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DOES EDUCATION MATTER? TESTS FROM EXTENSIONS OF COMPULSORY SCHOOLING IN ENGLAND AND WALES 1919-22, 1947, AND 1972

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Does Education Matter? Tests from Extensions of Compulsory Schooling in England and Wales 1919-22, 1947 and 1972

Gregory Clark and Neil Cummins*

August 28, 2020

Abstract

Schooling and social outcomes correlate strongly. But are these connections causal? Previous papers for England using compulsory schooling to identify causal effects have produced conflicting results. Some found significant effects of schooling on adult longevity and on earnings, others found no effects. Here we measure the consequence of extending compulsory schooling in England to ages 14, 15 and 16 in the years 1919-22, 1947 and 1972. From administrative data these increases in compulsory schooling added 0.43, 0.60 and 0.43 years of education to the affected cohorts. We estimate the effects of these increases in schooling for each cohort on measures of adult longevity, on dwelling values in 1999 (an index of lifetime incomes), and on the the social characteristics of the places where the affected cohorts died. Since we have access to all the vital registration records, and a nearly complete sample of the 1999 electoral register, we find with high precision that all the schooling extensions failed to increase adult longevity (as had been found previously for the 1947 and 1972 extensions), dwelling values, or the social status of the communities people die in. Compulsory schooling ages 14-16 had no effect, at the cohort level, on social outcomes in England.

1 Introduction

Across a wide variety of countries, educational attainment is correlated with better social outcomes: adult health, longevity, occupational status, earnings, and wealth (Cutler et al. (2011); Mackenbach et al. (2008)). However, attempts to establish a causal influence of education on social outcomes using compulsory schooling laws have produced conflicting results.

Here we look at three extensions of compulsory schooling in England and Wales in 1919-22, 1947 and 1972 which respectively raised the school leaving age to from 12 to 14, from 14 to 15 and from 15 to 16. To estimate the effects of the extension of compulsory education on average years of schooling, we have good administrative statistics for the various extensions showing the change in average years of schooling. As outcome variables for those who were subject to extra education after the compulsory schooling extensions we have the following:

*Gregory Clark; UC Davis, LSE and CEPR. Neil Cummins; LSE and CEPR. Thanks to Matt Curtis and Marianne Page for valuable comments and suggestions.

Table 1: Education Extensions in the UK, 1918-1944

Parliamentary Act	Full Implementation	Leaving Age	Treatment Birth Cohort	extra years schooling
1918	1922, September	14	1910-4th quarter*	+.43
1944	1947, 1st April	15	1933-2nd quarter	+.60
1944	1972, September	16	1957-4th quarter	+.43

Note: The treatment cohort represents the first cohort fully affected by the schooling extension. * For the 1922 extension (1918 Act), the pre treatment cohorts are taken as those born 1902-4th quarter to 1907-3rd quarter.

(1) Adult longevity. For those effected by the 1919-22 extension we have adult longevity information up to age 90 for the entire population. For those affected by the 1947 reform we can observe the share living to age 65, again for the whole population. For those affected by the 1972 reform we observe just the share living to age 45. However, here we have longevity at the population level. The death records in England and Wales give exact date of births for deaths registered in 1969-2nd quarter and later. So for most of adult longevity for the cohorts affected by the 1947 and 1972 extensions, we can date precisely the cohorts affected by the reform and those not affected.

(3) Property value in 1999. For a sample of those still alive in 1999, we can match those with rarer surnames to the electoral register, and from this get their address and an estimate of the average value of houses on their street in the years 1995-2015 from the UK Land Registry. To make this match, we have to match a birth record to the 1999 electoral register. We thus restrict these matches to men, since typically only unmarried women would still bear their birth surname. In the relevant years the birth register shows just the quarter of registration of births. The first cohort of men affected by the 1919 reform will be 94 in 1999, so this measure is at the end of their lives. However, the first cohorts affected by the 1947 and 1972 reforms will be 66 and 42 respectively, so in prime years. The numbers of men we are able to so match within five years of each extension is 18,258 for 1919-22, 83,126 for 1947 and 115,193 for 1972. The density of the data around the latter two extensions allows for very precise estimates of the effects.

(2) The social status of the registration districts that people died in for those affected by the 1922 and 1947 schooling extensions. If schooling raises earnings and occupational status, then we should observe greater concentration of deaths of the cohort that experienced more compulsory schooling in districts of higher average social status. Here we again observe location at death for the entire population for cohorts just before and after the compulsory education extensions.

Though the 1947 and 1972 schooling extensions have been extensively studied before, this is the first paper to study the effects of the 1919-22 extension. We also have novel social outcomes measured on a much larger scale, which are house values 1999, and the social status of the place of death. We find across all three measures and all three extensions of compulsory schooling, no sign of any positive social effects from the extension of schooling. They did not affect health, lifetime earnings, or the social status of places of death.

Earlier studies of schooling extensions in England have produced conflicting results, though a majority found strong positive effects. These studies are summarized in table 2. Harmon and Walker (1995) measure the effects of the 1947 and 1972 schooling extensions in the UK using data on the earnings of 34,336 employed males aged 18-64 surveyed 1978-1986 in the Family Expenditure Survey. The find an estimated 15% gain from an additional year of compulsory schooling. Oreopoulos (2006)

Table 2: The Effects of Compulsory Education Extensions in the UK

Authors	Outcome	Effects	Effect of 1 Extra Schooling Year
Harmon and Walker (1995)	Earnings	**+**	+15%
Oreopoulos (2006)	Wages	**+**	+15%
Devereux and Hart (2010)	Earnings	**+**/0	+6%/0
Machin et al. (2011)	Crime*	**+***	-11.6%*
Lindeboom et al. (2009)	Child Health	**0**	0
Silles (2009)	Health Status	**+**	+5%
Clark and Royer (2013)	Mortality	**0**	0
Dickson et al. (2016)	Children's Education	**+**	+0.1 *SD*
Dolton and Sandi (2017)	Earnings	**+**	+6%
Delaney and Devereux (2019)	Earnings Volatility	**+***	-0.1 *SD*/0

Notes: The quoted 'effect' is the one claimed by the authors in either the abstract, introduction or conclusion (the 'headline' finding). * indicates were the effect was negative: it is indicated by **+** as it is beneficial (for ease of interpretation). *SD* is standard deviation.

similarly finds that the extension of the school leaving age from 14 to 15 in Britain in 1947, and Northern Ireland in 1957, also created a 15% increase in wages from a year of schooling. However Devereux and Hart (2010), using the same data as Oreopoulos (2006), report a 0% gain in earnings for women, and a 5.5% gain for men (the Oreopoulos result they could show was partly created by programming errors in the original paper). With richer data on earnings from another much larger UK earnings survey they find for the 1947 schooling extension a year of schooling led again to a 0% gain for women, and now just a 3-4% gain for men. Using that same earnings survey Delaney and Devereux (2019) find that the 1972 extension had no statistically significant effect on earnings averaged across ages 20-60 for men or women, though a point estimate of a 6% gain from a year of additional schooling for men. Dolton and Sandi (2017), however, report that for the 1947 extension, rate of return estimates are surprisingly sensitive to the polynomials controlling for year and age effects. Returns "appear sensitive to the choice of the polynomial used to describe the underlying unobservable trends in education and earnings in the sample: our estimates range from 5–6% and statistically significant when using polynomials of order three or four to 0–3% and non-statistically significant when using polynomials of order one and two." (p. 100).

Clark and Royer (2013) found no effects on mortality rates from extensions of the school leaving age in the United Kingdom from 14 to 15 in 1947 and later from 15 to 16 in 1972 using data on the entire population until 2007. Yet Silles (2009) reports for the UK 1947 and 1972 schooling extensions "evidence of a causal relation running from more schooling to better health which is much larger than standard regression estimates suggest." Machin et al. (2011) report substantial effects of the 1972 England and Wales extension in reducing criminal conviction rates for ages 18-45 for the affected cohorts.

We consider below how our results fit with this existing body of literature. Our conclusion is that for England at least the best evidence now is for an absense of any effect of compulsory schooling on social outcomes.

1.1 Extensions of Compulsary Schooling 1919-22, 1947, 1972

The 1944 Education Act raised the school leaving age from 14 to 15. The raising of the leaving age occurred on April 1, 1947. Those born on April 1, 1933 and later had to attend school until the end of the term in which they turned 15. There are administrative data on the number of students on the rolls of schools, by age, on Dec 31 or Jan 30 in each of the years before and after the 1947 extension. The annual Reports of the Board of Education give these numbers for Dec 31 of each year for 1930-1938. There is a gap in the enrollment data for the war years. But the series resumes with the annual Reports of the Ministry of Education in 1947, which gives student numbers Dec 31, 1946 and 1947, then Jan 30, 1949-1952. From these reports we derive figure 1c, which shows the estimated fraction of 14 year olds enrolled in schools in each year 1930-1951.[1] That number is a constant 40% of the cohort through the years 1900-38, and also for 1946 before the reform was enacted in 1 April, 1947. In January 1948 enrollment rose to 85% of those aged 14, and by January 1949 it was 100%, consistent with full implementation of the new leaving age. This implies the extension of the compulsory education age to 15 increased education per person for the affected cohorts by an average of 0.6 of a year.[2]

The 1939 population survey, conducted on September 28 when all schools would be in session, shows that school leaving ages were enforced. This survey reports birth dates, so we can see for anyone under the mandated school leaving age of 14 whether or not they were listed in school. As figure 1b shows more than 99% of a sample of children under age 14 were listed as attending school. Of those aged 14 at the date of the Population Register, 33% were listed as being in school.

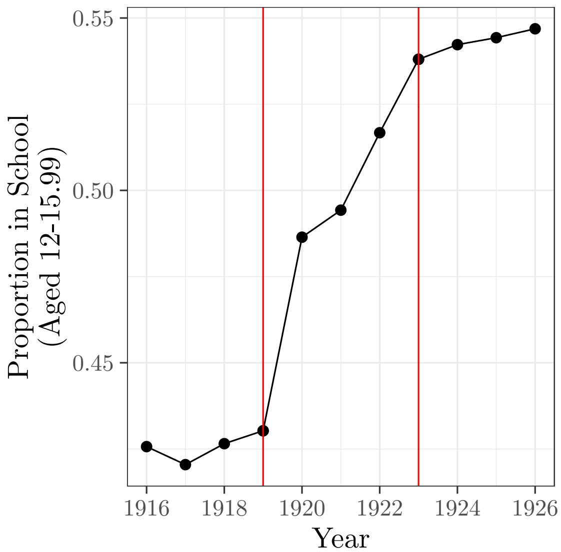
The 1972 increase of the school leaving age to 16 was implemented under the powers of the 1944 Act. Administrative data show that for the UK in 1970, only 57% of 15 year olds were in school. Thus the extension added at least 0.43 of a year of schooling to the cohort born September 1956 and later (Bolton, 2012, table 5).

The 1918 Education Act, which extended compulsory schooling from age 12 to 14, had a more drawn out introduction. Administrative data on school enrollment from the Reports of the Board of Education for those aged 12-16 in 1916-1926 show that the leaving age requirements of this Act were in fact introduced over 4 years. Figure 1b shows there was a sharp rise in school attendance by 12-14 year olds in the school year 1919-20, but then three more modest increases all the way to full implementation of the leaving age of 14 in the 1922-3 school year. The overall effect was an increase of 0.43 years of schooling, with 0.22 of that increase occurring in the school year 1919-20. Therefore in the estimates below the treatment cohorts are taken as births 1910-4th quarter to 1915-3rd quarter, and the pre-treatment cohorts as births 1902-4th quarter to 1907-3rd quarter.

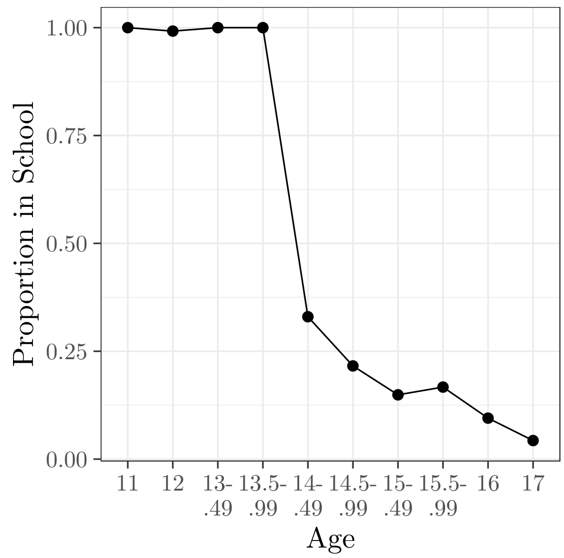
For convenience in the text below we refer to the three increases in compulsory schooling as the 1922, 1947 and 1972 reforms, reflecting the year of full implementation in each case.

[1] We estimate this fraction from the numbers of 13 year olds enrolled the previous year, allowing 1% for mortality, assuming all 13 years old were enrolled.

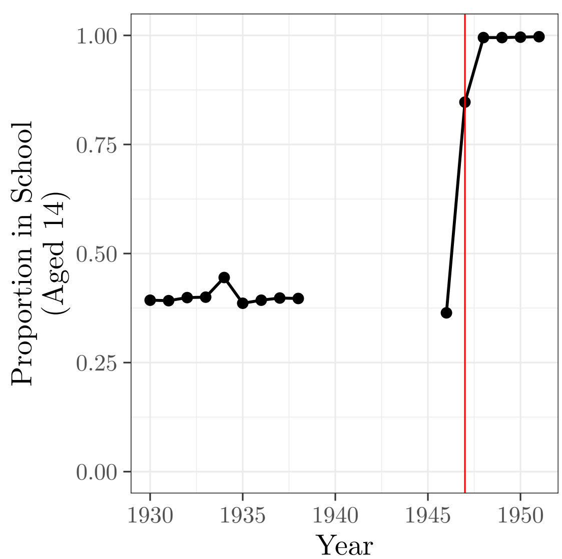
[2] This assumes all on the rolls attended school every day. But that is the implicit assumption of the returns to education literature.



(a) School Attendance, 1916-1926



(b) School Attendance, 1939



(c) School Attendance, 1930-1951

Figure 1: The Effects of the 20th Century Schooling Extensions
Sources: Board of Education Reports 1920-1927, UK Statistical Abstract 1927, Bolton (2012).

2 Data

2.1 Births and Deaths, 1837-2007

We compiled a database of all 127,760,704 births and 87,107,052 deaths, 1837-2007, for England and Wales by downloading the individual index entries from two websites: freebmd.com (1837-1980) and famliysearch.org (1980-2007). A separate online appendix details this procedure and is available at `http://neilcummins.com/ns_online_appendix.pdf`. After 1866, the death index records the date of death by quarter and the integer age at death. In 1970, calendar year of birth is instead recorded. By 1984, the index reports the exact date of birth. Also starting in 1984 the date of death is recorded by month instead of by quarter. Table 14 in the appendix reports details of the death records data and the calculations required to generate estimated ages at death and dates of birth.

For the analysis of lifespan we construct birth cohorts from the 100% sample of deaths in England and Wales for the 5 years before and after the reforms. For the analysis of survival rates, we calculate counts of births and deaths from the 100% vital records to estimate annual survival rates, by birth year, for each of the 5 years before and after the schooling reform.

2.2 The 1999 UK electoral roll

All voters in the UK were listed in the *1999 electoral roll*. We extracted these records from a CD-ROM *UK-Info Disk* (2000). 1999 was the last year that the complete, pre opt-out, electoral roll was available. (After 1999, registered voters could choose not to be reported on the electoral roll, which is often used by marketers to generate mass mailings. Thus the public electoral rolls 2002-2020 report details of only about half the electorate). Our extraction method resulted in 31,551,398 observations of forename, surname, specific address, and post-code for 1999.[3] While this is only 70% of the names on the roll, it represents essentially a 100% sample of names that can be uniquely linked to earlier birth records, since the missing names are all those of people with common surnames. This too is detailed further at `http://neilcummins.com/ns_online_appendix.pdf`.

For the analysis of the effects of compulsory schooling on house values in 1999 (section 4.3), we link the micro data by exact name. First we generate subsets of the 100% birth and death records, where the first name, surname and year of birth are unique combinations. We then link these data to each other by exact name. From this we calculate that there are potentially 24 million observed births alive in 1999. We link these records by exact name to the electoral roll. As the electoral roll does not record any birth or age information we include in our final sample only those who are both unique and exact matches. This is 730,353 men. The process is summarized, and counts reported for each linking stage, in figure 2. To guard against mismatches we match only for male first names, since women can acquire a new surname at marriage. The linking procedure is detailed, with linkage rates reported for each stage and a comparison of the linked sample to the population, in appendix section 15.

[3]Extracting the data from the 20 year-old CD-Rom interface was a technical challenge as only 250 records per individual search could be returned with an upper limit of 2,000 for any search criteria. Automation via jitbit Macro Recorder (`https://www.jitbit.com/macro-recorder/`) over several months resulted in 31 million duplicate free records. This represents a sample of 70% of the entire roll of 44 million voters. The sample is complete for rarer names but incomplete for common names (those with more than 2,000 holders in the voting roll) due to the 2,000 results per query limit.

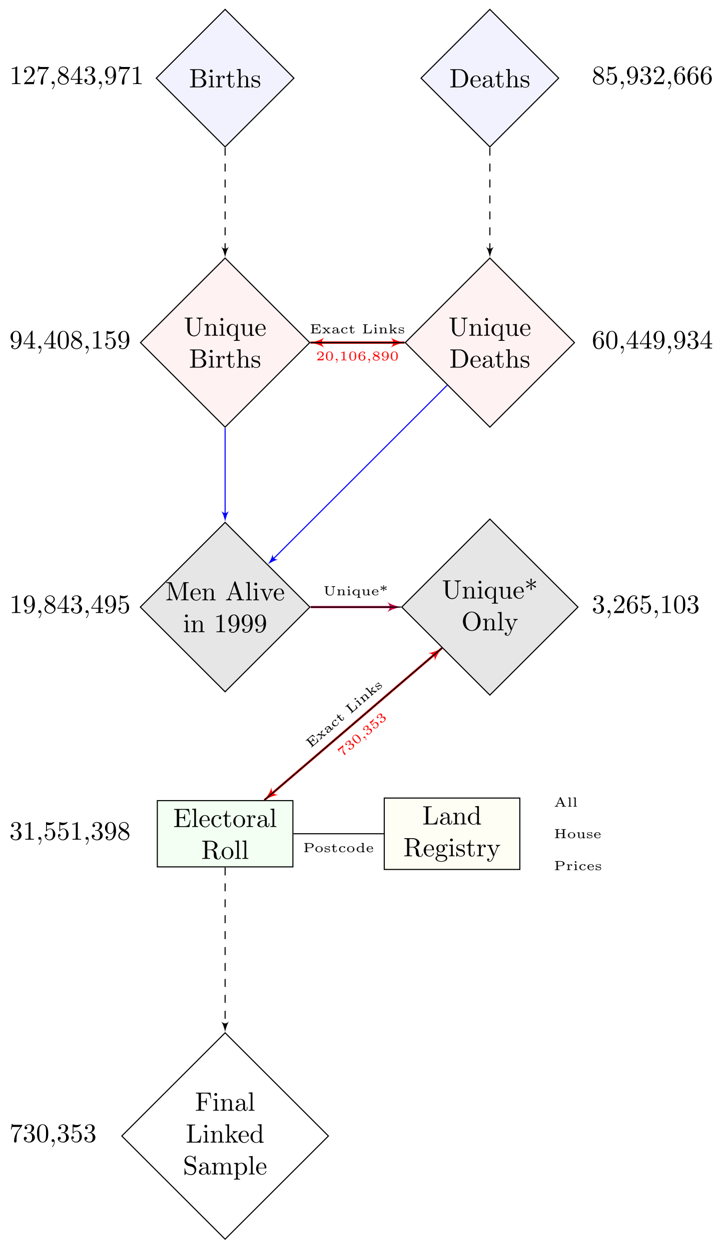


Figure 2: Linking between the Data-Sets, Counts and Method

Notes: Alive in 1999 is defined as those births between 1890 and 1981 that are not linked to a death or who die after 1998. "Unique" refers to unique forename, surname, birth year combinations in the case of births, and unique forename, surname, death year combinations in the case of deaths. Unique* refers to unique forename-surnames only.

2.3 The Land Registry, 1995-2005

We link the post code of the address of each individual from the electoral roll of 1999 to house price data by postcode in 1995-2005 from the land registry.[4] There are 1,758,312 postcodes in the UK so this is a highly specific estimate of average local house values, typically the average value across 10-20 houses. The resulting prices were deflated by the CPI to 2017 prices.[5] Average house value by postcode in 2017 prices varied in England from £8,000 to £24,000,000, with a pronounced right skew in the distribution. Therefore in the estimations of the effects of schooling we use the logarithm of house values to have an outcome that has a closer to normal distribution.

2.4 District of Death

We use the characteristics of the district of death to test for differential migration of the treated group to areas of higher socio-economic status. For death records 1984 and later, we have a precise date of birth to locate people and treated and untreated cohorts. For deaths 1922-1969 we get from the death record only an estimated year of birth, with some corresponding uncertainty on whether a person belongs to a treated or untreated cohort.

We can measure the socio-economic status of the district of death in a number of ways. First the infant mortality rate, and the average adult age of death by district, 1980-9, calculated from 100% birth and death records across 357 registration districts. Second we have for 340 census districts in 2001 the Index of Multiple Deprivation in 2019, where higher values on this index denote poorer average social conditions. These census districts can be matched to death registration districts. The Index of Multiple Deprivation varies from a high of 45 in Blackpool, Lancashire to a low of 5.5 in Hampshire.

Using data from our Families of England database which is a genealogical database of 400,000 individuals in family trees in England 1700-2020, we can see that for men individual occupational status is strongly correlated with the social characteristics of place of death, for men dying 1920 and later. Table 3 shows the correlation between occupational status, measured on a 0-1scale, with the index of multiple deprivation, with infant mortality rates 1980-89, and with average longevity for deaths in each district 1980-89.

[4]There are 12,414,008 transactions recorded in this 'Price paid' data, 1995-2005, which was downloaded from `http://prod.publicdata.landregistry.gov.uk.s3-website-eu-west-1.amazonaws.com/pp-complete.txt` (HM Land Registry, 2018).

[5]Using CPI data from https://www.ons.gov.uk.

Table 3: District Status Measures and Individual Occupational Rank

	District Characteristic		
	Infant Mortality Rate, Per 1,000	Adult Age at Death	Index of Multiple Deprivation
	(1)	(2)	(3)
Occupational Rank	-0.899^{*}	1.198^{***}	-11.035^{***}
	(0.432)	(0.170)	(0.718)
Constant	11.125^{***}	73.995^{***}	26.005^{***}
	(0.142)	(0.056)	(0.236)
Observations	4,696	4,696	4,696
R^2	0.001	0.010	0.048

Note: *p<0.05; **p<0.01; ***p<0.001
Source: 100% BMD Records and *Families of England* database

3 Methodology

3.1 Identification

We identify the effect of the compulsory education extensions in 1919-22, 1947 and 1972 by looking at a band width of five years around each innovation. The five year band was an arbitrary choice. However, the results for all outcomes would be very similar for other band widths since trends are linear and generally similar pre and post the intervention. Thus the pre- and post-cohorts for the first extension 1919-22 are those born 1902-4th quarter to 1907-3rd quarter and 1910-4th quarter to 1915-3rd quarter. For the 1947 extension the pre-cohort is 1928-2nd quarter to 1933-1st quarter, and the post-cohort 1933-2nd quarter to 1938-1st quarter. For 1972 the pre- cohort is September 1952 to August 1957, and the post-cohort September 1957 to August 1962.

For the 1947 and 1972 transitions, for each outcome y_i, we estimate the parameters of the following equations

$$y_{it} = a_0 + b_0 t, t = -5, -0 \quad (1)$$

$$y_{it} = a_1 + b_1 t, t = 0, 5 \quad (2)$$

for both the 5 years before the transition and the 5 years after, where t ranges from -5 to -0 in the pre-transition, and 0-5 post transition. Then the key question is the value of $a_1 - a_0$? For the 1919-22 transition we take $t = 0$ as births 1910.75, and estimate the parameters in equation (1) for t = -8, -3, where -3 is 1907.75. We exclude the period t = 0, -3 because in these years the evidence is of just partial implementation of the 14 year age limit for required schooling. Note that we only use a simple linear time trend here because the data in all cases is consistent with such a simple correction for trend in the 5 year pre and post intervals.

4 Results

4.1 Lifespan (1922 Extension)

Table 4 and figure 3 report the lifespan effects of the 1922 education extension for the treatment cohorts of 1910-1915.

Table 4: The Impact of the Education Extension, 1922

	Age at Death	
	Pre	Post
	(1)	(2)
Trend	0.212***	0.158***
	(0.007)	(0.007)
Female	3.307***	3.293***
	(0.021)	(0.021)
Constant	68.466***	68.197***
	(0.043)	(0.023)
Observations	2,470,887	2,356,456
R^2	0.010	0.010

Note: *p<0.05; **p<0.01; ***p<0.001
OLS estimation
5 years pre-extension: 1902.75-1907.75
5 years post-extension: 1910.75-1915.75

Longevity at age 15 for those born in the first full treatment year, births beginning 1910-4th quarter, is estimated from the 5 pre-reform birth years, 1902.75-1907.75, and also from the 5 post reform birth years 1910.75-1915.75, as in equations (1) and (2). Note that life expectancy by birth year shows little variation from year to year, and a linear upwards trend fits the data well in both the pre-and post years. The upwards trend in the pre- years is however higher at 0.212 years per year than the trend in the post- years of 0.158 years per year. The estimated lifespan for those born 1910.75 from the pre-reform data is 68.466 years at 15, 0.269 years greater than the estimate from the post extension data. Since the standard error of that difference is 0.049, the decline is statistically significant at the 1% level. However if the upward trend in the pre-reform years was 0.158 years per year as it was post-reform then the estimated lifespan for births 1910.75 would be 68.304 years from the pre-extension period, and 68.197 from the post-extension data. The conclusion is that the addition of 0.43 years of compulsory education by 1922 created no gains in

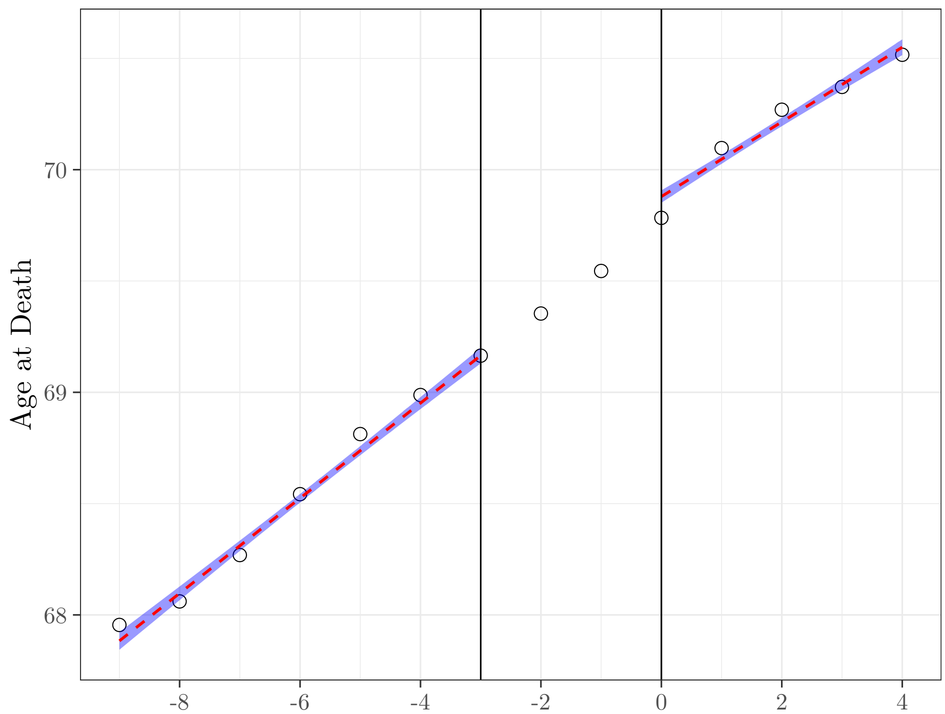


Figure 3: The Impact of the 1922 Education Extension on Lifespan

Source: 100% Sample of Death Records, 1901-1916. N=6,959,625. Year 0 = 1910.75.

lifespan at age 15.

Note in contrast that in Lleras-Muney (2005), state level variations in compulsory education in the USA suggested that an additional year of schooling increased adult longevity by 1.7 years. If any such effect existed in England then we would easily have been able to detect these effects of the 1922 reform with the data utilized above.

4.2 Survival Rates

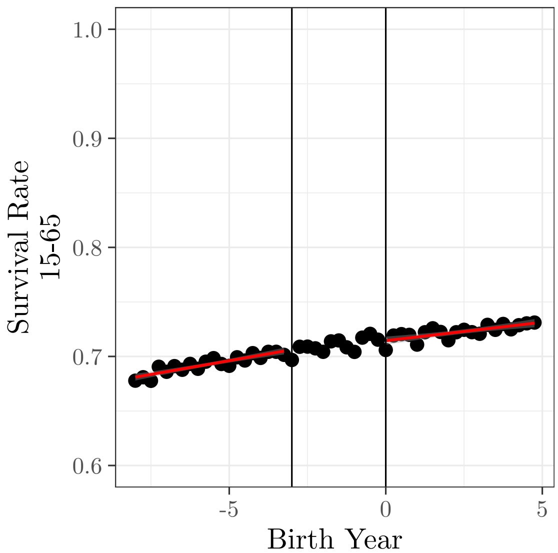
For the 1947 and 1972 extensions of compulsory education we cannot observe the effects on lifespan overall since the data on mortality only runs until 2007, and even in 2020 significant number of people born in the windows before and after the reform are still alive. So to look at the effects of the 1922, 1947 and 1972 extensions we use suvival rates ages 15-65 for the 1922 and 1947 extensions, and ages 15-45 for the 1972 extension. Figure 4 shows survival rates 15-65 for the five years before and after the 1922 and 1947 extensions of compulsory education, and survival rates 15-45 for the five years before and after the 1972 extension of compulsory education. For survival rates we are able to display averages by quarter. The figures suggest strongly that none of these education extensions had any effect on survival rates for the treated cohorts. Note also the smoothness of the trends pre and post the education extensions.

Tables 5, 6 and 7 report the regression estimates of the effects of the 1922, 1947 and 1972 extensions of compulsory education on survival, where the data is aggregated by estimated quarter of birth. The 1922 extension of compulsory education induces no significant increase in the probability of individuals at age 15 surviving to age 65. Similarly the 1947 extension of compulsary education is associated with exactly the same survival rate 15-65. Finally the 1972 extension of compulsary education produces again exactly the same survival rates 15-45. The standard errors on these estimates are also very small. For example for the 1947 compulsory education extension the estimated survival rate 15-65 pre- versus post- is the same 0.847, with the standard error on the difference being 0.002. Thus the extra 0.6 years of education for the cohort born at maximum induced a 0.004 increase in the fraction of people aged 15 living to 65.

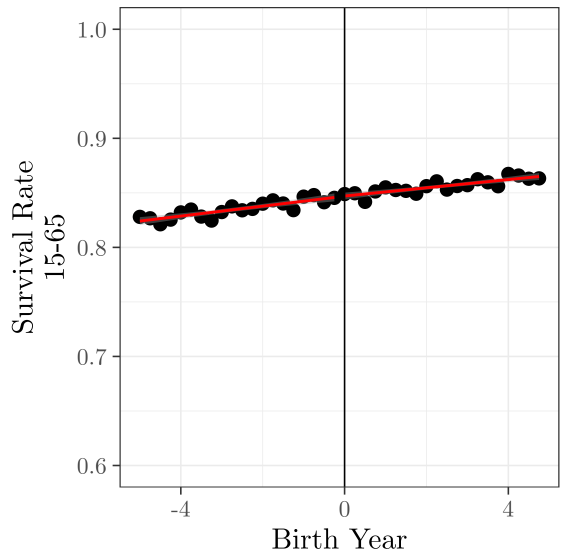
Overall, we see here clear and consistent evidence that additional years of education have no effect on overall longevity, or on survival rates 15-65 or 15-45. The general observed correlation of years of education and longevity seemingly reflects just a selection into education by more robust individuals.

4.3 House Values in 1999

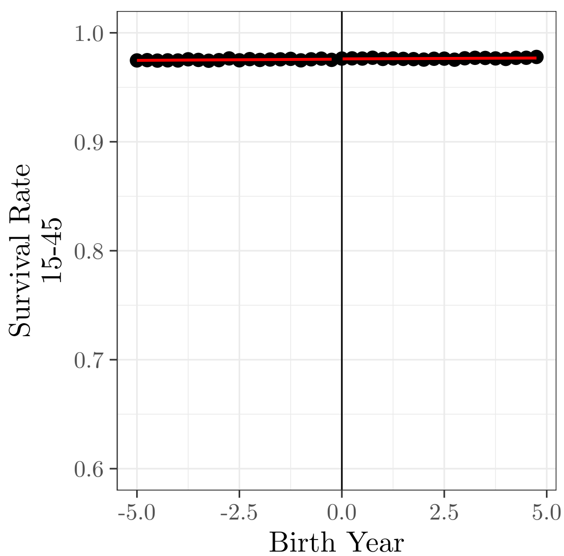
Overall we link 730,353 male births in England and Wales to their electoral roll address 1999, and thus establish their estimated house value in 1999. Figure 5 shows the average house value by year of birth from this linked group. There are clear life cycle effects. For the youngest voters, those aged 18-30, house values are relatively high, presumably because most of them are registered to vote at their parents' address. As they leave home their dwelling value declines, presumably again because they are renting or buying houses of lower value as young adults with lower initial earnings than their parents, and modest wealth accumulation. Then as people age from 35 to 55 house values increase. After age 55 average house values begin a modest decline as some people presumably move to smaller accomodation once their children leave home. We will thus expect to see around the birth cohorts first affected by educational extensions - 1910, 1933, 1957 - trends in average house values. Since the average postcode values are very asymmetric, to produce a distribution closer to normal



(a) 1922



(b) 1947



(c) 1972

Figure 4: The Impact of the Educational Extensions on Survival Rates, by Extension Year
Note: 95% Confidence interval displayed in all charts. *Source*: 100% Sample of Death Records. 1922 extension, birth years, 1902-1915. N=9,704,140. 1947 extension, birth years, 1928-1938. N=5,120,665. 1972 extension: birth years 1952-1962, N=5,741,332.

Table 5: The Impact of the 1922 Education Extension on Survival Rates

	Survival Rate, 15-65	
	Pre	Post
	(1)	(2)
Trend	0.005***	0.003***
	(0.001)	(0.001)
Constant	0.721***	0.714***
	(0.003)	(0.002)
Observations	20	20
R^2	0.834	0.602
Note:	*p<0.05; **p<0.01; ***p<0.001	
	OLS estimation	
	5 years pre-extension	
	5 years post-extension	
	N=7,355,401	

Table 6: The Impact of the 1947 Education Extension on Survival Rates

	Survival Rate, 15-65	
	Pre	Post
	(1)	(2)
Trend	0.005***	0.004***
	(0.001)	(0.001)
Constant	0.847***	0.847***
	(0.002)	(0.001)
Observations	20	20
R^2	0.754	0.731
Note:	*p<0.05; **p<0.01; ***p<0.001	
	OLS estimation	
	5 years pre-extension	
	5 years post-extension	
	N=5,133,290	

Table 7: The Impact of the 1972 Education Extension on Survival Rates

	Survival Rate, 15-45	
	Pre	Post
	(1)	(2)
Trend	0.0002*	0.0002
	(0.0001)	(0.0001)
Constant	0.976***	0.976***
	(0.0003)	(0.0002)
Observations	20	20
R^2	0.276	0.149
Note:	*p<0.05; **p<0.01; ***p<0.001	
	OLS estimation	
	5 years pre-extension	
	5 years post-extension	
	N=5,769,512	

we also take the logarithm of house values. We take house values in 1999 as being a good indicator of average lifetime incomes of individuals, and thus as a proxy for average lifetime earnings.[6] To this is a reasonable assumption the average of the natural logarithm of postcode values in 1999 was regressed against a measure of occupational rank for a sample of men where we also know their occupational status (an occupational rank that is scored 0-1). The results are reported in table 8. The elasticity of housing value to occupational rank, measured in this way, is 0.5.[7]

Figure 6 illustrates the before and after trends in house values, for men affected by the 1947 and 1972 compulsary education extensions. For the 1947 and 1972 episodes the trends on either side of the break are smooth and linear, and the data will produce a precise estimate of any effects of the educational extension on house values. For the 1922 extension there is much less data, the yearly averages are consequently noisier, and we have to project 1900.75-1905.75 birth cohort values to births 1908.75, because the reform took three years to be fully implemented. Thus the evidential value of the cohort house values is weak for this earlier extension of schooling, and is reported in the appendix.

Tables 9 and 10 report the regressions estimates for 1947 and 1972. In both cases the projected house value for the first post-extension cohort is the same or lower than that estimated for the pre-extension cohort. For the 1972 extension the estimated effect is a decline in house values of 0.6%, with a standard error of 0.7%. Thus we can say with 95% confidence that the additional 0.43 years of schooling produced less than a 0.6% increase in estimated household lifetime earnings, assuming a unit income elasticity of demand for housing. By implication the effect of an extra year of education would with 95% confidence be a gain of less than 1.4% in male lifetime earnings. The

[6]The income elasticity of housing demand is positive and typically in the range 0.7-1.0 (de Leeuw (1971)).

[7]This data is an individual level geneologcal-economic database, under construction: http://familiesofengland.com/.

Table 8: Postcode House Values and Occupational Rank

	ln(Postcode House Value)
ln(Occupational Rank)	0.509***
	(0.044)
Constant	12.548***
	(0.055)
Observations	793
R^2	0.147

Note: *p<0.05; **p<0.01; ***p<0.001
Source: Electoral Roll, Land Registry and *Families of England* database

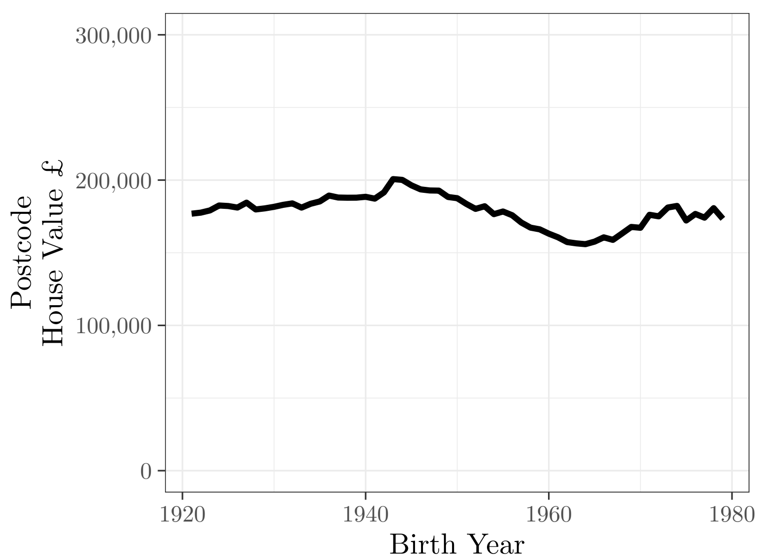
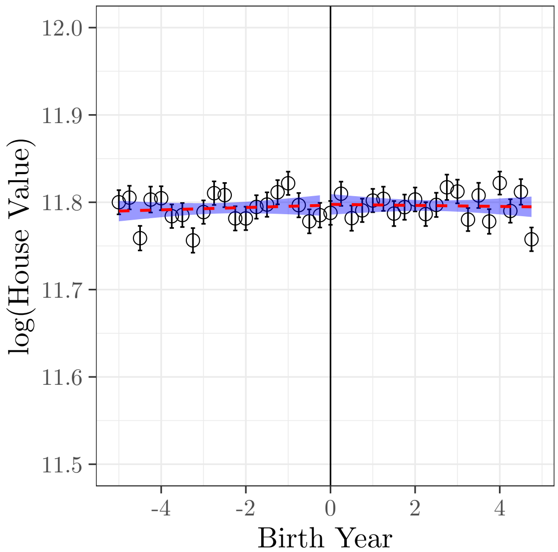
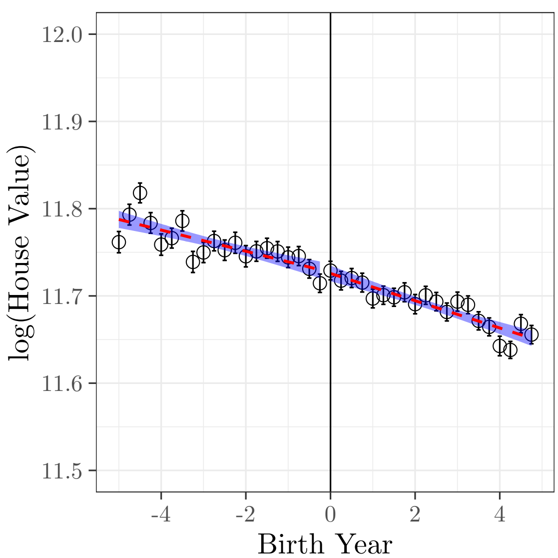


Figure 5: Postcode House Values (1999), by Birth Year 1920-1980
Source: linked electoral roll sample, births 1920-1980, $N = 592,874$.



(a) 1947



(b) 1972

Figure 6: The Impact of the 1947 and 1972 Educational Extensions on House Values
Note: 95% Confidence interval displayed in all charts. *Source*: linked electoral roll sample. 1947 extension, birth years, 1928-1938. $N = 77,642$. 1972 extension: birth years 1952-1962, $N = 121,903$.

1947 extension produces an estimated decrease of 0.1% in house values, with a standard error of 0.9%. We can thus be 95% confident that the rise in average house values for the affected cohort was less than 1.5%. By implication an additional year of schooling likely produced a decrease in permanent income of 0.2%. And we can be 95% confident that an additional year of schooling in 1947 produced less than a 2.5% increase in lifetime male earnings. But our best estimate would be that a year of additional schooling produced no gains in permament incomes for men in England.

Table 9: The Impact of the 1947 Education Extension on House Values in 1999

	ln(House Value)	
	Pre	Post
	(1)	(2)
Trend	0.001	-0.001
	(0.002)	(0.002)
Constant	11.797***	11.798***
	(0.006)	(0.006)
Observations	41,656	41,814
R^2	0.00001	0.00000
Note:	*p<0.05; **p<0.01; ***p<0.001	

Table 10: The Impact of the 1972 Education Extension on House Values in 1999

	ln(House Value)	
	Pre	Post
	(1)	(2)
Trend	-0.012***	-0.016***
	(0.002)	(0.002)
Constant	11.727***	11.726***
	(0.005)	(0.005)
Observations	54,844	61,905
R^2	0.001	0.001
Note:	*p<0.05; **p<0.01; ***p<0.001	

The precisely estimated absense of any significant earnings gains from an additional year of compulsory education in 1947 or 1972 is at odds with the literature on compulsary education and earnings in the UK cited above. However, as discussed above, for the 1972 extension while Delaney and Devereux (2019) estimate the gains for men 20-60 in weekly earnings to be 1.8%, it is with a standard error of 1.0%, and so is consistent with our estimate. For the 1947 extension if we take Devereux and Hart (2010) as the best estimate of the effects, in terms of the amount and quality of the earnings data, they give point estimates of the effects of the 1947 reform as a 0% gain for women in earnings from an additional year of compulsory schooling, and a 3-4% gain for men. However, for their preferred two-sample 2SLS estimate (with quartic age controls) the point estimate for another year of schooling is a 3.9% gain in male weekly earnings, but in this case with a 1.7% standard error (Table 4). At the lower 5% confidence interval the gain could be as little as 0.5%. If they instead use year of birth dummies, the estimate is a 3.0% gain in male weekly earnings, with a 1.9% standard error, and is not significantly different from 0 at the 5% level (table 4). Thus the results here are consistent with the Devereux and Hart estimate of the effect on earnings. The house value evidence here is simply more evidence, along with that on longevity, of the absense of significant social outcome effects from the 1922, 1947 or 1972 schooling extensions.

4.4 Social Characteristics of District of Death

Suppose, despite our failure to observe this, the 1922, 1947 and 1972 schooling extensions produced gains in earnings and improved health. Another way that this would appear empirically would be in terms of the characteristics of the districts people die in. There should be enhanced movement among those whose schooling was increased by compulsory schooling extensions to residential districts with more educated people, wealthier people, healthier people, and districts with fewer social problems such as crime. The death register in England and Wales recorded deaths in the interval 1923-2007 over as many as 1,073 registration districts.[8] We use the characteristics of the district of death to test for differential migration of the treated group to higher amenity locations.

Matching death districts to 317 census districts for 2001 we can get an estimate of the index of multiple deprivation for 2019 of the district of death of those born 1900-1912, on either side of the cohorts affected by the 1919-22 schooling extension. Figure 7 shows for the cohorts born on either side of the extension the Index of district of death by year of birth. As can be seen there is almost no year by year variation, and no sign of any break associated with more schooling for birth post 1908. Table 11 shows the detailed pre- and post-estimates of the natural logarithm of the index value for the birth cohort 1922. The estimates are essentially identical, with very low standard errors.

[8] The number of registration districts changed over time as districts were merged and split as the population grew and moved, and the structure of local administration was modernized. See https://www.freebmd.org.uk/district-list.html for an historic list of registration districts, and the period of time they covered.

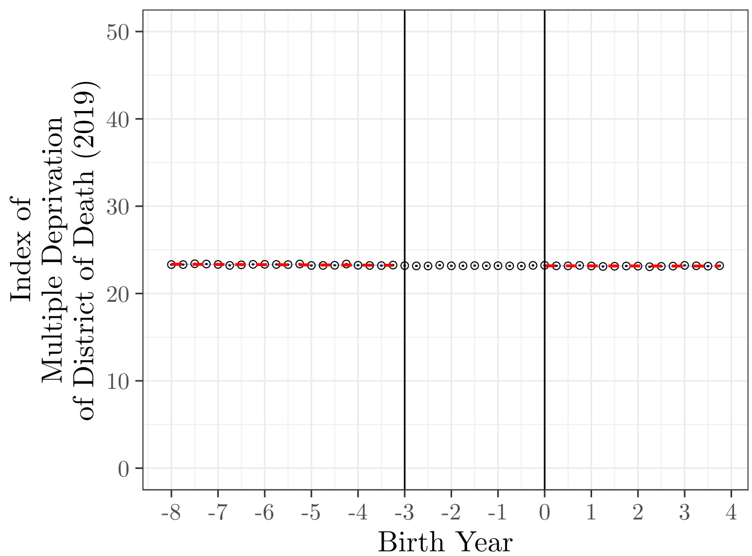


Figure 7: The Impact of the 1922 Educational Extensions on the Index of Multiple Deprivation of Place of Death

Note: Source for Index of Multiple Deprivation (2019): www.gov.uk. The period of births is 1902.75-1915.75. There are 317 Census Districts linked to the death registration districts. N=6,420,723. The sample size is bigger here than for table 11, since table 11 does not employ the three years 1919-22.

Table 11: The Impact of the 1922 Education Extension on the Index of Multiple Deprivation of District of Death

	ln(Index of Multiple Deprivation of District of Death)	
	Pre	Post
	(1)	(2)
Trend	-0.001^{***}	-0.0005^{*}
	(0.0002)	(0.0002)
Constant	3.066^{***}	3.068^{***}
	(0.001)	(0.0004)
Observations	2,415,021	2,509,356
R^2	0.00002	0.00000

Note: *p<0.05; **p<0.01; ***p<0.001
OLS Estimation
Deaths over age 15
5 years pre-reform
5 years post-reform

5 Interpretation

We see above that we can find no sign that the 1919-22, 1947 and 1972 extensions of compulsory schooling improved health, incomes, or social environment for the affected cohorts. The absense of health results is consistent with what Clark and Royer (2013) report for the 1947 and 1972 extensions. Yet, as we note in the introduction, Silles (2009) reports for the British 1947 and 1972 schooling extensions significant improvements in self reported health for individuals surveyed in the General Household Survey for England, Scotland and Wales in the years 1980-2004. The measures here are (1) Self-reported good health (2) No long-term illness (3) No activity-limiting illness (4) No work-preventing illness. Comparing the 3 years before the 1947 and 1972 extensions to the 3 years after the author reports that an additional year of schooling increases the probability of being in good health by 6.4 percentage points, lowers the probability of suffering for a long-term illness by 7.5 percentage points, reduces the probability of suffering from a long-term illness which limits activity by 5.1 percentages points, and reduces the probability of a work-preventing illness by 1.4 percentage points. However, if we look at figure 2 of the paper (figure 8 here), which summarizes the average values of these four health reports by birth year, it seems impossible to reconcile with these estimated effects. Look in particular at the top series for self-reported good health, where the schooling extensions in 1947 and 1972 should have produced an upwards jump of 0.04 and 0.03. That very smooth series shows no sign of any such discontinuity in 1947 or 1972. The value for 1947 is the same as that for 1946, and the value for 1972 is the same as for 1971. The raw data in figure 8 of the paper looks very consistent with an absense of any major effects of schooling extensions on health.

A second potential challenge to the results reported here are the results of Machin et al. (2011) reporting substantial effects of the 1972 England and Wales schooling extension in reducing criminal conviction rates for the affected cohorts. The authors found a near 5% reduction in criminal convictions for men aged 18-40 for the cohorts which experienced the increase in school years. However, as figure 9 shows, estimated criminal conviction rates, even detrended by year and age dummies, were varying substantially for the birth cohorts around those affected by the reform. The reform is estimated to have reduced conviction rates by 0.1 per 1,000. Yet in the 8 years before the reform conviction cohort rates varied by year by more than 0.2 convictions per 1,000 across the 8 cohort years, and in the 8 subsequent cohort years they also varied by 0.2 This variation is completely unexplained residual variation. In our estimates above we see instead either stability of the outcome variable on either side of the break, or a smooth near linear trend. Thus for the English extensions of compulsary education 1922, 1947, 1972 the evidence is that there were minimal consequences in social outcomes as a result of extensions of compulsary education. This result for England, where we can estimate the effects of compulsory education at a relatively precise 0, is hard to reconcile with the international literature on the effects of compulsory schooling, which is summarized in table 12.

That literature is a mix of papers which find significant positive effects of compulsory education on social outcomes, and papers which find little or no effect. For example, Acemoglu and Angrist (2001) find significant effects on earnings in the USA, and Oreopoulos (2007) significant effects on earings in Canada. Yet Pischke and von Wachter (2008) find no effects of compulsory schooling on wages in Germany, Meghir and Palme (2005) find only a 1.4% estimated effect of a year of additional schooling in Sweden, Oosterbeek and Webbink (2007) found no effects in the Netherlands, and Grenet (2013) found no effect in France. If the true effect was everywhere zero, and the range of results found here just the product of sampling error and confounding factors, then we would

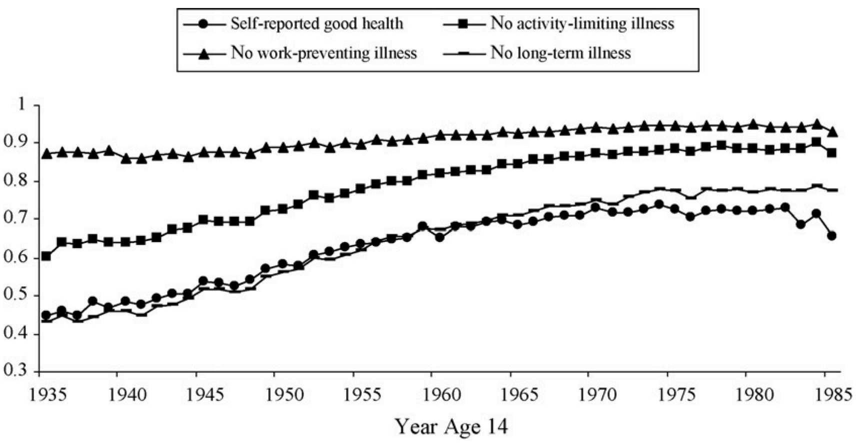


Fig. 2. Fraction reported various measures of health status.

Figure 8: Silles, 2009, figure 2

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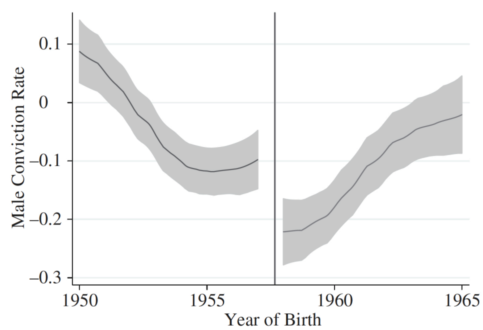

Fig. 2. *Crime Discontinuities Around the Compulsory School Leaving Age Increase*

Notes. Based on Offenders Index Data from 1972 to 1996, men aged 18–40. Graph shows residuals from regression of offence rate per 1,000 male population de-trended from a model containing GHS controls (proportion British born, proportion employed, proportion non-white and proportion living in London), year and age dummies. Lines denote kernel weighted smooth polynomial fit to data points before and after the discontinuity denoted by the vertical line. Grey shaded area is 95% confidence interval.

Figure 9: Machin, Marie and Vujić, 2011, figure 2

Table 12: The Effects of Compulsory Education Extensions

Authors	Year	Journal	Outcome	Effects	Citations
Angrist and Krueger	1991	QJE	Earnings, USA	**+**	2,859
Lleras-Muney	2005	REStud	Mortality, USA	**+**^{A}	1,499
Harmon and Walker	1995	AER	Earnings, UK	**+**	759
Oreopoulos	2006	AER	Wages, UK	**+**	736
Oreopoulos	2007	JPubE	Earnings, Health	**+**	562
Arendt	2005	EER	Health, Denmark	?	407
Machin et al.	2011	EJ	Crime	**+**	427
Clark and Royer	2013	AER	Mortality, UK	**0**	332
Silles	2009	EcEdRev	Health	**+**	278
Pischke and von Wachter	2008	REStat	Earnings, Germany	**0**	272
Albouy and Lequien	2009	JHE	Mortality, France	**0**	249
Kemptner et al.	2011	JHE	Health, Germany	**+**	196
Lindeboom et al.	2009	JHE	Child Health, UK	**0**	196
Devereux and Hart	2010	EJ	Earnings, UK	**+**/0	183
Stephens and Yang	2014	AER	Wages, employment, divorce, USA	0/**–**	167
Mazumder	2008	EP	Mortality, USA	**0**	150
Lager and Torssander	2012	PNAS	Mortality, Sweden	**0**	104
Grenet	2013	ScanJEcon	Earnings	**+**/0	101
Dickson et al.	2016	EJ	Children's Education	**+**	60
Black et al.	2015	JHE	Mortality, USA	0	13

Notes: QJE = Quarterly Journal of Economics, REStud =Review of Economic Studies, AER = American Economic Review, JPubE = Journal of Public Economics, EER = Economics of Education Review, EcEdRev = Economics of Education Review, REStat = Review of Economics and Statistics, JHE = Journal of Health Economics, EJ = Economic Journal, EP = Economic Perspectives, PNAS = Proceedings of the National Academy of Sciences, ScanJEcon = Scandinavian Journal of Economics. *A* Positive effect is beneficial. Citations from Google Scholar as of 11 Feb 2020.

expect to see equal numbers of studies reporting negative effects as were reporting positive ones. However, the absense of any published results showing a negative effect of compulsory education on earnings does not imply an absense of studies that found negative effects. For we fully expect that authors finding negative effects would decide that these results were clearly implausible, since no-one expects that education had a negative effect, and thus not suitable for publication. Thus we expect that this publication pattern of a mixture of positive and zero estimated effects reflects just a problem of publication biases in an area where there is a strong a priori assumption that if education has any effect, it is towards positive social outcomes. There is also some chance that papers which find a positive effect of education on social outcomes get less scrutiny and critical reading than those which find zero effect. We have arranged the papers in the survey table 12 in order of their citations in google. There seems to also be some tendency for papers which report positive effects of education to be more frequently cited. For example, taking just citations over the period 2016-20, the Lleras-Muney paper demonstrating strong positive effects of compulsory education on adult mortality rates in the USA got 539 citations. In contrast, Stephens and Yang (2014), which showed that allowing year of birth effects to vary across regions in the USA eliminates any positive association between education and a variety of social outcomes, got only 138 citations 2016-20. Similarly the more tightly observed and convincing Clark and Royer paper, demonstrating the absense of any effect of education on longevity in England, got only 300 citations 2016-20.

Thus we take the precisely estimated absense of effects here for England on health, house values, or district of death quality as good evidence for the likelihood that compulsory schooling nowhere has significant positive effects on social outcomes.

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A Details of Data Construction

The precise dates of full implementation of the extensions in compulsory education years are shown in table . The 1918 Act extensions of compulsory education were not fully implemented until the Autumn of 1922. That means at least some of those born before 1908 4th Quarter were able to leave school at ages 12 or 13. Since there were significant increases in school attendance as early as the 1919-20 school year, we measure the pre-reform cohorts as those born 1900 (4th quarter)-1905 (3rd quarter). None of the pre-reform cohort would be affected at all by the school year extension. For some of the outcome variables - house values 1999 for example - we can precisely match people to the pre and post treatment groups by quarter of the year. For others - such as survival rates 15-65 - we allocated people just to calendar years of birth. Thus for the 1922 reform the pre- calendar years would be 1903-1907, and the post- 1911-1915.

As noted in the text for 1866-1970 the death index records the date of death registration by quarter, and the integer age at death. For 1970-2006 the index reports the exact date of birth. 1970-1984 the data we have records just the year of death. 1984 and later the date of death is recorded by month instead of by quarter. Table 14 reports details of the death records data and the calculations required to generate ages at death and dates of birth.

Parliamentary Act	Full Implementation	Leaving Age	Treatment Birth Cohort
1870	1870, December	10	
1880	1880, December	10	
1893	1893	11	
1899	1899	12	
1918	1922, Sept	14	1910-4th quarter*
1944	1947, 1st April	15	1933-2nd quarter
1944	1972, September	16	1957-4th quarter

Table 13: Education Extensions in the UK, 1870-1944

Note: The treatment cohort represents the first cohort fully affected by the reform. * For the 1922 reform (1918 act), the pre treatment cohorts are taken as those born 1902.75-1907.75.

Table 14: Characteristics of the Death Register Data

Period	Recorded in Death Index	Age at Death	Date of Birth
1838-1866	Year and Quarter of Death	-	-
1866-1970	Year, Quarter of Death, and Integer Age at Death	Integer Age at Death +0.5	Year of Death+(Quarter of Death×3)-1.5-Age at Death
1970-1984	Year of Death and Exact Date of Birth	Year of Death+0.5-Date of Birth	Reported
1984-2006	Year and Month of Death and Exact Date of Birth	Year of Death+(Month-1)/12+(15/365)-Date of Birth	Reported

B The Linkage Process

Starting with the universe of births and deaths recorded in England and Wales, 1838-2007 (steps 1 and 2 in table 15), we extract a subset of unique births and deaths, based upon year, surname, first forename and the first letter of the second forename (steps 3 and 4).

We link these records to each other, births to deaths, by exploiting a useful feature of the death records after 1866; every death has either an age at death or a birth date recorded. This allows us to attribute births to deaths based on the concordance between the records of surname, first name, first letter of second forename and the year of birth. (We also allow the year of birth to be the preceding year in the case where the birth is recorded in the first quarter of the year.) The links are then examined to ensure that there is only one unique link, all duplicate links are dropped. The resulting data is thus reflects unique birth-death links (step 5).

Figure 10 reports the linkage rate for rare births to deaths, by year and quarter of birth, from 1838 to 1970. (Post 1970 we do not have the quarter of birth recorded, only the year of birth.) We are much more successful in linking births that are later on in the year. This is because births earlier in the year, and especially in quarter 1, are more likely to have truly been born in the preceding year. Therefore the listed birth year on the death record is far morel likely to be correct for those whose birth is recorded later in the year. From the unique birth file, we extract all male births who have not died (not linked to a birth) and are aged 20 and over by 1999 (step 6). We then only take the unique births from step 6 by surname, and forename, only (step 7). This is because the electoral roll of 1999 (step 8) does not contain age information and only a limited proportion (<10%) contain the first letter of the second forename. We therefore only link based on exact surname and forename. Only unique matches are kept, all duplicate matches are dropped. Where we have evidence from both sources on middle initials we only keep those that agree. Figure 11 reports the linkage rate from unique births to the electoral roll of 1999. Here the boundaries for the period windows analyzed across the education extension are marked. There are no discontinuities across these windows.[9] This electoral roll contains the post-code of the registered voter. By linking this to the average post-code value from the land registry, we have our final sample for analysis (step 9). Table 15 details the sample sizes for each of these steps. How representative is this final sample? Figure 12 reports the distribution of post-code house values for the entire electoral roll of 1999, compared with that for the linked sample, described in section 2.2. Table 16 reports the average and standard deviations for post-code house value. The linked sample is systematically a richer group than the English average. This could potentially be a result of a number of factors. Only registered voters are recorded on the electoral roll. It also is used by credit agencies for verification so those looking to get a mortgage are possibly more likely to be registered. Further, if the consistency of spelling between records is related to education and human capital we would be more likely to link the more educated. All of these biases suggest that we are likely to link a more elite sample than the population average. However, the key point is that this probability does not vary in a discontinuous way across are sample windows. In fact, being linked to the electoral roll could be interpreted as simple test of being more educated than those not linked. We do not see evidence for any structural breaks across the windows associated with schooling extensions.

We inspected a random sample of 200 linked records indviudally by looking them up on www.192.com where the elctroal rolls after 2000 are avaialble (these records contain age estimates, unlike the 1999 electroal roll). Table 17 reports the details of this process. The percentage of matches implied to be to wrong person is 4.5% if we assume all close matches correct. The percentage to wrong person

[9] The period windows marked in figure 11 are 1903.75 to 1913.75, 1927.25 to 1937.25 and 1951.75 to 1961.75.

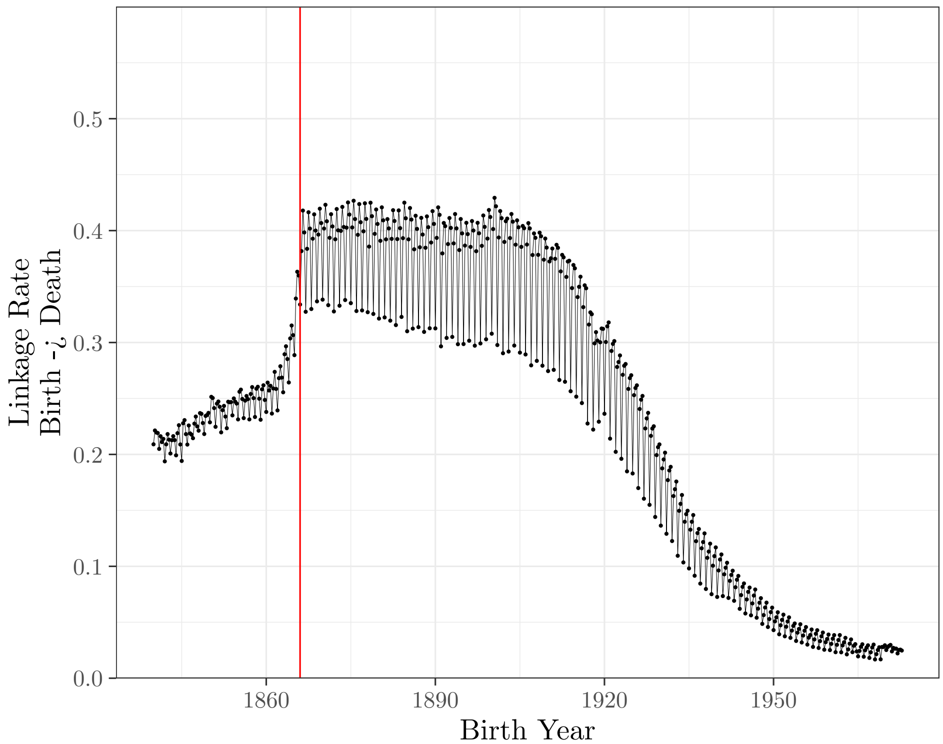

Figure 10: Linkage Rate, by year of birth and quarter, 1838-1970

Table 15: The Linkage Process

Step	Data	N	Years
1	All Births	127,843,971	1838-2007
2	All Deaths	85,932,666	1838-2007
3	Unique Births[1]	94,408,159	1838-2007
4	Unique Deaths[1]	60,449,934	1838-2007
5	Unique Births-Unique Deaths[1]	20,106,890	1838-2007
6	Births that are Male, Alive and Age >= 18 in 1999	19,843,495	1890-1979
7	Unique Births from step 6[2]	3,265,103	1890-1979
8	Electoral Roll, 1999	31,551,398	1999
9	Male Births linked to Electoral Roll[2]	730,353	1999

Notes: [1] Based upon exact surname, first name and first letter of second forename and the year of birth. [2] Based upon exact forename and surname only.

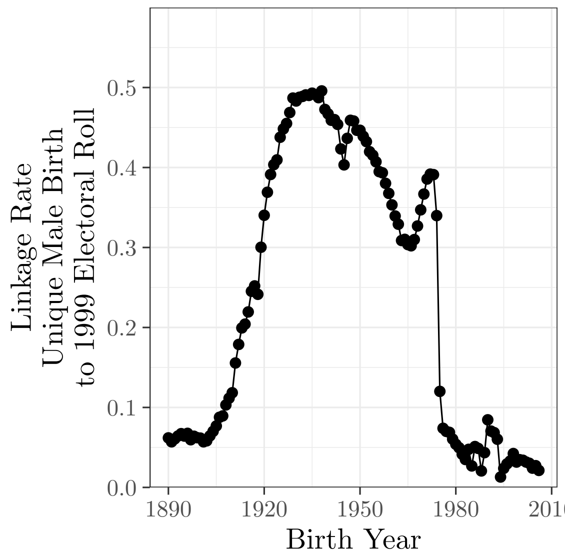

Figure 11: Linkage Rate from Unique Births, alive in 1999, linked to the Electoral Roll of 1999

Table 16: Comparison of Linked Sample and All, Electroal Roll of 1999

Measure	Linked	All
N	730,353.00	31,551,398.00
House Value, Mean	177,591.49	159,780.61
House Value, SD	158,157.87	132,792.50
Age, Mean	49.46	
Age, SD	17.98	

Source: Electoral Roll fo 1999 linked to Rare Births, still alive in 1999

if we assume the close matches might also be to wrong person in same ratio as for whole sample, 5.4%.

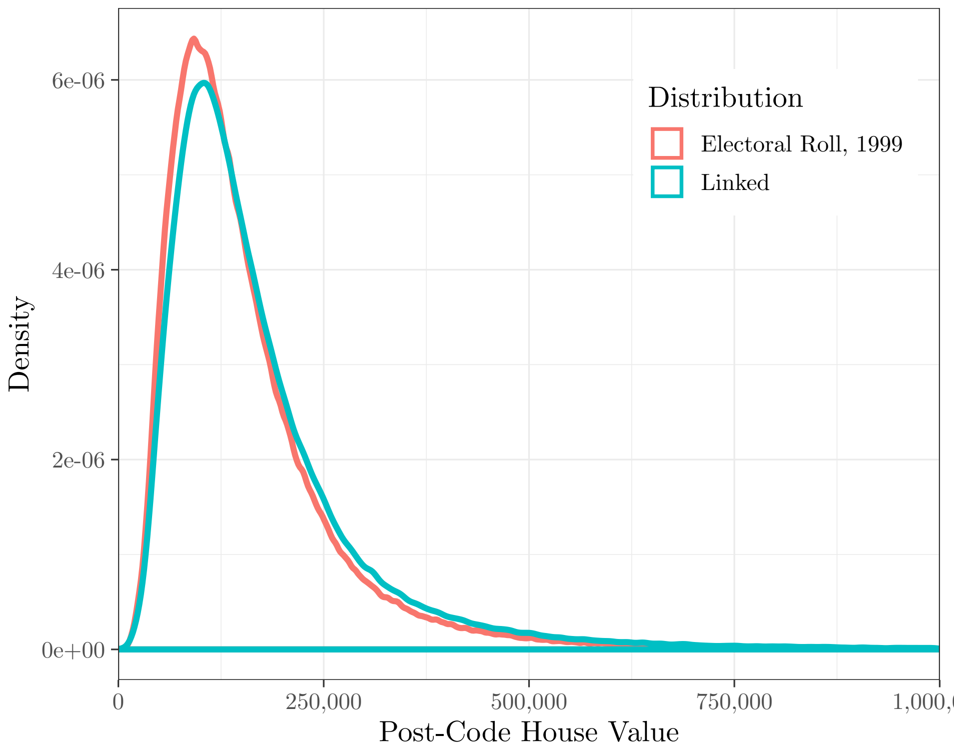

Figure 12: The Distribution of Post-Code House Values, the Electoral Roll of 1999 and the Linked Sample Compared

Table 17: Individual Verification of Links, random sample of 200

Type	Definition	Number
Complete Match	Same unique surname, first name, second initial, year	110
Close Match	Same unique surname, first name. Year within 3 years or additional initial in electoral roll	26
Wrong match	Unique match to person with wrong birthdate	2
Multiple match	Match also to person with same first name but different birth date	8
No match	No-one with same surname, firstname in published registers 2003-10	54
All		200

Note: The electoral register 1999 is complete. Published registers 2003-10 contain only voters who elected to make their information public.

C Additional Results

As noted in the text we know the registration district in which individuals died. Individuals with higher earnings, higher status occupations, or more education will be more likely to be found at death in registration districts with better social outcomes. We have two other measures of registration district characteristics. First the infant mortality rate 1980-9. Second the average adult age of death in 1980-9. Both of these measures are calculated from 100% birth and death records across 357 registration districts.

As can been seen in table 19 and figure 13, the 1922 extension of compulsory education does not lead to any substantial improvement in the infant mortality rates 1980-9 of the districts people are dying in. Table 19 shows an estimated decline of 0.008 infant deaths per 1000 births, which is statistically insignificant, and quantitatively tiny relative to the average child mortality rate of around 11.2. The trend in infant mortality rates of district of death is, however, lower for those born in the treated cohorts. If we project across the 3 years of the introduction of the higher school age using the post period trend then we see a decline of 0.035 in infant deaths per 1,000 in places of death, and this difference is statistically significant at the 5% level. Also even a decline of 0.035 is quantitatively very small. The general negative trend in the infant mortality rates of place of death for those born 1902-15 reflects just the movement in population in the twentieth century away from high infant mortality districts in the North of England.

If we measure the social quality of place of death by average adult longevity for those dying 1980-9 we similarly see no gains from additional schooling. As table 19 and figure 14 show there is a statistically significant, but quantitatively unimportant, decline in the life expectancy of districts of death for the post cohort of 0.023. Since again the pre and post trends differ, we can also estimate this using the post trend, which produces a statistically significant but quantitatively unimportant gain of 0.016 years of life expectancy in districts of death.

Table 18: The Impact of the Education Extension, 1922, Infant Mortality rate of District of Death

	Infant Mortality Rate of District of Death	
	Pre	Post
	(1)	(2)
Trend	-0.014^{***}	0.003
	(0.003)	(0.002)
Constant	10.970^{***}	10.994^{***}
	(0.015)	(0.006)
Observations	2,087,716	2,447,025
R^2	0.00001	0.00000

Note: *p<0.05; **p<0.01; ***p<0.001
OLS Estimation
Infant Mortality is N deaths per 1000
5 years pre-extension
5 years post-extension

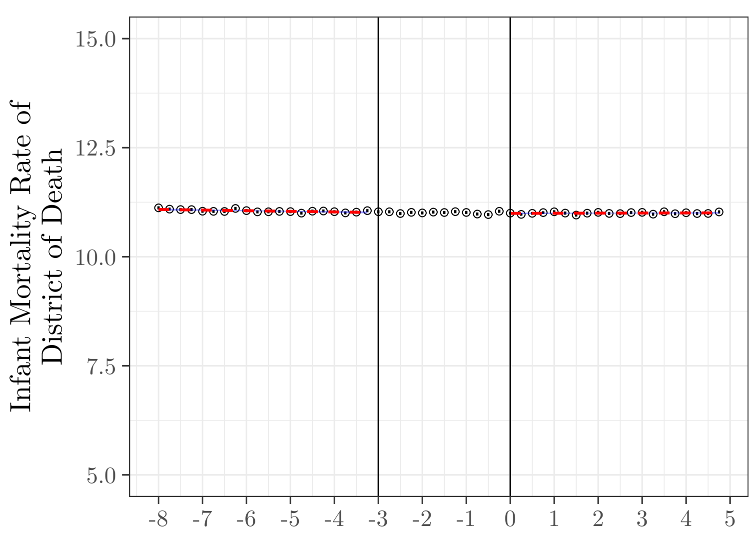

Figure 13: Infant Mortality Rate of District of Death, by Year of Birth, 1903-1913
Note: Infant Mortality is Number of Infant Deaths (ages 1 and under) per 1,000 Births. Year 0 = 1910.75.

Table 19: The Impact of the Education Extension, 1922, Adult Age at Death of District of Death

	Average Adult Age at Death of District of Death	
	Pre	Post
	(1)	(2)
Trend	−0.002	−0.011***
	(0.001)	(0.001)
Constant	74.649***	74.634***
	(0.007)	(0.003)
Observations	2,357,679	2,726,228
R^2	0.00000	0.00003

Note: *p<0.05; **p<0.01; ***p<0.001
OLS Estimation
Deaths over age 15
5 years pre-extension
5 years post-extension

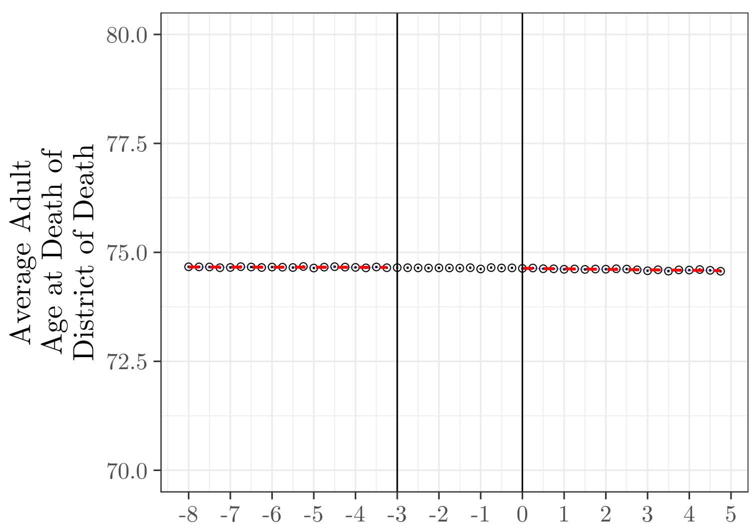

Figure 14: Adult Average Age at Death of District of Death, by Year of Birth, 1903-1913
Notes: Year 0 = 1910.75.

Table 20 reports the regression estimates of the effects of the 1922 schooling extension on house values in 1999. For the 1922 extension the point estimate is of a 4.9% gain in house values for the affected cohort, but here with a standard error of 3.9%.

Table 20: The Impact of the Education Extension, 1922, House Values in 1999

	log(1999 House Value)	
	Pre	Post
	(1)	(2)
Trend	0.010	−0.002
	(0.006)	(0.004)
Constant	11.799***	11.822***
	(0.035)	(0.011)
Observations	5,068	12,779
R^2	0.001	0.00001
Note:	*p<0.05; **p<0.01; ***p<0.001	

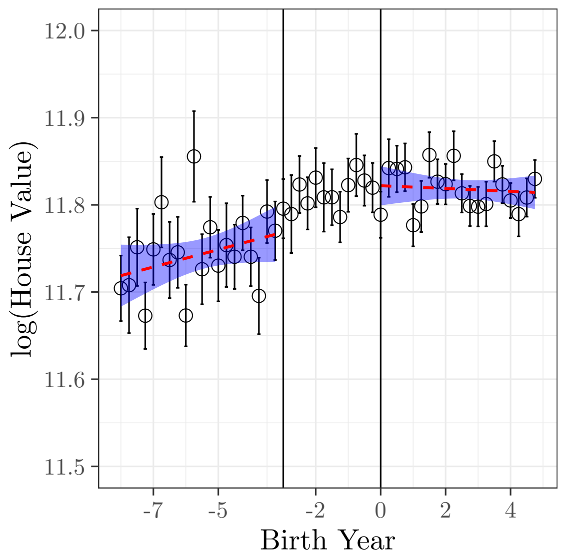

Figure 15: 1922

Note: 95% Confidence interval displayed. *Source*: linked electoral roll sample 1922 reform, birth years, 1903-1915. $N = 16,287$.