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Estimating the Effect of Education on Health: Old Instrument, New Insights?

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Abstract

This paper uses variation of education generated by the interaction of compulsory schooling laws and season of birth as an instrument to estimate the effect of education on mortality and self-reported health. Education is found to have protective effects on health which differ between cohorts. Threats to this in the literature common approach are identified and the following analysis implies that estimates possibly overstate the effect of education on health.

Key Words: Education, Health, Mortality, Instrumental Variable Estimation, Causality

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1 Introduction

There is a strong empirical relationship between education and health which is frequently debated by researchers and policymakers. Not only it is observed that individuals with higher educational attainment live longer than those with little education, but additionally, this gap has been increasing recently which is the result of a positive trend of longevity of the highly educated but stagnation among the low educated (Jemal et al., 2008). If policymakers attempt to close that gap by increasing education at the lower end of the education distribution, it is crucial to know whether the relationship between education and income is causal or the result of third factors. However, there is no clear evidence yet whether the effect of education on health is causal.

In this paper, I intend to contribute to the literature by exploiting exogenous variation in schooling generated by the interaction of season of birth with compulsory schooling laws as an instrument to identify effects of education on mortality and self-reported health using large U.S. survey data of adults. This approach which goes back to Angrist and Krueger (1991) has been used frequently in the literature, however, to my knowledge there is no study using mortality as an outcome. Moreover, the originality of my approach lies within discussing new evidence of a possible violation of the exclusion restriction by Buckles and Hungerman (2013) who find that maternal socioeconomic characteristics vary by birth seasons. I investigate indications of these findings in the data and discuss the consequences for the interpretation of the estimates in the main analysis of this paper.

I find significant and large effects of education on mortality and self-reported health and indications that these results differ between different cohorts and ethnic groups. The results appear to be robust to different specifications. Despite these findings, however, there are some indications in the data related to the evidence of Buckles and Hungerman (2013) which raise concerns that the instrument is not exogenous. I conclude that the instrumental variable estimates are possibly biased upwards.

The paper is organised as follows: Section 2 gives first a brief overview of theoretical considerations on the education-health relationship and describes transmission channels, secondly, it summarises previous findings of empirical studies. In Section 3, the institutional mechanism behind the season of birth instrument is explained and threats to the exclusion restriction are discussed. The empirical strategy is discussed in Section 4 followed by the presentation of the data in Section 5. Empirical results are presented in Section 6.

Section 7 discusses the validity of the empirical strategy; Section 8 concludes.

2 The Education-Health Relationship

There is a large body of theoretical and empirical research investigating the relationship between education and health. The basis for many theoretical considerations is the model proposed by Grossman (1972) who views health as a durable capital stock that depreciates over time and increases with investments. He argues that individuals with higher education are more efficient in producing health inputs such that they demand a higher stock of health capital. Cutler and Lleras-Muney (2006) provide an extensive and empirically motivated discussion of possible mechanisms which could account for the relationship between education and health. Additionally to productive efficiency, they consider education to have a direct effect on health because more educated individuals are better informed and use the information more efficiently to make health-related decisions. They also discuss indirect effects of education on health, for instance, that educated people have higher incomes, better health care access and work in safer environments. Furthermore, higher educated people tend to have a higher social rank and larger social networks which are associated with better health (Cutler & Lleras-Muney, 2006). Besides these causal channels of education, Cutler and Lleras-Muney (2006) note that the link between education and health can however be a result of health having an impact on educational attainment as well. For instance, Black et al. (2007) find that low birth weight children have lower education. Furthermore, Cutler and Lleras-Muney (2006) discuss the existence of third factors that are correlated with health and educational attainment such as parental investment in education and health of their children. Evidence for maternal education is provided by Black et al. (2005) who find a causal effect on sons' education while Lundborg et al. (2014) find positive effects on sons' health status. Another example is proposed by Fuchs (1980) who finds evidence that higher educated people have lower discount rates which are linked to more schooling and higher investment in health.

The theoretical discussion above suggests that there are many possible and often unobserved factors that could account for the empirically strong correlation between education and health. Hence, there are serious doubts about whether a control strategy like ordinary least squares (OLS) can produce unbiased and consistent estimates of the causal effect of education on health. However, from a policy perspective, it is crucial to know whether the effect of education on health is causal or a result of other factors. As a consequence, there have been many empirical studies in the last two decades investigating the causal effect of education on health using natural experiments. An early contribution is Adams (2002) who uses similar to this study U.S. data and quarter of birth as an instrument. He finds positive effects of education on self-reported health and functional limitations which are similar in magnitude to his OLS estimates. Also, the more recent study by Becchetti et al. (2018) uses quarter of birth as an instrument and finds effects of education on mental and physical functionalities of adults in Europe. Beginning with Lleras-Muney (2005), many studies are using exogenous variation from compulsory schooling reforms in different countries to estimate the causal effects of education on health. Using U.S. census data at group level, Lleras-Muney (2005) finds that education significantly lowers mortality. Being able to replicate these findings with a larger data set, Mazumder (2008) however observes that estimates for mortality become insignificant when he controls for state-specific time trends. In contrast to his findings for mortality, he estimates significant and robust effects of education on self-reported health. Similarly, Fletcher (2015) finds self-reported health improving effects but no significant effect of education on mortality using U.S. survey data including a sample of older adults.

School reforms in other countries have also been exploited with similar research designs. Using a Dutch school reform from 1928, Van Kippersluis et al. (2011) find that education significantly decreases mortality for their large sample of older adults by employing a fuzzy regression discontinuity design. For the United Kingdom, Clark and Royer (2013) also use a regression discontinuity framework for two different school reforms and are not able to reject the null hypothesis of no effect of education on mortality. Meghir et al. (2018) employ regression discontinuity and difference-in-differences designs to a school reform in Sweden but are not able to find significant effects on mortality. These findings stand in contrast to those of Fischer et al. (2021) who use two Swedish school reforms to isolate the effect of additional education from peer effects caused by one reform. They observe health gains in terms of self-reported health and mortality caused by the reform.

The study of Fischer et al. (2021) also highlights the concern that school reforms do often not only result in a change of years of education but are likely to have effects on the peer composition and quality of schooling. Despite weaker statistical power and other possible issues presented in Section 3, I do not expect the season of birth instrument to have large influence on peer composition as individuals should be as good as randomly distributed over birth seasons such that selectivity of dropouts does not affect peer composition. Furthermore, the instrument does not use a structural change in the school system as exogenous variation. As a consequence, the season of birth instrument should not result in quality changes in education.

The summary of results of empirical literature above shows that the evidence is still not clear on whether education has a causal effect on health. In their literature review, Galama et al. (2018) conclude that the heterogeneity of findings is to some extent due to different methodologies. Furthermore, causal effects might be only present at specific times, locations, or for specific populations. They note that effects often differ between men and women which is likely to be at least partially driven by different effects of education on earnings (Galama et al., 2018, pp.26–27). Hamad et al. (2018) evaluate the findings of studies using compulsory schooling laws to measure effects on health in a meta-analysis. They find heterogeneity across results but an overall mortality lowering effect of education when pooling estimates from various studies. The discussion of previous literature has highlighted that further research is required to investigate if there are causal effects of education on health.

3 Quarter of Birth and Educational Attainment

In their seminal paper, Angrist and Krueger (1991) use an individual's quarter of birth as an instrument to estimate the effect of compulsory schooling on earnings. Therefore, they establish that there exists a correlation between an individual's date of birth and educational attainment as a result of the interaction of season of birth with compulsory school attendance laws using U.S. census data for cohorts born between 1920 and 1959. In most states, students start school in the autumn of the year they turn six years old (Angrist & Krueger, 1991, p.980). As a result, children born early in the year are on average older than those born late in the year when they start school. At the same time, compulsory schooling laws require students to remain in school until they reach a certain age when they are allowed to legally drop out, depending on the state that is usually between the age of 16 and 18 (Angrist & Krueger, 1991, pp. 1012–1013). Angrist and

Krueger (1991) argue that if the fraction of students wanting to drop out of school is equally distributed over birth dates, students born early in the year who reach the legal dropout age are able to leave school while those born late in the same year are forced to stay in school despite their desire to drop out. Regarding the whole cohort, this implies that average years of education are slightly higher for those born late in the year compared to those born in spring. Using a difference-in-differences design, Angrist and Krueger (1991) estimate that the enrolment rate of sixteen year old born in 1944 is increased by 4 percentage points because of compulsory schooling laws comparing states with 16 years years of compulsory schooling to those with 17 and 18 years. They conclude that these laws affect a significant part of the population and prevent approximately one-third of potential dropouts in school as about 12% of the students left school at age 16 in states where it was legal in the year 1960. In Section 6, estimates of the effect of season of birth on educational attainment are presented to discuss further the utility of season of birth as an instrument for educational attainment.

For the variable quarter of birth to be a valid instrument, the critical assumption that an individual's quarter of birth only affects their health through the above-described channel of schooling has to be satisfied. However, there exist studies from both social and natural sciences which cast doubts if the so-called exclusion restriction could be violated because of a relation of season of birth with different characteristics of health and socioeconomic status (Bound et al., 1995). For instance, Buckles and Hungerman (2013) find evidence that maternal socioeconomic characteristics are not equally distributed over birth months. Using U.S. census data for birth cohorts between 1943–1980 and natality detail files, they observe that mothers of children born in winter are more likely to be white, have completed high school, be married and are less likely to be a teenager when giving birth. Furthermore, they show that controlling for maternal characteristics reduces the effect of quarter of birth on educational attainment. That indicates a violation of the exclusion restriction as it shows that family characteristics, which are likely to be associated with different health outcomes, are related to quarter of birth. Buckles and Hungerman (2013) also find evidence that the seasonal pattern of maternal characteristics is driven by planned births. They provide the explanation that women with higher socioeconomic status have stronger preferences and are more able to time the birth date. It is noteworthy that the effect of children born in winter having on average a lower maternal socioeconomic status is weaker in earlier years of their study. This supports the authors' hypothesis as hormonal contraception became more available in the 1960s.

Further concerns are raised by a large body of epidemiological literature finding patterns between season of birth and health conditions in later life like mortality and chronic diseases, see Vaiserman (2020) for an overview. For instance, there is strong evidence that schizophrenia is more often diagnosed in individuals born in winter and early spring which is associated with factors like climate conditions and maternal infection exposure during pregnancy (Sham et al., 1992; Tochigi et al., 2004). Moreover, Doblhammer and Vaupel (2001) find that life expectancy is the highest for individuals born in autumn both in the northern and the southern hemisphere using data from Austria, Denmark and Australia. They argue that this pattern is most likely a result of pre- and postnatal nutrition and disease exposure. The findings above suggest that season of birth affects health outcomes through a different channel than educational attainment which threats the validity of using season of birth as an instrument for identifying effects of education on health. The possible violations of the exclusion restriction presented in this section have to be taken into consideration when interpreting the results of this study in Section 6 and their implications are discussed in Section 7. The following section formalises the empirical strategy and key identifying assumptions.

4 Empirical Strategy

The ambition of this paper is to estimate a causal effect of education on mortality and self-reported health. The econometric model in Equation 1 can be used to estimate the effect of schooling ρ on the health outcome h_i where s_i denotes years of schooling of individual i, X_i is a vector of observed covariates and u_i an error term.

$$h_i = X_i'\beta + \rho s_i + u_i \tag{1}$$

However, the main regressor s_i is likely to be endogenous because of measurement error in schooling or relevant unobserved factors in u_i which are linked to health and educational attainment like parental investment or discount rates which are discussed in Section 2. As a consequence, OLS estimates of ρ within the above model are likely to be biased and inconsistent. Classical measurement error in schooling attenuates OLS estimates of ρ

towards zero, see e.g. Wooldridge (2010, pp.73–76). More important for this study are omitted unobserved variables which likely lead to an overestimation of the true parameter ρ by OLS estimation as the unobserved factors discussed above are likely to be positively correlated with both education and health. Intuitively, OLS estimates of ρ will include the part of the effect of the third factor on health which is correlated with education and thus be upward biased.

In the economic literature, a common approach to cope with an omitted variables problem in a single equation model is instrumental variable estimation (Angrist & Pischke, 2009, pp.115–116). Instrumental variable estimates are obtained in this paper using the two-stage least squares estimator (2SLS). Intuitively, the exogenous instrument predicts variation of the endogenous regressor s_i which is not correlated with the error term. This unproblematic part of the variation in the endogenous regressor is used to estimate the parameter ρ. Therefore, the following first and second stage equations summarise the models which are estimated:

$$s_i = X_i'\alpha + Y_i'\delta + Z_i'\pi + \nu_i \tag{2}$$

$$h_i = X_i'\beta + Y_i'\phi + \rho s_i + \varepsilon_i \tag{3}$$

The vector of instruments Z_i consists of three quarter of birth dummies interacted with year of birth dummies. The first-stage equation (2) estimates variation of schooling s_i predicted by the instruments. Those predicted values of schooling are used to estimate the second stage equation (3) and the parameter of interest ρ. As a result of including also year of birth dummies Y_i in the equation, this estimation strategy identifies within birth year differences in education by quarter of birth (Angrist & Krueger, 1991, pp.997–998).

To give the 2SLS estimates a causal interpretation, the following assumptions have to be satisfied (Angrist & Pischke, 2009, pp.151–155, 175–178). First, the instrument has to be as good as randomly assigned (conditioned on covariates). Later in the paper, I will discuss that this assumption might be only satisfied when one controls for age as those born earlier in the year are naturally slightly older. Secondly, it must be assumed that season of birth is only affecting health outcomes through the compulsory schooling channel. As already discussed in Section 3, there are some concerns that will be extensively addressed in Section 7. Thirdly, there has to be a strong first-stage effect which will be established in Section 6.1. Finally, monotonicity requires that all individuals affected by the instrument are affected in the same way (Angrist & Pischke, 2009, p.154). Regarding the quarter of birth instrument, that implies that for instance individuals born late in the year should not be able to drop out early despite not having reached the legal dropout age. This also depends on how effectively compulsory schooling laws were enforced for the cohorts examined in this paper. Angrist and Krueger (1991, p.993) argue for effective enforcement because of a combination of prohibition of child labour and enforcement of compulsory schooling.

If these assumptions are satisfied the estimates can be interpreted as the average causal effect of the group which is marginally affected by the instrument, the local average treatment effect (LATE) (Angrist & Pischke, 2009, p.155). In case the instrument is only as good as randomly assigned conditioned on covariates, Angrist and Pischke (2009, pp.175–178) summarise that the estimated parameter provides under usual circumstances a good approximation of a covariate-averaged LATE. Applied to this paper, the marginally affected are potential drop-outs who are forced to stay in school by compulsory school attendance laws. In the next section, I turn to the data used in this paper before estimating the models described above.

5 Data

The data used for the empirical analysis of this paper are from the National Health Interview Survey (NHIS) which is part of IPUMS, a database with publicly available micro data provided by the University of Minnesota (Blewett et al., 2019). The survey is conducted yearly and draws a sample of the civilian U.S. population in all 50 states including around 35,000 households per wave resulting in a repeated cross-section data structure. The NHIS follows a complex sampling procedure oversampling the Black and Hispanic population in some years, however, sample weights are provided to obtain a representative sample of the U.S. population and correct standard errors as samples are not drawn randomly from the population. This paper uses data from the 1986–1996 surveys for cohorts born between 1930 and 1949 resulting in 249,264 observations. These cohorts are selected as for them the relationship between season of birth and educational attainment is especially strong. For later cohorts, the relationship becomes weaker as compulsory schooling laws lose relevance due to increasing average levels of education (Angrist & Krueger, 1991).

The NHIS contains a large set of different health outcomes. This study uses mortality as a measure of health which has the advantage of being an objective and unambiguous measure reflecting lifespan health conditions, moreover, research findings can be compared easier to those of other studies (Galama et al., 2018). Also, the selection of 1930–1949 born has the advantage that these cohorts are significantly affected by mortality which is likely to make results more informative. The NHIS provides mortality information by matching survey participants with the National Death Index records in the years after the interview until the year 2015. Thus, the outcome variable constructed for this study specifies whether the person has died before 2015 or not which explains the relatively high mortality rates presented in Table 1. For individuals of the 1930s cohort, it measures whether they died before turning 76 to 85 depending on the birth year, and for the 1940s cohort between 66 and 75. Only about 2 percent of individuals are not eligible to be linked because of a lack of data leading only to a few missing observations. Moreover, sample weights are given to adjust for ineligible respondents for mortality analysis. Besides mortality, self-reported health where 1 represents excellent health and 5 poor health is used as health outcome variable. I use self-reported health in two different ways: First, as a dummy variable indicating fair or poor health which are the lowest two of in total five categories. Secondly, I treat it as cardinal, although being ordinally scaled, to facilitate interpretation and comparison to studies using it in a similar manner, see for example Mazumder (2008). Table 1 shows that females report on average worse health status although they live on average longer. This observation seems consistent with the findings of Van Doorslaer and Gerdtham (2003, p.1625) who observe for a sample of Swedish adults that males tend to report better health than females given the same mortality risk. The observation that different groups seem to have different thresholds for reporting health status highlights one important issue. Self-reported health can lead to biased estimates of the effect of schooling on health if different socioeconomic groups report the same true health status systematically different (Van Doorslaer & Gerdtham, 2003, p.1622). However, Van Doorslaer and Gerdtham (2003, p.1625) find no evidence that self-reported health status differs by education or income.

Furthermore, the NHIS includes information about individual demographic and so-

Table 1: Descriptive statistics for selected variables reported separately by sex and cohort.

	Males			Females				
	Mean	SD	*N*	Mean	SD	*N*	Min	Max
Panel A: 30s cohort								
Died	0.470	0.499	47,697	0.361	0.480	53,208	0	1
Health	2.394	1.193	48,394	2.523	1.169	54,094	1	5
Badhealth	0.175	0.380	48,394	0.197	0.398	54,094	0	1
School	12.408	3.490	47,887	11.989	3.018	53,622	0	18
Age	55.607	4.106	48,624	55.679	4.111	54,340	46	66
Black	0.117	0.322	48,253	0.147	0.354	53,899	0	1
Hispanic	0.071	0.256	48,326	0.073	0.260	54,021	0	1
MSA	0.760	0.427	48,624	0.763	0.426	54,340	0	1
Married	0.825	0.380	48,624	0.689	0.463	54,340	0	1
Panel B: 40s cohort								
Died	0.231	0.422	68,301	0.160	0.366	74763	0	1
Health	2.099	1.079	69,547	2.237	1.094	76127	1	5
Badhealth	0.105	0.307	69,547	0.126	0.331	76,127	0	1
School	13.283	3.178	68,928	12.858	2.905	75,625	0	18
Age	45.422	4.082	69,829	45.402	4.079	76,471	36	56
Black	0.112	0.315	69,287	0.143	0.350	75,875	0	1
Hispanic	0.079	0.270	69,417	0.083	0.276	76,009	0	1
MSA	0.779	0.415	69,829	0.787	0.409	76,471	0	1
Married	0.803	0.398	69,829	0.709	0.454	76,471	0	1

cioeconomic characteristics. Educational attainment is measured by the highest grade of schooling the individual has ever completed. This is an advantageous and informative measure for this study as it captures the individual's highest educational attainment and not time spent in school. The highest category is six or more years of college, which is coded as 18 years of education in total. As the focus of this paper lies on compulsory schooling and the highest group is relatively small, the education variable is treated as cardinal despite the highest education category being collapsed. As the youngest individuals of the studied cohorts were 36 years old when the interview was conducted, virtually all individuals can be expected to have completed their education. In Table 1, one can observe that males have on average slightly higher education than females in both cohorts and that the average years of schooling increased for both males and females by about 0.9 years.

For estimating the empirical models introduced in Section 4, core demographic variables serve as control variables. The discussion of the identification strategy has emphasised the need for information on individuals' quarter of birth and year of birth. Season of birth is given by birth month which is easily transformed into quarter of birth. However, there is no information on the year of birth given. As a result, I use age which is measured in years on the day of the interview and the exact date of the interview to impute year of birth. Therefore, individuals are assigned the difference between survey year and age if the interview is after the respondents birth month and the difference minus one if before. For about 8.4% of the observations interview and birth month coincide such that I count those interviewed after the 15th of the month as if they already had their birthday. It follows that roughly 4% of the observations are assigned to the wrong year. In Section 6.4, I analyse if results are sensitive to the imputation of birth year.

Furthermore, the NHIS includes information about marital status, race and residence in a metropolitan statistical area (MSA). Marital status is transformed into a dummy variable indicating if a person is married with their spouse present or not. For the variable race, there are many categories available, however, for the cohorts examined in this paper only the category Black/African-American is of significant size with a fraction of 12 percent, such that a dummy variable is used to control for individuals being Black. Despite race, information is given if an individual has Hispanic origin or ancestry. The groups of Blacks and Hispanics are in particular interesting as for Blacks mortality of the 1930s cohort is with about 49% significantly higher than for non-Hispanic Whites (40%) whereas Hispanics have even lower mortality (34%). This is an epidemiological paradox as Hispanics are on average socioeconomically more similar to Blacks than to non-Hispanic Whites (Markides & Coreil, 1986). Thus, in Section 6.3 I will examine if there are differences in the effect of education on health between these groups as well.

6 Results

6.1 First-Stage Results

In Section 3, the relationship between season of birth and educational attainment was introduced by summarising the findings of Angrist and Krueger (1991) using Census data. In this subsection, the strength of this relationship is estimated and implications for the empirical strategy are discussed. The results for the first-stage effect presented below intend to reproduce observations of Angrist and Krueger (1991) but using NHIS data.

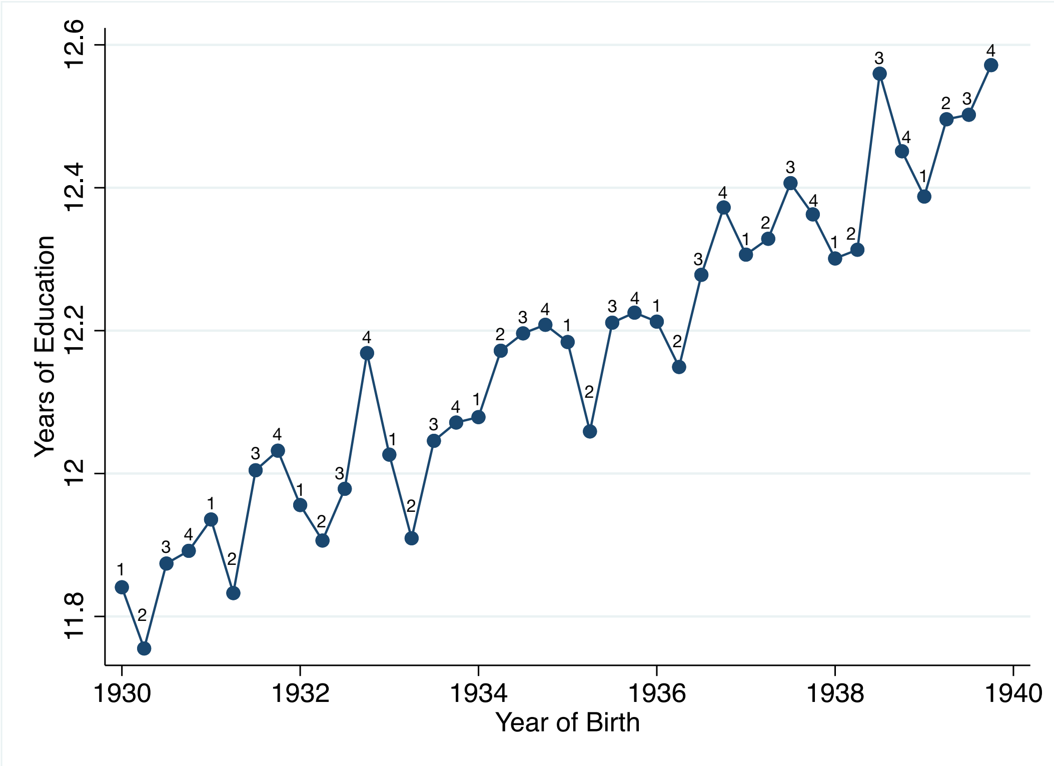

Figure 1: Average years of schooling per quarter of birth of the 1930s cohort. Numbers indicate quarter of birth.

To examine the effect of quarter of birth on education graphically, Figure 1 depicts the mean years of schooling per quarter and year of birth for males and females born in the 1930s. There is a clear trend of increasing average schooling, additionally, the graph reveals a pattern by birth seasons. Those born in the last two quarters of the year have in every year higher average education than those born early in the year. This birth season pattern is most likely not a result of the positive trend as individuals born in the same calendar year are attending the same grade (Angrist & Krueger, 1991, p.980). Furthermore, it is often observable that despite the positive trend individuals born in the fourth quarter have on average more schooling than those born in the first quarter of the next year. Reproducing a pattern of average education by season of birth similar to the one observed by Angrist and Krueger (1991) provides some visual evidence for a first-stage effect of season of birth on educational attainment.

To confirm that quarter of birth has a sufficiently strong effect on education, Table 2 presents the effects of the quarter of birth instrument on different outcomes of educational attainment estimated by OLS using NHIS data from 1986 to 1996 including males and females born in the 1930s and 1940s. Quarter of birth is measured by a set of dummies

Table 2: Effect of quarter of birth (QOB) on different education outcomes for 1930s and 1940s born men and women.

VARIABLES	(1) School	(2) High School	(3) College	(4) Master
QOB 1	-0.120***	-0.0153***	-0.0117***	-0.00581***
	(0.0179)	(0.00232)	(0.00258)	(0.00212)
QOB 2	-0.102***	-0.0172***	-0.00376	-0.00157
	(0.0188)	(0.00241)	(0.00281)	(0.00220)
QOB 3	-0.0302*	-0.00112	-0.00440	-0.00310
	(0.0176)	(0.00220)	(0.00275)	(0.00195)
Observations	245,961	245,961	245,961	245,961
R-squared	0.025	0.018	0.011	0.005
F-statistic:	21.77	28.81	8.15	2.67

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

for the first three quarters such that the fourth quarter functions as the reference category. Although the data are pooled, I do not control with a set of dummies for survey years as the data are collected relatively late in life for the included individuals such that the survey year is unrelated to quarter of birth and completed years of education. To account for the positive trend of education for the observed cohorts, I include a set of dummies for birth years. Column 1 shows the effect of the first three birth quarters of the year on educational attainment in years. One can observe that average schooling is 0.12 years lower in the first and about 0.1 years lower for individuals born in the second birth quarter of the year compared to the fourth quarter. Both estimates are significant at the one percent level. Furthermore, the absolute values of all three quarter of birth coefficients are descending which illustrates the expected relationship predicted by the institutional framework in Section 3 as students born early in the year reach earlier the legal dropout age. In addition to observing that observed effects are in line with predictions of the institutional framework, the hypothesis that the three quarter of birth dummies are jointly zero is rejected with an F-statistic of 21.77 at any common significance level. This suggests that the instruments are not weak although it should be noted that the low R-squared indicates that quarters of birth explain only a very small part of the variation in educational attainment.

Columns 2 to 4 present the effects of birth quarters on completed high school, college and master degrees. The share of individuals graduating from high school is about 1.5

percentage points lower in the first and 1.7 percentage points lower in the second quarter of birth compared to the fourth quarter. Both t-tests and F-test suggest a significant effect of quarter of birth on high school graduation. These findings are in line with the results of Angrist and Krueger (1991, pp.987–989) who furthermore argue that they are consistent with the predictions of the institutional framework as potential dropouts born late in the year could be forced to finish high school by compulsory schooling laws. They also state that season of birth should not be a good predictor of college and master graduation as university graduates are not constrained by compulsory schooling laws. In fact, the magnitude of the coefficients declines as well as F-statistics and R-squared which points towards a much weaker seasonal pattern of university education. Additionally, second birth quarter effects become very small and insignificant for both college and master graduation as an outcome. However, the coefficients of the first quarter dummy remain significant and relatively large with an about 1.2 percentage points lower fraction of college graduates and an about 0.6 percentage points lower fraction of master graduates. These findings stand in contrast to the results of Angrist and Krueger (1991, p.987) who find a 0.5 percentage points lower college graduation rate and 0.1 percentage points lower master graduation rate for the first quarter of birth compared to the fourth quarter. This observation raises concerns that other factors linked to season of birth than compulsory schooling laws determine educational attainment. As a result, I will come back to this issue in Section 7 and discuss implications for the validity of the quarter of birth instrument and possible consequences for the estimates presented in the following subsection.

6.2 Main Results

The analysis above establishes that quarter of birth has a sufficiently strong first-stage effect such that I turn to the main results starting with mortality as outcome followed by self-reported health. As a result of using pooled cross-sectional data, I use in every model specification a set of dummies indicating the survey year. In Table 3, panel A reports OLS and 2SLS estimates of the effect of education on mortality for males and panel B for females. Columns 1 and 2 contain OLS estimates of linear probability models which are in this case preferred to binary choice models to simplify comparisons between OLS and 2SLS estimates. The OLS estimates suggest that for males one more year of schooling is associated with an average decrease in mortality of about 2.1 percentage points. For females, the effect is slightly weaker with around 1.7 percentage points, nevertheless, the OLS estimates are statistically significant for both sexes.

Turning to the 2SLS estimates in columns 3 to 5, one notices that all specifications include age as a control variable. This is necessary in order to justify that quarter of birth is as good as randomly assigned conditioned on covariates. Omitting age would violate this assumption as quarter of birth is related to age because those born earlier in the year are slightly older and age is naturally related to mortality and other health outcomes. Column 1 of Table 10 in Appendix A.3 shows that 2SLS estimates omitting age are significantly higher. The most likely reason is that the reduced form estimates of the 2SLS estimator are upward biased as the quarter of birth coefficients incorporate a part of the effect of age on mortality.

The 2SLS results for males in panel A are significant at the 1%-level and suggest that one additional year of schooling results in a decline of mortality of 4.5 without controls and 5 percentage points when adding covariates as controls which is roughly twice as large as the OLS estimates. The estimates for females in panel B are only slightly smaller in absolute value than the ones for males and also statistically significant at the 1%-level. They indicate that with one additional year of schooling mortality is about 4.5 percentage points lower which is also closer to the OLS estimates. Both in panel A and B, estimates change only slightly when controlling for covariates in column 4 or adding a quadratic age term in column 5. The results are qualitatively similar to those of Lleras-Muney (2005, pp.209–211) using compulsory schooling reforms as instruments. She finds that one additional year of schooling lowers the 10-year death rate by at least 3.6 percentage points. However, these findings stand in contrast to Mazumder (2008, pp.9–10) who obtains smaller estimates which are statistically not significantly different from zero.

When interpreting the results with mortality as an outcome, one should have in mind that because of the use of survey data, there might be a bias driven by survival as people who are deceased cannot be drawn into the sample in later years anymore. If those with lower education die on average earlier and thus are less likely to be drawn into the sample, then the estimates could be biased towards zero, such that the estimates in this section understate the effect of education on mortality (Van Kippersluis et al., 2011, p.717). There is some evidence for this effect in the NHIS data as average mortality for the 1930s cohort declines from 0.43 in 1986 to 0.38 in 1996. However, controlling for survey years should at least partially account for this.

Having found some evidence for an effect of education on mortality, I turn to the more subjective measure of self-reported health. It should be noted that reports of subjective health status differ systematically between sexes such that comparisons of the magnitude of estimates should be interpreted cautiously (Van Doorslaer & Gerdtham, 2003, p.1625). Table 4 contains estimates with the binary outcome variable indicating fair or poor health. Panel A presents the effect of education on fair or poor health for males. Similarly to the mortality results, the OLS estimates are smaller than the 2SLS estimates and imply a decrease of low self-reported health status by about 2.3 percentage points with one more year of education compared to a 5.1 percentage points decrease indicated by 2SLS estimates. The 2SLS estimates for females in panel B are also larger than the OLS estimates and show statistically significant effects too. They suggest that an additional year of education reduces the probability of fair or poor health by about 5.2 percentage points. The estimates are close to the ones obtained by Adams (2002, p.105) who uses also quarter of birth as an instrument but different U.S. data. He finds that one more year of education increases the probability of good or better health by about 3.6 percentage points for males and 5.8 percentage points for females. Note that the coefficients are also quantitatively comparable with a different sign as his dummy indicator for good or better health is the opposite of fair or poor health within a five-step self-reported health scale.

Mazumder (2008, pp.11–12) finds a slightly larger effect for low self-reported health using school reforms for identification. His estimated effect of schooling on cardinal self-reported health is very close to those of this paper. Table 6 shows that for males a one-year increase in education is associated with a 0.16 step improvement in health while the effect for females is 0.23 steps.

At a first glance, it seems surprising that the 2SLS are larger in absolute value than the OLS estimates as the unobserved factors discussed earlier are most likely to result in an overestimation of the effect of education on health. One possible explanation discussed in Section 4 is that IV estimation accounts for measurement error in the endogenous variable. A further reason might be that 2SLS estimates the LATE instead of OLS which tries to estimate an average treatment effect. In the case of this study it is plausible that potential dropouts with low education have larger gains from additional schooling. The consequences of estimating the treatment effect of a compliant sub-population instead of

Table 3: OLS and 2SLS estimates for the effect of education on mortality.

VARIABLES	(1) OLS	(2) OLS	(3) 2SLS	(4) 2SLS	(5) 2SLS
Panel A: Males					
School	-0.0213***	-0.0194***	-0.0448***	-0.0503***	-0.0504***
	(0.000490)	(0.000499)	(0.0157)	(0.0178)	(0.0178)
Age	0.0213***	-0.00322	0.00280	0.00245	0.00865*
	(0.000248)	(0.00316)	(0.00322)	(0.00322)	(0.00522)
Age2		0.000246***			-6.23e-05
		(3.16e-05)			(4.11e-05)
Black		0.0692***		0.0231	0.0229
		(0.00514)		(0.0268)	(0.0268)
MSA		-0.0265***		0.00603	0.00605
		(0.00389)		(0.0191)	(0.0191)
Married		-0.106***		-0.0909***	-0.0909***
		(0.00396)		(0.00905)	(0.00904)
Observations	117,147	116,683	117,133	116,669	116,669
R-squared	0.105	0.116			
Panel B: Females					
School	-0.0166***	-0.0155***	-0.0445***	-0.0476***	-0.0463***
	(0.000497)	(0.000497)	(0.0167)	(0.0173)	(0.0173)
Age	0.0178***	-0.00212	0.0111***	0.0109***	0.0203***
	(0.000219)	(0.00296)	(0.00301)	(0.00302)	(0.00456)
Age2		0.000197***			-9.30e-05**
		(2.97e-05)			(4.06e-05)
Black		0.0460***		0.0211	0.0220
		(0.00417)		(0.0146)	(0.0145)
MSA		-0.0212***		-0.00172	-0.00257
		(0.00373)		(0.0112)	(0.0112)
Married		-0.0689***		-0.0617***	-0.0621***
		(0.00288)		(0.00485)	(0.00483)
Observations	129,620	129,059	129,599	129,038	129,038
R-squared	0.083	0.091			

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 4: OLS and 2SLS estimates for the effect of education on self-reported fair or poor health.

VARIABLES	(1) OLS	(2) OLS	(3) 2SLS	(4) 2SLS	(5) 2SLS
Panel A: Males					
School	-0.0247***	-0.0230***	-0.0542***	-0.0510***	-0.0509***
	(0.000505)	(0.000517)	(0.0133)	(0.0145)	(0.0145)
Age	0.00481***	-0.00719***	0.00295	0.00330	-0.00971**
	(0.000193)	(0.00234)	(0.00256)	(0.00247)	(0.00386)
Age2		0.000121***			0.000131***
		(2.37e-05)			(3.00e-05)
Black		0.0698***		0.0284	0.0286
		(0.00463)		(0.0223)	(0.0223)
MSA		-0.0324***		-0.00275	-0.00274
		(0.00402)		(0.0158)	(0.0158)
Married		-0.0467***		-0.0335***	-0.0335***
		(0.00318)		(0.00781)	(0.00783)
Observations	116,420	115,944	116,379	115,903	115,903
R-squared	0.075	0.084			
Panel B: Females					
School	-0.0292***	-0.0274***	-0.0549***	-0.0522***	-0.0521***
	(0.000483)	(0.000487)	(0.0129)	(0.0132)	(0.0132)
Age	0.00415***	0.00309	0.00304	0.00322	0.00160
	(0.000202)	(0.00241)	(0.00243)	(0.00239)	(0.00396)
Age2		1.07e-05			1.62e-05
		(2.41e-05)			(3.50e-05)
Black		0.122***		0.102***	0.103***
		(0.00466)		(0.0115)	(0.0115)
Metro1		-0.0295***		-0.0142	-0.0143
		(0.00426)		(0.00919)	(0.00916)
Married		-0.0529***		-0.0476***	-0.0476***
		(0.00267)		(0.00382)	(0.00380)
Observations	128,791	128,209	128,732	128,150	128,150
R-squared	0.072	0.091			

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

an average treatment effect on external validity are left for discussion in Section 7. Finally, the 2SLS can be strongly biased also in large samples because the instrument is too weak (Bound et al., 1995). I will come back to the two latter issues in Section 7.

The results above suggest that education has an effect on mortality which contributes to the scientific debate as there is no clear evidence for the U.S. population (Fletcher, 2015; Lleras-Muney, 2005; Mazumder, 2008). In contrast to the discussion of mortality, my findings regarding self-reported health are in line with the evidence in the literature as findings point towards a causal effect of education on self-reported health in the U.S. population (Adams, 2002; Fletcher, 2015; Mazumder, 2008).

6.3 Heterogeneity Analysis

In this subsection, I examine the heterogeneity of effects of education on health in different sub-populations. First, I investigate whether results differ between the 1930s and 1940s born to obtain a better understanding of what cohorts drive the results in Section 6.2. Secondly, I focus on the effect of education on mortality and self-reported health for Black and Hispanic Americans as in Section 5 it was observed that despite having a similar socioeconomic status, there are large differences in mortality between the two groups. This observation evokes the question if there might be differences in the production of health as well.

Table 7 shows estimates with mortality as the outcome for the 1930s cohort. Estimates for both males and females are smaller than those for the whole sample in Table 3 and not statistically significant which is likely the result of the lower estimated effect size and a smaller sample size which increases standard errors and results in a weaker first-stage effect. As a consequence, I cannot reject the hypothesis that education has no effect on mortality for the 1930s cohort. Results for the 1940s cohort are presented in Table 8. The estimates are larger for both males and females than those using the whole sample in Table 3 and suggest a decline in the probability to decease by around 6.8 percentage points. Furthermore, all estimates are statistically significant at the 1%-level. In summary, the cohort-specific results reveal larger health returns to education for those born in the 1940s. The reason for this effect is not clear from the data. One possible explanation for the discrepancy of the results for women born in the 1930s and 1940s could be differences in labour market participation resulting in higher income effects on health and other indirect

Table 5: OLS and 2SLS estimates for the effect of education on mortality for the groups of Blacks and Hispanics.

VARIABLES	(1) OLS	(2) OLS	(3) 2SLS	(4) 2SLS	(5) 2SLS
Panel A: Blacks					
School	-0.0192***	-0.0180***	-0.0391**	-0.0361**	-0.0357**
	(0.000876)	(0.000892)	(0.0173)	(0.0174)	(0.0174)
Age	0.0195***	0.00192	0.0179***	0.0173***	0.0274***
	(0.000458)	(0.00589)	(0.00528)	(0.00526)	(0.00930)
Age2		0.000177***			-0.000102
		(5.84e-05)			(7.72e-05)
MSA		-0.0234***		0.00688	0.00626
		(0.00792)		(0.0297)	(0.0297)
Married		-0.0552***		-0.0464***	-0.0466***
		(0.00527)		(0.00997)	(0.00996)
Observations	31,109	31,109	31,099	31,099	31,099
R-squared	0.084	0.088			
Panel B: Hispanics					
School	-0.00511***	-0.00498***	0.0100	0.00994	0.0103
	(0.000756)	(0.000758)	(0.0113)	(0.0112)	(0.0112)
Age	0.0157***	0.000766	0.0123*	0.0119*	0.0234**
	(0.000587)	(0.00703)	(0.00725)	(0.00721)	(0.0114)
Age2		0.000147**			-0.000115
		(6.91e-05)			(8.96e-05)
MSA		-0.0238**		-0.0337**	-0.0339**
		(0.0121)		(0.0144)	(0.0144)
Married		-0.0153**		-0.0235**	-0.0239**
		(0.00745)		(0.00973)	(0.00973)
Observations	15,895	15,895	15,890	15,890	15,890
R-squared	0.050	0.051			

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

effects of labour such as peer effects influencing health behaviours (Galama et al., 2018, pp.31–32). However, there are likely to be other effects as the effect is observed for males and females.

In Table 5, estimates of the effect of education on mortality are presented for Blacks in panel A and Hispanics in panel B. OLS estimates for Blacks are similar to those of the whole population in Table 3. 2SLS estimates are larger than the OLS estimates and significant at the 5%-level. They suggest a decrease in the probability of decease by 3.6–3.9 percentage points. It should be noted that the sample size is much smaller which makes inference more difficult such that the estimates seem relatively robust and provide evidence for an mortality reducing effect of education for Blacks. For the estimates in panel B, I drop the survey year 1992 from the sample to avoid autocorrelation as the same Hispanic individuals were interviewed again to receive a short panel. The estimates for Hispanics in panel B show that there are large discrepancies in the effect of education on mortality between Blacks in Hispanics. Even the OLS estimates show a much smaller effect suggesting that an additional year reduces mortality by about 0.5 percentage points. 2SLS are larger with a one percentage point increase but not significant at any common level. Estimates for self-reported fair or poor health in Table 9 show a different picture as 2SLS estimates for Blacks are small and insignificant and larger effects for Hispanics which are significant at the 5%-level are observed. Although being significant, point estimates for Hispanics are smaller than those for the whole population in Table 4. A possible explanation for the above findings could be that Hispanics have on average low baseline mortality such that the effects of education on mortality are small but there are subjective health gains. However, there is more research needed to investigate differences in channels of how education affects health between Blacks, Hispanics and other groups.

6.4 Sensitivity of Results

In this section, I assess the robustness of the estimates presented in Section 6.2 by testing alternative specifications. To test the influence of the choice of the instrument I use instead of quarter of birth month of birth as an instrument. First-stage effects of month of birth are summarised in Table 14 in Appendix A.4. One can observe that the F-statistic, testing if the effects on years of schooling of the eleven month dummies are jointly zero, is with 7.47 smaller than in the case of using quarter of birth. Nevertheless, the effect is sufficiently strong to be informative about the robustness of the results.

Table 11 shows results of the effect of education on mortality using month of birth as an instrument for educational attainment. Column 1 uses only the eleven month dummies and Columns 2–4 use month dummies interacted with year of birth dummies. When using only the eleven month of birth dummies as an instrument, the estimated effects both in panel A and panel B are larger than those in Table 3. However, standard errors are much larger such that 95% confidence intervals do not exclude the point estimates in Table 3. Columns 2–4 use also interaction terms between birth month and year of birth similar to the specifications presented in Section 6.2. Both in panel A and B, slightly smaller effects are estimated compared to those in Table 3 suggesting an about 3.5 percentage points decline in mortality for an additional year of schooling for males and 3.3 percentage points for females. In total, the estimated effects are relatively robust to using variation of educational attainment by birth month as an alternative instrument to quarter of birth variation.

The aforementioned Table 10 shows the consequences of adding single covariates to the 2SLS equations. As mentioned before, the estimate in column 1 implies that one should control for age to assure conditional independence of the instruments. Columns 2–4 show the effect of adding controls for being Black, living in a MSA and being married. As the estimates change only slightly, the instrument is likely to be independent of these covariates. Moreover, it rules out concerns that the variables marital status and living in a metropolitan area are bad controls and affect estimates as they are measured after individuals complete their education. Interestingly, adding covariates does not increase the precision of estimates as standard errors remain almost unchanged, see e.g. Angrist and Pischke (2009, p.176). This observation suggests that the discussed covariates do not reduce variation in the outcome variable although they had all significant effects on mortality in a multivariate OLS regression (see Table 3).

Finally, I assess whether the imputation of birth years by age and date of the interview has important consequences on the results. Therefore, I drop those observations which were interviewed in the month of their birthday as here imputation is only possible with some error reducing the sample size by 20,944 observations. Assuming that the interview date is assigned in a way such that it is random if birth month and month of the interview coincide, observations are dropped randomly and should not result in bias. Table 12 and Table 13 reproduce the results of education on mortality divided by sex and cohort discussed in Section 6.3 to be able to observe changes of effects precisely. Results show that the effects are qualitatively the same and magnitudes of coefficients differ only little in most cases. Only for males born in the 1930s effects are slightly higher and turn significant at the 10% or 5%-level depending on the specification which stands in contrast to the results presented in Table 8. In total, the results do not indicate that imputation of birth year results in a bias of the results.

7 Discussion

Having found that the estimates presented in Section 6 are relatively robust to different specifications, I turn to the discussion of the validity of the empirical strategy. This discussion is beside the empirical results in Section 6 a main focus of this paper as it analyses whether these 2SLS estimates have a causal interpretation which is crucial from the policy perspective. The first part of this section concentrates on internal validity by discussing if the instruments are too weak, examining potential violations of the exclusion restriction and discussing implications for the results in Section 6. In the second part, the external validity of IV estimates is discussed.

7.1 Internal Validity

It has been known for a long time that the 2SLS estimator produces consistent estimates but biased estimates in finite samples assuming that the exclusion restriction is satisfied (Angrist & Pischke, 2009, pp.205–208). Bound et al. (1995, pp.445–446) show that 2SLS estimates which suffer from weak instruments can be seriously biased even in large samples and connect their argument with a critique of the quarter of birth instrument. They argue that the inverse of the F-statistic of the excluded instruments provides a good approximation of the finite sample bias of the 2SLS estimator relative to the OLS estimator. Furthermore, adding interaction terms of year of birth and quarter of birth dummies potentially amplifies the bias as the number of instruments increases strongly, however, not the predictive power (Bound et al., 1995, p.449). Bound et al. (1995, p.448) express their concerns that some estimates of Angrist and Krueger (1991) with low F-statistics could exhibit quantitatively relevant bias. In the context of this paper, there are specifications, especially in Section 6.3 where the number of observations is low such that the instruments become less powerful. In the main specifications, however, F-statistics are usually large and thus imply only small bias provided the exclusion restriction holds.

Consequently, the exclusion restriction is the key assumption that has to be satisfied to obtain valid causal estimates. However, the concerns outlined in Section 3 that compulsory schooling laws are not the only channel how season of birth is related to education and health outcomes are not addressed yet. Buckles and Hungerman (2013) provide convincing evidence that season of birth is related to parental socioeconomic status. In particular, they find that mothers of children born in winter are on average less likely to have graduated from high school, to be White, to be married and are more likely to be a teenager at birth (Buckles & Hungerman, 2013, pp.712–713). This implies a correlation of the instrument with the error term as parental characteristics are unobserved but related to parental investment in education and health, see e.g. (Cutler & Lleras-Muney, 2006, p.11). These findings are consistent with the observations in Table 2 that the coefficients of the first birth quarter on college and master degree completion remain relatively large and significant. To further investigate which months are the drivers for this finding, I take advantage of the fact that the data also include month of birth. Table 14 in Appendix A.4 reproduces Table 2 but with a set of dummies for the first eleven months of the year. The results reveal that being born in January and February has a similar effect on college graduation as on high school completion which is counter-intuitive to the explanation by compulsory schooling laws as college students are not affected by them. For the months of May and June, coefficients for the month of birth effect are much smaller in absolute value for college than for high school graduation which is consistent with the prediction by the compulsory schooling mechanism proposed by Angrist and Krueger (1991). The findings suggest that there might be another factor besides compulsory schooling laws causing education to be lower on average for those born in winter.

Another possibility to test violations of the exclusion restriction, which are linked to the findings of Buckles and Hungerman (2013), is directly looking at the balancing of parents characteristics over the birth season of their children. This approach is somehow limited as parents' characteristics are only linked in the data if they belong to the same household which leads to very few observations for the cohorts regarded in this paper. Additionally, it is arguable if adults belonging to the same household as their parents represent well the whole population. Despite these issues, Table 15 in Appendix A.4 presents season of birth effects on parental education in Columns 1 and 2. Maternal education is for individuals born in the first quarter lower compared to the fourth, however, it is even lower in the third quarter. Consistent with the findings of Buckles and Hungerman (2013) is that the lowest paternal education is observed for individuals born in the first quarter but the effect is not statistically significant. In Column 3, quarter of birth dummies are regressed on a dummy indicating whether an individual is Black/African-American as this should be a good indicator for at least one parent being Black. The findings of Buckles and Hungerman (2013) that children born in winter are more likely to have a non-White mother cannot be confirmed in that way. The distribution of parents characteristics over their child's birth season does not provide convincing evidence for the findings of Buckles and Hungerman (2013), however, the approach in this paper is very limited because of the small and selective sample.

In summary, it still seems plausible that the empirically observed season of birth effect on education is a combination of the interaction of season of birth with compulsory schooling laws and seasonal variations in family background. As a result, I will discuss the possible inconsistency of the 2SLS estimates in Section 6 below.

In Appendix B, I present a proof that makes clear that the 2SLS estimator is inconsistent if the instruments are related to the error term of the structural equation. Furthermore, it provides some intuition for the direction of the bias. I now suppose that parental socioeconomic status does follow a seasonal pattern like the one observed by Buckles and Hungerman (2013) such that quarter of birth is correlated with the unobserved parental characteristics in the error term which are likely to affect individual health. As the seasonal pattern of education caused by parental characteristics and by compulsory schooling laws predicts lower education for those born in winter the correlation of the correlation with the error term is likely to be positive. From the probability limit of the 2SLS estimator in Appendix B follows that the effect of education on health will be overestimated.

This result suggests that one should be careful giving estimates using quarter of birth a causal interpretation as they might be upward biased and inconsistent. I suggest that future work using quarter of birth as an instrument should take possible violations of the key identifying assumption stronger in the focus. For instance, Becchetti et al. (2018) interpret their estimates of the effect of education on health as causal although the patterns

Buckles and Hungerman (2013) find are likely to be present also in Europe and possibly affect their estimates like those in this paper.

7.2 External Validity

Considering the discussion above about possible violations of the exclusion restriction, one should be very careful of giving the 2SLS a causal interpretation. Despite these concerns, I turn to the discussion of the external validity of this study as 2SLS does not intend to estimate an average treatment effect but a LATE and the policy relevance of this parameter is controversial. Heckman and Urzua (2010, p.35) argue for instance that even if the LATE is estimated consistently by 2SLS, it can be misleading for policy guidance as it might be different from the average treatment effect which is usually the parameter of interest. Imbens (2010, p.414) objects that although instrumental variable regression fails to identify an average treatment effect, LATE still can be an informative parameter depending on the context. Also, the LATE estimated in this paper can be informative from the policy perspective as the sub-population affected by the instrument are potential dropouts who are located at the lower end of the education distribution. If policymakers intend to reduce health inequality by increasing health on the lower end of the health distribution, estimates of the LATE in this paper could provide guidance, assuming internal validity would be high. Furthermore, combining estimated LATEs of several studies investigating different sub populations can build evidence about policy-relevant parameters (Imbens, 2010, p.404) such that the empirical strategy in this paper is reasonable and relevant from a policy perspective.

8 Conclusion

In this paper, I estimated the effect of educational attainment on mortality and self-reported health using quarter of birth as an instrument. I found significant and robust effects of education on mortality and self-reported health which seem to be driven by large effects for individuals born in the 1940s. Additionally, I observe that education does not seem to reduce mortality for Hispanics but for Blacks, however, more education seems to lead to self-reported health gains for Hispanics but not for Blacks.

I discussed evidence by Buckles and Hungerman (2013) that season of birth is related to parental socioeconomic background. There are some indications for these findings in the data such that there are serious concerns about the validity of the key identification assumption of this approach. I conclude that one should be careful to give the estimates in this paper a causal interpretation as there are concerns that they overstate the effect of education on health.

The discussion of the validity of the instrument has emphasised that there is a need to find different sources of exogenous variation in education to identify the effects of education on health. Furthermore, the instruments which are often used for identification, like the one used in this paper and compulsory schooling reforms, estimate effects at the lower end of the education distribution, however, variation in higher education should be taken more into the focus as here different effects might occur which are also important from a policy perspective. A further limitation of the results of this study is that it only provides evidence for the effects of education on mortality and self-reported health. For policy guidance, however, it is crucial to understand not only if but also through what channels education improves health.

Although my instrumental variable estimates are suspected to not estimate the causal effect of education on health for potential dropouts consistently, this paper sheds some light on the limitation of the approach using quarter of birth as an instrument for education.

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A Tables

A.1 Results for Cardinal Self-Reported Health

Table 6: OLS and 2SLS estimates for the effect of education on cardinal self-reported health.

VARIABLES	(1) OLS	(2) OLS	(3) 2SLS	(4) 2SLS	(5) 2SLS
Panel A: Males					
School	-0.105***	-0.0983***	-0.174***	-0.155***	-0.155***
	(0.00151)	(0.00154)	(0.0420)	(0.0454)	(0.0454)
Age	0.0196***	-0.0103	0.00605	0.00724	-0.0111
	(0.000627)	(0.00780)	(0.00791)	(0.00766)	(0.0117)
Age2		0.000302***			0.000184*
		(7.78e-05)			(9.42e-05)
Black		0.289***		0.205***	0.206***
		(0.0146)		(0.0692)	(0.0692)
MSA		-0.132***		-0.0718	-0.0720
		(0.0145)		(0.0504)	(0.0504)
Married		-0.170***		-0.143***	-0.143***
		(0.00967)		(0.0240)	(0.0240)
Observations	116,420	115,944	116,379	115,903	115,903
R-squared	0.119	0.131			
Panel B: Females					
School	-0.114***	-0.107***	-0.226***	-0.228***	-0.227***
	(0.00136)	(0.00135)	(0.0434)	(0.0437)	(0.0437)
Age	0.0172***	0.0308***	0.0105	0.0101	0.0393***
	(0.000597)	(0.00708)	(0.00717)	(0.00697)	(0.0124)
Age2		-0.000133*			-0.000291**
		(7.02e-05)			(0.000114)
Black		0.465***		0.372***	0.372***
		(0.0135)		(0.0371)	(0.0372)
MSA		-0.136***		-0.0614**	-0.0622**
		(0.0146)		(0.0304)	(0.0304)
Married		-0.112***		-0.0855***	-0.0861***
		(0.00836)		(0.0128)	(0.0127)
Observations	128,791	128,209	128,732	128,150	128,150
R-squared	0.111	0.133			

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

A.2 Heterogeneity Analysis

Table 7: OLS and 2SLS estimates for the effect of education on mortality in the 1930s cohort.

VARIABLES	(1) OLS	(2) OLS	(3) 2SLS	(4) 2SLS	(5) 2SLS
Panel A: Males					
School	-0.0219***	-0.0199***	-0.0264	-0.0311	-0.0289
	(0.000699)	(0.000695)	(0.0227)	(0.0266)	(0.0265)
Age	0.0292***	0.0373**	0.00702	0.00688	0.0340*
	(0.000819)	(0.0162)	(0.00530)	(0.00531)	(0.0180)
Age2		-6.94e-05			-0.000241
		(0.000145)			(0.000153)
Black		0.0572***		0.0372	0.0413
		(0.00852)		(0.0480)	(0.0478)
MSA		-0.0319***		-0.0191	-0.0218
		(0.00560)		(0.0311)	(0.0311)
Married		-0.115***		-0.109***	-0.110***
		(0.00697)		(0.0155)	(0.0155)
Observations	48,036	47,847	48,031	47,842	47,842
R-squared	0.056	0.066			
Panel B: Females					
School	-0.0184***	-0.0168***	-0.0234	-0.0279	-0.0281
	(0.000834)	(0.000849)	(0.0293)	(0.0279)	(0.0279)
Age	0.0249***	0.0266*	0.0189***	0.0187***	0.0327**
	(0.000780)	(0.0143)	(0.00521)	(0.00511)	(0.0157)
Age2		-1.77e-05			-0.000125
		(0.000129)			(0.000137)
Black		0.0419***		0.0323	0.0321
		(0.00680)		(0.0256)	(0.0256)
MSA		-0.0252***		-0.0188	-0.0187
		(0.00582)		(0.0174)	(0.0174)
Married		-0.0872***		-0.0832***	-0.0832***
		(0.00493)		(0.0112)	(0.0112)
Observations	53,795	53,567	53,790	53,562	53,562
R-squared	0.040	0.048			

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 8: OLS and 2SLS estimates for the effect of education on mortality in the 1940s cohort.

VARIABLES	(1) OLS	(2) OLS	(3) 2SLS	(4) 2SLS	(5) 2SLS
Panel A: Males					
School	-0.0209***	-0.0190***	-0.0650***	-0.0684***	-0.0689***
	(0.000584)	(0.000593)	(0.0234)	(0.0259)	(0.0259)
Age	0.0148***	0.0115	-0.000247	-0.000646	0.00605
	(0.000673)	(0.00918)	(0.00396)	(0.00402)	(0.0105)
Age2		4.03e-05			-7.36e-05
		(0.000101)			(0.000110)
Black		0.0771***		0.0137	0.0131
		(0.00718)		(0.0342)	(0.0342)
MSA		-0.0229***		0.0260	0.0265
		(0.00482)		(0.0265)	(0.0265)
Married		-0.0997***		-0.0781***	-0.0780***
		(0.00461)		(0.0125)	(0.0125)
Observations	69,111	68,836	69,102	68,827	68,827
R-squared	0.038	0.051			
Panel B: Females					
School	-0.0153***	-0.0145***	-0.0633***	-0.0679***	-0.0684***
	(0.000515)	(0.000503)	(0.0207)	(0.0220)	(0.0220)
Age	0.0122***	0.0152*	0.00509	0.00482	0.0188**
	(0.000537)	(0.00783)	(0.00373)	(0.00374)	(0.00937)
Age2		-3.19e-05			-0.000154
		(8.60e-05)			(9.98e-05)
Black		0.0496***		0.0109	0.0106
		(0.00475)		(0.0167)	(0.0168)
MSA		-0.0186***		0.0152	0.0155
		(0.00391)		(0.0144)	(0.0144)
Married		-0.0548***		-0.0485***	-0.0485***
		(0.00341)		(0.00484)	(0.00485)
Observations	75,825	75,492	75,809	75,476	75,476
R-squared	0.027	0.034			

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 9: OLS and 2SLS estimates for the effect of education on self-reported fair or poor health for the groups of Blacks and Hispanics.

VARIABLES	(1) OLS	(2) OLS	(3) 2SLS	(4) 2SLS	(5) 2SLS
Panel A: Blacks					
School	-0.0340***	-0.0317***	-0.0116	-0.00486	-0.00498
	(0.000802)	(0.000814)	(0.0158)	(0.0162)	(0.0162)
Age	0.00697***	-0.000726	0.0122**	0.0111**	0.000260
	(0.000419)	(0.00536)	(0.00485)	(0.00485)	(0.00857)
Age2		7.97e-05			0.000109
		(5.32e-05)			(7.11e-05)
MSA		-0.0693***		-0.113***	-0.113***
		(0.00725)		(0.0275)	(0.0275)
Married		-0.0757***		-0.0887***	-0.0886***
		(0.00480)		(0.00931)	(0.00931)
Observations	31,445	31,445	31,422	31,422	31,422
R-squared	0.077	0.086			
Panel B: Hispanics					
School	-0.0185***	-0.0182***	-0.0264**	-0.0268**	-0.0268**
	(0.000662)	(0.000662)	(0.0109)	(0.0108)	(0.0107)
Age	0.00705***	0.0135**	0.0123**	0.0120*	0.0102
	(0.000514)	(0.00644)	(0.00624)	(0.00620)	(0.0103)
Age2		-6.46e-05			1.75e-05
		(6.32e-05)			(8.35e-05)
MSA		-0.0126		-0.00708	-0.00706
		(0.0108)		(0.0131)	(0.0131)
Married		-0.0485***		-0.0443***	-0.0443***
		(0.00652)		(0.00841)	(0.00840)
Observations	18,773	18,773	18,762	18,762	18,762
R-squared	0.064	0.066			

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

A.3 Sensitivity of Results

Table 10: 2SLS estimates testing how single controls affect the coefficient of education.

VARIABLES	(1) 2SLS	(2) 2SLS	(3) 2SLS	(4) 2SLS	(5) 2SLS
School	-0.0827***	-0.0603***	-0.0599***	-0.0620***	-0.0591***
	(0.0144)	(0.0151)	(0.0152)	(0.0155)	(0.0151)
Age		0.00596**	0.00600**	0.00594**	0.00610**
		(0.00255)	(0.00252)	(0.00253)	(0.00255)
Black			0.0213		
			(0.0171)		
MSA				0.0156	
				(0.0120)	
Married					-0.0538***
					(0.00728)
Observations	246,732	246,732	245,707	246,732	246,732

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 11: 2SLS estimates with month of birth as an alternative instrument for measuring the effect of educational attainment on mortality.

VARIABLES	(1) 2SLS	(2) 2SLS	(3) 2SLS	(4) 2SLS
Panel A: Males				
School	-0.0841***	-0.0394***	-0.0347***	-0.0346***
	(0.0320)	(0.00857)	(0.00913)	(0.00913)
Age	8.01e-05	0.00317	0.00339	0.00968*
	(0.00447)	(0.00313)	(0.00307)	(0.00506)
Age2				-6.32e-05
				(4.08e-05)
Black			0.0464***	0.0464***
			(0.0146)	(0.0146)
MSA			-0.0107	-0.0107
			(0.0101)	(0.0101)
Married			-0.0983***	-0.0983***
			(0.00580)	(0.00580)
Observations	117,133	117,133	116,669	116,669
Panel B: Females				
School	-0.102***	-0.0334***	-0.0329***	-0.0324***
	(0.0303)	(0.00812)	(0.00814)	(0.00816)
Age	0.00660	0.0119***	0.0119***	0.0204***
	(0.00444)	(0.00286)	(0.00287)	(0.00448)
Age2				-8.49e-05**
				(4.02e-05)
Black			0.0326***	0.0329***
			(0.00785)	(0.00787)
MSA			-0.0107*	-0.0111*
			(0.00590)	(0.00590)
Married			-0.0649***	-0.0651***
			(0.00345)	(0.00345)
Observations	129,599	129,599	129,038	129,038

Standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

Table 12: OLS and 2SLS estimates for the effect of education on mortality in the 1930s cohort with droppped observations.

VARIABLES	(1) OLS	(2) OLS	(3) 2SLS	(4) 2SLS	(5) 2SLS
Panel A: Males					
School	-0.0219***	-0.0199***	-0.0336*	-0.0458**	-0.0429*
	(0.000699)	(0.000695)	(0.0203)	(0.0229)	(0.0228)
Age	0.0292***	0.0373**	0.0280***	0.0273***	0.0498***
	(0.000819)	(0.0162)	(0.00239)	(0.00248)	(0.0175)
Age2		-6.94e-05			-0.000197
		(0.000145)			(0.000153)
Black		0.0572***		0.0107	0.0159
		(0.00852)		(0.0415)	(0.0413)
MSA		-0.0319***		-0.00172	-0.00512
		(0.00560)		(0.0268)	(0.0267)
Married		-0.115***		-0.101***	-0.103***
		(0.00697)		(0.0140)	(0.0140)
Observations	48,036	47,847	48,031	47,842	47,842
R-squared	0.056	0.066			
Panel B: Females					
School	-0.0184***	-0.0168***	-0.0157	-0.0240	-0.0185
	(0.000834)	(0.000849)	(0.0251)	(0.0250)	(0.0246)
Age	0.0249***	0.0266*	0.0247***	0.0236***	0.0353**
	(0.000780)	(0.0143)	(0.00243)	(0.00242)	(0.0152)
Age2		-1.77e-05			-9.83e-05
		(0.000129)			(0.000135)
Black		0.0419***		0.0356	0.0404*
		(0.00680)		(0.0226)	(0.0222)
MSA		-0.0252***		-0.0210	-0.0241
		(0.00582)		(0.0157)	(0.0155)
Married		-0.0872***		-0.0846***	-0.0866***
		(0.00493)		(0.0104)	(0.0102)
Observations	53,795	53,567	53,790	53,562	53,562
R-squared	0.040	0.048			

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 13: OLS and 2SLS estimates for the effect of education on mortality in the 1940s cohort with droppped observations.

VARIABLES	(1) OLS	(2) OLS	(3) 2SLS	(4) 2SLS	(5) 2SLS
Panel A: Males					
School	-0.0209***	-0.0190***	-0.0666***	-0.0737***	-0.0741***
	(0.000584)	(0.000593)	(0.0192)	(0.0208)	(0.0209)
Age	0.0148***	0.0115	0.00869***	0.00791***	0.0125
	(0.000673)	(0.00918)	(0.00244)	(0.00260)	(0.0103)
Age2		4.03e-05			-5.03e-05
		(0.000101)			(0.000111)
Black		0.0771***		0.00702	0.00651
		(0.00718)		(0.0285)	(0.0286)
MSA		-0.0229***		0.0314	0.0317
		(0.00482)		(0.0216)	(0.0216)
Married		-0.0997***		-0.0759***	-0.0757***
		(0.00461)		(0.0104)	(0.0105)
Observations	69,111	68,836	69,102	68,827	68,827
R-squared	0.038	0.051			
Panel B: Females					
School	-0.0153***	-0.0145***	-0.0569***	-0.0600***	-0.0614***
	(0.000515)	(0.000503)	(0.0178)	(0.0187)	(0.0188)
Age	0.0122***	0.0152*	0.00776***	0.00750***	0.0201**
	(0.000537)	(0.00783)	(0.00199)	(0.00199)	(0.00888)
Age2		-3.19e-05			-0.000137
		(8.60e-05)			(9.70e-05)
Black		0.0496***		0.0165	0.0155
		(0.00475)		(0.0144)	(0.0145)
MSA		-0.0186***		0.0103	0.0112
		(0.00391)		(0.0124)	(0.0125)
Married		-0.0548***		-0.0495***	-0.0494***
		(0.00341)		(0.00452)	(0.00455)
Observations	75,825	75,492	75,809	75,476	75,476
R-squared	0.027	0.034			

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

A.4 Exclusion Restriction

Table 14: Effects of birth month on different education outcomes for 1930s and 1940s born men and women

VARIABLES	(1) School	(2) High School	(3) College	(4) Master
jan	-0.0519	-0.00560	-0.00976**	-0.00353
	(0.0316)	(0.00421)	(0.00443)	(0.00376)
feb	-0.0964***	-0.0137***	-0.0121***	-0.00638*
	(0.0338)	(0.00415)	(0.00456)	(0.00372)
mar	-0.109***	-0.0183***	-0.00842*	-0.00519
	(0.0329)	(0.00437)	(0.00431)	(0.00360)
apr	-0.0988***	-0.0146***	-0.00711	-0.00204
	(0.0320)	(0.00429)	(0.00487)	(0.00375)
may	-0.0314	-0.0136***	0.00361	0.00425
	(0.0338)	(0.00424)	(0.00506)	(0.00399)
jun	-0.0605*	-0.0138***	-0.000683	-0.00286
	(0.0334)	(0.00442)	(0.00449)	(0.00351)
jul	0.0104	4.30e-05	-0.00372	-0.00100
	(0.0332)	(0.00414)	(0.00473)	(0.00399)
aug	-0.0559*	-0.00467	-0.00368	-0.00180
	(0.0331)	(0.00407)	(0.00457)	(0.00380)
sep	0.0399	0.00460	-0.000351	-0.00291
	(0.0305)	(0.00404)	(0.00474)	(0.00361)
oct	0.0645**	0.00354	0.00612	0.00400
	(0.0316)	(0.00416)	(0.00479)	(0.00390)
nov	0.0556	0.00613	0.00247	0.00114
	(0.0346)	(0.00456)	(0.00489)	(0.00378)
age	0.00737	0.000288	0.00181***	0.00140***
	(0.00492)	(0.000542)	(0.000587)	(0.000402)
Observations	246,767	246,767	246,767	246,767
R-squared	0.025	0.018	0.012	0.005
F test:	7.47	8.21	3.17	1.85

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 15: Regressions of quarter of birth on parental education and being Black.

VARIABLES	(1) Mother's Education	(2) Father's Education	(3) Black
qob1	-0.226*	-0.136	0.00119
	(0.121)	(0.227)	(0.00195)
qob2	-0.177	0.0677	0.00106
	(0.122)	(0.223)	(0.00196)
qob3	-0.244**	0.148	0.00327*
	(0.115)	(0.212)	(0.00187)
Observations	7,511	2,749	247,197
R-squared	0.001	0.001	0.000

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

B Proof

The proof follows Wooldridge (2010, pp.92–94) using the simple notation of Angrist and Pischke (2009, p.206) with variable definitions of this paper to make it clearer. The causal model of interest (structural equation) with the causal effect of interest ρ, with endogenous regressor vector s for schooling, h vector of health outcome and ε an error term, is:

$$h = s\rho + \varepsilon \tag{4}$$

The first stage, with Z as the NxQ instrument matrix, is:

$$s = Z\pi + \nu \tag{5}$$

It follows that the 2SLS estimator, with $P_Z = Z(Z'Z)^{-1}Z'$, is (Angrist & Pischke, 2009, p.206):

$$\begin{aligned} \hat{\rho}_{2SLS} &= (s'P_Z s)^{-1} s'P_Z h && (6)\\ &= (s'P_Z s)^{-1} s'P_Z(s\rho + \varepsilon) && (7)\\ &= \rho + (s'P_Z s)^{-1} s'P_Z \varepsilon && (8) \end{aligned}$$

For consistency, I investigate the probability limit of $\hat{\rho}_{2SLS}$:

$$plim_{N\to\infty}\hat{\rho}_{2SLS} = \rho + plim_{N\to\infty}(s'P_Z s)^{-1}s'P_Z\varepsilon \tag{9}$$

Using Slutsky's Theorem, it follows:

$$\rho + plim_{N\to\infty}(s'P_Z s)^{-1} \cdot plim_{N\to\infty}s'P_Z\varepsilon \tag{10}$$

$$= \rho + plim_{N\to\infty}(\frac{1}{N}s'Z\frac{1}{N}(Z'Z)^{-1}\frac{1}{N}Z's)^{-1} \cdot plim_{N\to\infty}\frac{1}{N}s'Z\frac{1}{N}(Z'Z)^{-1}\frac{1}{N}Z'\varepsilon \tag{11}$$

Applying the law of large number and assuming general rank condition implies that the following terms converge in probability to constant matrices: $\frac{1}{N}s'Z \to \Sigma_{sZ}$, $\frac{1}{N}(Z'Z)^{-1} \to \Sigma_{ZZ}^{-1}$ and $\frac{1}{N}Z's) \to \Sigma_{Zs}$.

I focus only on the probability limit of $\frac{1}{N}Z'\varepsilon$ as this is relevant for the discussion in Section 7. To show convergence in probability to zero, the exclusion restriction has to hold, so now assume $E(Z'\varepsilon) = 0$. Such that:

$$plim_{N\to\infty}\frac{1}{N}E(Z'\varepsilon) = 0 \tag{12}$$

It follows:

$$plim_{N\to\infty}\hat{\rho}_{2SLS} = \rho + \Sigma_{sZ}\Sigma_{ZZ}^{-1}\Sigma_{Zs} \cdot \Sigma_{sZ}\Sigma_{ZZ}^{-1} \cdot \mathbf{0} = \rho \tag{13}$$

This result implies that the coefficient will be overestimated if $E(Z'\varepsilon) > 0$.