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

VERY PRELIMINARY AND VERY INCOMPLETE

PLEASE DO NOT CITE

WITHOUT PERMISSION OF THE AUTHORS

Damon Clark
University of Florida

Heather Royer
Case Western Reserve University

First Draft: March 2007
This Draft: April 20, 2007

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 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, Oreopoulos (2006) uses a regression discontinuity approach to compare individuals based on year of birth who turned 14 just after 1947 (affected) with those who turned 14 just before 1947 (not affected) and finds that the affected students were 80% less likely to leave school by age 14. We use a similar but even finer approach to assess whether these educational attainment differences translated into adult health differences. Specifically, we use micro-level data to construct measures of education and health outcomes by quarter of birth. This lead to better estimates of the impact of the policy change on educational attainment since only two-thirds of the 1933 birth cohort were affected by the law. 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 risk and self-reported health and health-related behaviors.

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 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 compulsory schooling legislation that led to stark, sudden changes in educational attainment across different cohorts born just a few months apart. Since there is no reason to suppose that mortality among these cohorts would have differed in the absence of the school leaving laws, any differences can be plausibly attributed to the causal effects of education. Moreover, a natural test of this identifying assumption is to assess whether individuals born before and after the compulsory schooling age change differ along non-education-related dimensions. This can be done with the data at hand: for example, we show that there are no differences across these cohorts in childhood mortality risk.

Using this regression discontinuity strategy, we show, as prior literature has, that the increased compulsory schooling age led to a sharp increase in completed schooling, in the region of one half of a year. However, despite this dramatic change in completed schooling, we observe only small differences in mortality and self-reported health. This is in marked contrast to the findings of earlier US studies, particularly Lleras-Muney (2005) who estimates large effects of education on mortality in the United States. A natural explanation for the different findings is that the affected UK cohorts enjoyed universal health insurance, in contrast to the cohorts studied by Lleras-Muney. While this explanation is appealing, it requires that access to health insurance affects health outcomes, a connection that has yet to be convincingly established (see for example, Card et al., 2004).[1]

II. Background

In the US and elsewhere, there is a large and persistent correlation between education and adult health outcomes. In relation 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).[2] 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 also strong (Grossman, 2005). Assuming this link is a causal one, any of one of three broad mechanisms could be underpinning it (see Cutler and Lleras-Muney (2006) for a more extensive discussion). First, the mechanism may be

[1] 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.

[2] As these literatures are not directly relevant to our proposed study, we do not provide details on their methods and findings.

an economic one. It is generally thought that there exists a causal relationship between education and economic resources (Card, 1999) and these economic resources may secure access to safer and more secure jobs, better medical care and cleaner and safer neighborhoods (Williams and Collins, 1995). Second, the mechanism may be a behavioral one. If education improves knowledge, as well as the ability to process information, more educated people may make better health choices (Grossman, 1972). Third, the mechanism may be a psychological one. For example, education may change preferences (Becker and Mulligan, 1997), status (Marmot, 1994) or a person's sense of control (Ross and Wu, 1995). These and other factors can influence exposure to stress and the ability to deal with it.

Despite documenting a strong correlation between education and adult health outcomes, prior observational studies do not in general confirm that a causal link exists.[3] As a result, some recent literature uses quasi-experimental evidence to assess the extent of any casual link. In relation 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).[4] Arguing that these 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.[5] 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.[6] In addition to mortality, various studies have used similar research designs to assess the impact of education on health outcomes and

[3] We read the previous literature as aiming mainly to document the observable relationship, not estimate the causal effect.

[4] We do not provide a detailed discussion of the other studies since they use research designs similar to that used by Lleras-Muney but look at outcomes other than mortality.

[5] 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.

[6] 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.

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 understanding of the effect of education on mortality, but they suffer two 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 fifty percent of the cohorts for which the reform was binding.

III. The 1947 Reform

We follow a long line of literature and argue that such a quasi-experiment is generated by changes to compulsory school laws which specify the minimum age at which individuals may leave full-time education.[7] In the US, these laws are made at the state level, with most of important changes taking place in the first half the twentieth century (Lleras-Muney, 2005). In the UK they are made 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. This change, introduced as part of the 1944 Education Act, altered the structure of secondary education in the UK. Previously, 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 from September-December, January-Easter, Easter-July). After the law, applicable from 1

[7] In the health context, Lleras-Muney (2005) and Adams (2002) have used school leaving laws to examine the impacts of education. Other studies have used compulsory school laws to analyze the effects of education on labor market outcomes (Oreopoulos, 2006), crime (Lochner and Moretti, 2004), intergenerational effects (Oreopoulos et al, 2006) and citizenship (Milligan et al, 2004).

April 1947, students had to stay in secondary school at least until the end of the term in which they turned 15. As a result, students born in 1932 (and 14 in 1946) were in the old regime: they could leave school at the end of the term in which they turned 14. Students born in 1934 (and 14 in 1948) were in the new regime: they could leave school at the end of the term in which they turned 15. Students born in 1933 were in the old or new regime depending when their birthday fell. Those with a birthday before the end of the Easter term of 1947 (roughly January-April, although term dates differ at the local level) were allowed to leave at the end of the Easter term of 1947 (i.e., aged 14). Those with a birthday after the end of the Easter term of 1947 were subject to the new law, and had to wait until the end of the summer term of 1948 (i.e., aged 15). Easter fell on April 6 in May 1947, hence we would none of those born in January-March, some of those born in April and all of those born in May-December to have been affected by the law change.

Visual inspection of the relationship between birth quarter and completed education (Figure 3b) shows a marked jump in completed education between the first and second quarter of birth in 1933. Assuming the law did not coincide with changes in non-education related factors that might influence mortality, comparisons of educational attainment and mortality among those reaching age 14 just before 1947 and those reaching age 14 just after 1947 can identify the causal effect of education on mortality. One potential concern might be that students born around 1933 had their education interrupted by the Second World War, since these children would have been around six years old at the time that War broke out in 1939 and seven or eight years old when England and Wales were most seriously affected (1940 and 1941). 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 in the first quarter of 1933 and the extent of any pre-treatment differences in outcomes across these groups can be tested.

An obvious question is what the extra year meant in practice. Students in secondary school under both the old and new regimes did not officially complete secondary school (and receive a school leaving certificate) until the end of grade 11. In practice, under both the old and new regimes, only students intending to proceed to University completed eleven grades of school, with the vast majority of students leaving before this. Hence for most students the law change meant an extra year of general classes. This is discussed in the 1947 Ministry of Education report (HMSO, 1948):

> "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)

While one might be worry that the sudden increase in education provision could somehow dilute the return to this extra year, Orepoulos (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 adult health.

IV. Estimation Approach

The main difficulty in estimating the impact of education on mortality is omitted variables. In particular, there are many variables (e.g., preferences) that influence both education and health and may therefore generate a spurious correlation between them. Typically, the researcher cannot control for all of these variables, hence cross-sectional regression estimates will be biased and unreliable. In contrast, in experimental or quasi-experimental settings, at least a portion of the variation in education may be independent of other determinants of mortality, allowing the causal effect of education to be identified with appropriate statistical techniques.

To exploit the quasi-experiment represented by the 1947 reform, we use a regression discontinuity approach. This this amounts to estimating regressions of the following form:

(1) $E_c = \beta_0 + \beta_1 D_c + X'_c \pi_c + f(c) + u_c$

(2) $Y_c = \alpha_0 + \alpha_1 D_c + X'_c \pi_c + g(c) + v_c$

where E_c denotes the average level of schooling for birth cohort c, Y_c is a measure of health outcomes, D_c is a dummy variable for whether the cohort is affected by the 1947 law (i.e., has a value of 1 for those affected and 0 otherwise), X_c is a vector of control variables and f(c) and g(c) are smooth functions of the birth cohorts (low-order polynomials). The random error terms, u_c and v_c capture the unobservable determinants of E_c and Y_c respectively. The inclusion of f(c) and g(c) allows for the smooth evolution of educational attainment and mortality over time. To verify that our estimates are not overly sensitive to the particular function chosen, we follow standard practice (e.g., Oreopoulos, 2006) and graph the relationship between birth cohort and outcome (to provide visual evidence of potential specification errors) and experiment with alternative specifications.[8]

The parameters of interest in equations (1) and (2) are β_1 and α_1. They represent the regression discontinuity estimates of the effect of the reform on education and health outcomes respectively. More specifically, they identify the "jump" in educational attainment and health outcomes associated with whether the cohort is affected by the law change. This is the sense in which our research design exploits the comparison of those just unaffected and just affected by the law. The identifying assumption underpinning it is that there would have been no such jump in the absence of the change in educational attainment induced by the law.

[8] We have also used local linear regression methods. The results are qualitatively similar.

These reduced-form parameters, β_1 and α_1, can then be combined together to provide an instrumental variables estimate of the causal effect of education on health outcomes. The ratio α_1/β_1 is the standard IV Wald estimate. Unfortunately, we are unable to estimate β_1 and α_1 using one dataset, as we describe later. In particular, for those who have died, we do not observe their educational attainment. A two-sample instrumental variables strategy can be used in this context, using one source to estimate equation (1) and another to estimate equation (2) using another (Angrist and Krueger, 1992). While this is potentially complicated by sample selection concerns (we only observe the first-stage relationship between cohort of birth and age at school completion for those alive in 1970 and later), our analysis provides limited support for this hypothesis and so future versions will implement these two-sample methods.

V. Data

As noted above, we will use a two-sample instrumental variables technique that involves separate estimation of the impact of the 1947 law on educational attainment and mortality. The educational attainment analyses use data from the General Household Survey (GHS), an annual survey of roughly 10,000 households (15,000 individuals) in England, Wales and Scotland. The survey began in 1971 and has in every year obtained data on both the respondent's age and the age at which the respondent left full-time education. For the years 1983-1998, individuals report several measures of self-reported health, and on a nearly-biennial basis the survey asks about smoking and drinking behaviors. We pool all waves of these data (1971-1998) to generate a large sample of individuals born on either side of the 1933 threshold.[9]

[9] After 1998, the GHS underwent a number of survey redesigns. We hope to include more recent surveys in future versions of this paper.

For the years 1986 and onward, we have finer measures of birth cohort (i.e., exact day of birth) than year of birth (as inferred from age and survey year). This distinction is important since, as discussed above, those born in the first quarter of 1933 were not affected by the reform. As a result, contrasts between people born in 1933 and 1932 will lead to biased estimates of the effect of the reform on age at school exit. While data limitations prevented previous analyses (Oreopoulos (2006) and Harmon and Walker (1995)) from using these finer measures, we will improve year-of-birth analyses by using a regression specification that more accurately reflects the policy experiment. Specifically, we specify the treatment variable D in equations (1) and (2) to be zero for those born in 1932 or before, 9/12 for the 1933 birth cohort (since only persons born in April and later were affected) and 1 for those born in 1934 and later. As shown below, this specification results in large and more precise estimates of the impact of the reform on educational attainment.

We use mortality data from the Human Mortality Database (http://www.mortality.org/). For several countries, this database provides births and deaths by year of birth, single year of age and sex. For England and Wales, 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).[10] From 1959, the GRO kept individual-level mortality records and we use these to disaggregate mortality by cause of death in one three categories: circulatory diseases, respiratory diseases, and other.[11] One potential issue with these data is mobility. In particular, the data exclude deaths occurring outside of the UK. If out-migration is an increasing function of education, our reduced-form estimates of the effect of the reform on mortality will be upward-biased. Our small reduced-form estimates suggest this mobility issue is not important.

[10] 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).

[11] Earlier publications did not disaggregate by cause of death and single year of age simultaneously.

In addition to mortality, in future drafts we plan to analyze health outcomes (such as self-reported and objectively measured indictors of hypertension and diabetes) available in the Health Surveys of England. We have requested these data with exact data of birth but have not yet received them. Began in 1991, the Health Surveys of England are annual surveys combining a questionnaire-based component and information obtained from a nurse visit, including physical measurements and the analysis of blood samples. We will pool all waves of these data from 1991 through 2004 to give us a large sample of roughly 100,000 adults born roughly 15 years on either side of the 1934 change.[12] These data used in conjunction with our research design afford us an excellent opportunity to study the effects of education on a wide range of health outcomes.

VI. RESULTS

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

Figure 1 displays the fraction of males and females as observed in the 1972-1998 UK General Household Surveys 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 1944 reforms, 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. As discussed above, the law did not bind for individuals born in the first three months of 1933. As a result, the

[12] This calculation is based on annual sample sizes of 20,000 (slightly conservative), 14 years of data and a fraction of the population in the relevant group of around 35% (in 2003, 37% of the sample are aged over 55).

Figure shows that an intermediate fraction of this cohort (around 40 percent) completed school before the age of 15. The relationship between year of birth and the fraction dropping out at age 14 or earlier is nearly identical to that for men.

Figure 2 displays the fraction of individuals dropping out at age 15 or earlier by year of birth. While this fraction has declined over time, we do not observe the sharp decreases seen in Figure 1. This pattern suggests that the individuals compelled to stay in school an extra year drop out as soon as possible, although for females there appears to be a small discontinuous decline in the fraction dropping out at age 15 for the 1932 versus 1933 birth cohorts. Figure 3 presents the mean age at school exit by yearly birth cohorts. The sudden discontinuous pattern observed in Figure 1 is also apparent here. For both men and women, the age at school exit is about 14.75 years for the 1932 birth cohort rising to 15 for the 1933 birth cohort.

Table 1 quantifies the relationships seen in Figures 1-3. In particular, this Table presents the corresponding regression estimates of equation (1) for 3 outcomes for males and females: fraction dropping out by 14, fraction dropping out by 15, and age when left school. These regressions specify f(c) as a third-degree polynomial in year 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). We define the post-1933 dummy variable as equal to 1 for 1933 and later cohorts and 0 otherwise and the revised post-1933 dummy variable as equal to 1 for 1934 and later cohorts, 9/12 for the 1933 cohort, and 0 for the pre-1933 cohorts.[13]

For each outcome, we present three estimates. The first estimate treats all individuals born in 1933 as treated (i.e., the post-1933 dummy variable specification used by Oreopoulos, 2006). The second excludes the 1933 birth cohort from the estimation sample, and the last allows the treatment effect to phase in (i.e., uses the revised post-1933 dummy variable specification). The results suggest

[13] This variable is equal to 9/12 for the 1933 cohort because approximately 75 percent of the 1933 birth cohort should be affected by the reform assuming a uniform distribution of births.

the reform reduced the fraction dropping out of school by age 14 by 40 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 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. Notice that the first set of estimates (based on the post-1933 dummy variable specification) consistently underestimate the effect of the reform by over 25 percent.

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.[14] 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).

With more precise birth date information, we can obtain more reliable estimates of the impact of the reform. Starting with 1986, we can observe an individual's exact date of birth in the General Household Survey. As a result, we can compare educational outcomes by quarter of birth. As seen in Figure 3b, while the overall pattern is consistent with Figure 3a, it is clear that individuals born in first quarter of 1933 were unaffected by compulsory schooling change. As a result, regression-based estimates based on quarter of birth (Table 2) are more precise than those based on

[14] See for example Card (1999).

year of birth, and suggest that we need not be concerned with weak instruments (the t-ratios range from 10 to 15). The point estimates are also slightly larger, suggesting an impact on age of school exit of over half a year for females. Despite the advantages offered by quarter of birth data, note that the point estimates are similar to those based on a year of birth analysis that excludes the 1933 birth cohort or uses the revised post-1933 dummy. This suggests that even data aggregated to the year of birth level will likely be useful for our purposes. Finally, to verify that the quarter of birth analysis is sufficient (i.e., we do not need to make finer comparisons based month or day of birth), we present the first-stage estimates using monthly birthdates. The results in Table 3 are comparable to those aggregated by quarter of birth. This finding is not surprising, since Figures 1-3 suggest the general trends in educational attainment will be well captured by a polynomial in quarter of birth.

The Reduced-Form Relationship Between Birth Cohort and Mortality

Given the observed relationships between birth cohort and age at school exit, if education does reduce mortality risk, we would expect that cohorts subject to the reform would have lower rates of mortality and higher rates of survival. To test this, we first plot mean years of survival between certain ages by birth cohort (Figure 4). Given that the "treatment" occurs at age 14, it is natural to look at mean years of survival from age 14 onwards (i.e. condition on those that have survived to age 14). Given that we have data up to 2003 and wish to compare outcomes within a broad window of birth cohorts (e.g., 1921-1951) we start by looking at survival between ages 14 and 55. This measure ranges from zero to 41: if all members of a birth cohort die aged 14, mean years of survival are zero; if all members survive until age 55, mean years of survival will be 41.

Mean survival between ages 14 and 55 are plotted in the top two panels of Figure 4. At least two features of these graphs are apparent. First, there is no obvious jump in mean years of survival between birth cohorts 1932 and 1934, as we would expect if educational attainment influenced mortality. Second, among men, expected survival to the left of the threshold is a highly non-linear function of birth cohort. This reflects the large number of men born in the 1920s that died during the Second World War, which explains why the same pattern is not apparent in the graph for women. More concrete evidence for this explanation comes from top right-hand graph in Appendix Figure 1, which shows that relative to cohorts born between 1931 and 1932, cohorts of men born between 1925 and 1926 and between 1928 and 1929 were much more likely to die between ages 15 and 21 (i.e., between 1940 and 1945).

Individuals born in 1921 would have been 24 years old when the Second World War finished in 1945. Hence mean survival rates between ages 25 and 55 should be unaffected by the War. Consistent with this hypothesis, the middle-left graph shows a much smoother relationship between birth year and mean survival rates among men. Again there is no obvious jump between cohorts born in 1932 and 1934 and again the relationship among women is smooth across the entire window. These graphs suggest that any changes in mortality resulting from the changes in educational attainment induced by the 1947 reform were small. Finally, the bottom two graphs of Figure 4 suggest that the affected and unaffected cohorts enjoyed similar mean survival rates between birth and age 14. This suggests that affected and unaffected cohorts were similar in at least one pre-treatment dimension.

Table 4 presents regression-based models of mean survival. Each panel considers mean survival between certain ages and presents separate estimates for men and women. For each panel and each sex group, four estimates are presented. These correspond to models that specify a cubic in birth cohort along with a post-1933 dummy (model (1)), a post-1933 dummy with the 1933 birth cohort excluded (model (2)) and a refined post-1933 dummy (model (3)). In addition, we present an estimate based on a model that specifies a post-1933 dummy with the 1933 birth cohort excluded and a cubic in birth year interacted with the post-1933 dummy.

The estimates in panel A suggest that the reform had at best very small effects on mean survival among women. None of the four estimates is statistically significant and the point estimates suggest increased survival rates of less than one month. Based on the confidence intervals implied by these estimates, effects of more than two months can be comfortably ruled out. These can be compared with the mean survival rate among the last pre-treatment cohort (1932) of roughly 40 years (standard deviation roughly 4.5). Effects on women's survival rates between ages 25 and 55 are even smaller (panel B) and effects on pre-treatment survival are small and switch signs across specifications, consistent with the visual impression of no effect given by Figure 4.

The picture for men is clouded by the effects of the Second World War. In particular, the estimated impact on mean survival between ages 14 and 55 is very sensitive to the birth cohort polynomial chosen. This is not surprising given the graph shown in Figure 4. When we look at survival rates between ages 25 and 55, the picture is much clearer: estimates are statistically insignificant, switch signs across specifications and allow us to rule out effects of more than two months additional survival. For men, pre-treatment survival estimates (panel C) are again consistent with no pre-treatment differences between unaffected and affected cohorts.

While mean survival rates are a natural means of summarizing mortality experiences, it is also useful to consider mortality rates across various ages. First, these will illuminate the effects of the reform on mortality over certain age ranges. For example, it may be that more educated individuals are less likely to choose dangerous occupations with a high risk of early death. These effects may show up in an analysis of the impact of the reform on mortality between ages 14 and 24, but may not be picked up by an analysis of mean survival from age 14 to age 55. Second, they will allow us to estimate the effect of the reform on mortality from particular causes. Since we only have these from 1959 onwards we will disaggregate the mortality rate between ages 35 and 55.

Mortality rates across various ages are presented in Figure 5. Not surprisingly, the same qualitative patterns seen in Figure 4 can also be seen here. In particular, there is scant evidence to suggest a jump in mortality rates from the 1932 birth cohort (unaffected) to the 1934 birth cohort (affected). There is also a highly non-linear pattern for mortality rates from age 14 to 24 for men, but the relationship looks a lot smoother when looking at mortality rates from age 25 to 34 (i.e., conditional on survival to 25). Finally, looking at the bottom pair of graphs, there is no evidence to suggest any pre-treatment differences in mortality rates.

The estimates presented in Table 5 support these assertions. For each panel and each sex group, four estimates are again presented, corresponding to the four models presented in Table 4. Starting again with women, panel A suggests insignificant and small effects of the reform on mortality between ages 14 and 24. Set against a mean mortality rate of 0.8 percentage points for the 1932 (pre-treatment) cohort, we can rule out mortality reductions resulting from the reform of larger than 0.2 percentage points. Estimated effects on mortality between ages 24 and 34 and 35 and 55 are again small and insignificant. For mortality rates between ages 35 and 55, the estimates suggest that reductions of more than 0.3 percentage points can be ruled out, less than 5% of the base mortality rate of 5.7 percentage points.

For men, the picture is again complicated by Second World War deaths, with the estimates based on a cubic spline in birth year (column (4)) pointing to large and significant negative effects on mortality between ages 14 and 24 and ages 25 and 34. As seen in Figure 5 however, these estimates are likely driven by the non-smooth relationship between birth cohort and mortality among the untreated birth cohorts (born before 1932). In panel A the problem is obvious; in panel B, there is a slight kink in mortality rates from age 25 to 34 that a cubic spline will be unable to pick up. In panel C, this specification again points to a negative effect, although this is statistically insignificant and small in magnitude (around three percent of the baseline mortality rate).

For both men and women, panels D and E look at the impact of the reform on specific causes of death between ages 35 and 55, namely circulatory or respiratory disease mortality rates.[15] Consistent with the estimated effects on all causes of death, there is no evidence to suggest significant effects on either measure for either men or women. Finally, consistent with Figure 5, the estimates in panel F suggest that there are no significant pre-treatment differences in mortality risk among affected and unaffected cohorts. Overall, the mortality analysis supports the validity of the research design and suggests that the reform had, at best, very small effects on mortality.

The Reduced-Form Relationship Between Birth Cohort and Self-Reported Health and Health-Related Behaviors

Since we observe no mortality differences between the 1933 and pre-1933 cohorts, we need not worry about sample selection concerns and can now proceed to looking at the effects of education on health and health-related behaviors. The General Household Surveys for selected years collected data on self-reported health and smoking and drinking behaviors. In future we will also exploit the Health Surveys of England which collect more extensive health data.

Figures 6-8 display the reduced-form relationships between birth cohort and the aforementioned outcomes. Although there is substantial variation in these outcomes across cohorts, reflective of their age differences, the cohort trends are smooth and do not exhibit any apparent discontinuities starting with the 1933Q2 birth cohort.

Table 7 presents comparisons based on year of birth in a manner analogous to Table 1. Interestingly, the results for men suggest that an extra year of education increases the likelihood of reporting being in good health whereas for women, the opposite is true. Both of these results are statistically significant at the standard levels of significance. This may reflect differences in how

---

[15] The two common pulmonary diseases are heart disease and stroke. The three most common respiratory diseases include pneumonia (23 percent of total respiratory mortality), cancers, and Chronic Obstructive Pulmonary Disease (COPD), which includes chronic bronchitis and emphysema (ONS, 2007).

women and men access their own health status. The implied instrumental variables estimates (Table 8)[16] are large – staying in school a year longer suggests more than a 10 percent increase in the likelihood of reporting being in good health. The results for drinking for men are also statistically significant but perverse in sign. The estimates imply that an extra year of education reduces the likelihood of drinking alcohol little to none by nearly 25 percent.

We can potentially improve upon these estimates by using quarter of birth rather than year of birth comparisons. In Tables 9 and 10, we provide the reduced-form and instrumental variables estimates using these more-refined contrasts. We examine additional outcomes in these tables (the reduced-form and instrumental variables estimates for these other outcomes using the year of birth type of analysis are available upon request). The point estimates are substantially smaller than those in Tables 7 and 8 and none of them are statistically significant, suggesting that the drinking and self-reported health results based on the year-of-birth comparisons may be spurious. These results are more consistent with Figures 6-8. Overall, it appears that education has little effect on self-reported health and drinking and smoking behaviors. However, one might argue that after taking into account the standard errors, the point estimates in Table 10 do not rule out economically meaningful hypotheses. The use of the Health Surveys of England, which have significantly more observations, will allow us to estimate these effects more precisely.

Testing the Assumptions Underlying the Regression Discontinuity Model

The assumptions underlying the regression discontinuity model are partially testable. Our analysis implicitly assumes that post-reform individuals are similar to pre-reform individuals along non-education-related characteristics. We can test the validity of this assumption at least in terms of observable characteristics. We have shown that there are no important differences in pre-treatment

[16] Note the instrumental variable estimates are not the ratio of the reduced-form estimates presented in Table 7 to the first-stage estimates presented in Table 1 because the samples are different.

mortality and with additional data that we are collecting we will be test whether there are cohort differences in out-of-wedlock birth rates.

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 such as Lleras-Muney (2005).

As mentioned above, 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. In particular, smoking behavior is highly correlated with socioeconomic status in the United States, but less well correlated in the UK, at least at the lower incomes along which our policy experiment is operating (Cutler and Glaeser, 2006).

REFERENCES

Adams, Scott J. (2002). "Educational Attainment and Health: Evidence from a Sample of Older Adults." *Education Economics* 10(1): 97-109.

Angrist, Joshua D. and Alan B. Kreuger (1992). "The Effect of Age at School Entry on Educational Attainment: An Application of Instrumental Variables with Moments from Two Samples", *Journal of the American Statistical Association*, 87(418), 328-336.

Angrist, Joshua D. and Victor C. Lavy (1999). 'Using Maimonides' Rule To Estimate The Effect Of Class Size On Scholastic Achievement", *Quarterly Journal of Economics* 114(2): 533-575

Arendt, Jacob Nielsen (2005). "Does Education Cause Better Health? A Panel Data Analysis Using School Reforms for Identification." *Economics of Education Review* 24(2): 149-160.

Arkes, Jeremy (2003). "Does Schooling Improve Adult Health?" RAND Working Paper, Santa Monica, CA.

Beckett, Megan (2000). "Converging Health Inequalities in Later Life – an Artifact of Mortality Selection?" *Journal of Health and Social Behavior* 41: 106-119.

Becker, Gary S. and Casey B. Mulligan (1997). "The Endogenous Determination of Time Preference", *Quarterly Journal of Economics*, 112(3), 729-758

Card, David E. (1999). "The Causal Effect of Education on Earnings," in Orley Ashenfelter and David E. Card, eds., *The Handbook of Labor Economics, Volume 3A*, Amsterdam: Elsevier.

Card, David and David S. Lee (2007), "Regression Discontinuity Inference with Specification Error", *Journal of Econometrics*, forthcoming.

Christenson, Bruce A. and Nan E. Johnson (1995). "Educational Inequality in Adult Mortality: An Assessment with Death Certificate Data from Michigan." *Demography* 32(2): 215-229.

Coleman, David A. (1995). "International Migrants in Host Countries in Four Continents", *International Migration Review*, 29(1) 155-206.

Cutler, David M. and Adriana Lleras-Muney (2006). "Education and Health: Evaluating Theories and Evidence", NBER Working Paper 12352, Cambridge, MA.

de Walque, Damien (2004). "Education, Information, and Smoking Decisions: Evidence from Smoking Histories, 1940-2000." World Bank Policy Research Working Paper No. 3362.

Deaton, Angus and Paxson, Christina (2004). "Mortality, Income and Income Inequality over Time in Britain and the United States", in Wise, David A (ed.), "Perspectives on the Economics of Aging", University of Chicago Press for NBER, 427-480

Department of Health and Human Services (2005). http://www.healthypeople.gov/About/goals.htm

Glied, Sherry and Adriana Lleras-Muney (2003). “Health Inequality, Education and Medical Innovation”, NBER Working Paper 9738, Cambridge, MA.

Grossman, Michael (1972). "On the Concept of Health Capital and the Demand for Health." *Journal of Political Economy* 80(2): 223-255.

Grossman, Michael (2005). “Education and Non-market Outcomes”, NBER Working Paper 11582, Cambridge, MA.

HMSO (1948). “Education in 1947”, Report of the Ministry of Education to Parliament, HMSO, London, UK.

Hoyert, Donna L. et al. (2001). “Deaths: Final Data for 1999.” *National Vital Statistics Reports* 49(8):table 23.

Hummer, Robert A., Richard G. Rogers, and Issac W. Eberstein (1998). “Sociodemographic Differentials in Adult Mortality: A Review of Analytic Approaches.” *Population and Development Review* 24(3): 553-578.

Imbens, Guido W. and Joshua D. Angrist (1994). “Identification and Estimation of Local Average Treatment Effects.” *Econometrica* 62 (2): 467–475.

Kitagawa, Evelyn M. and Philip M. Hauser (1968). “Educational Differentials in Mortality by Cause of Death: United States, 1960.” *Demography* 5(1): 318-353.

Jdanaov, Dmitri and Evgeny Andreev and Domantas Jasilionis and Vladimir M. Shkolnikov (2005), “Estimates of mortality and population changes in England and Wales over the two World Wars”, Demographic Research, Vol. 13, Article 16, pp 389-414.

Lantz, Paula M., James S. House, James M. Lepkowski, David R. Williams, Richard P. Mero, Jieming Chen (1998). “Socioeconomic Factors, Health Behaviors, and Mortality: Results from a Nationally Representative Prospective Study of US Adults.” *Journal of the American Medical Association* 279(21): 1703-1708.

Lee, David S. (2007). “Randomized Experiments from Non-random Selection in U.S. House Elections”, *Journal of Econometrics*, forthcoming.

Lee, David and David Card (2006). “Regression Discontinuity Inference with Specification Error.” *Journal of Econometrics*, Forthcoming.

Lleras-Muney, Adriana (2005). “The Relationship Between Education and Adult Mortality in the United States.” *Review of Economic Studies* 72: 189-221.

Lochner, Lance and Enrico Moretti (2004), “The Effect of Education on Crime: Evidence from Prison Inmates, Arrests and Self-Reports”, *American Economic Review*, 94(1), 155-189.

Lynch, Scott M. (2003). "Cohort and Life-Course Patterns in the Relationship Between Education and Health: A Hierarchical Approach." *Demography* 40(2): 309-331.

MacInnis, Bo (2006). "Does Education Impact Health? Evidence from the Pre-Lottery Vietnam Draft," mimeo, University of California, Berkeley.

Marmot, Michael G. (1994). "Social Differences in Health Within and Between Populations", *Daedalus*, 123(4) 197-216

Mazumder, Bhashkar (2007). "How Did Schooling Laws Improve Long-Term Health and Lower Mortality?" Federal Reserve Bank of Chicago, WP 2006-23 (revised January 24 2007), Chicago, Il.

McDonough, Peggy, David R. Williams, James S. House, and Greg J. Duncan (1999). "Gender and the Socioeconomic Gradient in Mortality." *Journal of Health and Social Behavior* 40(1): 17-31.

Milligan, Kevin and Enrico Moretti and Philip Oreopoulos (2004). "Does Education Improve Citizenship? Evidence from the U.S. and the U.K." *Journal of Public Economics*, 88 (9-10), 1667-1695.

O'Keeffe, Denis, J. (1975). "Some Economic Aspects of Raising the School Leaving Age in England and Wales in 1947". *The Economic History Review*, 28(3) 500-516.

Oreopoulos, Philip (2006)."Estimating Average and Local Average Treatment Effects of Education when Compulsory School Laws Really Matter ", *American Economic Review*, 96(1) 152-175.

Oreopoulos, Philip and Marianne Page and Ann Stevens (2006). "Does Human Capital Transfer from Parent to Child? The Intergenerational Effects of Compulsory Schooling", *Journal of Labor Economics*, 24(4), 729-760.

Pappas, Gregory, Susan Queen, Wilbur Hadden, and Gail Fisher (1993). "The Increasing Disparity in Mortality Between Socioeconomic Groups in the United States, 1960 and 1986." *New England Journal of Medicine* 329(2): 103-109.

Ross, Catherine E. and Chia-Ling Wu (1995). "The Links Between Education and Health." *American Sociological Review* 60: 719-745.

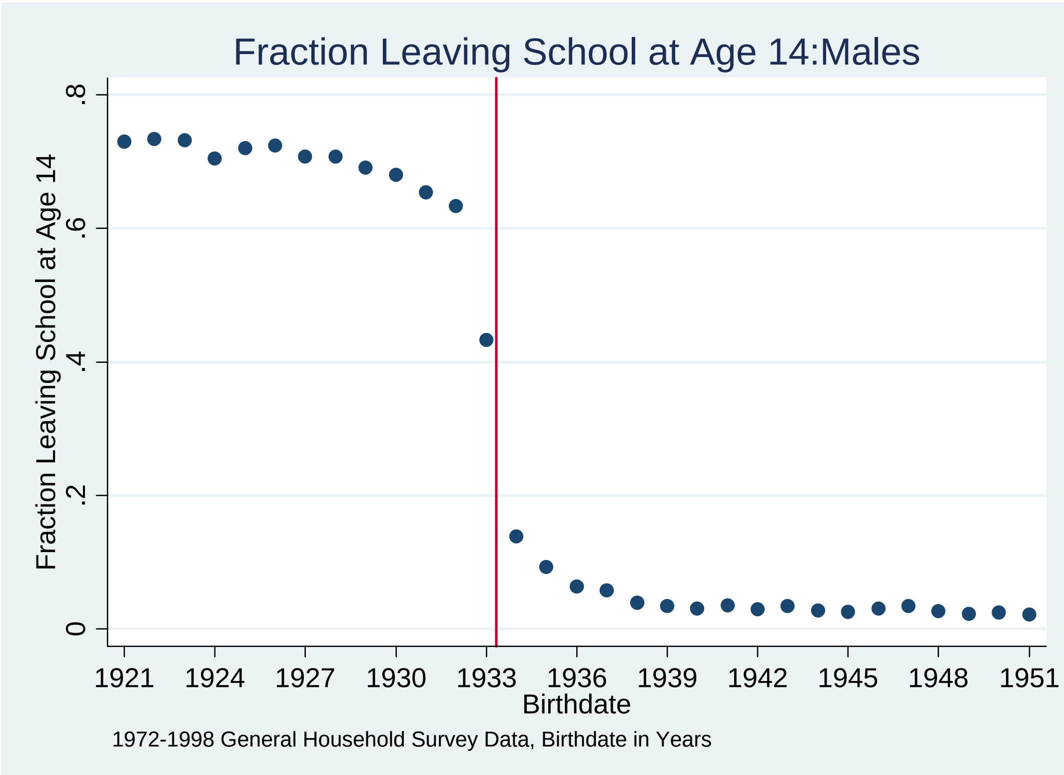
Thistlethwaite, D., and D. Campbell (1960). "Regression-Discontinuity Analysis: An Alternative to the Ex Post Facto Experiment." *Journal of Educational Psychology* 51: 309-317.

Trochim, W. (1984). *Research Design for Program Evaluation: The Regression-Discontinuity Approach*. Sage Publications: Beverly Hills.

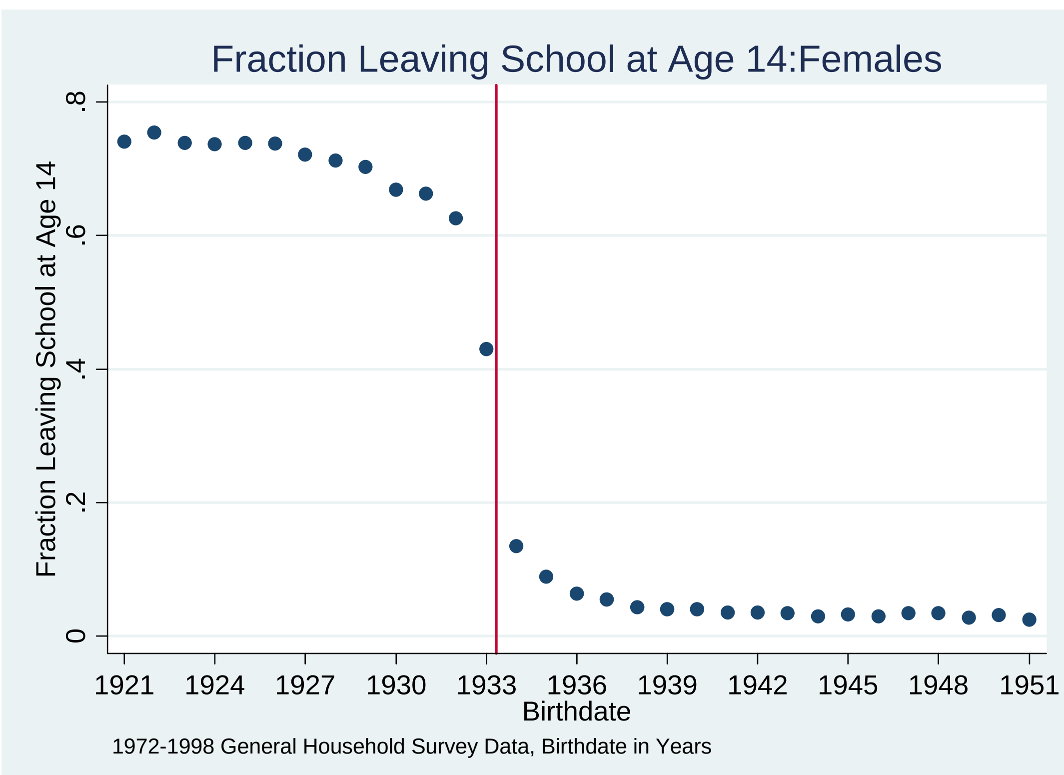
Wild, Sarah and Paul McKeigue (1997). "Cross Sectional Analysis of Mortality by Country of Birth in England and Wales, 1970-1992". *British Medical Journal*, 314(7082) 705-710.

Williams, David R. and Chiquita Collins (1995). "US Socioeconomic and Racial Differences in Health: Patterns and Explanations." *Annual Review of Sociology* 21: 349-386.

Figure 1

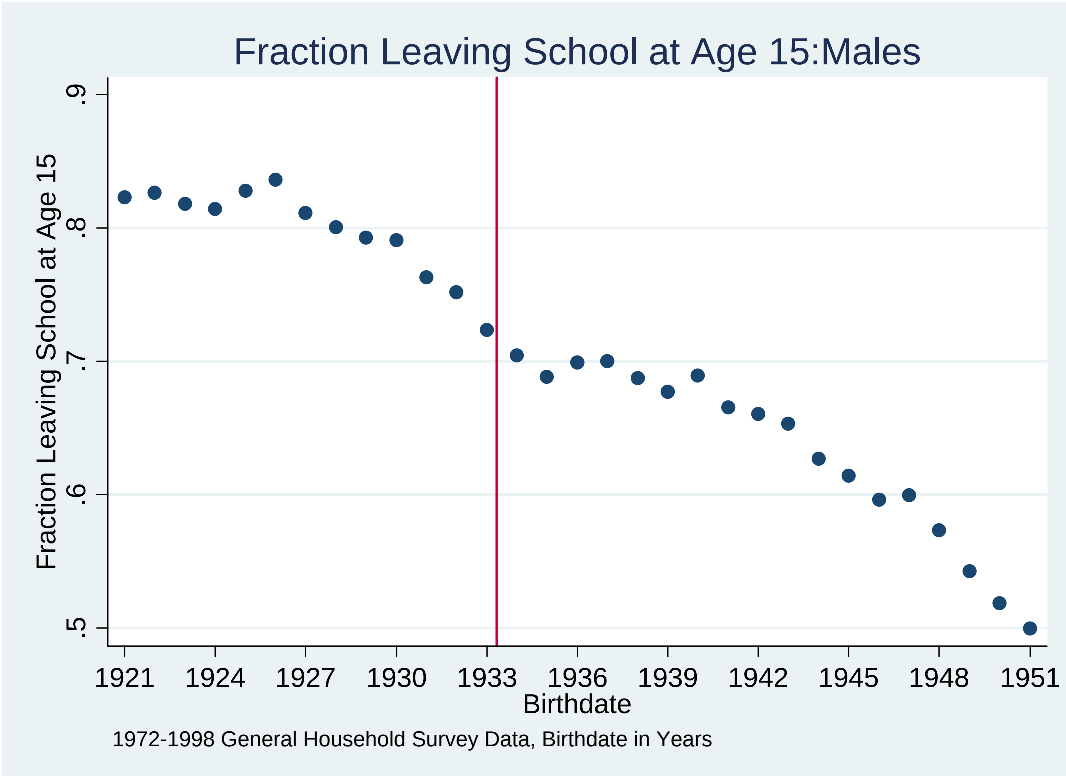


Fraction Leaving School at Age 14:Males
Fraction Leaving School at Age 14
0
.2
.4
.6
.8
1921
1924
1927
1930
1933
1936
1939
1942
1945
1948
1951
Birthdate
1972-1998 General Household Survey Data, Birthdate in Years



Fraction Leaving School at Age 14:Females
Fraction Leaving School at Age 14
0
.2
.4
.6
.8
1921
1924
1927
1930
1933
1936
1939
1942
1945
1948
1951
Birthdate
1972-1998 General Household Survey Data, Birthdate in Years

Figure 2


Fraction Leaving School at Age 15:Males
Fraction Leaving School at Age 15
.5
.6
.7
.8
.9
1921
1924
1927
1930
1933
1936
1939
1942
1945
1948
1951
Birthdate
1972-1998 General Household Survey Data, Birthdate in Years

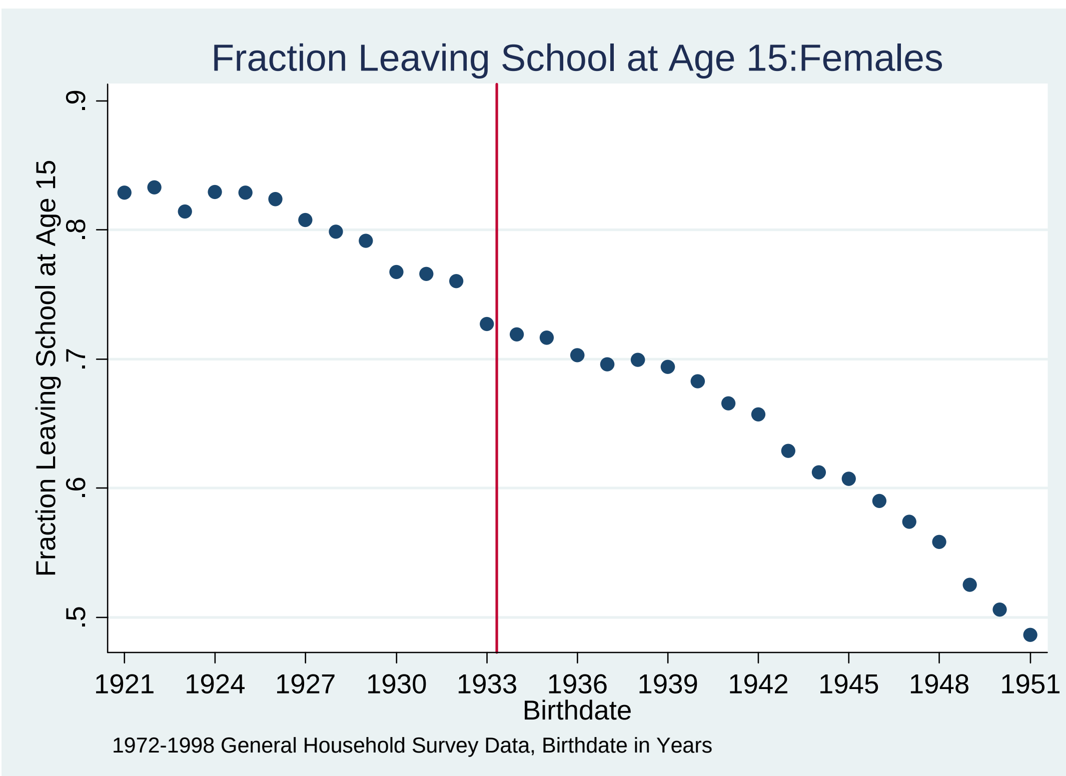
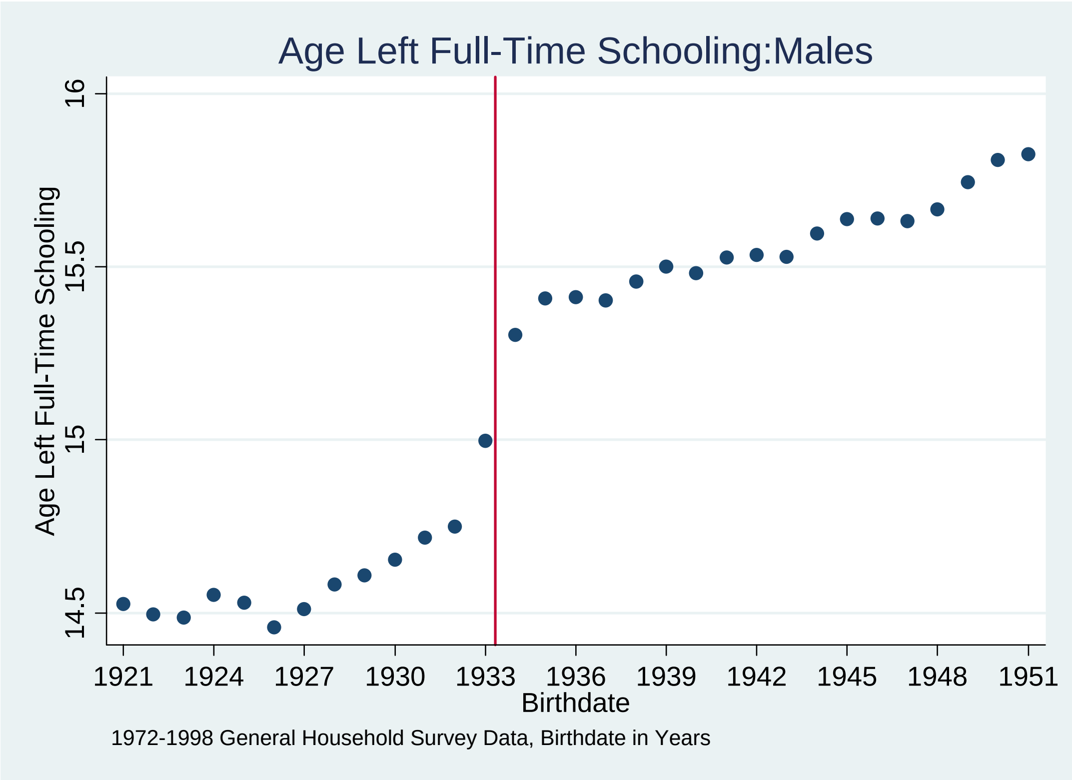

Fraction Leaving School at Age 15:Females
Fraction Leaving School at Age 15
.5
.6
.7
.8
.9
1921
1924
1927
1930
1933
1936
1939
1942
1945
1948
1951
Birthdate
1972-1998 General Household Survey Data, Birthdate in Years

Figure 3a


Age Left Full-Time Schooling:Males
Age Left Full-Time Schooling
16
15.5
15
14.5
1921
1924
1927
1930
1933
1936
1939
1942
1945
1948
1951
Birthdate
1972-1998 General Household Survey Data, Birthdate in Years

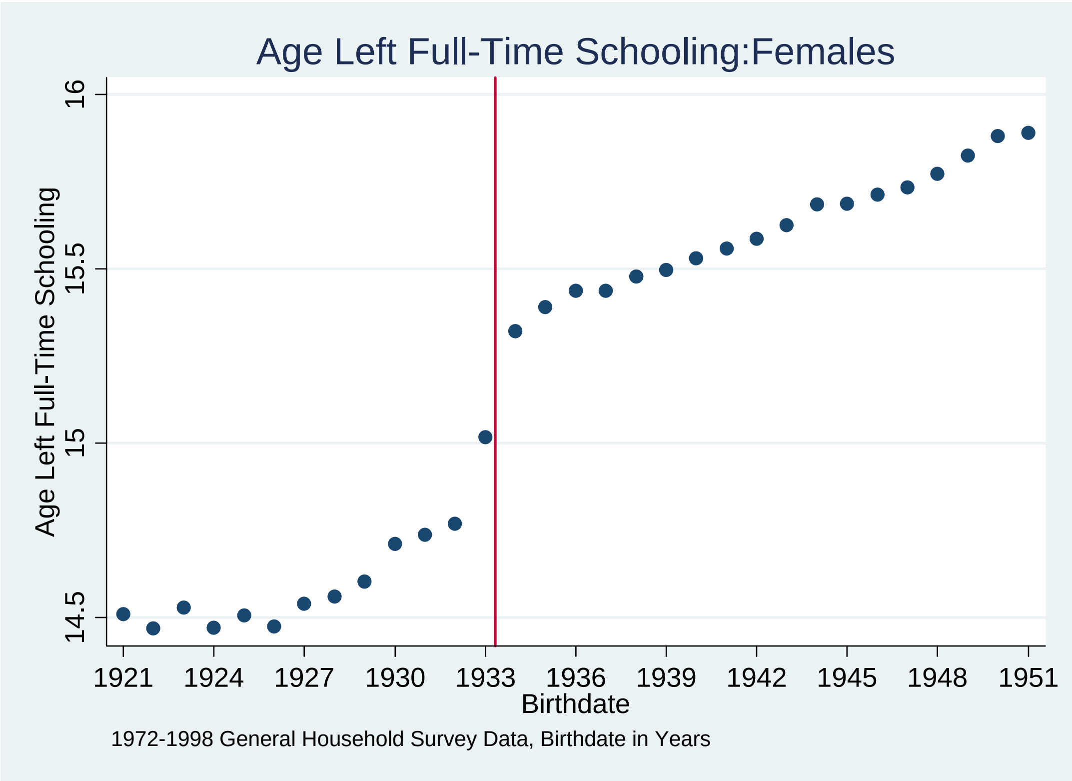
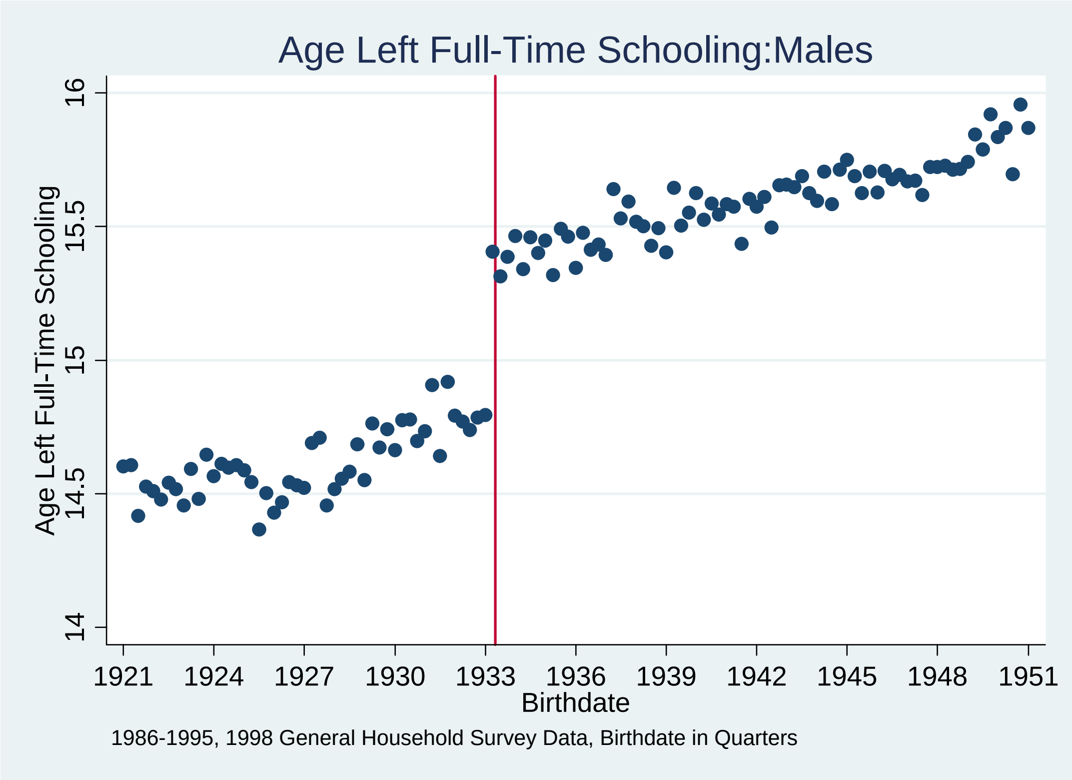

Age Left Full-Time Schooling:Females
Age Left Full-Time Schooling
16
15.5
15
14.5
1921
1924
1927
1930
1933
1936
1939
1942
1945
1948
1951
Birthdate
1972-1998 General Household Survey Data, Birthdate in Years

Figure 3b


Age Left Full-Time Schooling:Males
Age Left Full-Time Schooling
14
14.5
15
15.5
16
1921
1924
1927
1930
1933
1936
1939
1942
1945
1948
1951
Birthdate
1986-1995, 1998 General Household Survey Data, Birthdate in Quarters

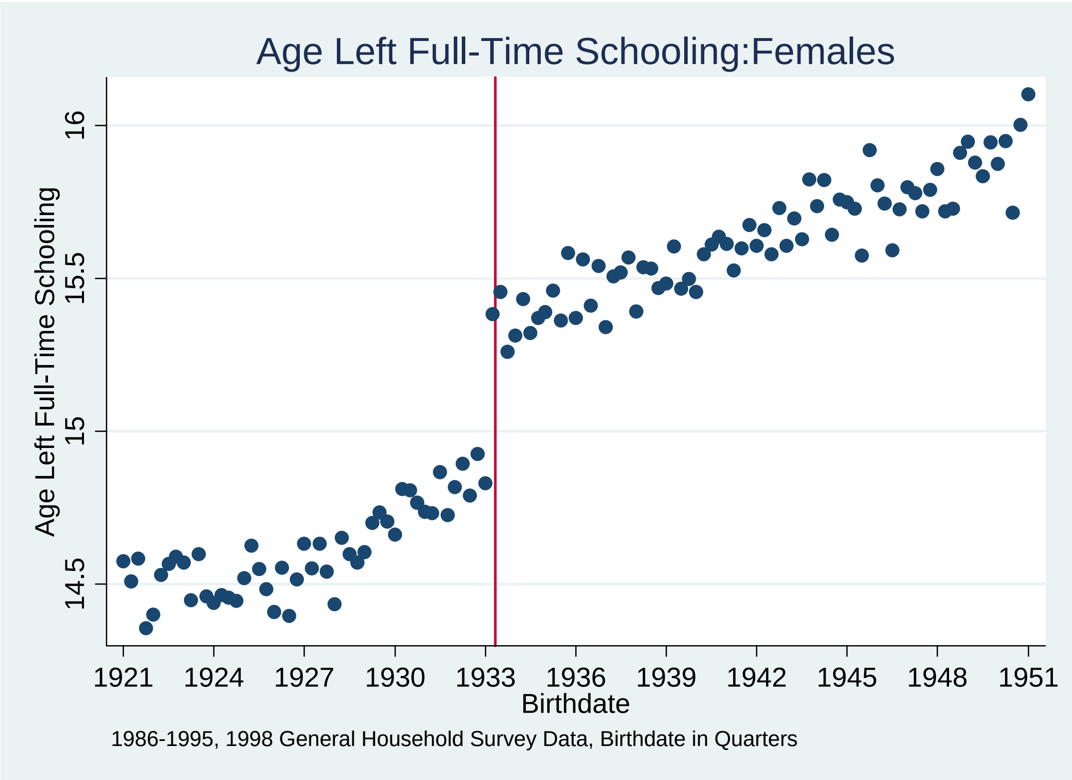
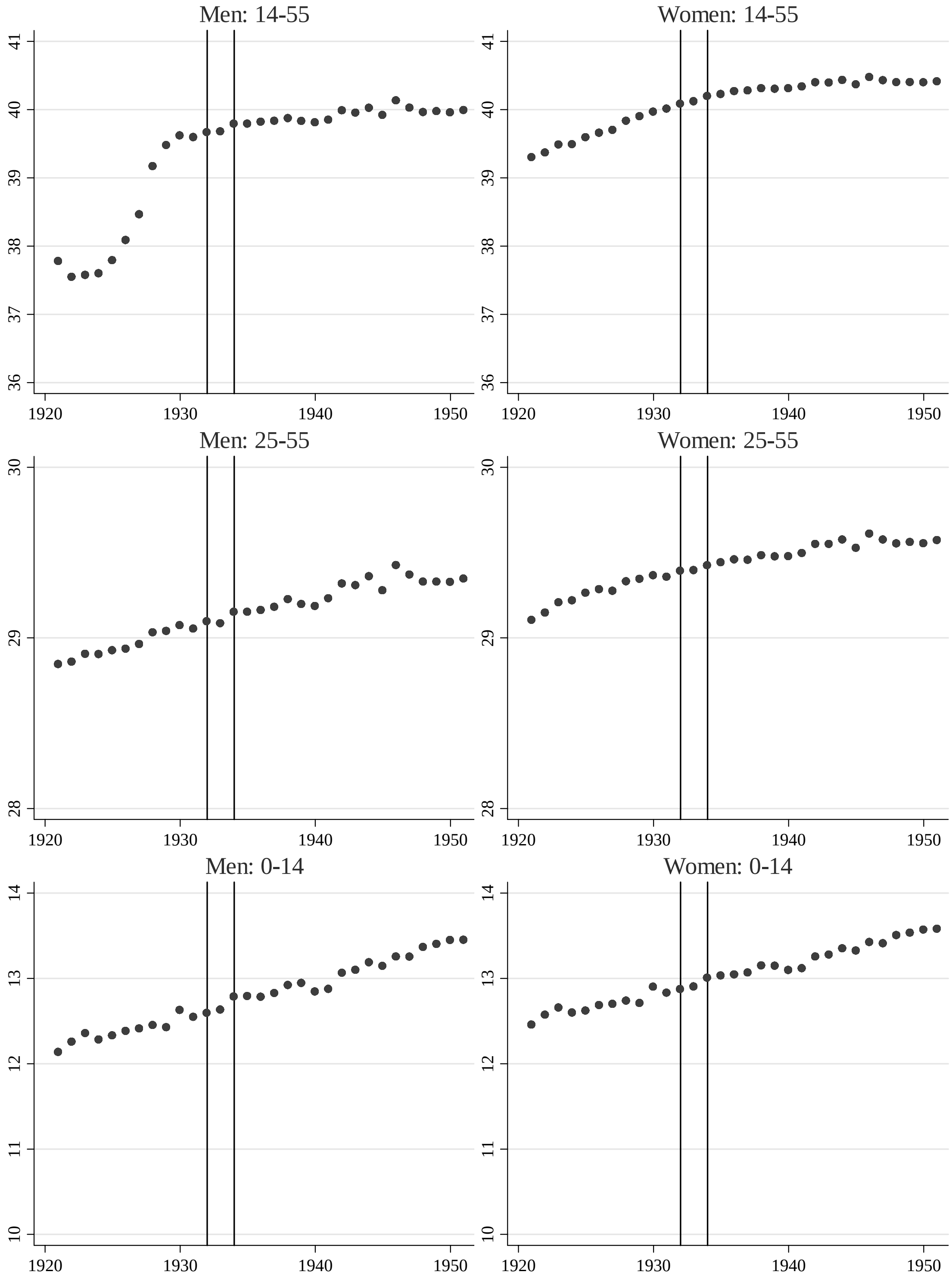
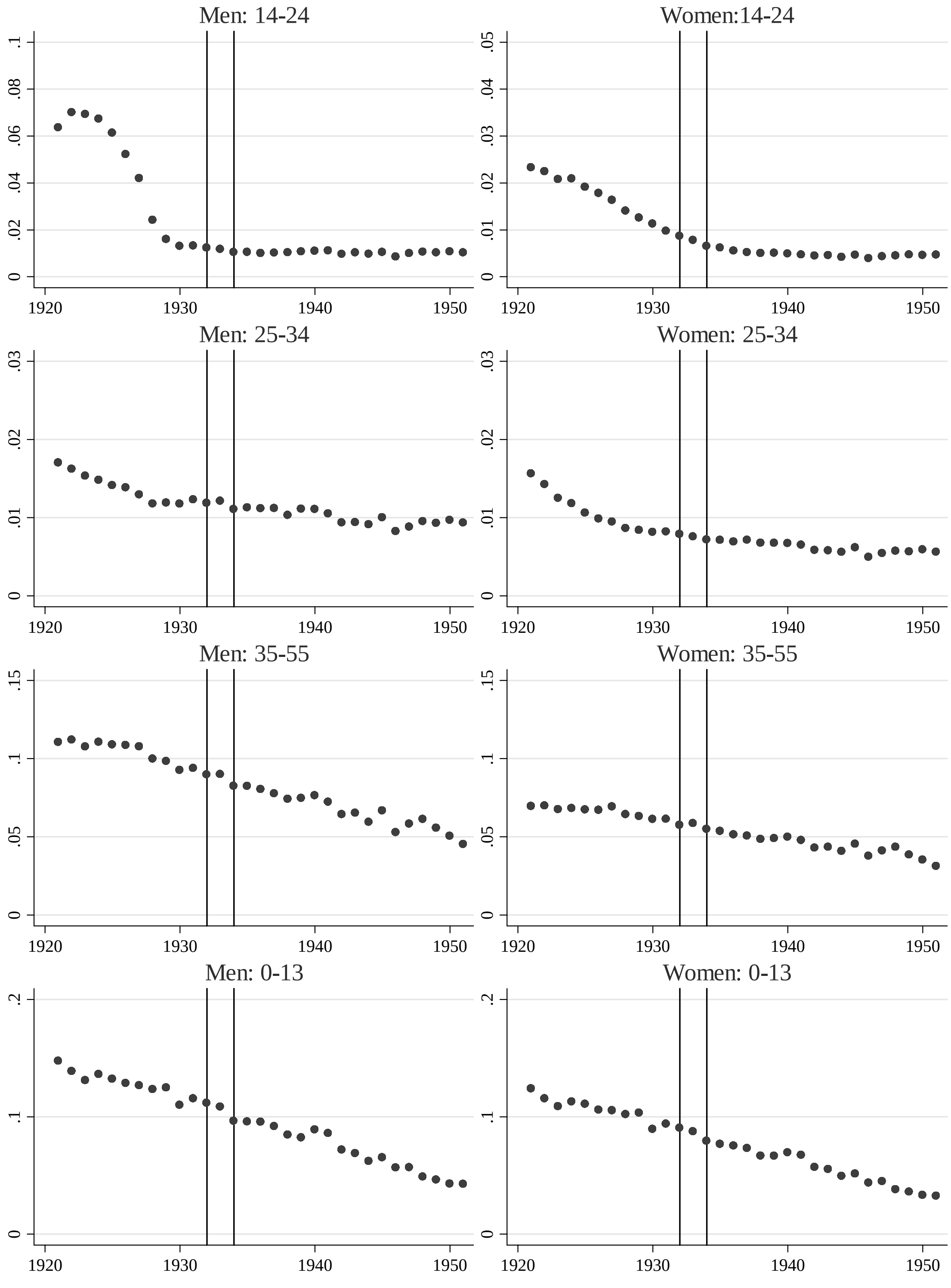

Age Left Full-Time Schooling:Females
Age Left Full-Time Schooling
14.5
15
15.5
16
1921
1924
1927
1930
1933
1936
1939
1942
1945
1948
1951
Birthdate
1986-1995, 1998 General Household Survey Data, Birthdate in Quarters

Figure 4: Mean years of survival between various ages



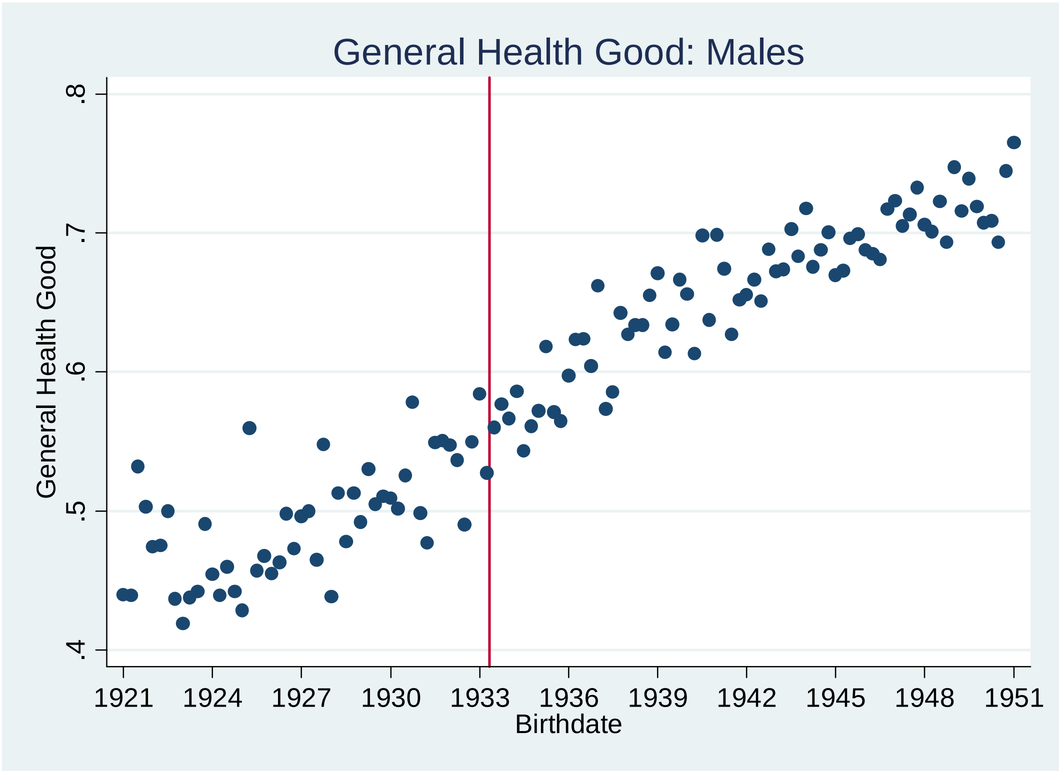
Note: Vertical lines refer to birth cohorts 1932 and 1934.

Figure 2: Mortality rates between various ages



Note: Vertical lines refer to 1932 and 1934.

Figure 6


General Health Good: Males
General Health Good
.4
.5
.6
.7
.8
1921
1924
1927
1930
1933
1936
1939
1942
1945
1948
1951
Birthdate

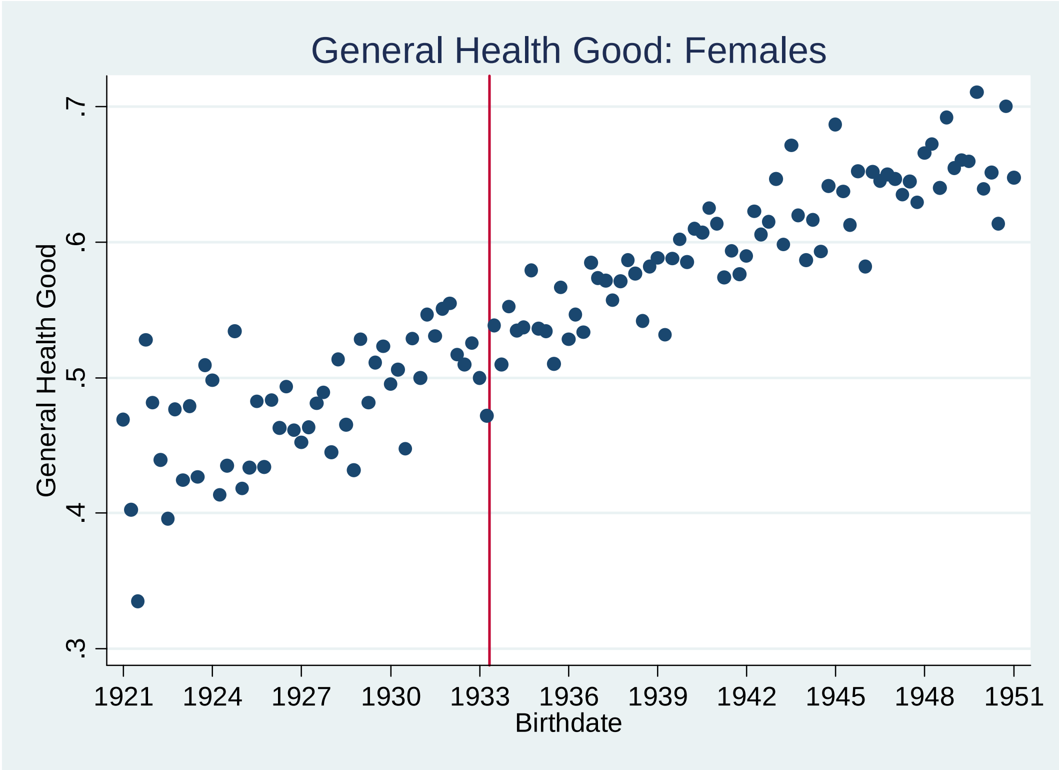
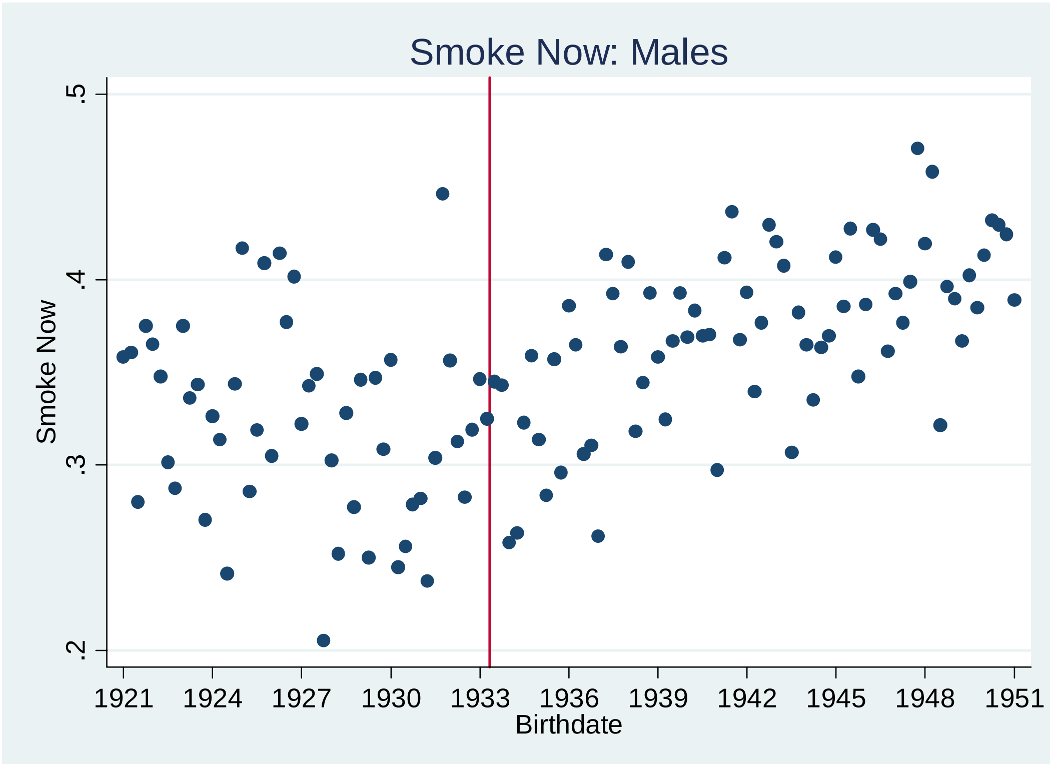

General Health Good: Females
General Health Good
.3
.4
.5
.6
.7
1921
1924
1927
1930
1933
1936
1939
1942
1945
1948
1951
Birthdate

Figure 7


Smoke Now: Males
.5
.4
.3
.2
Smoke Now
1921
1924
1927
1930
1933
1936
1939
1942
1945
1948
1951
Birthdate

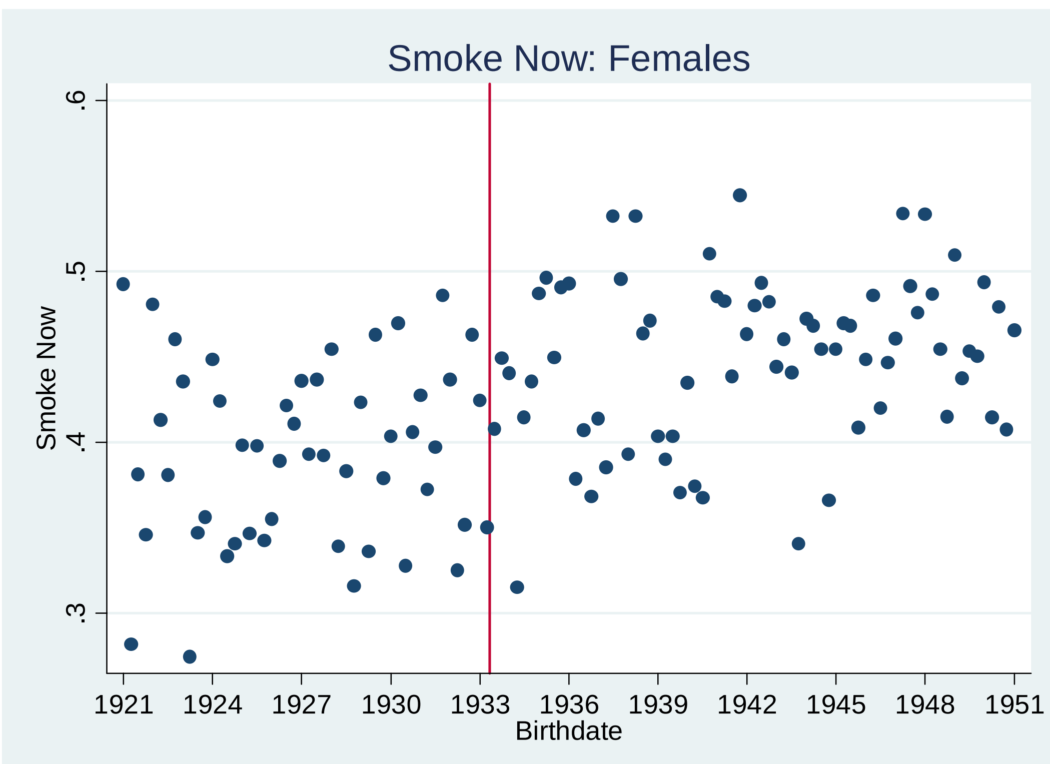
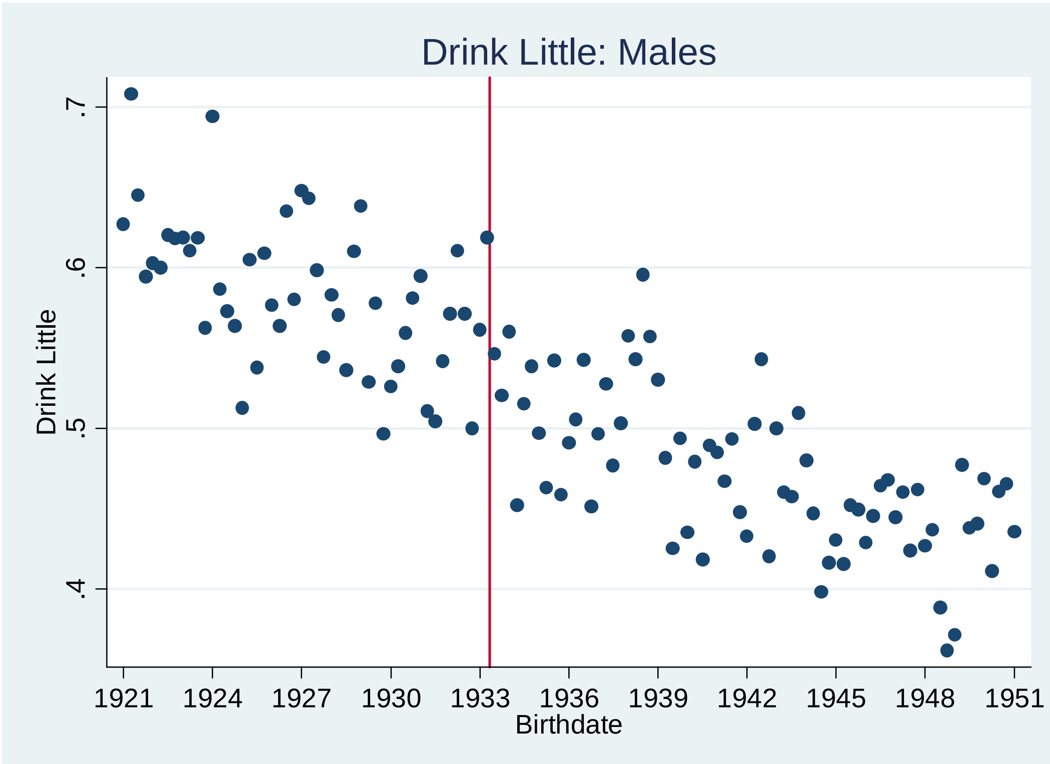
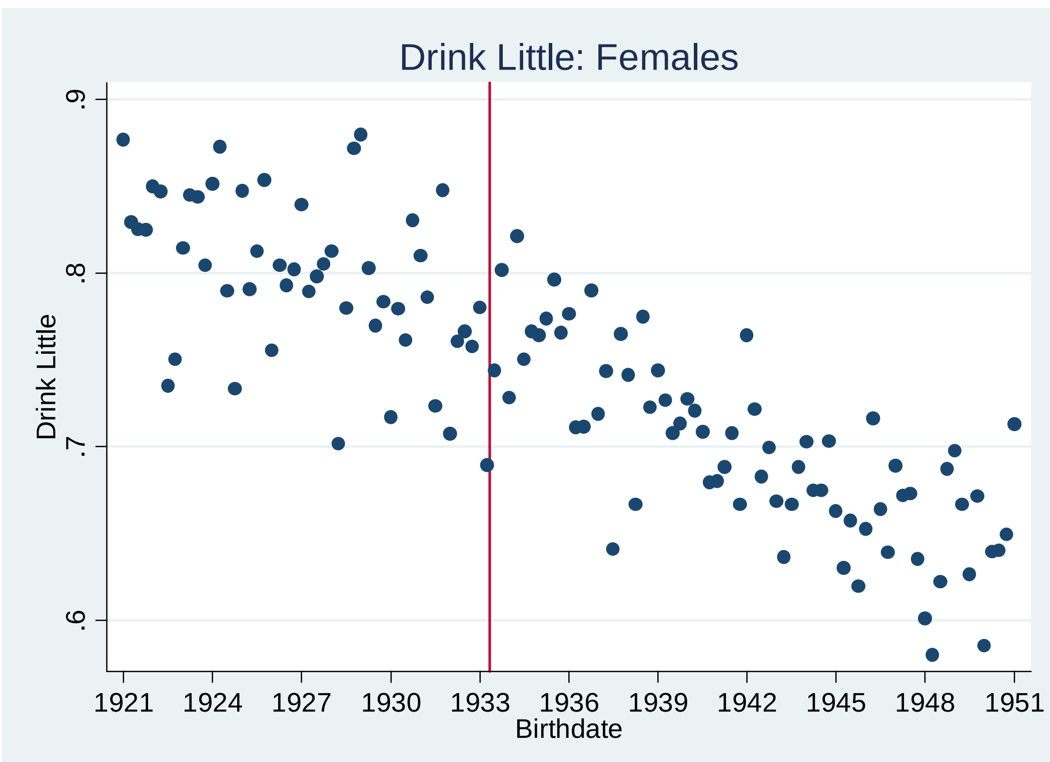

Smoke Now: Females
.6
.5
.4
.3
Smoke Now
1921
1924
1927
1930
1933
1936
1939
1942
1945
1948
1951
Birthdate

Figure 8

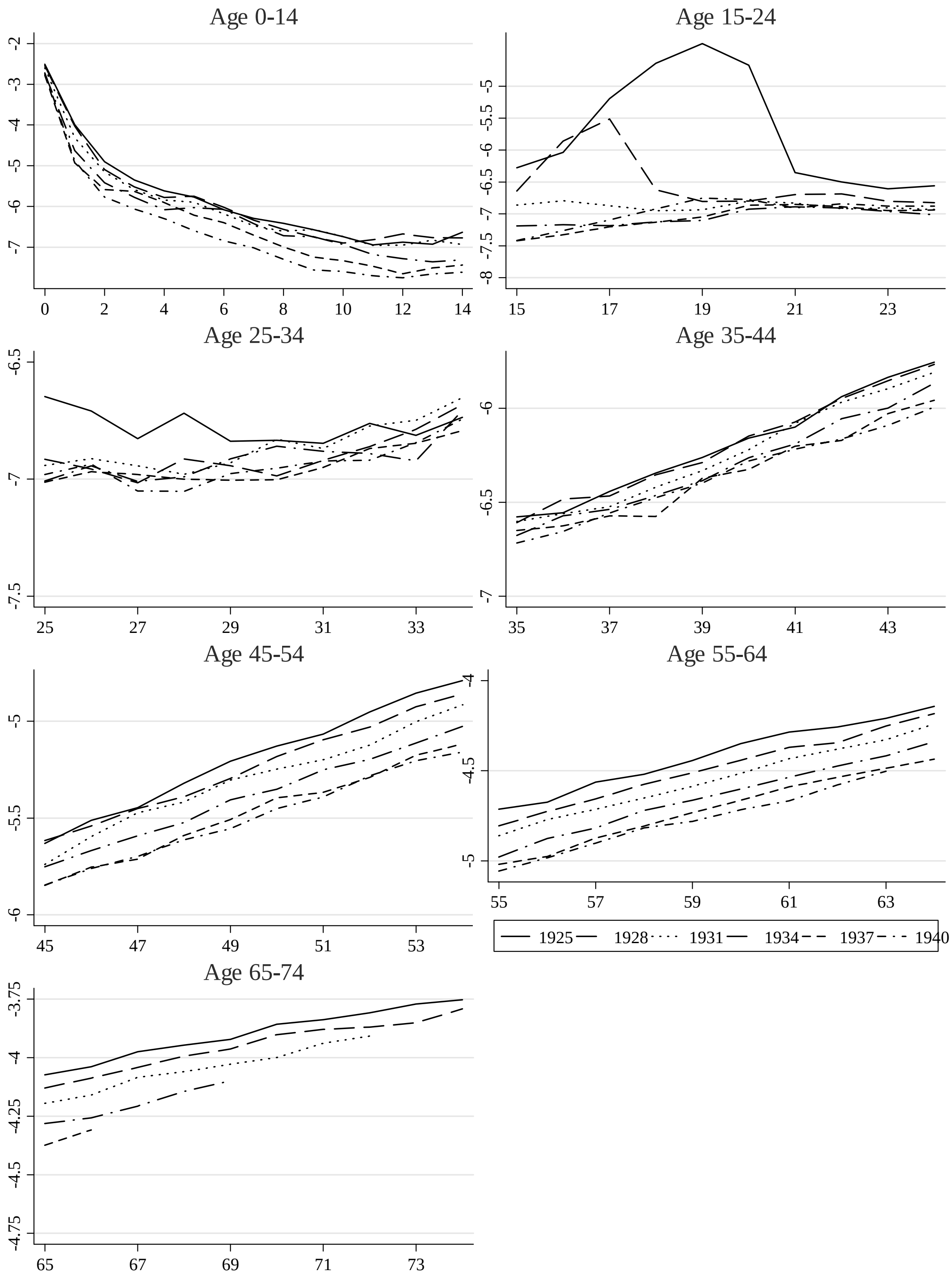


Drink Little: Males
Drink Little
.4
.5
.6
.7
1921
1924
1927
1930
1933
1936
1939
1942
1945
1948
1951
Birthdate



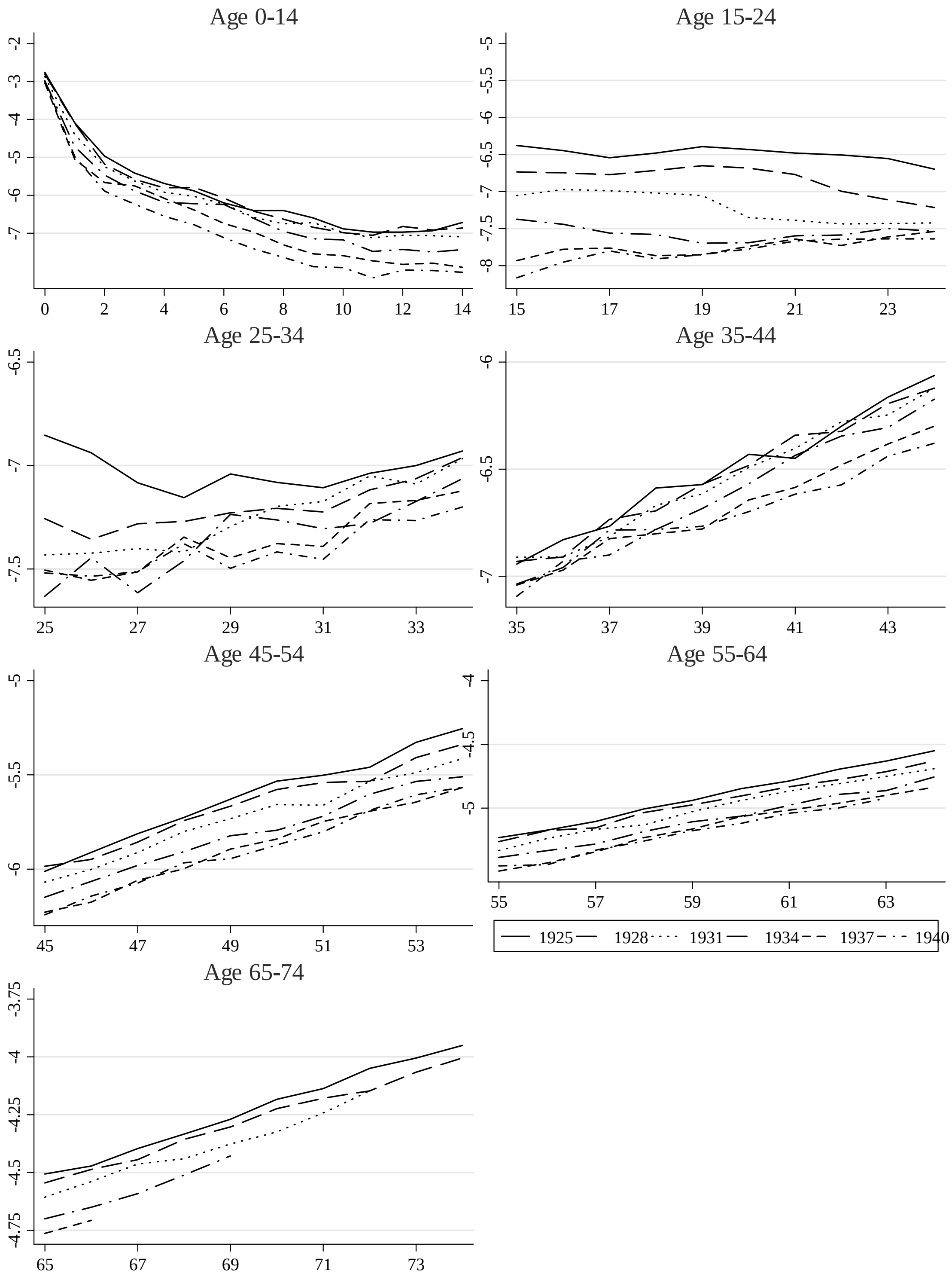
Drink Little: Females
Drink Little
.6
.7
.8
.9
1921
1924
1927
1930
1933
1936
1939
1942
1945
1948
1951
Birthdate

Appendix Figure 1: Log odds of dying (men)



Notes: Cohorts are pairs of adjacent birth cohorts. Hence “1925” includes birth cohorts 1925 and 1926, “1928” includes 1928 and 1929 and so on.

Appendix Figure 2: Log odds of dying (women)



Notes: Cohorts are pairs of adjacent birth cohorts. Hence “1925” includes birth cohorts 1925 and 1926, “1928” includes 1928 and 1929 and so on.

Table 1
First-Stage Regression Discontinuity Estimates of the Compulsory Schooling Change
Year of Birth Analysis

Males

	Fraction Dropping Out by 14			Fraction Dropping Out by 15			Age Left School		
	-0.273	-0.431	-0.402	-0.014	-0.021	-0.019	0.255	0.423	0.390
	(0.079)	(0.014)	(0.060)	(0.010)	(0.010)	(0.009)	(0.089)	(0.033)	(0.059)
Post-1933 dummy	X	X		X	X		X	X	
Exclusion of 1933 cohort		X			X			X	
Revised post-1933 dummy			X			X			X
Mean of Dependent Variable	0.28	0.28	0.28	0.69	0.69	0.69	15.24	15.24	15.24
Observations	116,558	113,173	116,558	116,558	113,173	116,558	116,558	113,173	116,558

Females

	Fraction Dropping Out by 14			Fraction Dropping Out by 15			Age Left School		
	-0.279	-0.440	-0.407	-0.005	-0.014	-0.012	0.273	0.438	0.404
	(0.079)	(0.013)	(0.059)	(0.009)	(0.010)	(0.009)	(0.086)	(0.039)	(0.055)
Post-1933 dummy	X	X		X	X		X	X	
Exclusion of 1933 cohort		X			X			X	
Revised post-1933 dummy			X			X			X
Mean of Dependent Variable	0.28	0.28	0.28	0.70	0.70	0.70	15.20	15.20	15.20
Observations	122,451	118,915	122,451	122,451	118,915	122,451	122,451	118,915	122,451

Notes: Table gives the estimated discontinuities in the specified outcome due the 1947 compulsory school age change. All regressions estimated using individual-level General Household Survey data for the years 1986-1996 and 1998 and include the 1921-1951 British-born birth cohorts.
All regressions include a cubic polynomial in year of birth interacted with post dummy along with age fixed effects. Robust standard errors clustered on quarter of birth 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 2
First-Stage Regression Discontinuity Estimates of Compulsory Schooling Change
Quarter of Birth Analysis

Males

	Fraction Dropping Out by 14	Fraction Dropping Out by 15	Age Left School
	-0.491	-0.005	0.442
	(0.018)	(0.015)	(0.039)
Mean of Dependent Variable	0.14	0.68	15.38
Observations	42,246	42,246	42,246

Females

	Fraction Dropping Out by 14	Fraction Dropping Out by 15	Age Left School
	-0.531	-0.027	0.538
	(0.016)	(0.012)	(0.040)
Mean of Dependent Variable	0.14	0.68	15.41
Observations	44,528	44,528	44,528

Notes: Table gives the estimated discontinuities in the specified outcome due the 1947 compulsory school age change (i.e., coefficient on post-1933 dummy). All regressions estimated using individual-level General Household Survey data for the years 1972-1996, 1998 and include the 1921-1951 British -born birth cohorts. All regressions include a cubic polynomial in quarter of birth interacted with post dummy along with age and year fixed effects. Robust standard errors clustered on quarter of birth in parentheses. The post-1933 dummy is equal to 1 for cohorts born in 1933Q2 and later and 0 otherwise. The mean of the dependent variable is measured for the 1933Q2 cohort.

Table 3
First-Stage Regression Discontinuity Estimates of Compulsory Schooling Change on Age Left School
Month of Birth Analysis

Males

	Fraction Dropping Out by 14	Fraction Dropping Out by 15	Age Left School
	-0.477	0.019	0.399
	(0.016)	(0.014)	(0.038)
Mean of Dependent Variable	0.16	0.69	15.36
Observations	42,246	42,246	42,246

Females

	Fraction Dropping Out by 14	Fraction Dropping Out by 15	Age Left School
	-0.511	-0.000	0.487
	(0.014)	(0.014)	(0.039)
Mean of Dependent Variable	0.11	0.59	15.56
Observations	44,528	44,528	44,528

Notes: Table gives the estimated discontinuities in the specified outcome due the 1947 compulsory school age change (i.e., coefficient on post-1933 dummy). All regressions estimated using individual-level General Household Survey data for the years 1972-1996, 1998 General Household Surveys and include the 1921-1951 British-born birth cohorts. All regressions include a cubic polynomial in month of birth interacted with post dummy along with age and year fixed effects. Robust standard errors clustered on month of birth in parentheses. The post-1933 dummy is equal to 1 for cohorts born in 1933Q2 and later and 0 otherwise. The mean of the dependent variable is measured for the April 1933 cohort.

Table 4: Reduced-form regression discontinuity estimates of school reform on years of survival between various ages

	Men (1)	Men (2)	Men (3)	Men (4)	Women (1)	Women (2)	Women (3)	Women (4)
Panel A: Years of survival between ages 14 and 55								
	Men (mean=39.660, sd=5.340)				Women (mean=40.079, sd=4.526)			
	-0.256	-0.389	-0.355	1.161	0.024	0.027	0.027	0.018
	(0.183)	(0.233)	(0.208)	(0.266)	(0.023)	(0.030)	(0.029)	(0.054)
Post-1933 dummy	X	X		X	X	X		X
1933 cohort excluded		X		X		X		X
Revised post-1933 dummy			X				X	
Polynomial*dummy				X				X
Panel B: Years of survival between ages 25 and 55								
	Men (mean=29.094, sd=3.696)				Women (mean=29.391, sd=3.062)			
	-0.023	-0.015	-0.020	0.061	-0.011	-0.005	-0.008	0.003
	(0.023)	(0.028)	(0.028)	(0.040)	(0.014)	(0.016)	(0.016)	(0.030)
Post-1933 dummy	X	X		X	X	X		X
1933 cohort excluded		X		X		X		X
Revised post-1933 dummy			X				X	
Polynomial*dummy				X				X
Panel C: Years of survival between birth and age 14								
	Men (mean=12.590, sd=4.084)				Women (mean=12.869, sd=3.688)			
	0.048	0.076	0.068	0.044	0.040	0.062	0.056	-0.012
	(0.047)	(0.044)	(0.046)	(0.120)	(0.036)	(0.033)	(0.035)	(0.112)
Post-1933 dummy	X	X		X	X	X		X
1933 cohort excluded		X		X		X		X
Revised post-1933 dummy			X				X	
Polynomial*dummy				X				X

Notes: Mean and standard deviations refer to the 1932 (i.e., last pre-reform) birth cohort. Each model uses birth cohort data from 1921 to 1951 except where indicated. Each model includes a cubic function in birth cohort (defined using year of birth), except where indicated. Robust standard errors in parentheses.

Table 5: Reduced-form regression discontinuity estimates of school reform on mortality rates (percentage points)

Panel A: Mortality rates between ages 14 and 24							
Men (mean=1.23)				Women (mean=0.856)			
(1)	(2)	(3)	(4)	(1)	(2)	(3)	(4)
0.543	0.818	0.738	-3.215	-0.133	-0.130	-0.139	-0.032
(0.460)	(0.505)	(0.472)	(0.644)	(0.069)	(0.080)	(0.077)	(0.092)
Panel B: Mortality rates between ages 25 and 34							
Men (mean=1.19)				Women (mean=0.788)			
0.124	0.117	0.127	-0.230	0.081	0.086	0.088	-0.085
(0.051)	(0.059)	(0.060)	(0.084)	(0.039)	(0.045)	(0.045)	(0.039)
Panel C: Mortality rates between ages 35 and 55							
Men (mean=8.97)				Women (mean=5.74)			
0.127	0.039	0.051	-0.304	0.026	-0.040	-0.012	0.318
(0.226)	(0.271)	(0.300)	(0.457)	(0.130)	(0.144)	(0.148)	(0.289)
Panel D: Mortality Rates between ages 35 and 55 (Circulatory Diseases)							
Men (mean=4.10)				Women (mean=1.43)			
0.077	0.025	0.046	-0.136	-0.022	-0.027	-0.025	-0.068
(0.243)	(0.303)	(0.071)	(0.169)	(0.029)	(0.033)	(0.042)	(0.074)
Panel E: Mortality rates between ages 35 and 55 (Respiratory Diseases)							
Men (mean=0.390)				Women (mean=0.287)			
0.049	-0.032	-0.026	0.003	-0.027	-0.023	-0.027	0.037
(0.072)	(0.020)	(0.023)	(0.027)	(0.017)	(0.020)	(0.020)	(0.017)
Panel F: Mortality Rates between birth and age 14							
Men (mean=11.2)				Women (mean=9.0)			
-0.302	-0.511	-0.444	-0.178	-0.306	-0.469	-0.421	0.238
(0.343)	(0.324)	(0.338)	(0.872)	(0.263)	(0.241)	(0.254)	(0.816)

Notes: Models (1)-(4) refer to those in Table 4. Mean and standard deviations refer to the 1932 (i.e., last pre-reform) birth cohort. Each model uses birth cohort data from 1921 to 1951 except where indicated. Each model includes a cubic function in birth cohort (defined using year of birth), except where indicated. Robust standard errors in parentheses.

.

Table 7
Reduced-Form Regression Discontinuity Estimates of the Compulsory Schooling Change
Year of Birth Analysis

Males

	Health Good			Smoke Now			Drink Little		
	0.025	0.030	0.030	-0.014	-0.014	-0.015	-0.028	-0.080	-0.069
	(0.010)	(0.012)	(0.012)	(0.013)	(0.014)	(0.014)	(0.032)	(0.019)	(0.026)
Post-1933 dummy	X	X		X	X		X	X	
Exclusion of 1933 cohort		X			X			X	
Revised post-1933 dummy			X			X			X
Mean of Dependent Variable	0.56	0.56	0.56	0.44	0.44	0.44	0.59	0.59	0.59
Observations	51,488	49,981	51,488	47,278	45,856	47,278	33,790	32,757	33,790

Females

	Health Good			Smoke Now			Drink Little		
	-0.053	-0.045	-0.050	0.020	0.015	0.017	-0.023	0.000	-0.004
	(0.010)	(0.011)	(0.012)	(0.019)	(0.019)	(0.023)	(0.014)	(0.017)	(0.023)
Post-1933 dummy	X	X		X	X		X	X	
Exclusion of 1933 cohort		X			X			X	
Revised post-1933 dummy			X			X			X
Mean of Dependent Variable	0.52	0.52	0.52	0.52	0.52	0.52	0.75	0.75	0.75
Observations	56,784	55,078	56,784	43,960	42,729	43,960	35,359	34,335	35,359

Notes: Table gives the estimated discontinuities in the specified outcome due the 1947 compulsory school age change. All regressions estimated using individual-level General Household Survey data for available years and include the 1921-1951 British-born birth cohorts. All regressions include a cubic polynomial in year of birth interacted with post dummy along with age and year fixed effects. Robust standard errors clustered on quarter of birth 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.

Table 8
Instrumental Variable Regression Discontinuity Estimates of the Effect of Age at School Exit
Year of Birth Analysis

Males

	Health Good			Smoke Now			Drink Little		
	0.081	0.067	0.070	-0.041	-0.031	-0.032	-0.068	-0.140	-0.128
	(0.039)	(0.028)	(0.033)	(0.036)	(0.026)	(0.028)	(0.068)	(0.035)	(0.042)
Post-1933 dummy	X	X		X	X		X	X	
Exclusion of 1933 cohort		X			X			X	
Revised post-1933 dummy			X			X			X
Mean of Dependent Variable	0.56	0.56	0.56	0.44	0.44	0.44	0.59	0.59	0.59
Observations	51,488	49,981	51,488	47,278	45,856	47,278	33,790	32,757	33,790

Females

	Health Good			Smoke Now			Drink Little		
	-0.162	-0.095	-0.110	0.107	0.040	0.052	-0.085	0.001	-0.012
	(0.060)	(0.025)	(0.037)	(0.132)	(0.053)	(0.081)	(0.072)	(0.044)	(0.062)
Post-1933 dummy	X	X		X	X		X	X	
Exclusion of 1933 cohort		X			X			X	
Revised post-1933 dummy			X			X			X
Mean of Dependent Variable	0.52	0.52	0.52	0.52	0.52	0.52	0.75	0.75	0.75
Observations	56,784	55,078	56,784	43,960	42,729	43,960	35,359	34,335	35,359

Notes: Table gives the instrumental variables estimates of the effect of a one year increase in the age at school exit. All regressions estimated using individual-level General Household Survey data for the available years and include the 1921-1951 British-born birth cohorts. All regressions include a cubic polynomial in year of birth interacted with post dummy along with age fixed effects. Robust standard errors clustered on year of birth 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 9
Reduced-Form Regression Discontinuity Estimates of Compulsory Schooling Change
Quarter of Birth Analysis

Males

	Health Good	Health Bad	Long Illness	Limited Activity	Ever Smoked	Smoke Now	Age Started Smoking	Drink Now	Drink Lots	Drink Little
	0.002	0.030	0.014	0.023	0.009	0.011	0.046	-0.002	-0.015	-0.017
	(0.016)	(0.010)	(0.019)	(0.021)	(0.013)	(0.024)	(0.196)	(0.013)	(0.011)	(0.022)
Mean of Dependent Variable	0.53	0.19	0.52	0.57	0.81	0.33	16.76	0.91	0.04	0.62
Observations	39,691	39,691	42,197	18,019	22,344	19,148	14,233	22,337	21,251	21,251

Females

	Health Good	Health Bad	Long Illness	Limited Activity	Ever Smoked	Smoke Now	Age Started Smoking	Drink Now	Drink Lots	Drink Little
	-0.018	-0.014	0.025	-0.006	0.015	-0.016	-0.319	0.012	0.003	0.006
	(0.014)	(0.012)	(0.012)	(0.015)	(0.018)	(0.027)	(0.396)	(0.016)	(0.004)	(0.020)
Mean of Dependent Variable	0.47	0.14	0.50	0.64	0.66	0.35	19.06	0.83	0.02	0.69
Observations	43,680	43,680	44,469	18,821	24,611	17,375	11,498	24,605	22,102	22,102

Notes: Table gives the estimated discontinuities in the specified outcome due the 1947 compulsory school age change (i.e., coefficient on post-1933 dummy). All regressions estimated using individual-level General Household Survey data for the years 1972-1996, 1998 General Household Surveys and include the 1921-1951 British-born birth cohorts. All regressions include a cubic polynomial in quarter of birth interacted with post dummy along with age and year fixed effects. Robust standard errors clustered on quarter of birth in parentheses. The post-1933 dummy is equal to 1 for cohorts born in 1933Q2 and later and 0 otherwise. The mean of the dependent variable is measured for the 1933Q2 cohort.

Table 10
Instrumental Variable Regression Discontinuity Estimates of the Effect of Age at School Exit
Quarter of Birth Analysis

Males

	Health Good	Health Bad	Long Illness	Limited Activity	Ever Smoked	Smoke Now	Age Started Smoking	Drink Now	Drink Lots	Drink Little
	0.029	0.011	-0.000	0.017	0.005	0.016	-0.314	-0.006	-0.022	-0.001
	(0.043)	(0.030)	(0.023)	(0.031)	(0.037)	(0.031)	(0.347)	(0.021)	(0.015)	(0.030)
Mean of Dependent Variable	0.53	0.19	0.52	0.57	0.81	0.33	16.76	0.91	0.04	0.62
Observations	39,691	39,691	42,197	18,019	22,344	19,148	14,233	22,337	21,251	21,251

Females

	Health Good	Health Bad	Long Illness	Limited Activity	Ever Smoked	Smoke Now	Age Started Smoking	Drink Now	Drink Lots	Drink Little
	-0.035	-0.049	0.026	0.019	0.518	-0.019	-0.200	0.030	0.008	0.022
	(0.048)	(0.059)	(0.016)	(0.019)	(1.191)	(0.038)	(0.676)	(0.145)	(0.013)	(0.059)
Mean of Dependent Variable	0.47	0.14	0.50	0.64	0.66	0.35	19.06	0.83	0.02	0.69
Observations	43,680	43,680	44,469	18,821	24,611	17,375	11,498	24,605	22,102	22,102

Notes: Table gives the instrumental variables estimates of the effect of a one year increase in the age at school exit. All regressions estimated using individual-level General Household Survey data for the available years and include the 1921-1951 British-born birth cohorts. All regressions include a cubic polynomial in quarter of birth interacted with post dummy along with age and year fixed effects. Robust standard errors clustered on year of birth in parentheses. The post-1933 dummy is equal to 1 for cohorts born in 1933Q2 and later and 0 otherwise. The mean of the dependent variable is measured for the 1933Q2 cohort.