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Abstract

While there is no doubt that health is strongly correlated with education, whether schooling exerts a causal impact on health is not yet firmly established. We exploit Dutch compulsory schooling laws in a Regression Discontinuity Design applied to linked data from health surveys, tax files and the mortality register to estimate the causal effect of education on mortality. The reform provides a powerful instrument, significantly raising years of schooling, which, in turn, has a large and significant effect on mortality even in old age. An extra year of schooling is estimated to reduce the probability of dying between ages of 81 and 88 by 2-3 percentage points relative to a baseline of 50 percent. High school graduation is estimated to reduce the probability of dying between the ages of 81 and 88 by a remarkable 17-26 percentage points but this does not appear to be due to any sheepskin effects of finishing high school on mortality beyond that predicted linearly by additional years of schooling.

JEL Classification: *D30, D31, I10, I12*

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1. Introduction

Inequalities in health and life expectancy by education are striking. For example, in the Netherlands individuals with a university or college degree live, on average, 6 to 7 years longer than those that finished only primary school. The difference in life expectancy in good health is as much as 16 to 19 years (CBS, 2008). Such disparities in health and mortality by education have been documented for many countries (Grossman and Kaestner, 1997; Smith and Kington, 1997; Mackenbach *et al.*, 1997; Cutler and Lleras-Muney, 2008). Indeed, Michael Grossman has claimed that "*years of formal schooling completed is the most important correlate of good health*" (Grossman, 2003, p.32). Yet, very few studies have found robust evidence of a causal impact of education on health. The correlation could also stem from childhood ill-health constraining educational attainment (Perri, 1984; Behrman and Rosenzweig, 2004; Case *et al.* 2005) and confounding factors such as ability and time preference (Fuchs, 1982; Auld and Sidhu, 2005; Deary, 2008). Establishing whether education causally impacts on health is essential to the formation and evaluation of education and health policies.

The purpose of this paper is to establish whether education has a causal impact on mortality using exogenous variation in education that comes from a compulsory schooling law in the Netherlands. This reform, which increased the educational attainment of the population suddenly and strongly, provides a valuable instrument within a regression discontinuity design. We observe a very large sample of individuals between the ages of 80 and 88. This makes it possible to investigate whether education has long run effects on health that result in an extended life. If education effects are present at this age, then it is highly likely that they exist at younger ages, such that our estimate can be interpreted as a lower bound on the total impact of education on mortality.

Lleras-Muney (2005), exploiting changes in compulsory schooling laws in more than 30 US states, estimates that an additional year of schooling reduces the 10 year probability of dying by at least 3.6 percentage points. However, the analysis is based on only an approximation to mortality derived from the change in cohort size between subsequent censuses. Further, Mazumder (2008) demonstrates that the magnitude and the significance of the effect are not robust to the inclusion of state-specific cohort trends. Clark and Royer (2007) measure mortality somewhat more directly than Lleras-Muney (2005) and Mazumder (2008) from the number of deaths divided by the number of births of a cohort. They find that the 1947 extension to the school leaving age in the UK had a strong impact on educational attainment, but no significant effect on mortality. Albouy and Lequien (2008) exploit two compulsory schooling reforms in France in 1923 and 1953. They do not find a significant effect of years of schooling on mortality, which is observed directly from micro data, but neither did the reform have a very strong impact on years of schooling.

While the weight of existing evidence does not support an effect of education on mortality (Clark and Royer, 2007; Mazumder, 2008; Albouy and Lequien, 2008), some studies (but not all) using compulsory schooling laws as a source of exogenous variation in education find an effect on self reported health outcomes (Spasojevic, 2003; Oreopoulos, 2006; Mazumder, 2008; Silles, 2009) and hospitalizations (Arendt, 2008). An obvious weakness of evidence based on self-reported health is that reporting thresholds may vary with education (e.g. Bago d'Uva *et al*. 2008) and, if this is the case, the resulting bias would not be corrected by instrumenting education.[1]

The main weakness of most of the analyses of mortality, with the notable exception of Albouy and Lequien (2008), is that they rely on approximate measures of mortality at the cohort level rather than directly observed survival at the individual level. We observe a very large number of individuals from linked Dutch administrative and survey data for which a mortality follow-up is available. Given the strong instrument, provided by the compulsory schooling reform, and tremendous power, generated by the sample size and micro data, it is quite unlikely that we will fail to detect an effect of education on mortality should one exist in the population. A potential limitation of the previous literature is that it is concerned with the health returns to additional years of schooling, ignoring the nature and quality of education (Feinstein *et al.* 2006; Cutler *et al.* 2008). In addition to estimating the impact of an extra year of schooling on mortality, we estimate the health returns to completion of (the Dutch equivalent to) high school and investigate whether this has a discrete beneficial effect beyond that predicted linearly from the equivalent additional years of schooling.

We find that education significantly lowers mortality. An additional year of schooling reduces the probability of dying between the ages of 81 and 88 (inclusive) by more than 2 percentage points, relative to a baseline probability of 50 percent The

[1] If individuals with more schooling tend to understate their health, as some of the evidence suggests (Bago d'Uva *et al.* 2008), then impact of education on health will be underestimated.

instrumental variable (IV) estimate is larger than the ordinary least squares (OLS) estimate, indicating downward bias in estimates that fail to take account of endogeneity. A significant impact of education on mortality is found only with the larger one of our samples, suggesting that lack of statistical power is a potential explanation for the failure of most previous studies to find an effect. Finally, high school graduation reduces the probability of dying between 81 and 88 by as much as 17 to 26 percentage points but this does not appear to due to any sheepskin effects of finishing high school on mortality beyond that predicted linearly by additional years of schooling.

The paper is organized as follows. Section 2 provides background information on the Dutch reforms that enable us to estimate the causal impact of education on mortality. The data and methods are discussed in section 3. The main results are presented in section 4. Section 5 investigates whether there are additional gains in survival from high school graduation over and above those implied by the years of schooling this entails. Section 6 considers the lessons learnt and the limitations of our study.

2. Compulsory Schooling in the Netherlands

The first compulsory schooling law, mandating 6 years of education, was introduced in the Netherlands in 1900 (Dodde, 2000). In the years before World War I several attempts were made to increase the compulsory years of schooling to 7, and even 8. Due to the war, a law raising the minimum school leaving age came into force only from 1st of January 1922. The law required children to follow primary school at least from their 7th birthday until they had completed at least 7 years of schooling, or had reached the age of 14 (Hentzen, 1928, p. 4). However, since most schools did not have the resources to offer the 7th year of schooling, the law was never enforced (Hentzen, 1932). In 1924, the number of years of compulsory schooling was officially reversed to 6 and the increase postponed until 1930. But an improvement in the economy prompted parliament to ask for the rise of the minimum school leaving age to be brought forward. The government agreed in 1927 and enforced the increase from 1st of July 1928 (Hentzen, 1932; De Graaf, 2000).

Before that date pupils were allowed to leave school as soon as they had completed all 6 grades of primary school. Children born in 1916 who started school at age 6 - about half of this cohort did (CBS, 1951) - could be 12 and have finished all 6 classes of primary school by the 1st of July 1928. This cohort was not fully affected by the reform. In contrast, children born in 1917 were at most 11 on the 1st of July 1928, and could not have met the conditions to leave school under the pre-1928 legal regime. Therefore, the cohort born in 1917 was the first to be (fully) affected by the 1928 reform and to be forced to complete 7, rather than 6, years of schooling. Importantly, since most primary schools had only 6 grades, the 1928 reform induced – as we will show later – a large proportion of pupils to enter secondary school.

Compulsory schooling was raised further to eight years in 1942 under the German occupation with the aim of promoting the German language. A lack of competent teachers and materials, and resistence from the population, meant that both the quality of the extra education and compliance with the law were very questionable (Meijsen, 1976; HSG[2], 1946/1947). After the war, a new law confirmed the increase in the minimum years of schooling to eight, but deferred its enforcement to January 1st 1950 given high rates of school absence and the unpopularity of the war-time increase, circumvention of which had become almost a heroic deed (HTK[3], 1946/1947). Later increases in the minimum school leaving age in 1969, 1975 and 1985 are too recent to investigate their potential mortality effects.

The introduction of the seventh and the eighth year of compulsory schooling was a major breakthrough in the Dutch educational system, since it forced pupils to go beyond primary school and start secondary schooling. However, since between 1938 and 1949 the percentage that attended an additional level of education after primary school increased from 50 to 73% (CBS, 1951), the target population for the 1950 reform was much smaller than for the initial reform in 1928. We have confirmed that the 1950 reform did induce some individuals to obtain more schooling, but the effect is not large and so does not provide a strong instrument for education. In the analysis we restrict attention to the 1928 reform, which, as will become apparent, had a strong impact on educational attainment.

[2] HSG refers to notes of both the Lower House (House of Representatives) and the Upper House (Senate)
[3] HTK refers to notes of the Lower House (House of Representatives)

3. Data and Methods

3.1 Data

Our data are linked survey and administrative records from Statistics Netherlands. We use the annual cross-sectional general household survey (POLS) 1997-2005, the tax records (RIO) for 1998, and the Cause-of-Death register for 1998 until 2005 inclusive. All these files are linked to the Dutch Municipality Register (GBA), which covers, *inter alia*, year of birth, sex, province, and ethnicity.

The POLS samples a representative cross-section of the non-institutionalised Dutch population ranging from around 10000 to 80000 respondents per year[4]. It collects extensive information on demographic and socioeconomic characteristics. The respondent's education is recorded by two variables: the highest level followed and that finished on the standard Dutch categorization (*Standaard Onderwijs Indeling* (SOI) *1998*)[5]. The SOI is very close to the International Classification of Education (ISCED) and is easily converted into years of schooling following standard guidelines (SHARE, 2007).[6] In the relatively few cases that individuals reported to have followed a higher level than they finished, we take the average of the corresponding years.[7] Through linkage with the Cause-of-Death register we are able to observe death and its cause for all POLS respondents who died between 1998 and 2005.

The RIO is a huge administrative tax-register covering one third of the Dutch population, i.e. around five million observations per year. Apart from detailed income information, it also contains demographics. By linking the RIO to the Cause-of-Death register we observe, again, mortality for all individuals. Unfortunately, education information is not available in the RIO, but, as will be explained below, it is still possible to combine estimates from the linked RIO-death register with those from the POLS to obtain IV estimates of the impact of education on mortality.

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[4] Specifically 34.439 in 1997, 80.789 in 1998, 42.605 in 1999, 37.482 in 2000, 24.231 in 2001, 22.259 in 2002, 25.163 in 2003, 21.706 in 2004, and 10.378 in 2005.

[5] SOI is missing in the 2003 wave of the POLS. However, there is a highly similar educational variable available in that year, which is redefined into SOI and ISCED.

[6] The Dutch SOI consists of 7 levels of education: toddler school, primary education (6 years), lower vocational secondary education (10 years), higher general secondary education (13 years, similar to high school), first phase higher education (15 years, intermediary vocational education), second phase higher education (16 years, higher vocational education) and third phase higher education (17 years, university education).

[7] We drop a very small number of individuals who report to have followed a lower level than the one they report finishing. Also, a few individuals who claimed to have followed a level to which they do not have access given their highest level completed are disregarded.

The CBS Linked Data are unique in the context of data that have been used previously to estimate the impact of education on health in the sense that they provide a mortality follow-up of both a sample survey (POLS, 1997-2005) and a very large administrative database (RIO, 1998). So, individual mortality is observed, and with the POLS we observe both education and mortality for the same sample observations. The mortality record provides 8 years of follow-up by cause of death. Given that the cohorts affected by the reform we study were born around 1917, we observe these individuals in their 80s. Pre-sample selective mortality might be important, especially if education does reduce the risk of an early death. But in this case, our estimates will provide a lower bound on the impact of education on life expectancy. We estimate the effect of years of schooling on the probability of dying between 80 and 88 (inclusive), conditional on being alive at age 80[8]. This allows us to examine whether there are long-term returns of education on life expectancy.

3.2 Identification strategy and estimation

We exploit the 1928 compulsory schooling law as an instrument for education within the framework of a Regression Discontinuity Design (RDD) (Thistlethwaite and Campbell, 1960; Trochim, 1984; Hahn *et al.*, 2001; Lee and Card, 2008; Imbens and Lemieux, 2008; Van der Klaauw, 2008; Lee and Lemieux, 2009). All analyses are done separately for males and females. Year of birth (cohort) determines whether the individual is exposed to the reform, so sorting around the threshold is absent. Furthermore, not all individuals exposed to the reform are induced by it to change their education. We therefore have a discrete running-variable Fuzzy Regression Discontinuity set-up (Lee and Card, 2008) and the appropriate estimator is parametric Two Stage Least Squares (2SLS), in which years of education is instrumented by the reform. Under the standard assumptions of RDD (Hahn *et al*. 2001; Van der Klaauw, 2002), this provides an estimate of the Local Average Treatment Effect (LATE) of an additional year of education on mortality for individuals in the cohort exposed to the reform that were induced to stay at school.

[8] An estimated 27 percent of Dutch males born in 1917 and 51 percent of females is still alive at age 80 and, for this cohort, life expectancy at birth was around 60 for males and 68 for females (*Human Mortality Database,* University of California, Berkeley (USA), and Max Planck Institute for Demographic Research (Germany) www.mortality.org)

In the first stage, we regress years of education on a flexible polynomial in cohort[9], the indicator of whether the individual is exposed to the reform, and wave dummies[10]. The usual concern about the strength of the instrument applies here. If the instrument is weakly correlated with the possibly endogenous variable, then the IV estimator will be biased in the same direction as OLS (Bound *et al*. 1995; Staiger and Stock 1997). F-tests on the strength of the instrument are reported. Moreover, the flexibility and goodness-of-fit of the polynomials are tested using the G-test suggested by Lee and Card (2008). We allow for non-random specification error in the polynomial by computing robust standard errors clustered at the cohort level (Lee and Card, 2008).

A reduced form linear probability model of the binary mortality variable on the same variables as in the first stage is also estimated. Again a G-test on the flexibility and goodness-of-fit of the polynomial is performed (Lee and Card, 2008). For the models that pass the relevant tests, the full 2SLS is estimated.

A drawback of using the POLS in this analysis is the potential lack of power for an outcome such as mortality. With around five million observations, this is not a problem with the RIO data but, unfortunately, this dataset does not include education. However, since in the exactly identified case the coefficient of interest in 2SLS estimation can be written as the ratio of two reduced form coefficients, it is possible to perform these two reduced form regressions using separate samples from the same population. This technique has been labeled Two Sample Two Stage Least Squares (TS2SLS) (Angrist and Krueger, 1992; Arellano and Meghir, 1992; Inoue and Solon, 2009). The standard error of the TS2SLS coefficient is obtained by the delta method (Devereux and Hart, 2008).

[9] Following Lee and Lemieux (2009), we allow the polynomials to differ on either side of the threshold since otherwise the information on one side of the threshold is used in the estimation of the trend on the other side, which is against the spirit of a RDD. Eight models with different degrees of flexibility of the polynomial are estimated - linear, quadratic, cubic and quartic in cohort with and without interactions with the reform dummy. We present results only for the linear and quadratic models (with and without interactions) as these prove to be sufficiently flexible.

[10] Wave dummies are entered in the analysis of the POLS data only to correct for potential bias due to the differential mortality probabilities within the observation period across the cross-sections. That is, someone observed in the 1997 POLS is more likely to have died by 2005 than someone in the 2004 POLS. Since the wave dummies are included in the mortality regression, they are also included in the years of schooling regression.

4. Main Results

4.1 OLS estimates

Table 1 gives OLS estimates of the impact of an additional year of schooling on the probability of dying in the 1998-2005 period. For males, the reduction in the probability of dying is 1.4 percentage points per additional year of schooling, and this falls to 1.1 points, but still remains strongly significant, when both a cohort trend (quartic polynomial) and covariates (marital status, ethnicity, province, and city size) are controlled for. For females, there is also a significant but slightly smaller effect of 0.8-0.9 percentage points.

There is clearly a negative association between education and mortality in this population. Our aim in the remainder of the analysis is to establish whether there is a causal component in this correlation.

4.2 First stage results

Figures 1a (males) and 1b (females) show years of education completed by cohort, where a non-parametric lowess smoother is estimated on both sides of the reform threshold. For males, there is a very clear and large discontinuity in educational attainment at the first cohort that is fully affected by the 1928 reform - the 1917 birth-year cohort. The average years of schooling increases by more than 0.8 at the threshold. For females, there is no such discontinuity, suggesting that the 1928 reform did not have a strong impact on the schooling of females and does not provide a useful instrument. Given this, we do not pursue the estimation of a causal effect for women.

Table 2 shows linear regression estimates of variation in years of education in relation to the reform indicator (1 if 1917 cohort or later), controlling for different specifications of cohort trends and for wave dummies. In all specifications the magnitude of the coefficient of the reform indicator lies in the range of 0.60-1, indicating that the 1928 reform raised the average years of schooling by between seven months and one year. The reform indicator is strongly statistically significant in all models, which is confirmed by the extremely strong robust F-tests of the instrument, passing all criteria proposed in the literature (Bound *et al.* 1995; Staiger and Stock, 1997; Stock and Yogo, 2002).[11] Finally, the G-test indicates that all polynomials are sufficiently flexible. Given the spirit of RDD (Lee and Lemieux, 2009) and the lower AIC values of the more flexible models 3 and 4, these will be the focus of our discussion.

We now check robustness of the first-stage results to the choice of bandwidth around the threshold (see e.g. Lee and Lemieux, 2009). The base case uses five birth-year cohorts before and after the reform to estimate a reliable polynomial through cohorts. Identification relies on the cohorts being interchangeable, and this will only hold if the cohorts are sufficiently close to each other. Yet, focusing attention on just a few cohorts around the threshold reduces power. Therefore, the estimation is repeated using ten and three cohorts on either side of the 1917 threshold – see table 3. Overall, using ten cohorts, the coefficient of the reform is very similar to our base case, except for model 4 where the coefficient sharply declines and becomes more consistent with estimates from the other models. The reform indicator is still highly significant, with the F-tests showing no evidence of a weak instrument (Bound *et al.* 1995; Staiger and Stock, 1997; Stock and Yogo, 2002). G-tests are not presented but they give no indication that the polynomials are insufficiently flexible. The reform dummy remains strongly significant when using only three cohorts on either side of the threshold, and although the magnitude of the coefficient increases somewhat, it remains in the same range. With only three cohorts the G-tests (not shown) are less satisfactory, but this is to be expected given the limited number of observations from which to fit the polynomial.

Overall, changing the bandwidth does not affect the significance of the instrument and the magnitude of its effect is quite robust. This supports our use of observations further away from the threshold, but as an additional check on whether this introduces bias, we examine robustness to controlling for covariates (marital status, ethnicity, province and city size) that may possibly differ across the five-year span on either side of the reform threshold. While covariates can safely be excluded with a valid RDD, when using observations further away from the threshold covariates might be used to correct for possible differences in underlying

[11] The Stock and Yogo critical values (Stock and Yogo, 2005) can only give approximate confirmation of the strength of the instruments since they are computed for an iid error model and here we have made the F-tests robust to heterokesdasticity and clustering at the cohort level. Baum *et al.* (2007) suggest relying on the Staiger and Stock (1997) rule of thumb that the (robust) F-statistic should exceed 10 for weak instruments not to be considered a problem. This is satisfied for all models.

characteristics (Lee, 2006). The results given in the third panel of Table 3 confirm that the coefficient of the reform dummy hardly changes relative to the baseline for each specification, and statistical significance remains strong.

A final robustness check examines whether potential always-takers bias the estimated effect of the reform on educational attainment. The reform affected the individuals who would have dropped out at the age of 12 prior to the 1928 law but became legally required to stay in school until at least their 13th birthday. Individuals who would have continued school irrespective of the reform, the so-called 'always-takers', are of no use in our analysis, and only cause downward bias in the estimated effects. Since it might be argued that individuals who finished university or higher vocational education are likely to have continued school anyway, irrespective of the law change, we check robustness to the exclusion of these individuals. Comparison of the estimates in the fourth panel of table 3 with those in table 2 reveals that the magnitude and statistical significance of the coefficients hardly changes, so that potential always-takers do not appear to bias the results.[12]

We conclude that the 1928 reform had a sudden and strong positive impact on years of schooling for males and so provides a strong instrument for education. The magnitude and statistical significance of the effect is robust to several checks, so that we can safely move on to the second stage.

4.3 Two Stage Least Squares (2SLS) estimates

Figure 2, showing the probability of dying between 1998 and 2005 for male cohorts born between 1912 and 1922, reveals a small downward discontinuity at the 1917 threshold. The reduced form estimate is negative in three of the four models presented in table 4, but the effect is never significant. The G-tests indicate that the models are sufficiently flexible. Controlling for covariates, changing the bandwidth, and excluding potential always-takers changes the magnitude of the coefficient on the reform dummy marginally in the models, but it always remains insignificant[13].

Since the first stage results for education support the strength of the instrument, and the reduced form results for mortality confirm that the polynomials

[12] A necessary condition for the validity of excluding these individuals as always-takers is that the reform did not have an impact on the probability of attaining a university degree or higher vocational qualification. It turns out that indeed we cannot reject a zero effect of the reform on these levels of educational (results available upon request).

[13] Results available upon request.

provide a sufficiently flexible specification, we proceed to Two-Stage Least Squares estimates, which are given in table 5. The point estimates of the effect of years of education on mortality are all negative and in the order of magnitude of 0.02-0.03, except for model 1. However, the effect is never close to statistical significance and, on the basis of these estimates, we cannot reject the hypothesis that education has no impact on mortality.

4.4 Two Sample Two Stage Least Squares (TS2SLS) estimates

Despite the large discontinuity in educational attainment produced by the 1928 reform, the POLS data provide no evidence that years of schooling has a significant causal impact on mortality. However, this could simply be due to a lack of power if there are an insufficient number of observations in the relevant cohorts. Using the much larger RIO sample to estimate the mortality effect allows investigation of this possibility.

TS2SLS is consistent under the assumption that the two samples are drawn from the same underlying population (Angrist and Krueger, 1992; Arellano and Meghir, 1992). A potential concern in our case is that the first stage is estimated using the POLS cross-sections for 1997-2005, while the second stage is estimated using the RIO 1998 with a mortality follow-up, so that, strictly speaking, the underlying populations are not identical. More specifically, the RIO linked mortality register data provides information on mortality between 1998 and 2005 for a sample of individuals alive in 1998, while individuals who have died or been institutionalized subsequent to 1998 are not available for the POLS cross-section samples in the period 1999-2005. The first stage estimate may therefore be biased if survivors are affected differently by the reform than decedents. To investigate this, the first stage is estimated separately for survivors and decedents in the period 1998-2005. Moreover, an additional check is performed by estimating the first stage using only the POLS 1998 observations. Results are shown in table 3, where we also present estimates using all 1997-2005 POLS observations (as in table 2) without wave dummies since these are not necessary in the reduced form for mortality using the follow-up to the 1998 RIO (see footnote 10). Excluding the wave dummies (panel 5) has little or no impact on the estimates. Although significance is greater for decedents, the point estimates of the effect of the reform are of the same order of magnitude for survivors (panel 6) and decedents (panel 7), except for model 4. Furthermore, point estimates of the effect of the reform obtained using just the 1998 POLS (panel 8) are in the same range as using all waves. We conclude that the point estimates obtained using POLS data from 1997-2005 show little or no bias and can safely be used to produce the first stage estimate for TS2SLS.

Figure 3 shows the probability of dying in the period 1998-2005 for cohorts born between 1912 and 1922 using the 1998 RIO. The trend is smoother than in the corresponding graph produced from the POLS data[14] (figure 2) because of the larger sample size. Although the discontinuity at the reform threshold seems small, there does appear to be a small downward shift in the trend at that point. The corresponding reduced form estimates presented in the second panel of table 4 confirm that the 1928 reform induced changes that reduced the probability of dying between the ages of 81 and 88 (inclusive) by 1 to 3 percentage points, an effect that is significant in all specifications except model 1. Controlling for covariates does not change the results, while widening the bandwidth slightly increases the magnitude of the effect[15].

The TS2SLS estimates presented in table 5 indicate that, except for the linear model, an additional year of schooling reduces the probability of dying between the ages of 81 and 88 by between 2 and 3 percentage points. The estimates are statistically significant at the 10 percent level or less. Given that around 50 percent of males who completed 6 years of schooling, corresponding to primary school completion, died between the ages of 81 and 88, our estimates suggest that one extra year of schooling reduced the probability of dying by 4-6 percent compared to the baseline.

5. Are there additional returns to high school graduation?

While it is conventional to study the health (and other) returns to additional years of education, one might question whether health-related knowledge is acquired linearly irrespective of the stage of education completed. An additional year in lower vocational education is likely to constitute quite a different learning experience from an additional year in high school, such that the impact of each on health outcomes may be expected to differ (Feinstein *et al.* 2006). Moreover, linearity imposes the potentially restrictive assumption that the health gains obtained on moving from lower

[14] Note that the average mortality rate in the 1998 RIO is higher than in the POLS data, which is due to the fact that the POLS data samples a new cross-section of survivors every year between 1998 and 2005.
[15] Results available upon request.

vocational school (10 years) to high school (13 years) are only 3/4 of the gains following graduation from high school (13 years) to university (17 years). While the evidence for 'sheepskin effects' of education certificates on health is mixed (Cowell, 2006; Chevalier and Feinstein, 2007; Cutler and Lleras-Muney, 2008), and *a priori* it is difficult to imagine why a certificate should bring health returns over and above those of the years of schooling necessary to obtain the certificate, estimates of the health impact of years of schooling overlooks potentially useful information on how the returns vary with the nature of schooling, as represented by the level of education completed.

In order to establish whether there is evidence of non-linearity and heterogeneity in the impact of education on health, in this section we estimate the effect of high school completion on mortality and compare this with the effect predicted from the linear specification. The 1928 reform increased the compulsory years of schooling from 6 to 7. Since primary school in the Netherlands consists of 6 years, this reform induced many individuals to enter secondary school. Indeed, as shown in figure 5, the percentage that finished high school appears to have increased dramatically due to the reform.

If, after controlling for the level of education, years of schooling have no additional impact, then the causal effect of the level of schooling on mortality can be identified. Yet, clearly with just one instrument we cannot identify the causal effect of all levels of schooling. It turns out, however, that the reform only had a significant impact on the propensity to complete (i) high school, and (ii) higher vocational education[16]. The causal effect of high school completion on mortality can then be identified if higher vocational education does not have any additional health benefits on top of those acquired by completing high school. Alternatively, if the reform induced pupils to complete high school but not higher vocational school conditional on finishing high school then we can identify the average effect of high school and higher vocational education completion on mortality. While debatable, these assumptions are not necessarily more restrictive than those necessary to identify the effect of years of schooling.

The first stage results are presented in table 6. It can be seen that the 1928 reform strongly and significantly impacted on the propensity to finish high school.

[16] Results available upon request.

Around 10 percent of the individuals born in these cohorts were induced to complete high school by the reform. Combining these estimates with the reduced form mortality estimates from table 4, gives the TS2SLS estimate of the effect of high school graduation on mortality presented in table 7, which is between 17 and 26 percentage points in our preferred, more flexible models. Coupling this result with the fact that around 50 percent of males who did not complete high school died between the ages of 81 and 88, the probability of dying in this age range is reduced by a remarkable 34-52 percent for those that finished high school.

Given that high school graduation amounts to seven additional years of schooling for the compliers who, before the reform, would have ended their education after primary school, estimates from the models linear in years of education imply that high school graduation reduces the probability of dying between the ages of 81 and 88 by 14-19 percentage points (i.e. 7×2-7×2.7). This is below the estimate of a 17-26 point decrease generated by the binary model of high school completion, but not substantially so. Moreover, as explained above, it might be that the 17-26 percent partly stems from the completion of higher vocational education if this level had any additional health benefits on top of those provided by high school completion. Consequently, while there may be some 'sheepskin effects' of high school graduation, beyond what is predicted by the years of schooling it entails, there magnitude would appear to be rather modest.

6. Discussion

Education significantly and substantially reduces mortality even in old age. Our analysis reveals that, for Dutch males surviving to the age of 81, an additional year of schooling reduces the probability that they will die before reaching 89 by 2-3 percentage points, or 4-6% relative to the baseline probability. This suggests that the well-documented large correlation between education and health outcomes (Grossman and Kaestner, 1997; Smith and Kington, 1997; Mackenbach *et al.*, 1997; Cutler and Lleras-Muney, 2008) is not spurious but stems (at least partly) from a causal effect of education on health, and consequently mortality.

Our study exploits a compulsory schooling reform introduced in the Netherlands in 1928 which provides a strong and exogenous instrument for the educational attainment of Dutch males. The average years of schooling increased by 0.6-1.0 due to the reform, a result that is robust to several specification checks. In spite of the strong instrument, the treatment effect estimated from a smaller sample (n=3650) is not significant. But by exploiting mortality data in a larger sample (n=66891), and combining this with education information from the smaller sample, we do find a significant impact of education on mortality. Our analysis is based on mortality observed at the individual level, rather than that approximated from changes in cohort sizes, or from a comparison of birth and death rates. The power provided by our sample size and individual level data is a great advantage over previous studies (Lleras-Muney, 2005; Clark and Royer, 2007; Mazumder, 2008), while compared to Albouy and Lequien (2008) the strength of the instrument and the age at which our affected cohort is observed might explain why we find a significant impact whereas they do not.

A distinguishing feature of our study is that we estimate the impact of schooling on mortality for individuals aged above 80. There is obviously pre-sample selective mortality and our estimates cannot be taken as indicative of the mortality effect of education at all ages. However, provided that education has a non-positive effect on mortality at all ages, then our estimates constitute a lower bound on the total effect of education on life expectancy.

Our IV estimates of the impact of schooling on mortality are larger than those obtained from OLS. This is consistent with measurement error exerting a greater downward bias on the OLS estimate than any upward bias arising from unobservables, such as ability and time preference that increase investments in both education and health. Indeed, IV estimates of the impact of education on earnings are often greater than those obtained from OLS (e.g. Card, 1999) and this has also been observed in estimates of the health returns to education (Arkes, 2003; Arendt, 2005; Lleras-Muney, 2005; Oreopoulos, 2006; Silles, 2009). But one should be cautious in comparing the IV and OLS estimates since they estimate different parameters. OLS seeks to estimate the average treatment effect (ATE) across the population, while the RDD IV identifies the LATE among compliers at the threshold of the 1928 reform (Hahn *et al.*, 2001; Van der Klaauw, 2002; Imbens and Lemieux, 2008). It is quite plausible that the treatment effect is larger at a lower level of education (Auld and Sidhu, 2005) such as those forced to stay on at school by the reform. From a policy perspective, this is clearly an interesting group. We wish to know the impact of education reforms on individuals whose behaviour is changed by them.

The fact that we find an effect implies that there are very long run returns to education. Not only does education raise earnings over the life cycle, it also extends the horizon of the lifetime. This is an important finding in the context of rising education levels and the ageing of populations worldwide. As more and better educated individuals reach old age, we can anticipate that mortality rates among the elderly will fall further and populations will become even more 'grey'. Of course, this need not mean that health and social care needs rise since falling mortality rates will reflect improving levels of health, but it does mean that pensions will be stretched further to meet the consumption needs of an extended old age. The labour market returns to education need to be invested to provide for the health returns in the form of extended life.

The other side of the coin is that poorly educated individuals die earlier, enjoying a less extended period of retirement. There is a double injustice here. Not only does a lack of education lead to a deprivation of life itself, but it implies a lower return on investments in pensions made over the working life. On equity grounds, a case could be made for varying the retirement age with education[17], although the moral hazard effects induced by such a policy would probably render it undesirable.

Our results imply that education policies can be important instruments for tackling health inequalities. Design of effective policies requires knowledge of the causal mechanisms responsible for the impact of education on mortality in old age that we find. Two broad hypotheses have been advanced to explain an impact of education on health. Grossman (1972) argues that, through information acquisition and processing skills, education raises the productivity of investments in health. This hypothesis is consistent with US evidence on educational disparities in the adoption of new medical technology (Goldman and Lakdawalla; 2001; Lleras-Muney and Lichtenberg, 2002; Glied and Lleras-Muney, 2003; Cutler *et al.* 2008) and the management of illness (Goldman and Smith, 2002). In the context of the Dutch universal system of health insurance, inequality in access to medical care is a less plausible reason for education to impact on health, although differences in the management of disease could not be so easily dismissed.

[17] In the Netherlands this argument was made earlier by Bovenberg *et al.* (2006), who proposed to let the legal retirement age vary with life expectancy in order to make the pension system fairer and more robust to ageing and intergenerational tensions.

A second hypothesis is that education operates through health behaviour - diet, exercise, smoking, drinking etc. (Muurinen, 1982). Empirical work has confirmed that the lower educated do indeed indulge in less healthy behaviour (Feinstein *et al.*, 2006; Cutler and Lleras-Muney, 2008) and that lifestyle acts as a mediator between education and health (Contoyannis and Jones, 2004; Balia and Jones, 2008). The cohorts studied in this paper were 42-52 when the US Surgeon General's Report on Smoking and Health was published in 1964. US studies report that the better educated were more likely to quit smoking, or not take it up, after the publication of evidence on its risks (De Walque, 2007; Grimard and Parent, 2007). Although most smoking-related deaths occur before the age of 80, Peto *et al.* (1992) estimate that 39% of deaths of Dutch males in 1995 above the age of 70 are smoking related. We investigated whether there was any evidence of a mechanism through smoking behaviour by comparing the estimated treatment effects of education on different causes of death. While we did find a significant effect on deaths from respiratory diseases and on all deaths categorised as smoking attributable mortality (US Department of Health and Social Service, 1989; Peto *et al.*, 1992), there was no consistent evidence of a larger effect on these than on other causes of death (results available upon request). Further research seeking to unravel the pathways responsible for the causal impact of education on mortality is clearly warranted.

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Tables

Table 1: OLS estimates of the effect of years of education on the probability of dying between 1998 and 2005 of cohorts born between 1912 and 1922.

Males			
Years of Education	-0.015***	-0.013***	-0.011***
	(0.002)	(0.002)	(0.002)
Cohort	NO	YES	YES
Covariates	NO	NO	YES
Females			
Years of Education	-0.009***	-0.008***	-0.008***
	(0.002)	(0.002)	(0.002)
Cohort	NO	YES	YES
Covariates	NO	NO	YES

*** p-value < 0.1, ** p-value < 0.05, *** p-value < 0.01**

Notes: Data are from the 1997-2005 POLS. Cohort refers to a quartic polynomial in cohort. Covariates include wave dummies, marital status, province, city size and ethnicity. Standard errors are given in parenthesis.

Table 2: First Stage OLS estimates of impact of 1928 Compulsory Schooling Law on years of education controlling for cohort and wave dummies. Cohorts 1912-1922, males. N=3650.

Variable	Model 1	Model 2	Model 3	Model 4
Reform	0.685***	0.669***	0.636***	1.039***
	(0.099)	(0.109)	(0.112)	(0.050)
Cohort	0.013	0.016	0.039	-0.241***
$Cohort^2$		-0.001		-0.049***
Reform*Cohort			-0.032	0.121**
$Reform*Cohort^2$				0.073***
Wave98	-0.243	-0.243	-0.242	-0.244
Wave99	-0.189	-0.189	-0.190	-0.194
Wave00	-0.660**	-0.660**	-0.661**	-0.661**
Wave01	0.128	0.128	0.128	0.125
Wave02	-0.098	-0.099	-0.099	-0.106
Wave03	-0.322	-0.322	-0.323	-0.328
Wave04	0.738**	0.737**	0.737**	0.734**
Wave05	-0.165	-0.166	-0.167	-0.165
Constant	9.676***	9.695***	9.743***	9.443***
AIC	19566.78	19566.76	19566.66	19565.42
F-statistic	47.55	37.94	32.21	436.08
G-statistic	0.17	0.17	0.16	0.02

*** p-value < 0.1, ** p-value < 0.05, *** p-value < 0.01**

Notes: Data are from the 1997-2005 POLS. Models 1-4 refer to linear and quadratic polynomials (models 1 and 2, respectively) which are allowed to differ on either side of the threshold (models 3 and 4, respectively). "Reform" is 1 if 1917 cohort or later. The "Cohort" variable is centred on the 1917 cohort. AIC is the Akaike Information Criterion. F-statistic is for test of significance of 'reform', and is robust to clustering at cohort level and heteroskedasticity. G-statistic is the test statistic of the flexibility of the cohort polynomial, which follows a F(J-K,N-J) distribution, where J is the number of cohorts used in the estimation, K is the number of parameters, and N is the number of observations (Lee and Card, 2008). Standard errors (in parenthesis for "Reform") are robust to heteroskedasticity and clustering at the cohort level.

Table 3: Robustness checks on OLS estimates of impact of 1928 Compulsory Schooling Law on years of education, males.

Variable	Model 1	Model 2	Model 3	Model 4
10 birth year cohorts either side of reform (1907-1927). N=7845				
Reform	0.690***	0.751***	0.787***	0.619***
	(0.078)	(0.080)	(0.075)	(0.139)
F-statistic	77.65	88.68	110.90	19.76
3 birth year cohorts either side of reform (1914-1920). N=2218				
Reform	0.800***	0.825***	0.819***	1.197***
	(0.058)	(0.055)	(0.058)	(0.053)
F-statistic	192.58	228.68	197.90	512.77
5 birth year cohorts (1912-1922) with control for covariates. N=3650				
Reform	0.629***	0.665***	0.642***	0.926***
	(0.099)	(0.092)	(0.084)	(0.038)
F-statistic	40.45	52.75	58.38	590.30
5 birth year cohorts excluding those that finished university/college or PhD. N=3448				
Reform	0.659***	0.572***	0.522**	1.029***
	(0.139)	(0.175)	(0.204)	(0.203)
F-statistic	22.59	10.74	6.54	25.79
5 birth year cohorts (1912-1922) without wave dummies. N=3650				
Reform	0.692***	0.681***	0.651***	1.082***
	(0.105)	(0.117)	(0.124)	(0.053)
F-statistic	43.40	33.80	27.44	423.0
5 birth year cohorts (1912-1922) among Survivors. N=1991				
Reform	0.681*	0.864**	0.757**	1.384***
5 birth year cohorts (1912-1922) among Decedents. N=1659				
Reform	0.698***	0.575***	0.567***	0.689***
5 birth year cohorts (1912-1922), just 1998 sample. N=833				
Reform	0.911**	0.806**	0.760**	0.917*

*** p-value < 0.1, ** p-value < 0.05, *** p-value < 0.01**

Notes: Data are from the 1997-2005 POLS. "Reform" is 1 if 1917 cohort or later. Models 1-4 and F-statistic as in Table 2. Standard errors (in parenthesis for "Reform") are robust to heteroskedasticity and clustering at the cohort level.

Table 4: Reduced Form OLS estimates of the impact of the 1928 Compulsory Schooling Law on the probability of dying between 1998 and 2005 inclusive. Cohorts 1912-1922, males.

Variable	Model 1	Model 2	Model 3	Model 4
Reduced Form OLS estimates using the POLS 1997-2005. N=3650.				
Reform	0.000	-0.013	-0.020	-0.029
	(0.023)	(0.021)	(0.020)	(0.027)
Cohort	-0.031***	-0.028***	-0.020***	-0.005
$Cohort^2$		-0.001		0.003
Reform*Cohort			-0.013**	-0.036
Reform*$Cohort^2$				-0.001
Wave98	-0.055**	-0.055**	-0.055**	-0.055**
Wave99	-0.092***	-0.092***	-0.092***	-0.092***
Wave00	-0.148***	-0.148***	-0.148***	-0.148***
Wave01	-0.224***	-0.224***	-0.224***	-0.224***
Wave02	-0.306***	-0.307***	-0.307***	-0.307***
Wave03	-0.403***	-0.403***	-0.403***	-0.404***
Wave04	-0.453***	-0.453***	-0.453***	-0.453***
Wave05	-0.562***	-0.563***	-0.563***	-0.563***
Constant	0.649***	0.663***	0.675***	0.692***
AIC	4830.1	4829.5	4829.1	4828.8
G-Statistic	0.47	0.40	0.36	0.33
Reduced Form OLS estimates using the RIO 1998 follow-up. N=66891				
Reform	-0.008	-0.014*	-0.018***	-0.029***
	(0.009)	(0.007)	(0.005)	(0.004)
Cohort	-0.037***	-0.036***	-0.032***	-0.018***
$Cohort^2$		-0.000*		-0.002**
Reform*Cohort			-0.007***	-0.027***
Reform*$Cohort^2$				-0.001*
Constant	0.625***	0.633***	0.640***	0.655***
AIC	92278.0	92277.0	92274.5	92275.6
G-statistic	1.21	0.95	0.60	0.25

*** p-value < 0.1, ** p-value < 0.05, *** p-value < 0.01**

Notes: "Reform" is 1 if 1917 cohort or later. AIC and G-test as in table 2. Standard errors (in parenthesis for "Reform") are robust to heteroskedasticity and clustering at the cohort level.

Table 5: RDD estimates of the impact of years of education on the probability of dying between the ages of 80-88 (2SLS) or 81-88 (TS2SLS) . Cohorts 1912-1922, males.

Variable	**Model 1**	**Model 2**	**Model 3**	**Model 4**
2SLS estimates from 1997-2005 POLS (N=3650)				
Years of	0.000	-0.019	-0.031	-0.028
Education	(0.034)	(0.030)	(0.027)	(0.025)
TS2SLS estimates from 1997-2005 POLS (N=3650) & RIO 1998 follow-up (N=66891)				
Years of	-0.011	-0.020*	-0.027***	-0.026***
Education	(0.013)	(0.011)	(0.009)	(0.004)

*** p-value < 0.1, ** p-value < 0.05, *** p-value < 0.01**

Notes: Both the POLS and RIO data are linked to the Cause-of-Death Register (DO). Years of Education is instrumented by the 1928 compulsory schooling reform. Models 1-4 as in table 2 for 2SLS and as this without wave dummies for TS2SLS. Standard errors (in parenthesis) are robust to heteroskedasticity and clustering at the cohort level and are obtained by the Delta method for the TS2SLS estimation.

Table 6: First Stage OLS estimates of impact of 1928 Compulsory Schooling Law on the probability of completing high school. Cohorts 1912-1922, males. N=3650.

Variable	Model 1	Model 2	Model 3	Model 4
Reform	0.096***	0.108***	0.105***	0.105***
	(0.016)	(0.015)	(0.013)	(0.010)
Cohort	-0.000	-0.002	-0.005***	0.007
Cohort^2		0.001***		0.002***
Reform*Cohort			0.006	-0.021**
Reform*Cohort^2				0.001
Wave98	-0.030	-0.030	-0.030	-0.030
Wave99	-0.027	-0.027	-0.027	-0.027
Wave00	-0.081**	-0.081**	-0.081**	-0.081**
Wave01	-0.002	-0.002	-0.002	-0.002
Wave02	-0.034	-0.034	-0.034	-0.035
Wave03	-0.030	-0.031	-0.030	-0.031
Wave04	0.077*	0.077*	0.077*	0.078*
Wave05	-0.065	-0.064	-0.064	-0.064
Constant	0.347***	0.334***	0.334***	0.347***
AIC	5102.6	5102.1	5102.4	5101.8
F-statistic	36.51	54.86	65.97	111.40
G-statistic	0.18	0.13	0.16	0.09

*** p-value < 0.1, ** p-value < 0.05, *** p-value < 0.01**

Notes: Data are from the 1997-2005 POLS. "Reform" is 1 if 1917 cohort or later. AIC, F-test and G-test as in table 2. Standard errors (in parenthesis for "Reform") are robust to heteroskedasticity and clustering at the cohort level.

Table 7: RDD estimates of the impact of high school completion on the probability of dying between the ages of 81 and 88. Cohorts 1912-1922, males.

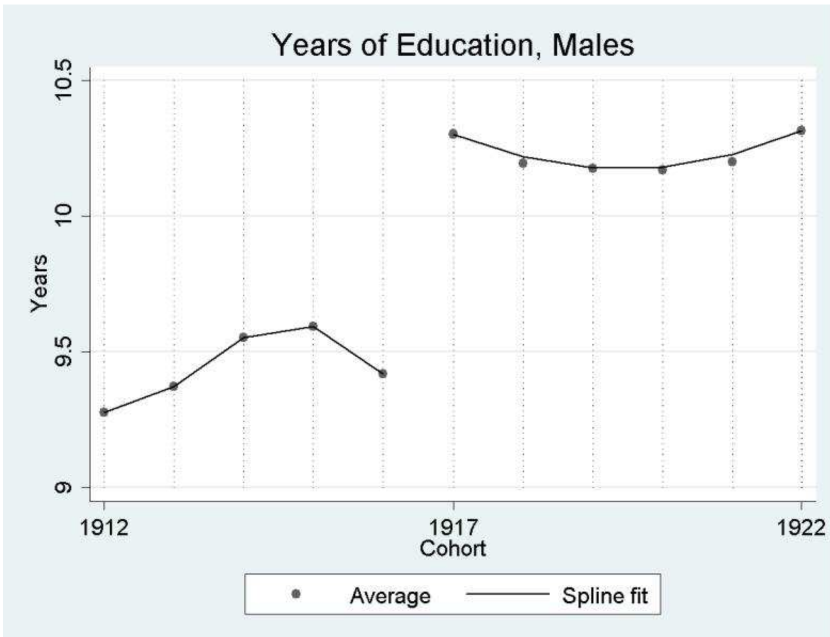
Variable	Model 1	Model 2	Model 3	Model 4
High school	-0.078	-0.128*	-0.168***	-0.264***
completion	(0.096)	(0.067)	(0.049)	(0.044)

*** p-value < 0.1, ** p-value < 0.05, *** p-value < 0.01**

Notes: Data are from the 1997-2005 POLS linked to the Cause-of-Death Register (DO) (N=3650) and the RIO 1998 linked to DO (N=66891). Models 1-4 as in table 2. Standard errors (in parenthesis) are robust to heteroskedasticity and clustering at the cohort level, and are obtained by the Delta method.

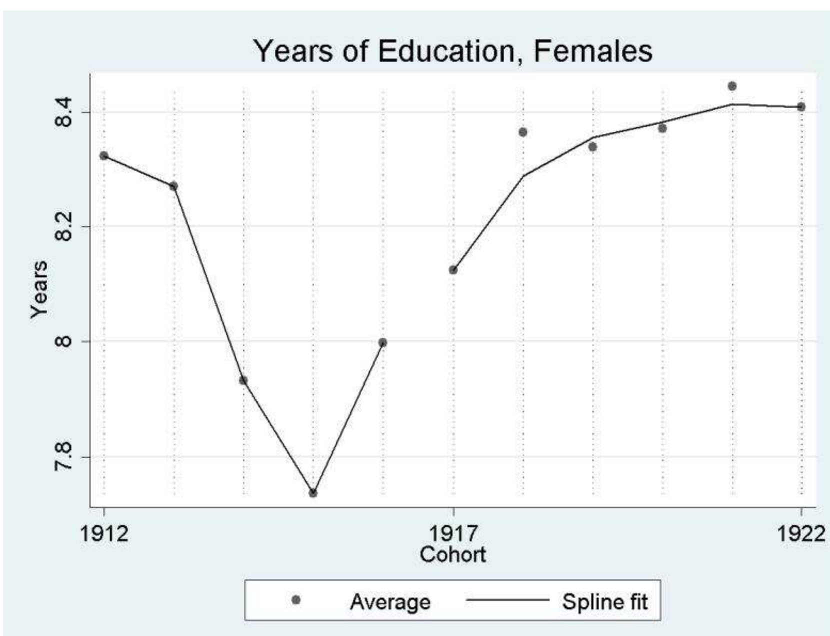
Figures

Figure 1a: Years of education by cohort. 1912-1922 birth-year cohorts, males.



Notes: Data are from the 1997-2005 POLS.

Figure 1b: Years of education by cohort. 1912-1922 birth-year cohorts, females.

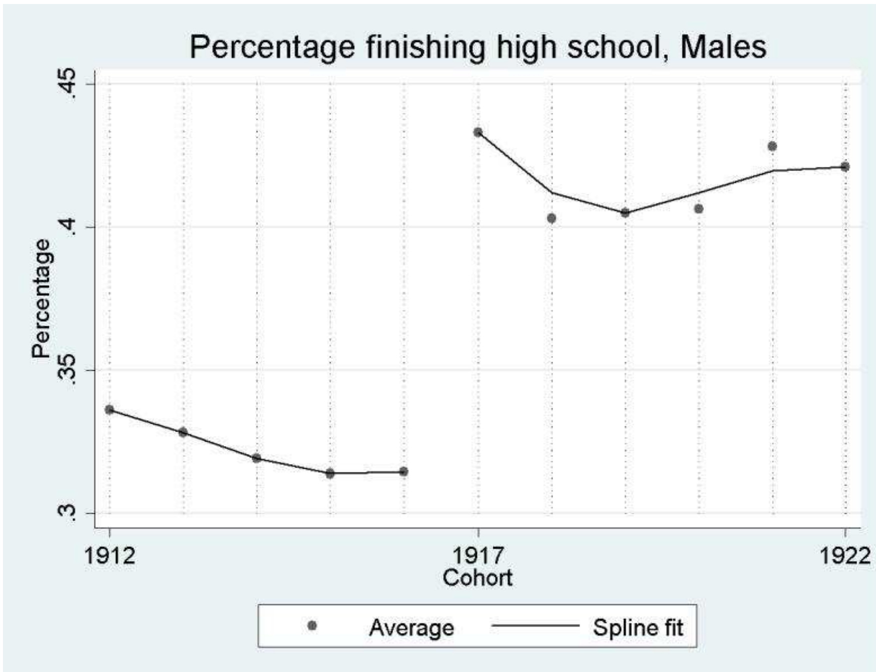


Notes: Data are from the 1997-2005 POLS.

Figure 3: Mortality rate, percentage that died in the period 1998-2005, by cohort. Cohorts 1912-1922, males, POLS 1997-2005.

Figure 4: Mortality rate, percentage that died in the period 1998-2005, by cohort. Cohorts 1912-1922, males, RIO 1998.

Figure 5: Percentage finishing high school by cohort. Cohorts 1912-1922, males.



Notes: Data are from the 1997-2005 POLS.