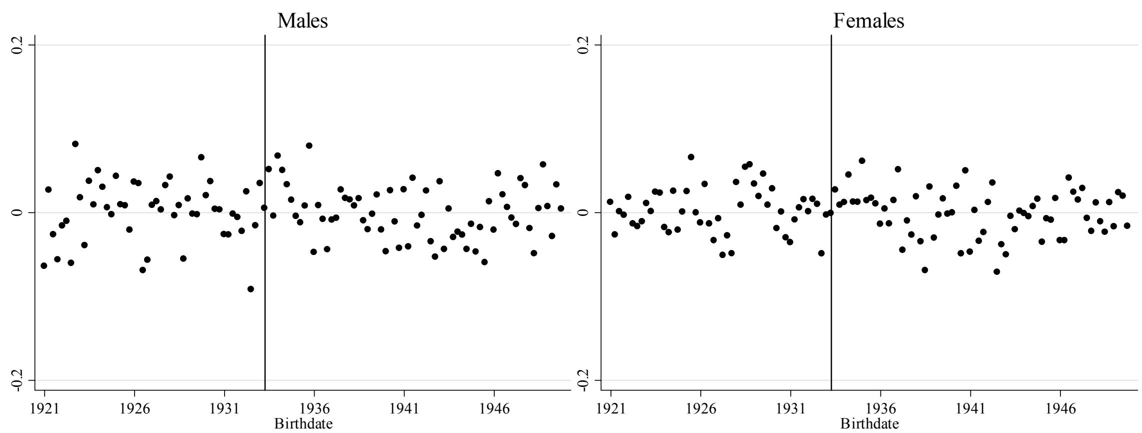
Notes: data from the Registrar General's Quarterly Return.

Figure 13a: Mother smoked (age-adjusted)



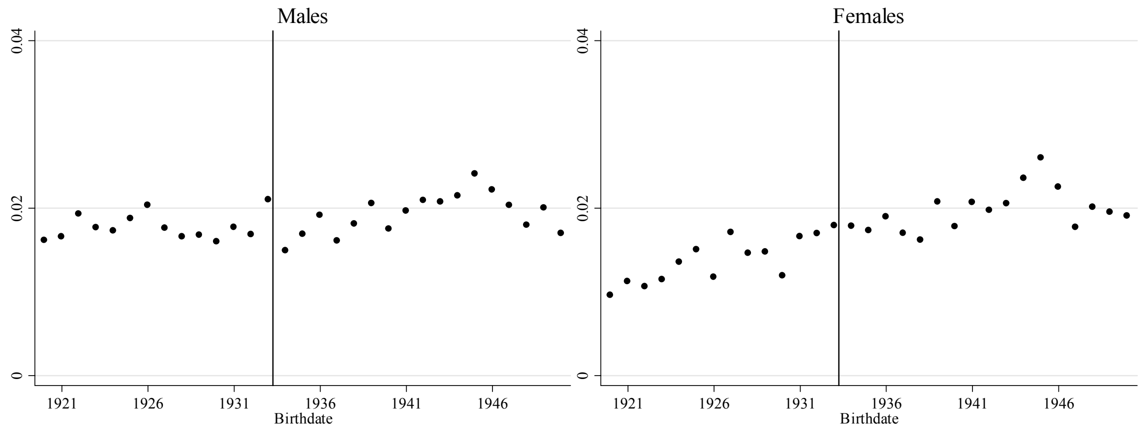
Notes: The dots are the regression-adjusted fractions of each quarterly birth cohort that report that their mother smoked when they were a child. The regression adjustment is done for quarter of birth and age at the time of the survey. Age is measured in quarters and in both cases adjustment is done via the inclusion of a full set of dummy variables. Data from pooled waves of the Health Survey of England 1991-2004. N=18,801 (males), 22,275 (females). The vertical lines correspond to the second quarter of 1933.

Figure 13b: Father smoked (age-adjusted)



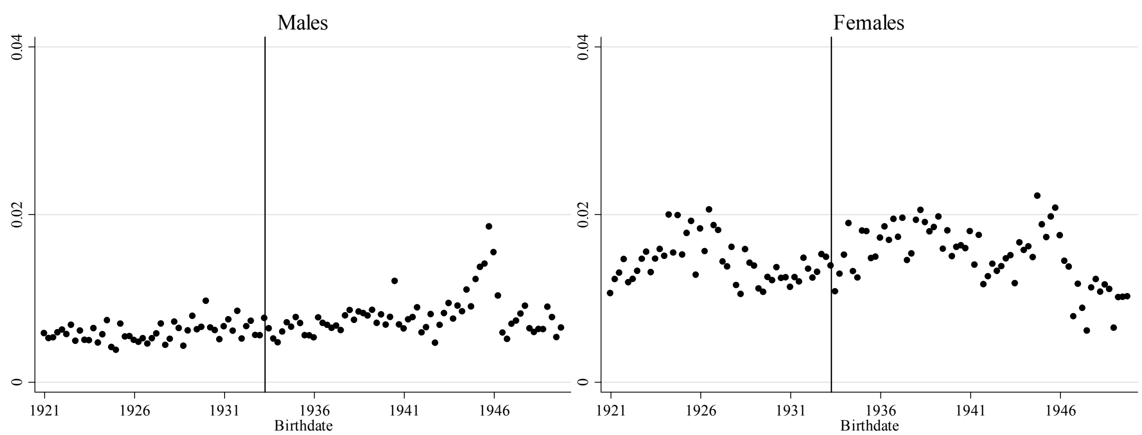
Notes: The dots are the regression-adjusted fractions of each quarterly birth cohort that report that their father smoked when they were a child. The regression adjustment is done for quarter of birth and age at the time of the survey. Age is measured in quarters and in both cases adjustment is done via the inclusion of a full set of dummy variables. Data from pooled waves of the Health Survey of England 1991-2004. N=18,801 (males), 22,275 (females). The vertical lines correspond to the second quarter of 1933.

Figure 14a: Robustness check: proportion of each year-of-birth cohort observed in the 1996 Canadian Census



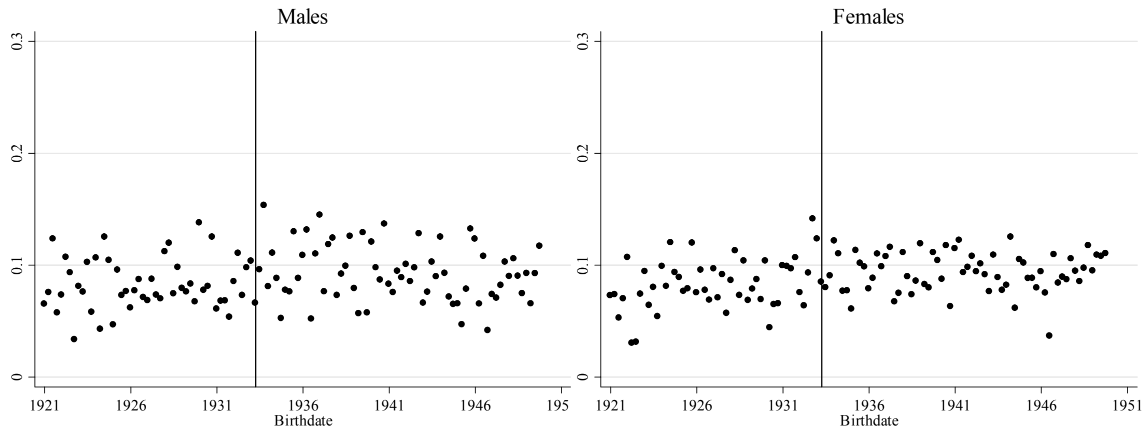
Notes: the graphs show the estimated number of citizens in the 1996 Canadian census (based on the 3% PUMS data) born in the UK as a fraction of the number of live births in England and Wales in the relevant quarter.

Figure 14b: Robustness check: proportion of each quarter-of-birth cohort observed in the 1980 US Census



Notes: the graphs show the estimated number of citizens in the 1980 US census (based on the 5% PUMS data) born in England and Wales as a fraction of the number of live births in England and Wales in the relevant quarter.

Figure 14c: Born outside of the UK



Notes: The dots are the fractions of each quarterly birth cohort that report being born outside of the UK. Data from pooled waves of the Health Survey of England 1991-2004. N=20,405 (males), 23,362 (females). The vertical lines correspond to the second quarter of 1933.

Table 1a: Effects of the Compulsory School Change on Educational Achievement of Males

	Left aged<=14		Left aged<=15		Left aged<=16		Left aged<=17		Left aged<=18		Age left FTE	
	1920-50	1929-39	1920-50	1929-39	1920-50	1929-39	1920-50	1929-39	1920-50	1929-39	1920-50	1929-39
Post-Change Dummy	-0.432	-0.376	-0.039	-0.055	0.002	-0.014	0.006	-0.004	0.011	-0.012	0.452	0.461
	(0.026)	(0.043)	(0.018)	(0.027)	(0.012)	(0.016)	(0.013)	(0.022)	(0.011)	(0.019)	(0.050)	(0.070)
Mean of Dependent Variable	0.128	0.128	0.651	0.651	0.844	0.844	0.894	0.894	0.922	0.922	15.560	15.560
Observations	25736	9444	25736	9444	25736	9444	25736	9444	25736	9444	25736	9444

Notes: Data from pooled waves of the Health Survey of England 1991-2004. In each wave, respondents were asked "At what age did you leave full-time education?".

Table 1b: Effects of the Compulsory School Change on Educational Achievement of Females

	Left aged<=14		Left aged<=15		Left aged<=16		Left aged<=17		Left aged<=18		Age left FTE	
	1920-50	1929-39	1920-50	1929-39	1920-50	1929-39	1920-50	1929-39	1920-50	1929-39	1920-50	1929-39
Post-Change Dummy	-0.428	-0.458	-0.075	-0.135	-0.009	-0.044	-0.005	0.015	-0.000	0.014	0.517	0.608
	(0.028)	(0.024)	(0.030)	(0.022)	(0.020)	(0.021)	(0.013)	(0.015)	(0.014)	(0.024)	(0.090)	(0.085)
Mean of Dependent Variable	0.099	0.099	0.609	0.609	0.827	0.827	0.893	0.893	0.922	0.922	15.667	15.667
Observations	29956	10636	29956	10636	29956	10636	29956	10636	29956	10636	29956	10636

Notes: Table gives estimated effect of compulsory schooling law change on various indicators of educational achivement derived from the question "at what age did you leave full-time education?". All regressions estimated using pooled waves of the Health Survey of England. All regressions include a fully-flexible cubic polynomial in quarter of birth as well as quarter of birth (1, 2, 3, 4) dummies and age dummies (measured in quarters of a year). Robust standard errors are presented in parentheses. The post-1933 dummy is equal to 1 for cohorts born in the second quarter of 1933 and later and 0 otherwise. The mean of the dependent variable is measured for cohort born in the second quarter of 1933.

Table 2a
Reduced-Form Effects of Compulsory Schooling Change on Male Mortality for 4 Time Periods (1970-2003, 1970-1981, 1981-1992, 1992-2003)

	1921-1951 Birth Cohorts				1929-1939 Birth Cohorts			
All-Cause Mortality	1970-2003	1970-1981	1981-1992	1992-2003	1970-2003	1970-1981	1981-1992	1992-2003
Post-Change Dummy	6.98	0.80	-0.33	6.00	6.49	1.11	0.40	4.85
	(3.30)	(0.44)	(1.61)	(1.73)	(4.80)	(0.63)	(1.95)	(2.49)
Mean of Dependent Variable	235.76	27.24	66.58	140.76	235.76	27.24	66.58	140.76
Circulatory Disease Mortality								
Post-Change Dummy	4.01	0.81	0.02	3.00	5.21	0.88	0.60	3.61
	(1.73)	(0.32)	(0.92)	(0.85)	(2.53)	(0.53)	(0.79)	(1.68)
Mean of Dependent Variable	106.27	14.53	32.11	59.10	106.27	14.53	32.11	59.10
Respiratory Disease Mortality								
Post-Change Dummy	0.59	-0.21	0.62	0.14	-0.07	-0.04	0.67	-0.69
	(0.55)	(0.08)	(0.17)	(0.40)	(0.84)	(0.16)	(0.25)	(0.56)
Mean of Dependent Variable	17.41	1.25	3.13	12.93	17.41	1.25	3.13	12.93
Non-Respiratory, Non-Circulatory Disease Mortality								
Post-Change Dummy	2.57	0.35	-0.96	2.86	1.29	0.18	-0.87	1.93
	(1.60)	(0.23)	(0.74)	(0.89)	(1.94)	(0.30)	(1.32)	(0.85)
Mean of Dependent Variable	112.08	11.46	31.34	68.73	112.08	11.46	31.34	68.73

Notes: Table gives estimated effect of compulsory schooling law change on male mortality rates (deaths/1000 births) for the time period specified (i.e., 1970-2003, 1970-1981, 1981-1992, or 1992-2003). Specifically, it is the estimated coefficient on the variable that 0 for cohorts born before the 2nd quarter of 1933 and 1 for cohorts born during the 2nd quarter of 1933 and after. All regressions estimated using death and birth counts by quarter and year of birth. All regressions include a fully-flexible cubic polynomial in birthdate in quarters along with quarter of birth fixed effects. Robust standard errors are presented in parentheses. Data come from the 1970-2000 Office of National Statistics Death Files using those born between 1921 and 1951 (first set of columns) or using those born between 1929 and 1939 (second set of columns). The mean of the dependent variable is measured for persons born in the 2nd quarter of 1933.

Table 2b

Reduced-Form Effects of Compulsory Schooling Change on Female Mortality for 4 Time Periods (1970-2003, 1970-1981, 1981-1992, 1992-2003)

	1921-1951 Birth Cohorts				1929-1939 Birth Cohorts			
All-Cause Mortality	1970-2003	1970-1981	1981-1992	1992-2003	1970-2003	1970-1981	1981-1992	1992-2003
Post-Change Dummy	5.13	1.52	0.66	2.90	3.30	1.16	-1.33	3.23
	(2.52)	(0.39)	(0.72)	(1.69)	(4.42)	(0.81)	(1.40)	(2.40)
Mean of Dependent Variable	156.91	19.02	43.84	93.06	156.91	19.02	43.84	93.06
Circulatory Disease Mortality								
Post-Change Dummy	1.15	0.70	-0.30	0.80	-0.21	0.13	-1.14	0.91
	(0.95)	(0.27)	(0.25)	(0.69)	(1.92)	(0.54)	(0.53)	(1.12)
Mean of Dependent Variable	45.71	5.44	11.11	28.84	45.71	5.44	11.11	28.84
Respiratory Disease Mortality								
Post-Change Dummy	0.17	0.15	0.10	-0.04	0.85	0.22	-0.00	0.70
	(0.34)	(0.07)	(0.14)	(0.29)	(0.51)	(0.17)	(0.19)	(0.45)
Mean of Dependent Variable	13.32	1.05	2.56	9.66	13.32	1.05	2.56	9.66
Non-Respiratory, Non-Circulatory Disease Mortality								
Post-Change Dummy	3.62	0.50	0.84	2.13	2.61	0.77	-0.19	1.62
	(1.61)	(0.23)	(0.67)	(1.09)	(2.58)	(0.41)	(1.16)	(1.38)
Mean of Dependent Variable	97.88	12.52	30.17	54.56	97.88	12.52	30.17	54.56

Notes: Table gives estimated effect of compulsory schooling law change on female mortality rates (deaths/1000 births) for the time period specified (i.e., 1970-2003, 1970-1981, 1981-1992, or 1992-2003). Specifically, it is the estimated coefficient on the variable that 0 for cohorts born before the 2nd quarter of 1933 and 1 for cohorts born during the 2nd quarter of 1933 and after. All regressions estimated using death and birth counts by quarter and year of birth. All regressions include a fully-flexible cubic polynomial in birthdate in quarters along with quarter of birth fixed effects. Robust standard errors are presented in parentheses. Data come from the 1970-2000 Office of National Statistics Death Files using those born between 1921 and 1951 (first set of columns) or using those born between 1929 and 1939 (second set of columns). The mean of the dependent variable is measured for persons born in the 2nd quarter of 1933.

Table 3: Effects of the Compulsory Schooling Change on Self-Reported Health

	Health Fair or Bad					Health Bad				
	1920-1950			1929-1939		1920-1950			1929-1939	
	(1)	(2)	(3)	(4)	(5)	(1)	(2)	(3)	(4)	(5)
	OLS	RF	IV	RF	IV	OLS	RF	IV	RF	IV
	-0.089	-0.006	-0.011	-0.008	-0.015	-0.041	-0.000	-0.000	-0.013	-0.024
	(0.004)	(0.016)	(0.033)	(0.025)	(0.046)	(0.002)	(0.009)	(0.019)	(0.016)	(0.029)
Mean of dependent variable	0.343	0.343	0.343	0.343	0.343	0.087	0.087	0.087	0.087	0.087
Observations	44366	55692	55692	20080	20080	44366	55692	55692	20080	20080

	Reduced Activity					Long Illness				
	1920-1950			1929-1939		1920-1950			1929-1939	
	(1)	(2)	(3)	(4)	(5)	(1)	(2)	(3)	(4)	(5)
	-0.024	-0.009	-0.019	-0.005	-0.008	-0.033	0.011	0.022	-0.004	-0.008
	(0.003)	(0.013)	(0.028)	(0.021)	(0.039)	(0.005)	(0.013)	(0.027)	(0.016)	(0.031)
Mean of dependent variable	0.191	0.191	0.191	0.191	0.191	0.614	0.614	0.614	0.614	0.614
Observations	44341	55663	55663	20074	20074	44351	55675	55675	20073	20073

	Cancer					Hypertension				
	1920-1950			1929-1939		1920-1950			1929-1939	
	(1)	(2)	(3)	(4)	(5)	(1)	(2)	(3)	(4)	(5)
	-0.003	-0.007	-0.013	-0.010	-0.019	0.002	0.014	0.030	-0.018	-0.033
	(0.001)	(0.005)	(0.010)	(0.007)	(0.014)	(0.002)	(0.013)	(0.028)	(0.017)	(0.031)
Mean of dependent variable	0.033	0.033	0.033	0.033	0.033	0.141	0.141	0.141	0.141	0.141
Observations	44366	55692	55692	20080	20080	44366	55692	55692	20080	20080

Notes: Table gives estimated effect of compulsory schooling law change on various self-reported health indicators. All regressions estimated using pooled waves of the Health Survey of England. All regressions include a fully-flexible cubic polynomial in quarter of birth as well as quarter of birth (1, 2, 3, 4) dummies and sex and age dummies (measured in quarters of a year). Robust standard errors are presented in parentheses. The post-1933 dummy is equal to 1 for cohorts born in the second quarter of 1933 and later and 0 otherwise. The mean of the dependent variable is measured for cohort born in the second quarter of 1933. The first two indicators are derived from a five-point scale: "very bad", "bad", "fair", "good", "very good". The "OLS" estimates in columns (1) are OLS effects of an additional year of education with age and quarter of birth controls. The samples used in column (1) include only those reporting leaving full-time education at age 16 or lower. The "RF" estimates in column (2) are reduced-form estimates of the effect of the reform on these outcomes and the "IV" estimates in columns (3) are instrumental variables estimates of the effect of an additional year of full-time education on these outcomes. See text for details.

Table 4: Effects of the Compulsory Schooling Change on Self-Reported Smoking

	Started smoking, currently smoking					Started smoking, stopped smoking				
	1920-1950			1929-1939		1920-1950			1929-1939	
	(1)	(2)	(3)	(4)	(5)	(1)	(2)	(3)	(4)	(5)
	OLS	RF	IV	RF	IV	OLS	RF	IV	RF	IV
	-0.058	-0.012	-0.025	0.001	0.001	0.028	0.021	0.043	0.039	0.071
	(0.003)	(0.015)	(0.031)	(0.017)	(0.031)	(0.004)	(0.016)	(0.033)	(0.021)	(0.042)
Mean of dependent variable	0.207	0.207	0.207	0.207	0.207	0.493	0.493	0.493	0.493	0.493
Observations	44340	55660	55660	20068	20068	44340	55660	55660	20068	20068

Notes: Table gives estimated effect of compulsory schooling law change on various self-reported smoking indicators. All regressions estimated using pooled waves of the Health Survey of England. All regressions include a fully-flexible cubic polynomial in quarter of birth as well as quarter of birth (1, 2, 3, 4) dummies and sex and age dummies (measured in quarters of a year). Robust standard errors are presented in parentheses. The post-1933 dummy is equal to 1 for cohorts born in the second quarter of 1933 and later and 0 otherwise. The mean of the dependent variable is measured for cohort born in the second quarter of 1933. The variables "Started smoking, currently smoking" and "Started smoking, stopped smoking" are derived from a series of questions about current and previous smoking behavior. See notes to Table 3.

Table 5: Effects of the Compulsory Schooling Change on BMI and Blood Pressure

	BMI					Obese (BMI>=30)				
	1920-1950			1929-1939		1920-1950			1929-1939	
	(1)	(2)	(3)	(4)	(5)	(1)	(2)	(3)	(4)	(5)
	OLS	RF	IV	RF	IV	OLS	RF	IV	RF	IV
	-0.268	0.150	0.310	-0.091	-0.179	-0.029	0.040	0.083	0.043	0.084
	(0.039)	(0.176)	(0.363)	(0.278)	(0.548)	(0.004)	(0.020)	(0.044)	(0.032)	(0.062)
Mean of dependent variable	27.497	27.497	27.497	27.497	27.497	0.251	0.251	0.251	0.251	0.251
Observations	40036	50412	50412	18299	18299	40036	50412	50412	18299	18299

	Blood Pressure					Stage 1 Hypertension				
	1920-1950			1929-1939		1920-1950			1929-1939	
	(1)	(2)	(3)	(4)	(5)	(1)	(2)	(3)	(4)	(5)
	-0.526	0.625	1.233	-0.065	-0.109	-0.006	-0.009	-0.019	-0.003	-0.005
	(0.108)	(0.588)	(1.213)	(0.803)	(1.352)	(0.003)	(0.013)	(0.025)	(0.024)	(0.041)
Mean of dependent variable	143.622	143.622	143.622	143.622	143.622	0.147	0.147	0.147	0.147	0.147
Observations	32926	41612	41612	15044	15044	32926	41612	41612	15044	15044

Notes: Table gives estimated effect of compulsory schooling law change on BMI and blood pressure. All regressions estimated using pooled waves of the Health Survey of England. All regressions include a fully-flexible cubic polynomial in quarter of birth as well as quarter of birth (1, 2, 3, 4) dummies and sex and age dummies (measured in quarters of a year). Robust standard errors are presented in parentheses. The post-1933 dummy is equal to 1 for cohorts born in the second quarter of 1933 and later and 0 otherwise. The mean of the dependent variable is measured for cohort born in the second quarter of 1933. Blood pressure is measured as the average of the systolic and diastolic readings, which are the mean of the second and third (out of four) readings in each case. See Table 3 for details of the difference between columns (1), (2) and (3).

Table 6a
Reduced-Form Effects of Compulsory Schooling Change on Male Mortality Rates Between 1947 and 1970
Year of Birth Analysis

	Probability of Death Between 1947 and 1970		
Post-Change Dummy	-0.99	-2.74	-1.95
	(1.16)	(1.07)	(1.32)
Post-1933 dummy	X	X	
Exclusion of 1933 cohort		X	
Revised post-1933 dummy			X
Mean of Dependent Variable	26.53	26.53	26.53
Observations	26	25	26

Notes: Table gives estimated effect of compulsory schooling law change on male mortality rates (deaths/1000 persons alive at the beginning of 1947) for the 1947-1970 time period. All regressions estimated using the Human Mortality Database. All regressions include a fully-flexible cubic polynomial in year of birth. Robust standard errors are presented in parentheses. The post-1933 dummy is equal to 1 for cohorts born in 1933 and later and 0 otherwise and the revised post-1933 dummy is equal to 1 for cohorts born in 1934 and later, 9/12 for cohorts born in 1933, 0 for all other cohorts. The mean of the dependent variable is measured for the 1933 cohort.

Table 6b
Reduced-Form Effects of Compulsory Schooling Change on Female Mortality Rates Between 1947 and 1970
Year of Birth Analysis

	Probability of Death Between 1947 and 1970		
Post-Change Dummy	0.32	-0.63	-0.10
	(0.66)	(0.63)	(0.77)
Post-1933 dummy	X	X	
Exclusion of 1933 cohort		X	
Revised post-1933 dummy			X
Mean of Dependent Variable	17.48	17.48	17.48
Observations	26	25	26

Notes: Table gives estimated effect of compulsory schooling law change on female mortality rates (deaths/1000 persons alive at beginning of 1947) for the 1947-1970 time period. All regressions estimated using the Human Mortality Database. All regressions include a fully-flexible cubic polynomial in year of birth. Robust standard errors are presented in parentheses. The post-1933 dummy is equal to 1 for cohorts born in 1933 and later and 0 otherwise and the revised post-1933 dummy is equal to 1 for cohorts born in 1934 and later, 9/12 for cohorts born in 1933, 0 for all other cohorts. The mean of the dependent variable is measured for the 1933 cohort.

Table 7a
Reduced-Form Effects of Compulsory Schooling Change on Male Mortality Rates Between Birth and 1947
Year of Birth Analysis

	Probability of Death Between Birth and 1947		
Post-Change Dummy	-10.77	-18.50	-15.70
	(5.51)	(5.33)	(5.99)
Post-1933 dummy	X	X	
Exclusion of 1933 cohort		X	
Revised post-1933 dummy			X
Mean of Dependent Variable	108.40	108.40	108.40
Observations	26	25	26

Notes: Table gives estimated effect of compulsory schooling law change on male mortality rates (deaths/1000 births) for the birth-1947 time period. All regressions estimated using the Human Mortality Database. All regressions include a fully-flexible cubic polynomial in year of birth. Robust standard errors are presented in parentheses. The post-1933 dummy is equal to 1 for cohorts born in 1933 and later and 0 otherwise and the revised post-1933 dummy is equal to 1 for cohorts born in 1934 and later, 9/12 for cohorts born in 1933, 0 for all other cohorts. The mean of the dependent variable is measured for the 1933 cohort.

Table 7b
Reduced-Form Effects of Compulsory Schooling Change on Female Mortality Rates Between Birth and 1947
Year of Birth Analysis

	Probability of Death Between Birth and 1947		
Post-Change Dummy	1.94	-2.29	0.09
	(4.54)	(4.73)	(4.75)
Post-1933 dummy	X	X	
Exclusion of 1933 cohort		X	
Revised post-1933 dummy			X
Mean of Dependent Variable	87.40	87.40	87.40
Observations	26	25	26

Notes: Table gives estimated effect of compulsory schooling law change on female mortality rates (deaths/1000 births) for the birth-1947 time period. All regressions estimated using the Human Mortality Database. All regressions include a fully-flexible cubic polynomial in year of birth. Robust standard errors are presented in parentheses. The post-1933 dummy is equal to 1 for cohorts born in 1933 and later and 0 otherwise and the revised post-1933 dummy is equal to 1 for cohorts born in 1934 and later, 9/12 for cohorts born in 1933, 0 for all other cohorts. The mean of the dependent variable is measured for the 1933 cohort.

Appendix Table 1a
Reduced-Form Effects of Compulsory Schooling Change on Male Mortality (1970-2003) by Degree of Birth Cohort Polynomial

	1921-1951 Birth Cohorts					1929-1939 Birth Cohorts				
All-Cause Mortality										
Post-Change Dummy	2.67	10.67	6.98	9.96	8.56	9.59	10.80	6.49	5.37	-22.07
	(1.46)	(2.40)	(3.30)	(4.36)	(6.00)	(1.43)	(3.06)	(4.80)	(7.93)	(19.08)
Mean of Dependent Variable	235.76	235.76	235.76	235.76	235.76	235.76	235.76	235.76	235.76	235.76
Circulatory Disease Mortality										
Post-Change Dummy	1.78	5.34	4.01	5.71	5.46	4.91	6.15	5.21	6.33	-10.03
	(0.71)	(1.20)	(1.73)	(2.05)	(2.91)	(0.88)	(1.62)	(2.53)	(4.47)	(10.32)
Mean of Dependent Variable	106.27	106.27	106.27	106.27	106.27	106.27	106.27	106.27	106.27	106.27
Respiratory Disease Mortality										
Post-Change Dummy	0.58	1.21	0.59	1.12	1.42	0.99	1.17	-0.07	-0.14	-5.78
	(0.45)	(0.46)	(0.55)	(0.79)	(1.13)	(0.43)	(0.69)	(0.84)	(1.47)	(3.47)
Mean of Dependent Variable	17.41	17.41	17.41	17.41	17.41	17.41	17.41	17.41	17.41	17.41
Non-Respiratory, Non-Circulatory Disease Mortality										
Post-Change Dummy	0.34	4.05	2.57	2.94	1.56	3.73	3.48	1.29	-0.80	-6.33
	(0.89)	(1.27)	(1.60)	(1.95)	(2.52)	(0.81)	(1.33)	(1.94)	(3.60)	(8.89)
Mean of Dependent Variable	112.08	112.08	112.08	112.08	112.08	112.08	112.08	112.08	112.08	112.08
Polynomial Degree	1st	2nd	3rd	4th	5th	1st	2nd	3rd	4th	5th

Notes: Table gives estimated effect of compulsory schooling law change on 1970-2003 male mortality rates (deaths/1000 births) for various control functions for quarter of birth (e.g., 1st degree polynomial, 2nd degree polynomial, and so forth). Specifically, it is the estimated coefficient on the variable that 0 for cohorts born before the 2nd quarter of 1933 and 1 for cohorts born during the 2nd quarter of 1933 and after. All regressions estimated using death and birth counts by quarter and year of birth. All regressions include age fixed effects. Robust standard errors are presented in parentheses. Data come from the 1970-2003 Office of National Statistics Death Files using those born between 1921 and 1951. The mean of the dependent variable is measured for persons born in the 2nd quarter of 1933.

Appendix Table 1b

Reduced-Form Effects of Compulsory Schooling Change on Female Mortality (1970-2003) by Degree of Birth Cohort Polynomial

	1921-1951 Birth Cohorts					1929-1939 Birth Cohorts				
All-Cause Mortality										
Post-Change Dummy	2.20	7.20	5.13	5.02	4.95	6.62	6.18	3.30	7.91	-29.86
	(1.10)	(1.70)	(2.52)	(3.27)	(4.82)	(1.24)	(2.60)	(4.42)	(7.42)	(14.74)
Mean of Dependent Variable	156.91	156.91	156.91	156.91	156.91	156.91	156.91	156.91	156.91	156.91
Circulatory Disease Mortality										
Post-Change Dummy	0.78	2.24	1.15	1.08	1.15	1.94	1.30	-0.21	-2.22	-19.85
	(0.41)	(0.63)	(0.95)	(1.36)	(2.04)	(0.40)	(1.09)	(1.92)	(2.87)	(4.46)
Mean of Dependent Variable	45.71	45.71	45.71	45.71	45.71	45.71	45.71	45.71	45.71	45.71
Respiratory Disease Mortality										
Post-Change Dummy	0.32	0.75	0.17	0.55	0.72	0.66	0.48	0.85	2.75	2.61
	(0.39)	(0.33)	(0.34)	(0.34)	(0.45)	(0.39)	(0.43)	(0.51)	(0.82)	(3.97)
Mean of Dependent Variable	13.32	13.32	13.32	13.32	13.32	13.32	13.32	13.32	13.32	13.32
Non-Respiratory, Non-Circulatory Disease Mortality										
Post-Change Dummy	0.87	4.14	3.62	3.04	3.10	3.78	4.24	2.61	7.40	-12.60
	(0.78)	(1.17)	(1.61)	(1.94)	(2.77)	(1.00)	(1.66)	(2.58)	(4.48)	(10.75)
Mean of Dependent Variable	97.88	97.88	97.88	97.88	97.88	97.88	97.88	97.88	97.88	97.88
Polynomial Degree	1st	2nd	3rd	4th	5th	1st	2nd	3rd	4th	5th

Notes: Table gives estimated effect of compulsory schooling law change on 1970-2003 female mortality rates (deaths/1000 births) for various control functions for quarter of birth (e.g., 1st degree polynomial, 2nd degree polynomial, and so forth). Specifically, it is the estimated coefficient on the variable that 0 for cohorts born before the 2nd quarter of 1933 and 1 for cohorts born during the 2nd quarter of 1933 and after. All regressions estimated using death and birth counts by quarter and year of birth. All regressions include age fixed effects. Robust standard errors are presented in parentheses. Data come from the 1970-2003 Office of National Statistics Death Files using those born between 1921 and 1951. The mean of the dependent variable is measured for persons born in the 2nd quarter of 1933.