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Abstract

There is a well-documented large positive correlation between education and health and yet it remains unclear as to whether this is a causal relationship. Potential reasons for this lack of clarity include estimation using different methods, analysis of different populations and school reforms that are different in design. In this paper we assess whether the type of school reform, the instrument and therefore subgroup identified and the modelling strategy impact the estimated health returns to education. To this end we use both Regression Discontinuity and Difference in Differences applied to two Swedish school reforms that are different in design but were implemented across overlapping cohorts born between 1938 and 1954 and follow them up until 2013. We find small and insignificant impacts on overall mortality and its common causes and the results are robust to regression method, identification strategy and type of school reform. Extending the analysis to hospitalisations or self-reported health and health behaviours, we find no clear evidence of health improvements due to increased education. Based on the results we find no support for a positive causal effect of education on health.

Keywords: Health returns to education, demand for medical care

JEL Classification: *I12, I18, I26*

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1 Introduction

The existence of an education gradient in health has been documented across various countries and for a variety of health measures including mortality, disability and various measures of morbidity (see for example, Mackenbach et al. 2003, 2008; Marmot et al. 2012; O'Donnell et al. 2013). A range of theories has been posited to explain the existence of this gradient including the suggestion that education has a causal effect through its impact on health production, or through impacting differences in financial resources, preferences or self-empowerment, understanding of information or better access to information (see for e.g. Cutler and Lleras-Muney 2006; Grossman 2006; Mackenbach 2012 for overviews). To test these theories we need credible instruments. A substantive part of the more recent empirical literature has used differences in compulsory schooling as a source of exogenous variation for years of education. Two recent reviews of the literature on the impact of education on health (Cutler and Lleras-Muney, 2012; Grossman, 2015) have found the literature hard to summarise.

In this paper we address this lack of clarity in the literature looking at the causal effect of education on health. We do this by pinning down many of the potential reasons that have been given to explain the differences in the findings in the literature. Suggestions for differences in results across studies include estimation on different populations and for different periods (Cutler and Lleras-Muney, 2012) or that the instruments used affect different groups (Grossman, 2015; Clark and Royer, 2013). Clark and Royer (2013) arguably provide the most convincing evidence so far from Britain's compulsory schooling law changes using month of birth and Regression Discontinuity (RD) design and find small or zero impacts of education on health. However, the reforms were implemented overnight, nationwide. The reforms were also enacted during very different periods (1947 and 1972). It may be that overnight implementation of the reforms reduced the wage returns to schooling because of the sudden change in supply of more educated workers, and this could impact a key potential channel for education to influence health. There is also cause for concern that the cohorts of 1947 and 1972 are not comparable. For example, Gathmann et al. (2015) provide indicative evidence that results based on pre WWII cohorts show larger impacts on mortality than results based on post WWII cohorts and this may be due to different base level mortality rates.

In this paper we consider the long-term impact of education on mortality and wider measures of health. We want to know whether the type of school reform has an impact on the results, holding other variables constant. To this end we use two school reforms in Sweden that increased years of schooling for cohorts born very close together in time but different in nature. The first reform increased minimum years of schooling from 7 years to 8 years but only for those who were not eligible or unwilling to take the academic track. This was rolled out to about half of Sweden's municipalities. The second reform increased minimum years of schooling from 7 or 8 years (depending on municipality) to 9 years and introduced a new national comprehensive school system involving a change in peer groups and the introduction of a new national curriculum. The 9 year reform was rolled out nationally but phased in across municipalities and has been found to have had a sizeable impact on years of schooling (Meghir and Palme, 2005; Holmlund, 2007; Lundborg et al., 2014; Hjalmarsson et al., 2015). In fact by utilising a methodological improvement in the measurement of years of schooling we show that the 9 year reform had impacts on years of schooling twice that of previous estimates. This paper is the first paper to use the 8 year reform. It was rolled out extensively across Sweden and we show that it had a sizeable impact on years of education.

The particular set-up assessed in this paper is rather unique because the reforms are overlapping in the sense that they occur on average 7 years apart within municipalities. This means the reforms impacted individuals from similar backgrounds who entered similar labour market structures under a similar health system. That is, under similar labour market structures any returns to education should be similar for similarly aged cohorts. For each reform, the nature of their phased roll out means we can compare two groups born in the same year but who received different amounts of compulsory schooling based on where they were born. An advantage of this paper is therefore that we are able to pin down the impacts of the reforms separate of cohort effects. Also, because the reforms were rolled out over time, the impacts of the reforms are less likely to suffer the same level of general equilibrium effects that may be a concern if the reforms were rolled out nationwide overnight.

To help further isolate potential variables that explain differences across studies we employ both difference-in-differences (DiD) and RD design to identify the causal impact of the reforms on health. RD uses the cut-off for the school year, the 1st of January, combined with reform year for each municipality. Using birth date in years and months, those too old and therefore born before the reform year cut-off are not assigned reform status and those born on or after are. There are as many cut-offs as there are years of reform implementation and we average over these to estimate the overall impact of the reform. Our DiD strategy compares across cohorts in municipalities that didn't implement the reform to those that did. To assess if modelling approach matters, we perform analysis of mortality using both linear regression and Cox proportional hazard regression for lifetime duration analysis.

Our data is based on the universe of Swedes born between 1932 and 1959. We link our various administrative records together using each individual's unique personal identification number enabling us to assess the mortality and health outcomes of about 1.2 million individuals. We consider the reform status of individuals born between 1938 and 1954. Mortality data is provided by the Swedish Cause of Death Database and our observation period follows our individuals up to the end of 2013 which means our oldest cohort born in 1938 and subject to reform is 75 years old when our period of observation ends. We also consider leading causes of death. We complement this analysis using Swedish hospital administrative data and a large survey (The Swedish Health and Living Standards Survey) considering self-reported health and health related behaviours. The survey data allows us to consider both contemporaneous health and health related behaviours and therefore gives us the potential to pick up effects using a more sensitive measure of health and also explain the pathways of any effects we find. Compared to previous Swedish studies which only consider the 9 year reform using DiD (Spasojevic, 2010; Lager and Torssander, 2012), the data we use has a much longer follow-up with more up to date data, more outcome variables and a larger sample size and we analyse this data using RD in addition to DiD. Meghir et al. (2017) also make the same contributions as we do over and above those of prior Swedish studies. In comparison to Meghir et al. (2017), our major contribution is that we introduce a new reform which has the novelty of allowing us to perform instrumental variable analysis of the causal effect of education on health, compare two different types of school reform across over-lapping cohorts. Further, we utilise a better measure of schooling that captures the full effect of the reform which dramatically changes the interpretation of the 9 year reform and we introduce new survey data measuring self-reported health and health behaviours.

In the following section we describe the two Swedish compulsory schooling reforms. In section 3 we introduce the data and in section 4 we outline our empirical strategy. In section 5 we present the results. Our findings show that there was a sizeable increase in years of education due to both reforms but these increases did not lead to an improvement in life expectancy or health. The results are robust to reform, modelling choice, identification strategy and health outcome. We then discuss the results in section 6 and argue that the results are not only robust internally, they also have validity beyond the Swedish context. Finally, we conclude in section 7.

2 The Swedish compulsory schooling reforms

During the 1950s and 1960s in Sweden, a large number of municipalities raised the minimum years of compulsory schooling from 7 to 8 years gradually over a period between 1941 to 1962 (affecting birth cohorts born between 1927 to 1948). This is illustrated in figure 1 (See "Old primary school 8-Year"). We call this the 8 year reform. From 1948 to 1969 municipalities then gradually replaced the old system with a new comprehensive school system that also raised the minimum years of schooling to 9 years (affecting birth cohorts born between 1938 and 1958). Again, this is illustrated in figure 1 (See "New Compulsory school"). We call this the 9 year reform. This section introduces provides some background information on these reforms, leaving a more detailed description to Appendix A.

Prior to both reforms, students attended a common compulsory primary school (*Folkskola*) up to and including the 6$^{\text{th}}$ grade. After the sixth grade, good performing students (defined by an assessment) had the option to switch to an academic educational track and study at a three year junior secondary school (*Realskola*).[1] Those who continued on at primary school studied up to seventh grade. Attending junior secondary school allowed students to continue to higher secondary school (*Gymnasium*) and later university, an option denied to those who stayed on at primary school. Only a small minority went on to junior secondary school, the vast majority of students stayed on and completed compulsory education at primary school.[2]

The 8 year reform was a simple extension of the minimum years of schooling within the municipality for those students who did not choose to go to junior secondary school. The reform was seen as an opportunity to give more time for the students to learn without any specific changes to the curriculum. It is therefore a credible instrument for years of education. The 9 year reform introduced the Swedish comprehensive school system and was very different in character to the 8 year reform, as not only did it increase the minimum years of schooling

[1]Students could also start junior school after the 4$^{\text{th}}$ grade and study a four year track.
[2]For the cohorts we consider the share was 16% (cohort 1938) and 30% (cohort 1951).

within the municipality to 9 years (from 7 or 8 years) it also postponed tracking of students with the aim of fostering greater equality of opportunity (Holmlund, 2007). The reform also introduced a new national curriculum. The removal of early tracking is likely to have broadened the peer group mix in the new comprehensive school system as higher ability students who would have gone to junior secondary school now shared the same class as their lower ability peers for longer compared to students under the old system. The curriculum changes may well have led to quality changes in schooling.

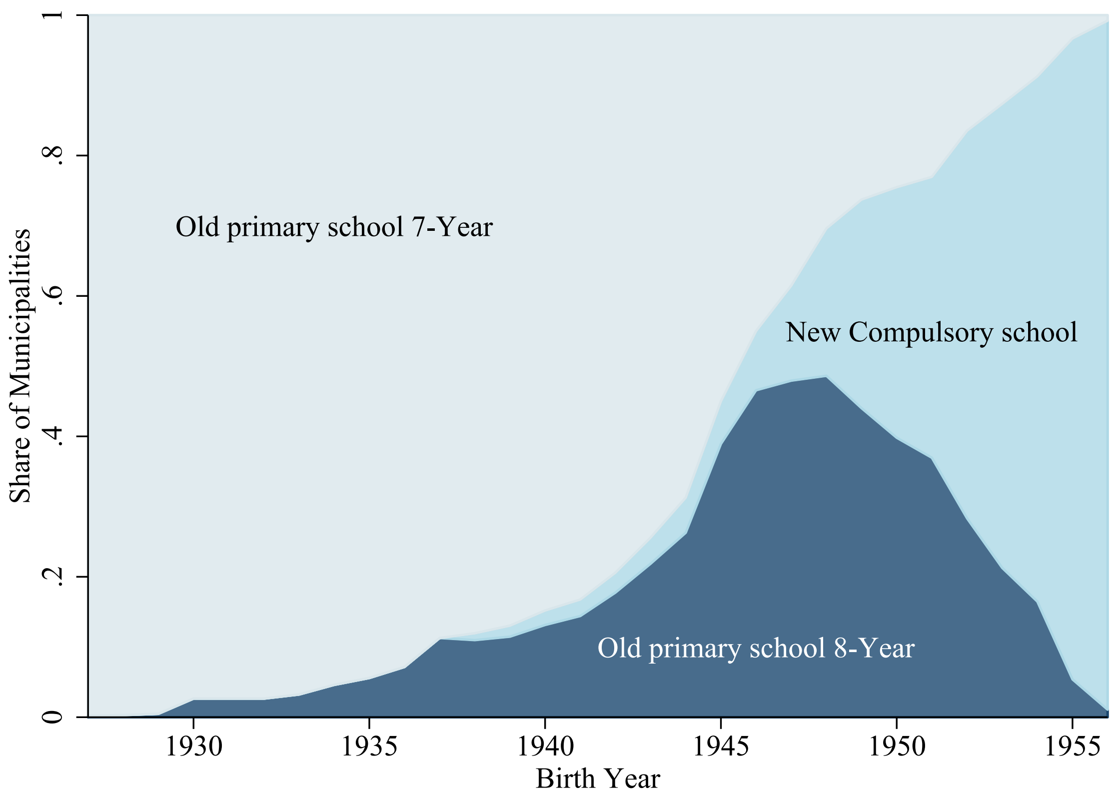

Fig. 1: Share of municipalities by length of compulsory education

Notes: This figure shows the proportion of municipalities in Sweden who have the 7 year old primary school system, the 8 year old primary school system and the new 9 year compulsory school system by birth cohort.

Both reforms were rolled out at municipality level over time and this phased roll out was at the discretion of the municipalities. The number of municipalities who had implemented an 8$^{\text{th}}$ compulsory year gradually increased from 33 in 1946/47 to 207 in 1958/59. The early birds in this development tended to be more urban and included most of the larger cities. Smaller municipalities followed and in the end more than half of Sweden's more than 1,000 municipalities had introduced a mandatory 8$^{\text{th}}$ grade before implementing the comprehensive

9 year school reform. The previous literature referred to the 8 year extension as a rare phenomenon mainly occurring in the largest cities (Holmlund, 2007). In fact the majority of municipalities had rolled out a compulsory schooling length of eight years before the 9 year comprehensive school was introduced. Figure 1 illustrates this development.[3] Both reforms were rolled out in such a way that within the same school older students were under the old regime and younger students were under the reformed compulsory schooling regime. For the 8 year reform this simply meant that younger cohorts studied a year longer than older cohorts. For the 9 year reform this meant two curricula were being taught within the same school.

The roll out of the reforms was not random. The 8 year reform roll out was chosen by the municipalities themselves, both whether or not to implement at all and the timing. The 9 year reform in its early phase was introduced with explicit intention to evaluate the policy reform. The process became less strict later on. In section 4 we set out how we control for this to identify the exogenous variation in schooling we are after.

3 Data

To quantify the impact of the two education reforms on health outcomes we employ both population based administrative data and survey data. The full population administrative data is drawn from the Swedish Interdisciplinary Panel (SIP)[4]. We consider the universe of those born in Sweden between 1932 and 1959, who survived to 16 years of age, and had not emigrated from Sweden by 2012.[5] These cohorts consist of 2,789,494 individuals.

To identify individuals as exposed or unexposed to the reforms we assign treatment status based on year of birth and place of residence. Information on the timing of the 8 year reform in each municipality was gathered from the Swedish National Archives. For the introduction of the 9 year reform we rely on a dataset as used in Hjalmarsson et al. (2015), of which an earlier version is described in detail in Holmlund (2007). Information on municipality

[3]In the early years of the introduction of the 9 year comprehensive school reform (between 1949 and 1962), the reform was introduced as a *social* experiment in certain areas (Marklund, 1982). The National School Board chose the areas from a group of applicants to form a representative set based on observable municipality characteristics (Holmlund, 2007). In 1962 the Swedish parliament finally decided that all municipalities should be obliged to offer the new comprehensive school system and by 1969 all municipalities were to have the new system in place.

[4]This is based upon Statistics Sweden's Multiple Generation dataset to which all other datasets are then linked using personal identifiers. It is administered at the Centre for Economic Demography, Lund University, Sweden. The present study was approved by the *Lund University Regional Ethics Committee, DNR 2013/288.*

[5]Very limited intergenerational information is available before 1932 explaining our chosen start point

of residence which we use to infer reform assignment is obtained from the 1960 and 1965 censuses.[6] In the empirical analysis we consider the reform status of cohorts born between 1938 and 1954 and use cohorts born before and after only as control groups. Our endpoint for the analysis, the cohort born in 1954, is chosen because this is the last cohort questioned in the 1970 census for which we have enough years to measure their years of schooling. Both reforms were rolled out in different parts at different times within the cities of Stockholm, Gothenburg and Malmo and therefore we exclude those resident in these cities. From the original sample we have reform assignment for both reforms for 2,108,696 individuals.

Data on schooling is obtained from the 1970 census and this is combined with post schooling attainments from the Education administrative database. We derive a measure of years of education by assigning the years typically associated with different types of schooling (from the census in 1970) and post-schooling qualifications from Education administrative database and take the sum as an approximation for the total years of education. This approach is an important innovation in how to measure years of education in Sweden as it allows us to capture the effect of the 8 year reform on years of education and also better captures the effect of the 9 year reform. Previous Swedish population administration data based studies have approximated years of education by the average length associated with the highest educational qualification (Hjalmarsson et al., 2015; Lager and Torssander, 2012; Lundborg et al., 2014). We cannot use this approach for the 8 year reform because there is no information on whether an individual went to 7 year or 8 year primary school. These are clumped together in the same category in the variable capturing highest educational qualification. Using just administrative based information on highest qualification also means the impact of the 9 year reform is under-reported as this approach cannot distinguish between someone who attained more than

[6]For cohorts born between 1943 and 1948, we assume that place of residence in 1960 is also the municipality where they went to school. For cohorts born on or after 1949 we follow the suggestion of Holmlund (2007) and use the place of residence as recorded in the 1965 census. For those born before 1943 we use place of residence of the mother (father if information is missing for the mother and if both parents are missing we use own place of residence in 1960) as recorded in the 1960 census. An alternative approach would be to use the *place of birth* as an approximation of place of residence during childhood. In theory this approach has a nice intention to treat interpretation and avoids being susceptible to potential parental choice of reform assignment, itself linked to the child's ability. However, for the cohorts we consider births were increasingly occurring at a hospital and before 1947 the hospital was recorded as the place of birth which often did not coincide with the place of residence of the parents. Up to 1947 therefore, the place of birth becomes increasingly uninformative as a measure of place of residence for the child at birth. From 1947 this practice changed and the place of birth of the child was recorded as the place of residence of the mother (Holmlund, 2007). In figure B.1 in the appendix we show that the misclassification from hospital recording is sizeable. As near half of our cohorts are born before 1947 and Meghir and Palme (2005) provide evidence that inter-municipality migration is very small (and therefore parental response to the reforms is small if there is one at all), we do not follow this approach.

compulsory schooling but had different amounts of compulsory schooling e.g. two people who received vocational training but received different amounts of compulsory schooling would be given the same number of years of education using just the information from the administrative data on highest educational qualification. Our new method captures the impact of the 9 year reform more accurately as it distinguishes schooling and post schooling achievements when calculating years of schooling.[7] How we construct our years of education variable is a key contribution of the paper, as it not only allows us to capture the impact of the 8 year reform, it also improves the measurement of the impact of the 8 year reform as we shall show later.[8] Our final sample size for those we also have information on years of education is 2,022,174.

Data on cause of death and cause of inpatient care at hospital is obtained from the Swedish Cause of Death database and the Hospital Patient database (stays at a hospital of more than 24 hours) and merged using a personal identifier to our main dataset. The mortality data covers the years from 1964 and cause specific information is from 1969 and runs up to 2013. We consider the whole observation period and therefore measure the impact on death up to age 75 for the oldest cohort.[9] The inpatient data covers the years 1987 up to 2012. We therefore consider the impact on hospitalisations up to age 74. Underlying cause in both datasets is recorded according to the 7$^{\text{th}}$, 8$^{\text{th}}$, 9$^{\text{th}}$ and 10$^{\text{th}}$ versions of the International Classification of Diseases (ICD) depending on year of death/hospital admission. The data also includes date of death, date of hospital visit (admission and discharge) and length of hospital visit. We consider the most common causes of death and hospital visits and also the number of hospital days from inpatient records (see appendix B table B.1 for variable ICD codings). In table 1 we present the means of our administrative data based outcomes variables by reform and whether treated or untreated. We include only those born within 10 years of

[7] We still have downward measurement error for cohorts born after 1951 because the years of schooling measure in the census measures schooling up to age 19. For cohorts born 1952-1954 we impute years of schooling using the modal years of schooling measured in the census corresponding to each level of further education achieved as measured in the Education administrative data from earlier cohorts (1948-1950). For cohorts after 1954 we assign 7 years of schooling for those with post schooling Swedish Education Nomenclature 2000 (SUN2000) classification of less than 200 and 9 years with a SUN2000 classification of 200-300. This adjusts for the downward bias of measuring individuals' schooling before they have reached 19 but will still underestimate the reform effect.

[8] This is particularly important for interpretation of prior studies that used the 9 year reform as an Instrumental Variable for years of schooling as they will have biased their causal estimates upwards using years of education based on highest qualification from the Education administrative database.

[9] We consider death by 2013 so that we capture as much data as possible. We could consider death by a certain age, but then we would lose a lot of information given most deaths are for older ages.

reform implementation as this is the population we use in our main analysis. We observe that on average for our 8 year reform sample the untreated have 9.4 years of schooling, 19% have died by 2013 and predominantly of cancer and have spent 30 days in hospital whereas the treated population have 10.9 years of schooling, only 9% have died by 2013 and have spent 21 days in hospital. For the 9 year reform sample on average the untreated are again less educated, more have died by 2013 and are more likely to have had a hospital visit and for longer compared to those treated. For both reforms, the treated are younger and this explains a large part of the differences in health outcomes between our treated and untreated groups. We control for this in our analysis.

Table 1: Descriptive statistics - administrative data

Variable:	8 Year Reform		9 Year Reform	
	Untreated	*Treated*	*Untreated*	*Treated*
Years of education	9.4	10.9	10.1	11.5
Dead	0.199	0.090	0.128	0.063
Proportion dead due to:				
Cancer	0.080	0.035	0.050	0.021
Circulatory Disease	0.050	0.019	0.029	0.012
External Causes	0.010	0.011	0.011	0.012
Other	0.059	0.026	0.038	0.019
Days at hospital	29.7	21.3	24.0	20.3
Proportion who had a hospital visit due to:				
Cancer	0.111	0.066	0.084	0.051
Circulatory Disease	0.156	0.083	0.113	0.062
External Causes	0.121	0.092	0.106	0.092
Other	0.313	0.269	0.284	0.293
Observations	215,846	318,557	640,093	607,715

Notes: This table shows the means for education and health outcomes for those treated and not treated by each reform and born within 10 years of the first cohort impacted by the reform.
Source: SIP. Own calculations.

The survey data stems from the Swedish survey on living standards (ULF) (Statistics Sweden, 2008).[10] The survey includes self-reported health and health behaviour variables which we consider as valuable complements to population based administrative data on cause of death. We consider binary indicators for smoking behaviour, obesity, self-reported anxiety or worry and self-reported fair or bad health (in contrast to good).[11] In table 2 we present the means of our education and health outcomes. The years of education means correspond

[10]The ULF survey is a well respected survey used for a wide range of research topics and in recent years has formed the Swedish part of the European Union Statistics on Income and Living Conditions (EU-SILC).

[11]We define a binary variable *bad or fair health* equal to one if self-reported health is reported as fair or poor. Smoke Daily is a binary indicator, indicating one if smoked daily in the past 30 days prior to interview, zero otherwise. Anxiety is a binary variable, one indicating whether the individual self-reported having heightened anxiety, concern or worry, zero otherwise. Obese is a binary indicator derived from information on height and weight creating a Body Mass Index (BMI), one indicating a BMI of 30 or more, zero otherwise.

to those for the administrative data in table 1 which suggests the sampling frame of the survey data does well to represent the population estimate. Whilst self-reported *fair or bad health* and obesity are more common amongst the untreated samples, *smoke daily* and *anxiety, concern* are actually more common amongst the treated population. The survey itself is carried out by face-to-face interviews of a randomly selected sample of the population. The sample size is about 7,500 individuals per year and data is reported for years 1980 through to 2012. We therefore have 32 years of data. Information from different Swedish censuses and on education attainment from the Education administrative data is linked to individuals in the survey.

Table 2: Descriptive statistics - survey data

Variable:	8 Year Reform		9 Year Reform	
	Untreated	*Treated*	*Untreated*	*Treated*
Years of education	9.5	10.9	10.2	11.5
N	3,360	4,840	9,847	9,306
Fair or bad health	0.261	0.183	0.213	0.166
N	3,349	4,832	9,830	9,294
Smoke daily	0.226	0.276	0.255	0.268
N	3,325	4,813	9,776	9,257
Obese	0.129	0.090	0.105	0.083
N	2,139	2,857	6,004	5,510
Anxiety, concern etc	0.149	0.150	0.146	0.149
N	2,256	3,132	6,437	6,050

Notes: This table shows the means for education and health outcomes for those treated and not treated by each reform and born within 10 years of the first cohort impacted by the reform.
Source: ULF-Survey. Own calculations.

4 Empirical Strategy

4.1 Identifying the impact of the reforms

In this section we outline the two empirical identification strategies we use to identify the impact of the reforms on health outcomes: DiD and RD design. The purpose of using two identification strategies is that it provides a sense of how robust the findings are. The two methods rely on different identifying assumptions to identify the causal impact of the reforms and potentially estimate the impact for different populations of compliers. We are therefore able to assess if this is important to the conclusions we draw.

The impacts of the reforms on education, mortality, hospitalisations and self-reported health outcomes are modelled in a linear setting, using either OLS or a Linear Probability

Model (LPM) depending on the outcome variable, as is standard for both DiD and RD. Our DiD empirical strategy utilises the fact that the education reforms were introduced slowly over time across municipalities in Sweden. Two individuals born in the same year but one resident in a reform municipality and the other not have different exposures to compulsory schooling. This provides us with variation in reform exposure both over time and across municipalities. However, the implementation was not random as discussed in Holmlund (2007). To control for this we difference across municipalities and across birth cohorts using dummy variables for both. Our linear DiD model is:

$$H_{i,c,m} = \beta_0^{DiD} + \beta_1^{DiD} Z_{c,m} + \beta_2^{DiD} C + \beta_3^{DiD} M + \beta_4^{DiD} trend_m + \epsilon_{i,c,m}; \tag{1}$$

where i indicates an individual, c the birth cohort, m the municipality, and $Z_{c,m}$ is a variable equal to one for individuals assigned as exposed to the reform using their date of birth and place of residence, zero otherwise. $H_{i,c,m}$ is our outcome of interest, C is a vector of birth year cohort dummies, M is a vector of municipality dummies, *trend* is a vector of municipality specific trends and β_0^{DiD} is a constant term. The coefficient β_1^{DiD} measures the impact of the reforms on our outcome measures. We estimate equation (1) separately for each reform.

The empirical strategy utilising RD design involves identifying the reform effect within municipalities based on the year the school reform was introduced and the cut-off date for the school year, which is the 1st of January. Individuals born before the reform year cut-off are not assigned as exposed to the reform and those born on or after the cut-off are assigned as exposed. The forcing variable, $T_{i,m}$, is birth date measured in years and months from the reform cut-off date in their municipality. Our linear RD model takes the form:

$$H_{i,c,m} = \beta_0^{RD} + \beta_1^{RD} Z_{c,m} + f(T_{i,m}) + \mu_{i,c,m}; \tag{2}$$

where i, c, m, $Z_{c,m}$ are as for equation (1). The coefficient β_1^{RD} captures the average impact of the reforms across all of the municipality level cut-offs. The identifying assumption is that the outcome variable is a smooth function of our forcing variable and that after adequately modelling this function, $f(T_i)$, any jump found at the cut-off is due to the education reform and not some other unobserved variable. To capture the birth cohort relationship with the outcome variable, we model $f(T_i)$ using a polynomial in years-months from the cut-off, T_i, estimated separately either side of the cut-off combined with dummies for gender and month of birth, to control for seasonality effects.[12] To choose our preferred function for T_i we followed the approach of Imbens and Lemieux (2008) and included progressively higher order polynomials in T until the additional polynomials were insignificant. At the same time care was taken to not over-fit the model, a concern raised by Gelman and Imbens (2017). We found that a second order polynomial was sufficient for all outcomes.

It is potentially of concern that we include individuals much older or younger than the first cohorts impacted by the reform within a municipality. To deal with this we use a bandwidth of up to 10 years for both our DiD and RD regressions so that only those born up to 10 years before or after the first cohort impacted by the reform are included in the analysis.[13] We also test the sensitivity of bandwidth choice and whether municipality level trends are important.

We consider mortality as our main health outcome. In addition to modelling mortality using an LPM we also consider time until death during the observation period using a Cox proportional hazard model (Cox and Oakes, 1984) together with our DiD and RD identification strategies. In this way, we model the conditional probability of dying in the next period given survival to the current period. By considering the survival nature of our data we use more information, potentially increasing efficiency. It also allows us to deal with censoring because of survival beyond the sample period of 2013 and is also a natural choice when considering causes of death. If a particular cause of death is reduced by the reforms, by construction this means other causes of death will be increased for a given level of mortality. Cox models, under the independent competing risks assumption, deal with this. In our application of the Cox model we estimate duration until death, d, and using DiD we stratify on municipality of residence and include dummies for birth cohort which gives us:

$$I_{1,i,c,m}(d|X) = I_{0,m}(d)exp[\delta_0^{DiD} + \delta_1^{DiD} Z_{c,m} + \delta_2^{DiD}\gamma_c + \delta_3^{DiD} trend_z]; \tag{3}$$

where $I_{0,m}$ is the baseline hazard stratified by municipality, cohort specific fixed effects are given by γ_c, time trends for municipalities that roll out the reform in the same year are given by $trend_z$, and subscripts i, c and m, and $Z_{c,m}$ are as for equation (1).[14] δ_1^{DiD} is the reform

[12]For analysis of the survey data we also include survey year dummies to control for age and year effects.

[13]We say up to ten years because for the earliest cohort we only have cohorts up to 6 years older to compare to and for later cohorts we only have cohorts up to 5 years younger to compare to.

[14]For estimation to be feasible we have to limit the municipality trends estimation to municipalities that

impact on mortality. For our RD design, the Cox model takes the form:

$$I_{1,i,c,m}(d|X) = I_0(d)exp[\delta_0^{RD} + \delta_1^{RD} Z_{c,m} + f(T_{i,m})]; \tag{4}$$

where I_0 is the baseline hazard and subscripts i, c and m, and variables $Z_{c,m}$ and $f(T_{i,m})$ are as for equation (2). The coefficient δ_1^{RD} is the impact of the reform on mortality within each municipality averaged over all municipalities.

All of the models we have outlined above are reduced form models. We also apply Two Stage Least Squares (2SLS) using our linear equations (1) and (2) as the first stages with years of education YE in place of H as the dependent variable and instrumented with reform status. Our second stages are:

$$H_{i,c,m} = \alpha_0^{DiD} + \alpha_1^{DiD} \widehat{YE}_{i,c,m} + \alpha_2^{DiD} C + \alpha_3^{DiD} M + \alpha_4^{DiD} trend_m + v_{i,c,m}; \tag{5}$$

$$H_{i,c,m} = \alpha_0^{RD} + \alpha_1^{RD} \widehat{YE}_{i,c,m} + f(T_{i,m}) + u_{i,c,m}; \tag{6}$$

where α_1^{DiD} and α_1^{RD} are the coefficients on years of education and are our coefficients of interest. Both α_1^{DiD} and α_1^{RD} are identified by the variation in years of education that comes from the variation generated by the school reforms.

To identify our 2SLS coefficients we need to assume that the reforms affected our health outcomes only via their effects on years of education (the exclusion restriction) and that reform exposure is as good as random given our control strategies. The exclusion restriction would be violated if the reforms had other impacts on students over and above their impact on years of education that then impacted on health. For the 8 year reform the education system remained the same and so did the curriculum. However, for the 9 year reform, in addition to the increase in years of compulsory schooling, both the curriculum and the school system were changed. Prior to the 9 year reform, students were selected into different schools based on their academic ability. The 9 year reform abolished this and instead, students were kept in the same school and classes until the ninth grade.[15] Tracking has been found to impact educational achievement and later life outcomes (Betts et al., 2011) suggesting that its removal

roll out the reform in the same year rather than model trends for each individual municipality.

[15]There were some exceptions, where tracking was used for some subjects but overall students were much more mixed.

may have had an impact on educational quality. The removal of tracking will also have changed the peer group mix that students were exposed to, potentially impacting learning, health related behaviours and even assortive mating. Peer effects have been found for health outcomes and health related behaviours such as drinking, smoking and drug use (see for e.g. Sacerdote et al. 2011 for an overview on peer effects). In addition to the change in tracking, the 9 year reform coincided with the introduction of a new national curriculum. Although we have no evidence of the impact on quality this change made, it seems reasonable to assume that it had some impact on the variation in quality of schooling across the municipalities.

In order to use the 9 year reform as an IV we have to assume that both the changes to the tracking system and the introduction of a national curriculum had no impact on schooling quality or peer effects that could in turn impact our health outcomes. A number of articles have made this claim (Spasojevic, 2010; Lundborg et al., 2014) whilst others view this as controversial and focus on estimating the reduced form effect of the reforms (Meghir and Palme, 2005; Meghir et al., 2017). In this paper we take the latter view but present IV estimates based on the 9 year reform as a way of quantitative comparison to the 8 year reform, a reform which we argue more convincingly meets the exclusion restriction requirements.

4.2 First stage results and diagnostic tests

Both of our identification strategies build upon our method of treatment status assignment performing well. In addition to this and the exclusion restriction, our 2SLS estimates require our education reform impacts on schooling to be as good as random given our control strategies. For our DiD estimates our control strategy hinges on the assumption that conditional on birth cohort and municipality fixed effects, exposure to treatment is as good as random. For our RD estimates our control strategy hinges on the assumption that conditional on our modelling of age relative to the first cohort within the municipality impacted by the reform, there are no jumps in the error term at the cut-off. In this case, any jumps we do find in years of schooling at the cut-off can then be assumed to be as good as random. In this section we establish the existence of a first stage and provide some diagnostic tests to assess the plausibility of our identification assumptions.

In figure 2 we present the raw data of the probability of having achieved 8 years of old primary schooling (left hand side panel) and 9 years of schooling (right hand side panel)

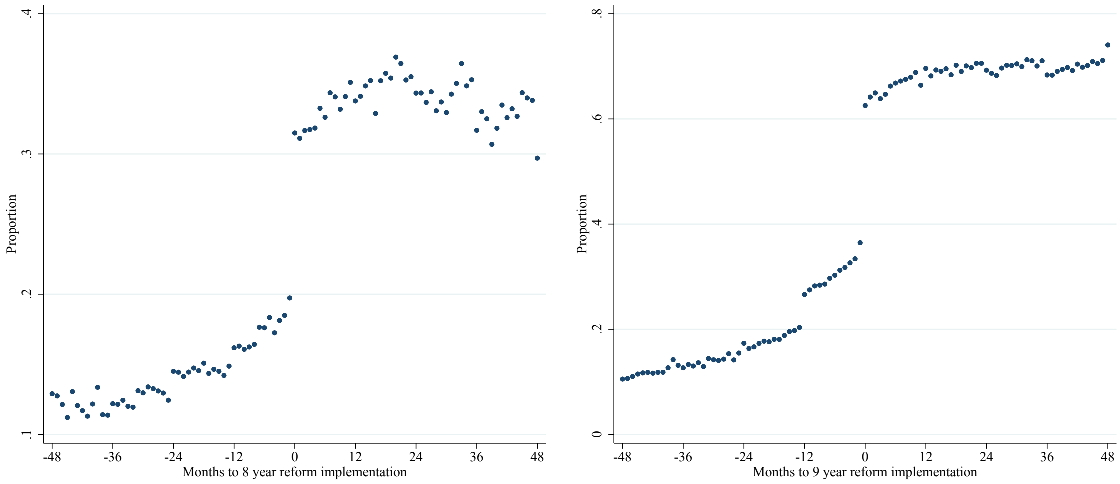

Fig. 2: Effect of the reforms on the proportion with the new minimum years of schooling

Notes: Scatter plots of the proportion with the new minimum years of schooling by age in months measured as months to reform implementation in their municipality. Left panel is for the 8 year reform, right panel the 9 year reform. Reform implementation is at time zero. *Source:* SIP. Own calculations.

against birth cohort measured in months relative to the first cohort impacted by each reform respectively. For each monthly bin the proportion with 8 years/9 years of schooling is plotted. We see that there is an increasing trend with time until exposure and that at the cut-off there are clear jumps in the proportion with the new minimum years of schooling. Note that it is entirely expected that a proportion of students have the new compulsory schooling before reform implementation. Students who repeat a grade would naturally receive an extra year of schooling. For both reforms it is also documented that there was partial roll out that was non-mandatory prior to the reform becoming mandatory. We also see a jump in the proportion with the new compulsory years of schooling in period t-1 and this is much clearer for the 9 year reform. Hjalmarsson et al. (2015) suggest that the pre-reform increase in schooling is due to either measurement error in the exposure variable or due to pupils being in the wrong grade based on their age due to choosing to repeat a grade. Hjalmarsson et al. (2015) cite evidence that grade repetition was not a common occurrence for those in the old 7 year primary school system but grade repetition and dropping out was for those who were tracked into the junior secondary school. Those at junior secondary school who were born a year too early but had dropped out would have normally gone back to old primary school, but because of the reform they would have instead been caught by the 9 year school reform and would as a consequence be a year older than their peers in the same class. This last explanation fits with what we see in the data. There is possibly a small jump in t-1 for the 8 year reform and this fits with the reported observation that grade repetition was not very common in the old primary school system. There is however a clear jump in t-1 for the 9 year reform, and this is quite likely in part driven by dropouts from junior secondary school.[16]

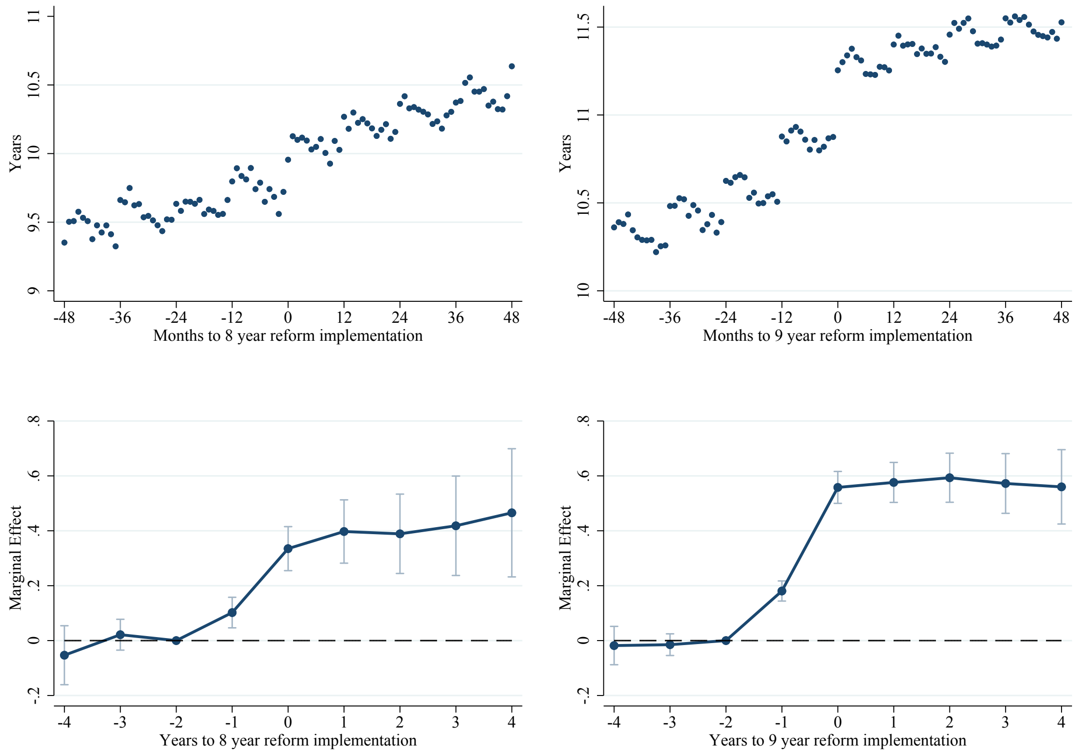

Fig. 3: Effect of the reforms on years of education

Notes: Top two panels: Scatter plots of mean years of schooling by age in months measured as months to reform implementation in their municipality where the first cohort impacted is at zero. The bottom two panels: plot regression coefficients of an individual's birth year relative to the first reform cohort in their municipality on years of education (spikes represent the 95% confidence interval for each coefficient estimate). Municipality and birth year fixed effects and municipality level time trends are controlled for, a bandwidth of 10 years is used and clustered standard errors are estimated at the municipality level. Category 4 is four or more years after the first reform cohort. The reference category is *two years before the first reform cohort* (t-2).
Source: SIP. Own calculations.

In figure 3 the top two panels present the raw data in a similar fashion to that of figure

[16]In the appendix we also show figures equivalent to figure 3 but for the proportion with just 7 years of schooling, just 8 years of schooling and just 9 years of schooling. These all confirm the jump at the cut-off. They also confirm the pre-reform jump in t-1. The jump in t-1 for the 9 year reform coincides with a clear drop in 8 years of old primary school in t-1 suggesting it is driven by individuals dropping out of junior secondary school after one year in municipalities who had introduced the 8 year reform. We can also see that there is measurement error in the exposure variable as after the 8 year reform there are still some with 7 years schooling. Similarly for the 9 year reform there is still a proportion with old primary school after reform exposure. This is partly explained by partial implementation in the municipality where exposure is given as 1 if just part of the municipality enacted the reform.

2, this time with years of education on the y-axis. The bottom two panels of figure 3 are event study graphs from our DiD regressions and show the conditional marginal effect and the corresponding 95% confidence interval for each year cohort relative to the first cohort impacted by the reform (t=0) on years of education. The reference cohort is the cohort born two years before the first treated birth cohort. The estimates are from a regression controlling for municipality and birth cohort fixed effects and standard errors are clustered at the municipality level. From figure 3 we can see a jump in the average years of schooling due to both reforms and that this jump is larger for the 9 year reform.

Table 3 presents the regression results of the impact of the reforms on years of education for all individuals, and also split by gender. Column (1) in table 3 presents the results for the 8 year reform on years of education for all individuals and we find an increase of 0.23 years of schooling using RD and 0.27 years of schooling using DiD. Column (4) presents the results for the 8 year reform for all individuals and we find an impact of 0.39 years using RD and 0.53 years using DiD. Note that the 9 year reform estimates presented here are much larger than previously documented (see e.g. Holmlund 2007; Lundborg et al. 2014; Meghir et al. 2017) and the reason is because we use a different measure of years of education which better captures the impact of increased compulsory schooling on years of education. Indeed, using a schooling measure calculated in the same way as Holmlund (2007) we find that the impact of the 9 year reform on years of schooling is 0.3 years using DiD and 0.25 years using RD, both much smaller than our preferred estimates presented in table 3 (see model (7) of table B.3 in the appendix for the administrative data based education variable results).

Table 3: Compulsory schooling reforms' impact on education

	(1)	(2)	(3)	(4)	(5)	(6)
	8 year reform			9 year reform		
	All	Females	Males	All	Females	Males
Difference in Difference	0.272***	0.227***	0.316***	0.523***	0.493***	0.551***
	(0.023)	(0.029)	(0.026)	(0.019)	(0.019)	(0.023)
F-stat	140.64	59.44	147.39	779.52	688.97	576.09
N	534,403	264,237	270,166	1,247,808	613,317	634,491
Regression Discontinuity	0.230***	0.208***	0.251***	0.392***	0.349***	0.433***
	(0.023)	(0.031)	(0.030)	(0.023)	(0.027)	(0.024)
F-stat	101.29	44.25	72.36	299.31	172.71	317.89
N	534,403	264,237	270,166	1,247,808	613,317	634,491

Notes: This table shows the impact of the 8 year and 9 year school reforms on years of education. Each coefficient is from a separate regression by reform, method and group. DiD specification includes birth cohort and municipality fixed effects and municipality level time trends. Regression discontinuity estimates have separate second polynomials in the running variable either side of the cut-off and a full set of dummy variables for month of birth and gender. Bandwidth of up to 10 years is used for both DiD and RD. Robust standard errors clustered by municipality level (for DiD) and by the running variable (for RD) are in parentheses. Testing the null of the coefficient: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$
Source: SIP. Own calculations.

The 9 year reform had a larger impact on years of education compared to the 8 year reform. This makes sense because the 9 year reform increased years of schooling by two years for students who were in municipalities only offering 7 years of primary school and one year for those offering 8 years, whereas the 8 year reform was just a single year increase for all municipalities affected. The 8 year reform was also predominantly rolled out in urban areas that had less potential compliers as more students in urban areas went to junior secondary school. The results split by gender show that the impact for males is slightly larger across modelling strategies and reforms. The F-statistic results suggest across the board that we have a strong first stage, based on the the rule of thumb for weak instruments of an F-statistic above 10 (Stock et al., 2002). In table B.3 in the appendix we investigate whether the results in table 3 are sensitive to our choice of bandwidth and inclusion of municipality specific linear trends (DiD only). Using bandwidths with a range of 2 years to up to 12 years we find that the point estimates change only slightly (see columns (1-6) in table B.3). Inclusion of trends in our DiD estimates makes little difference to the point estimates.

The event study graphs in figure 3 show increases in years of education for the birth cohort born a year before the first treated birth cohort (t-1) although this is much smaller for the 8 year reform. Hjalmarsson et al. (2015) find the same pattern when including lags of the treatment in their assessment of the 9 year reform. We also find that our RD estimates are consistently smaller than our DiD estimates. The increase we observe for the t-1 cohort in the figure explains most of the difference between our RD and DiD estimates. The RD estimates only capture the impact of the reforms in the reform period whereas the DiD estimates capture some of the pre-reform treatment differences. We illustrate this by dropping the t-1 cohort as a sensitivity test (see model (8) table B.3 in the appendix) and find that the gap between the RD and DiD estimates largely disappears.

We conclude that both the 8 year and 9 year reforms lead to increased years of education, that the increases were slightly larger for males and that the 9 year reform actually had much more bite than previous research has suggested. Our RD estimates are smaller than our DiD estimates and this is likely due to DiD capturing more compliers to the reform. This is a consequence of them capturing different sub-populations.[17]

We have shown that the reforms coincide with substantial increases in years of education using both DiD and RD and therefore that our method of reform assignment is working well. In addition to a strong first stage regression, our 2SLS estimates require exposure to reform to be as good as random, conditional on our control strategies. This may be violated if selective migration to and from reform municipalities occurred, either to escape or gain access to the reform. In previous work assessing the 9 year reform, both Meghir and Palme (2005) and Holmlund (2007) have tested for selective migration and have found that it was not a problem. We are not able to test it for the 8 year reform but make the assumption that the results of both Meghir and Palme (2005) and Holmlund (2007) apply to the earlier reform as well. We view this as a plausible assumption given that the 8 year reform was just a pure years of schooling change and would have provided much less of a reason to move compared to the comprehensive 9 year school reform - a reform that itself led to very limited selective migration.

Our estimates are robust to the inclusion of lags and leads and various forms of model specification but there may still be concern that our error term remains correlated with our explanatory variables, in particular reform assignment. In figure 4 we perform an RD diagnostic test of manipulation of the forcing variable in the spirit of McCrary (2008) by

[17] We have also tested whether seasonal variation in years of schooling changes after the reforms. We find no impact for the 8 year reform and a small negative impact for the 9 year reform on years of schooling but not on later health outcomes. This is consistent with work looking at the impact of school starting age on longer term labour market outcomes, that also finds both an impact on years of schooling but also no impact on later life outcomes (Fredriksson and Öckert, 2014). Including separate monthly dummies each side of the threshold however comes at a severe loss of efficiency, we therefore choose to model month effects without a reform interaction.

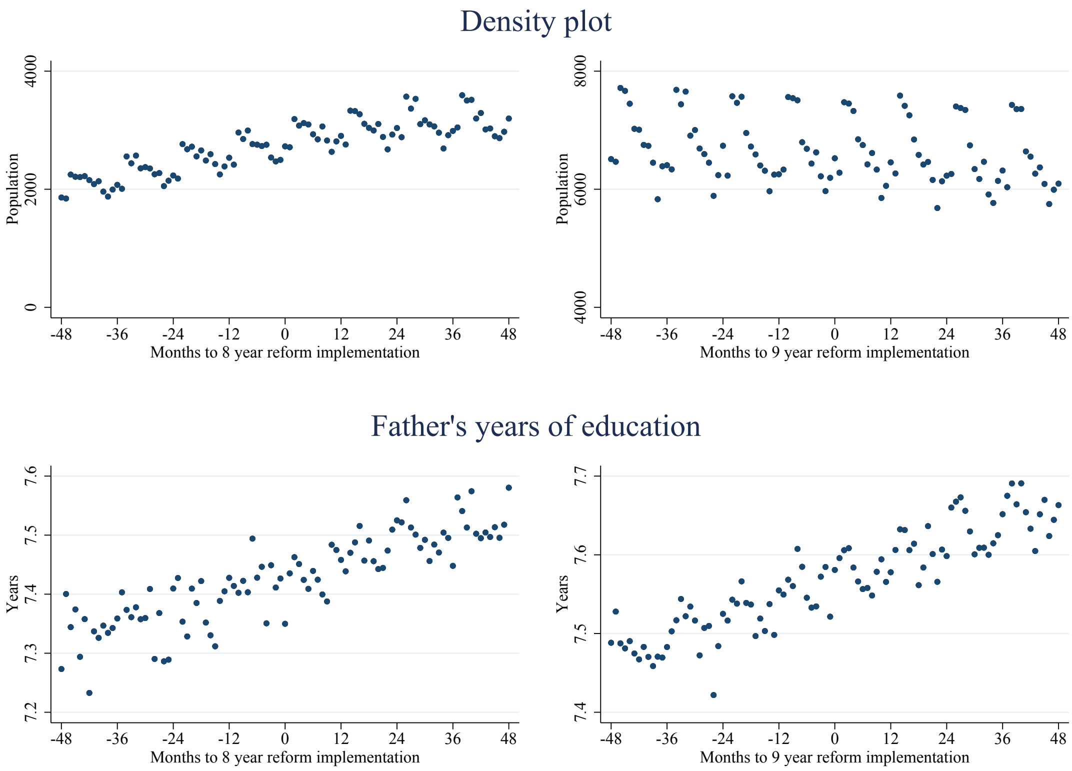

Fig. 4: Diagnostic tests

Notes: Top two panels: Density plots of age measured as the distance in months to reform in their municipality (the first cohorts to be impacted are at zero). The bottom two panels: Placebo tests of reform status on father's years of education - plotted as mean father's years of schooling in monthly bins of months to reform.
Source: SIP. Own calculations.

plotting the population density by age relative to the first cohort in the municipality impacted by the reform (top two panels). We observe no obvious jump at the cut-off point and therefore no clear changes in the fertility timing decisions around the reform. We also present scatter plots of father's education by age relative to the first reform birth cohort and again see no clear jump in father's years of schooling at the cut-off. In addition we perform a batch of balancing tests of predetermined characteristics and reform assignment in order to assess our exclusion restriction in table 4. The results show that when we control only for birth cohort fixed effects (columns 1 and 4) our predetermined characteristics are predicted by reform status. The correlations also go the way we might expect: the reforms were introduced earlier in areas where parents were more educated and had better jobs. The inclusion of municipality fixed effects and municipality specific time trends and hence our DiD strategy (see columns 2 and 5) reduces the size of the coefficients bringing them down to zero. Similarly in columns

(3) and (6) using RD to identify the impact of the reforms we find the size of the coefficients tends towards zero and they are insignificant. Whilst this evidence is just indicative that our reforms are not correlated with our error terms, they provide certain credibility to our strategies. In sum, we have shown that our assignment method works well and that with the application of our DiD and RD strategies we have provided support to our claim that reform exposure is as good as random.

Table 4: Diagnostics: Balancing test for differences in predetermined characteristics by reform status

	(1)	(2)	(3)	(4)	(5)	(6)
	8 YEAR REFORM			9 YEAR REFORM		
	OLS	DiD	RDD	OLS	DiD	RDD
PANEL A: MOTHER						
Years of Education	0.095*	-0.009	-0.009	0.074**	-0.002	0.005
	(0.039)	(0.007)	(0.009)	(0.026)	(0.004)	(0.006)
Blue collar worker	0.033***	0.002	0.001	0.002	-0.001	-0.000
	(0.007)	(0.002)	(0.003)	(0.005)	(0.001)	(0.002)
White collar worker	0.032***	0.004	-0.002	0.019**	0.000	-0.000
	(0.008)	(0.002)	(0.002)	(0.006)	(0.001)	(0.001)
No occupation	-0.068***	-0.006*	0.002	-0.021*	0.000	-0.001
	(0.012)	(0.002)	(0.003)	(0.009)	(0.002)	(0.002)
PANEL B: FATHER						
Years of Education	0.118	-0.005	-0.011	0.128***	0.005	0.005
	(0.065)	(0.011)	(0.014)	(0.038)	(0.007)	(0.009)
Blue collar worker	0.043***	-0.001	0.004	0.010	-0.000	-0.004
	(0.012)	(0.003)	(0.004)	(0.008)	(0.002)	(0.002)
White collar worker	0.060***	-0.001	0.000	0.032**	0.002	0.005*
	(0.017)	(0.003)	(0.003)	(0.012)	(0.002)	(0.002)
No occupation	-0.004*	0.001	0.002	0.001	0.000	-0.000
	(0.002)	(0.001)	(0.002)	(0.002)	(0.001)	(0.001)

Notes: This table shows impact of reform status on various predetermined characteristics. Columns (1) and (4) are simple associations controlling for year of birth. Columns (2) and (5) are estimates from a DiD regression. Columns (3) and (6) are estimates from a RD design regression using a 2nd polynomial in age from reform estimated either side of the cut-off, and dummies for month of birth and gender. All estimates use a bandwidth of up to 10 years and robust standard errors clustered at the municipality level (age on months level for RD) and these are shown in parentheses. Testing the null of the coefficient: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$
Source: SIP. Own calculations.

5 Results

5.1 Mortality

5.1.1 All cause mortality by 2013

In this section we analyse the impact of the school reforms on mortality risk by 2013 (dying by age 75). In figure 5 we show the risk of dying by 2013 and we observe no impact of the

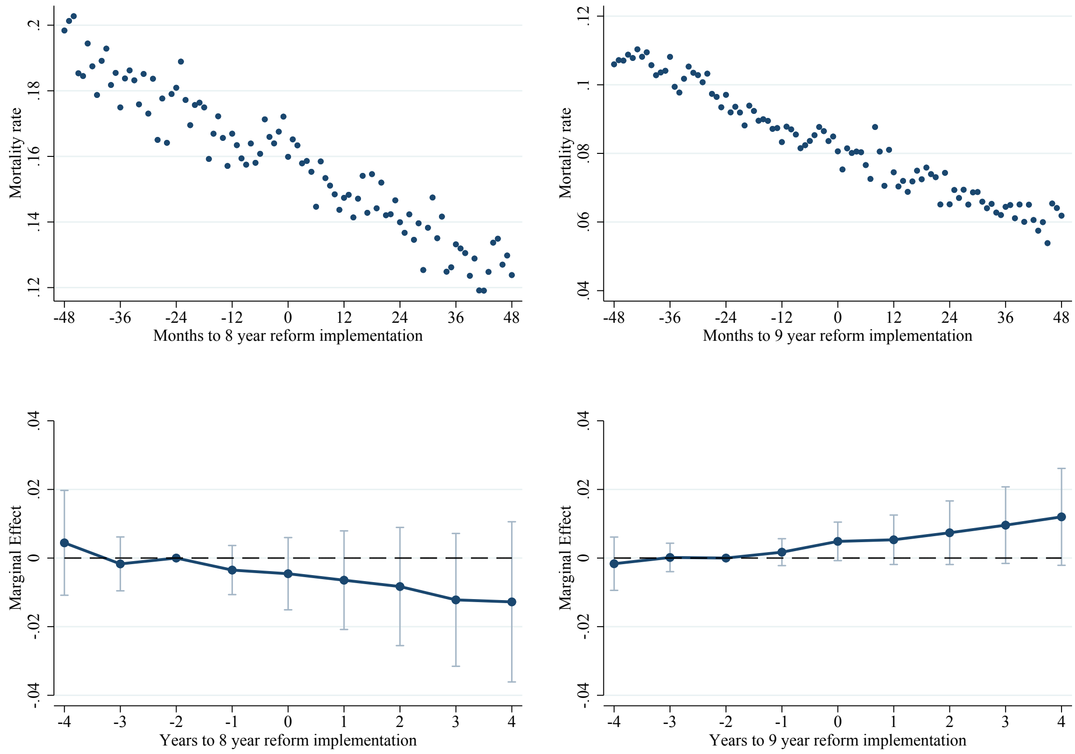

Fig. 5: Impact of the reforms on mortality by 2013

Notes: These figures plot the relationship between time to reform and mortality. The top two figures are raw data scatter plots. The bottom two figures are event study figures of coefficients from DiD regression. See notes for figure 3.
Source: SIP. Own calculations.

reforms on overall mortality in the raw data or in the event study figures.

In table 5 we present the LPM regression results for mortality. In column (1) we present the linear association of years of education with mortality probability controlling for year of birth only. This is estimated only for those not treated and with pre or post reform minimum years of schooling. This is to give an idea as to the strength of the education gradient in health for the sub-populations impacted by the reforms. The estimates confirm the finding in the wider literature that education is positively (negatively) associated with health (mortality). Columns (2) and (3) present our Reduced Form (RF) and 2SLS (IV) results respectively using our DiD identification strategy. Columns (4) and (5) present our RF and IV RD based results respectively.

Table 5: Regression results: OLS estimates and reform effects on overall mortality.

	OLS (1)	RF-DiD (2)	IV-DiD (3)	RF-RD (4)	IV-RD (5)
PANEL A: FEMALES AND MALES					
8 Year Reform Impact	-0.0167***	-0.0003	-0.0011	0.0027	0.0119
	(0.0013)	(0.0022)	(0.0082)	(0.0025)	(0.0112)
9 Year Reform Impact	-0.0100***	-0.0003	-0.0005	-0.0016	-0.0041
	(0.0006)	(0.0011)	(0.0021)	(0.0014)	(0.0034)
PANEL B: FEMALES					
8 Year Reform Impact	-0.0163***	0.0024	0.0106	0.0029	0.0139
	(0.0017)	(0.0029)	(0.0131)	(0.0033)	(0.0161)
9 Year Reform Impact	-0.0099***	0.0001	0.0002	-0.0022	-0.0064
	(0.0007)	(0.0014)	(0.0028)	(0.0017)	(0.0048)
PANEL C: MALES					
8 Year Reform Impact	-0.0096***	-0.0028	-0.0088	0.0026	0.0104
	(0.0019)	(0.0031)	(0.0097)	(0.0039)	(0.0159)
9 Year Reform Impact	-0.0063***	-0.0009	-0.0016	-0.0010	-0.0022
	(0.0008)	(0.0017)	(0.0031)	(0.0018)	(0.0042)

Notes: This table presents the OLS, reduced form and 2SLS regression estimates on mortality. Mortality is modelled using an LPM of death by 2013. Sample sizes/No. of deaths: Panel A, 8 year reform (534,403/71,640); Panel A, 9 year reform (1,247,808/120,382); Panel B 8 year (264,237/28,613); Panel B 9 year (613,317/47,531); Panel C 8 year (270,166/43,723); Panel C 9 year (634,491/72,851). Each coefficient estimate is from a separate regression. Column (1) is the association of years of education with mortality controlling for year of birth and the sample is limited to those not treated and with pre or post reform minimum years of schooling. Columns (2) and (3) are reduced form and 2SLS regression estimates using our DiD specification which includes birth cohort and municipality fixed effects, municipality level time trends and a bandwidth of up to 10 years. Columns (4) and (5) are reduced form and 2SLS regression estimates using regression discontinuity and have separate second polynomials in the running variable either side of the cut-off and a full set of dummy variables for month of birth, control for gender and bandwidth of up to 10 years. Robust standard errors clustered by municipality level (for DiD) and by the running variable (for RD) in parentheses. Testing the null of the coefficient: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$
Source: SIP. Own calculations.

In Panel A of table 5 the results are for all individuals. The reduced form estimates for the 8 year reform are -0.03 percentage points using DiD and 0.27 percentage points using RD and are much smaller than the OLS correlations. The standard errors rule out even moderately large sized effects. The IV estimates for the 8 year reform are -0.11 percentage points using DiD and 1.2 percentage points using RD (relative impacts of -0.8% and 8.9% respectively) with corresponding 95 percent confidence interval for DiD of -1.5 to 1.4 percentage points and using RD of -1 to 3.3 percentage points. The OLS estimate of column (1) is both larger and more negative than both IV point estimates and lies outside both the DiD and RD 95% confidence intervals. Testing for endogeneity of years of schooling in the OLS estimates, we reject the OLS estimates based on the RD IV results but not using the DiD results.[18] The 9 year reform reduced form impacts are similar in magnitude to those found for the 8 year

[18]The test used is a test based on difference-in-Sargan statistics (C-statistic).

reform but the standard errors are half the size compared to those from the 8 year reform regressions. As noted in section 4.1 there are strong arguments for not using the 9 year reform as an IV, but if we do we reject the OLS estimates using DiD but not RD.

In panels B and C of table 5 we split the results by gender as there are both biological and social differences between the genders that could potentially lead to different health responses to the reforms. The OLS correlations are stronger for females than for males but in general, we find no clear gender specific differences in our reduced form estimates or our causal IV estimates.

5.1.2 All cause mortality sensitivity analysis

In table 6 we present alternative estimates to the LPM results of table 5, this time based on Cox regression, modelling the proportional hazard function of the probability of dying in the next period. In column (1) of table 7 we confirm the LPM findings of table 5, that there is a significant positive association between education and health and that this is stronger for females. The reduced form estimates using Cox proportional hazard regression for all cause mortality (columns 2 to 5) mirror the LPM findings; we find no significant impact of either reform on mortality, that the impacts are very close to zero and there are no discernible differences in response to the reforms between the genders.

In the appendix, tables B.4 and B.5, we present sensitivity analysis that assesses the robustness of the results to choice of bandwidth and removal of trends in the LPM and the Cox DiD analysis and find the conclusions are unaffected by these modelling choices. In our analysis of the impact of the reforms on education in section 4.2 we found there to be measurement error in our reform assignment and a positive jump in years of schooling for individuals one year too old and that this was much larger for the 9 year reform. This lead to a large discrepancy between the RD estimates and the DiD estimates, but including a doughnut in our regressions (removing the t-1 cohort) removed the difference between the RD and DiD estimates. Our LPM and Cox DiD and RD estimates are in absolute terms very close to one another. However we test if controlling for the t-1 cohort affects our conclusions in column (8) of tables B.4 and B.5 in the appendix and the conclusions remain the same for the 8 year reform and for the 9 year reform using DiD. Using RD we find that our LPM results show a mortality reducing effect, however not for the Cox regression results suggesting this is not robust to modelling strategy. We conclude that we find no evidence of a causal effect of years of schooling on mortality and that this general result is robust to modelling choices, type of school reform and identification strategy.

Table 6: Cox proportional hazard estimates of survival till 2013

		8 YEAR REFORM		9 YEAR REFORM	
	COX-PH (1)	COX-DiD (2)	COX-RD (3)	COX-DiD (4)	COX-RD (5)
FEMALES AND MALES					
Reform Impact	0.9150***	0.9905	1.0136	0.9888	0.9925
	(0.0059)	(0.0169)	(0.0190)	(0.0130)	(0.0160)
N	138,460	534,403	534,403	1,247,808	1,247,808
No. Deaths	30,633	71,640	71,640	120,382	120,382
FEMALES					
Reform Impact	0.9013***	1.0112	1.0285	0.9970	0.9828
	(0.0094)	(0.0296)	(0.0319)	(0.0206)	(0.0254)
N	67,737	264,237	264,237	613,317	613,317
No. Deaths	12,105	28,613	28,613	47,531	47,531
MALES					
Reform Impact	0.9561***	0.9790	1.0039	0.9821	0.9989
	(0.0080)	(0.0196)	(0.0264)	(0.0167)	(0.0176)
N	70,723	270,166	270,166	634,491	634,491
No. Deaths	18,528	43,027	43,027	72,851	72,851

Notes: This table presents the impact of the compulsory school reforms on cause specific mortality. Each coefficient estimate is from a separate regression. Column (1) is from a Cox proportional hazard regression of the impact of years of schooling for those not treated and with pre or post reform minimum years of schooling. Columns (2) and (4) are regression results from out DiD specification which includes birth cohort fixed effects, municipalities that reformed in the same year level time trends stratified by municipality and a bandwidth of up to 10 years. Columns (3) and (5) are RD estimates and have separate second polynomials in the running variable either side of the cut-off and a full set of dummy variables for month of birth and gender and a bandwidth of up to 10 years. Robust standard errors clustered by municipality level (for DiD) and by the running variable (for RD) in parentheses. Testing the null of the coefficient: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$
Source: SIP. Own calculations.

5.1.3 Cause specific mortality by 2013

To test whether the reforms had competing impacts by cause of death that potentially offset each other or whether diseases more amenable to health related behaviours, specifically cancer, circulatory diseases and external causes show a response to the reforms we consider some leading causes of mortality by 2013. Figure 6 presents the raw data for the average within municipality relationship between birth cohort and cause specific mortality and there are no discernible jumps at the reform cut-offs.

In table 7 we present the Cox independent competing risks regression estimates for leading causes of mortality. The RF Cox estimates presented in columns (2-5) are all smaller than those found in column (1). For both the 8 year and 9 year reforms we find no cause specific impacts. The potential exception is the impact of the 9 year reform on deaths due to other causes. However, we only find a significant positive impact using DiD and a negative but insignificant impact using RD. It is therefore not robust to identification strategy.

Table 7: Cox proportional hazard independent competing risk results: Impact of the reforms on causes of mortality

		8 year reform		9 year reform	
	COX-PH (1)	COX-DiD (2)	COX-RD (3)	COX-DiD (4)	COX-RD (5)
Cancer					
Reform Impact	0.9526***	1.0221	1.0167	1.0232	0.9851
	(0.0103)	(0.0291)	(0.0280)	(0.0215)	(0.0289)
No. Deaths	11,831	28,255	28,255	45,079	45,079
Circulatory disease					
Reform Impact	0.8659***	0.9603	0.9776	1.0077	0.9747
	(0.0109)	(0.0334)	(0.0395)	(0.0298)	(0.0403)
No. Deaths	8,091	16,792	16,792	26,086	26,086
External causes					
Reform Impact	0.9199***	0.9578	0.9403	0.9483	0.9917
	(0.0258)	(0.0535)	(0.0690)	(0.0336)	(0.0464)
No. Deaths	1,527	5,650	5,650	13,834	13,834
All other causes					
Reform Impact	0.9101***	0.9772	1.0449	0.9399***	1.0270
	(0.0108)	(0.0291)	(0.0354)	(0.0218)	(0.0307)
No. Deaths	9,184	20,943	20,943	35,383	35,383
N	138,460	534,403	534,403	1,247,808	1,247,808

Notes: See notes for table 6
Source: SIP. Own calculations.

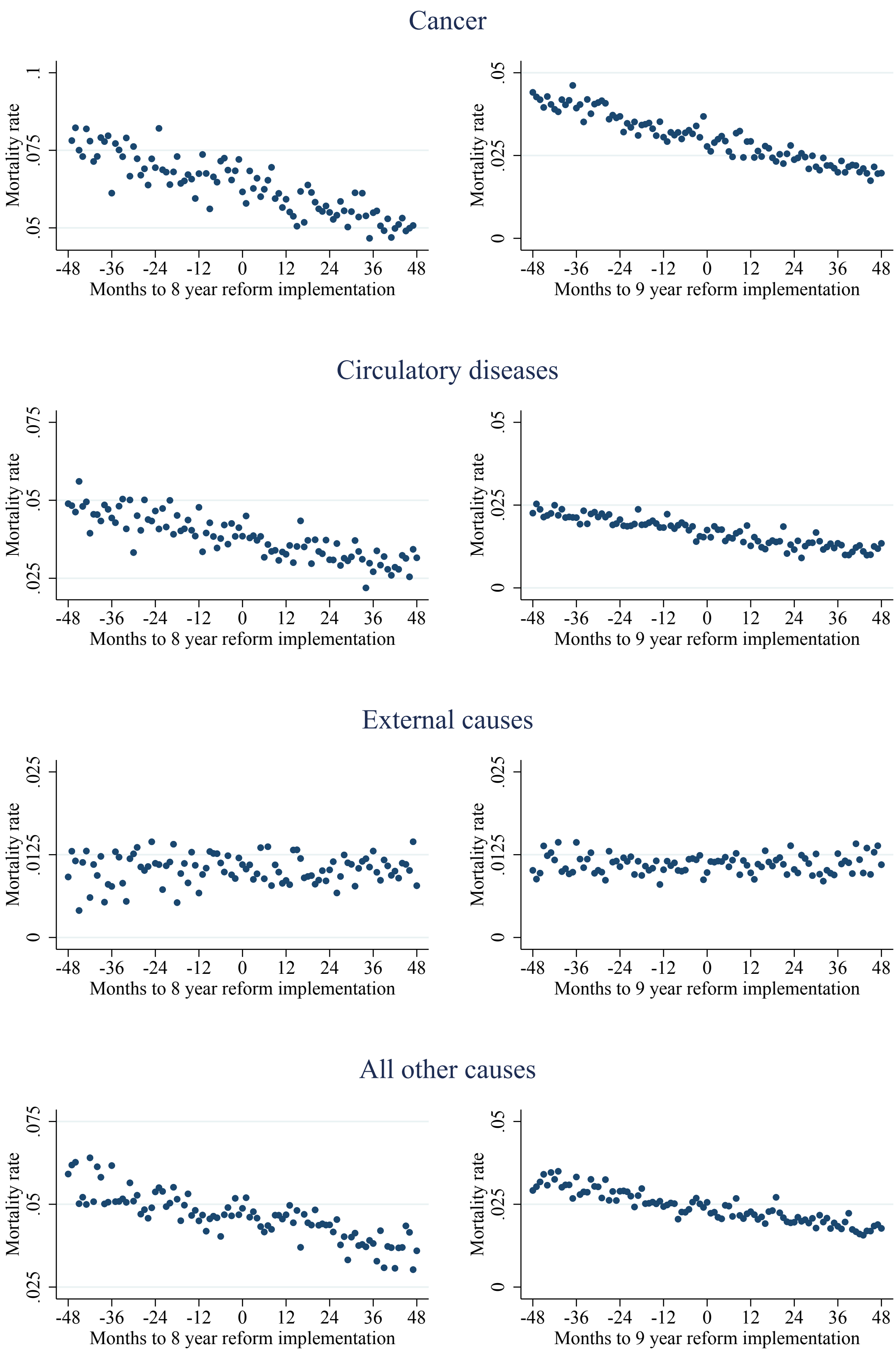

Fig. 6: Impact of the reforms on cause specific mortality by 2013

Notes: Scatter plots of cause specific mortality rate by age in months measured as the age difference of each individual from the first birth cohort in their municipality to be impacted by the reform (the first cohorts to be impacted are at zero).
Source: SIP. Own calculations.

5.2 *Hospital admissions*

5.2.1 All cause days admitted to hospital

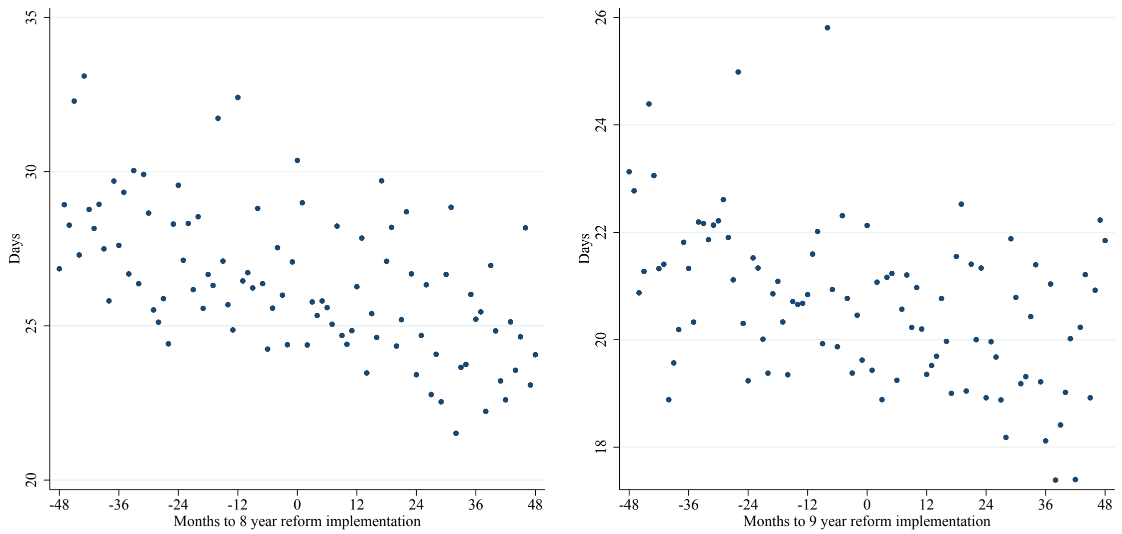

Fig. 7: Impact of the reforms on days admitted to hospital by 2012

Notes: This figure plots the relationship between birth cohort and first cohort impacted by the reform and days admitted to inpatient care (a stay over-night). See notes for figure 5.
Source: SIP. Own calculations.

In this section we assess the impact of the school reforms on a potentially more sensitive measure: inpatient hospital admissions. Specifically, days admitted to inpatient hospital care and cause specific inpatient hospital admissions are considered. In the previous section we found no impact on overall mortality over the observation period. This means we can assess other health measures without concern for mortality impacts affecting our results. Figure 7 presents the relationship between age relative to the first reform cohort and days admitted to hospital. We find no visually discernible impact of the reforms on days admitted to hospital at the reform cut-offs (0 represents the first cohorts in the municipality impacted by the reforms).

In table 8 we present the regression results for days in hospital for both reforms. Column (1) of table 8 shows the simple association of years of education for those not treated and with pre or post reform minimum years of schooling. There is a substantial and highly significant negative relationship between years of schooling and days admitted to hospital. The OLS results suggest that for an additional year of schooling, individuals will have about 1.5 fewer days at hospital, or a 6 to 7% reduction. The 8 year reform reduced form point estimates using both DiD and RD are found in columns(2) and (4) and are equal to 0.27 and 0.85 days for DiD and RD respectively (relative impacts of 1.1% and 3.4% respectively) and are insignificant, small and positive. The 8 year reform IV point estimates are 1 and 3.7 days for DiD and RD respectively and also insignificant (increases of 4.1% and 14.9% respectively). The reduced form point estimates for the 9 year reform are -0.07 and 0.2 days for DiD and RD respectively and insignificant. The corresponding IV estimates report increases of -0.6% and 2.4% and are substantially smaller than the OLS correlations. Although both our 8 year and 9 year reform based IV estimates are quite different to the OLS estimates, we are unable to reject the OLS estimates.

5.2.2 Cause specific hospital admissions

Hospital admissions can occur for a variety of reasons and differences in education levels may push the quantity of medical care use in different directions. For example particular admissions due to health shocks are potentially more likely amongst those who have invested less in their health which may be a function of having received less education. On the other hand admissions that are in themselves health investments such as screening and examinations or preventative care or early detection may be a function of having received more education. To assess if there are counteracting effects of education we consider three leading reasons for hospital admission: cancer, circulatory diseases and external causes. In figure 8 we present the raw data as scatter plots of the probability of admission due to specific causes by age relative to the first cohort impacted by the reform in each municipality. Eyeballing the data, there are no obvious jumps at the reform implementation cut-offs in the hospitalisation rates by cause.

In table 8 column (1) we see that the association of years of education is negative and significant for circulatory diseases, external causes and all other causes but not for cancer. We test for jumps using RD in table 8 alongside DiD regression and our reduced form estimates (columns (2) and (4)). The results using the 8 year reform find no significant impact of the reform on the probability of inpatient care due to the specific causes we consider except for external causes, where using RD our IV estimate of -1.6 percentage points is nearly four times as large as our OLS estimate and twice as large as our DiD IV estimate. The results using the 9 year reform find evidence of an impact of the reform on the probability of inpatient

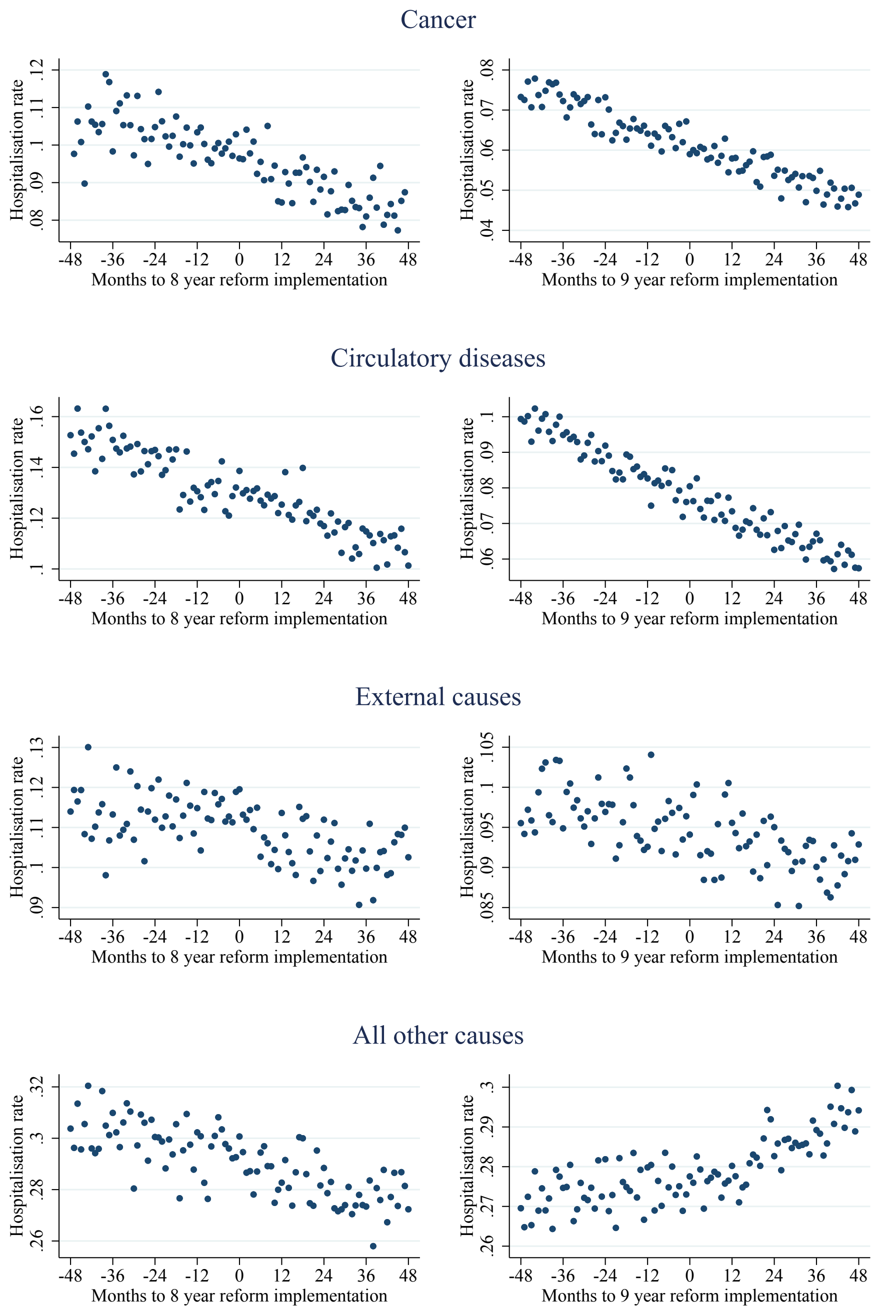

Fig. 8: Impact of the reforms on probability of hospital admission by cause by 2012

Notes: Scatter plots of the probability of hospital admission by age in months measured as the age difference of each individual from the first birth cohort in their municipality to be impacted by the reform (the first cohorts to be impacted are at zero).
Source: SIP. Own calculations.

care, through a reduction in other causes related admissions of -1.5 percentage points using our IV estimate. We should note that we make no adjustment for multiple hypothesis testing here and any attempt to do so would remove any significance we have found here. In general we do not find convincing evidence of an impact of education on cause specific inpatient care that is robust to identification strategy.

5.2.3 Sensitivity analysis of impact on hospital admissions

In the appendix table B.6, we test the sensitivity of these results to bandwidth choice and inclusion and removal of linear trends in the DiD regression specification. In table B.6 we estimate the reduced form results for days of inpatient care of table 8 varying the bandwidth between 2 to up to 12 years (see columns (1) - (7) in table B.6). Compared to the results for mortality and years of education, the inpatient days results are more sensitive to bandwidth choice but the choice of a 10 year bandwidth does not impact the conclusions. The exclusion of municipality specific linear trends in our DiD analysis makes little difference to the estimates. Inclusion of a dummy for the t-1 cohort increases the coefficient size across all model types and both reforms, but also the standard errors. In summary we conclude that there is no evidence of an impact of either reform on hospital admissions.

Table 8: Linear regression results: Impact of the reforms on inpatient hospital admissions

	Mean (1)	OLS (2)	RF-DiD (3)	IV-DiD (4)	RF-RD (5)	IV-RD (6)
Panel A: 8 Year Reform						
Days at hospital	24.71	-1.4768***	0.2745	1.0083	0.8469	3.6832
		(0.3283)	(0.4855)	(1.7969)	(0.7659)	(3.3420)
Probability of hospital admission due to:						
Cancer	0.08	-0.0008	0.0004	0.0014	0.0003	0.0014
		(0.0010)	(0.0017)	(0.0064)	(0.0017)	(0.0074)
Circulatory diseases	0.11	-0.0101***	0.0022	0.0082	0.0024	0.0106
		(0.0012)	(0.0017)	(0.0064)	(0.0024)	(0.0107)
External causes	0.10	-0.0045***	-0.0023	-0.0085	-0.0036**	-0.0158**
		(0.0010)	(0.0019)	(0.0071)	(0.0018)	(0.0078)
Any other cause	0.29	-0.0128***	-0.0034	-0.0125	-0.0028	-0.0121
		(0.0015)	(0.0026)	(0.0095)	(0.0035)	(0.0151)
N		138,460	534,403	534,403	534,403	534,403
Panel B: 9 Year Reform						
Days at hospital	22.17	-1.6405***	-0.0707	-0.1354	0.2070	0.5287
		(0.1684)	(0.4180)	(0.7986)	(0.4980)	(1.2650)
Probability of hospital admission due to:						
Cancer	0.07	0.0005	-0.0004	-0.0007	-0.0009	-0.0024
		(0.0004)	(0.0009)	(0.0018)	(0.0011)	(0.0029)
Circulatory diseases	0.09	-0.0089***	0.0017*	0.0033*	0.0006	0.0015
		(0.0005)	(0.0010)	(0.0020)	(0.0014)	(0.0035)
External causes	0.10	-0.0051***	0.0005	0.0009	-0.0001	-0.0002
		(0.0005)	(0.0012)	(0.0023)	(0.0016)	(0.0040)
Any other cause	0.29	-0.0110***	-0.0010	-0.0019	-0.0061**	-0.0156***
		(0.0007)	(0.0017)	(0.0033)	(0.0024)	(0.0060)
N		397,961	1,247,808	1,247,808	1,247,808	1,247,808

Notes: This table presents the impact of the compulsory school reforms on inpatient hospital admissions. All coefficients are from separate regressions. See notes for table 5.

5.3 Health and health related behaviours

In this section we consider the impact of schooling on health outcomes and health related behaviours. These health measures are arguably more sensitive to the potential mechanisms in which education may influence health compared to mortality and hospital visits. So whilst mortality and hospital visits are outcomes that are objectively measured and available for the whole population, they require major health events to occur making them relatively insensitive measures of the impact of education on health. Even though we have found no impact on mortality or hospitalisations up to the age of 75, it is still possible that we may see an impact in more sensitive measures such as health behaviours or in self-reported measures of current health.

In table 9 we present the regression estimates of the school reforms on various self-reported health outcomes and health related behaviours using our survey data. Column (1) shows the simple OLS correlation estimates of years of education on health for those untreated and with years of education equal to or one more year than the legal minimum. Education is found to be associated with lower probability of having fair or bad health and a lower probability of being obese for our population. The cohorts we use in this analysis therefore exhibit the same positive education gradient in self-reported health and health related behaviours that is observed for mortality and hospital admissions in this paper and that has also been observed more widely in the literature.

The reduced form estimates are found in columns (2) and (4) of table 9. These are modelled in the same way as for mortality and hospital admissions with the addition of dummies for survey year but the removal of municipality specific trends. Even though we observe a strong and significant effect of both reforms on years of education we find significant impacts of neither the 8 year reform nor the 9 year reform on health or health behaviours.

Focussing on the health variables where there is a significant correlation observed in column (1) between education and health (fair or bad health and obesity), we find that the 8 year reform based IV results show quite large relative drops and in the same direction as the OLS estimates. Our DiD and RD based IV results for fair and bad health report relative drops of -30% and -13% respectively and are large in comparison to the OLS results of a relative effect of -6%. For obesity our DiD and RD based IV results find relative impacts of -47% and -6% respectively. However, our estimates are not precisely estimated and this is with over 30 years of survey data.[19]

The reduced form estimates for the 9 year reform using both DiD and RD on *fair or bad health* imply small positive effects of the reform which is in contrast to the OLS regression estimates. Incidentally, our results indicate that the findings of Spasojevic (2010) who finds a causal impact of the 9 year reform on self-reported health are not robust to model specification. Whilst we are using a different dataset, we mirror her analysis on our dataset. In her paper she only controls for cohort fixed effects (our column 1 results) and the results in column (2) show that controlling for differences across municipalities which are also correlated to background characteristics (see table 4) explain away her findings.

We test the sensitivity of these results to using a smaller bandwidth, found in the appendix,

[19]The 8 year reform is a strong instrument using RD but not quite so strong using DiD. See table B.2 in the appendix.

table B.7, and the results show our conclusions remain the same. We find no clear impact of the reforms on measures of self-reported health or health behaviours.

Table 9: Education effects on self-reported health and health behaviours

	OLS (1)	RF-DiD (2)	IV-DiD (3)	RF-RD (4)	IV-RD (5)
PANEL A: 8 YEAR REFORM					
Fair or bad health	-0.019*	-0.021	-0.063	-0.017	-0.028
	(0.010)	(0.018)	(0.056)	(0.027)	(0.043)
N	2,691	8,181	8,181	8,181	8,181
Mean	0.31	0.21	0.21	0.21	0.21
Smoke daily	-0.008	0.001	0.004	-0.004	-0.006
	(0.009)	(0.021)	(0.061)	(0.026)	(0.041)
N	2,661	8,138	8,138	8,138	8,138
Mean	0.24	0.26	0.26	0.26	0.26
Obese	-0.029***	-0.022	-0.052	-0.005	-0.007
	(0.009)	(0.019)	(0.048)	(0.030)	(0.035)
N	1,683	4,996	4,993	4,996	4,996
Mean	0.15	0.11	0.11	0.11	0.11
Anxiety, concern etc.	-0.004	-0.007	-0.018	-0.029	-0.033
	(0.011)	(0.022)	(0.055)	(0.032)	(0.037)
N	1,776	5,388	5,385	5,388	5,388
Mean	0.17	0.15	0.15	0.15	0.15
PANEL B: 9 YEAR REFORM					
Fair or bad health	-0.018***	0.005	0.010	0.004	0.012
	(0.004)	(0.011)	(0.021)	(0.016)	(0.045)
N	12,866	19,124	19,122	19,124	19,124
Mean	0.28	0.19	0.19	0.19	0.19
Smoke daily	-0.007*	-0.011	-0.021	-0.012	-0.032
	(0.004)	(0.014)	(0.025)	(0.016)	(0.045)
N	12,741	19,033	19,030	19,033	19,033
Mean	0.27	0.26	0.26	0.26	0.26
Obese	-0.010***	0.001	0.002	0.013	0.036
	(0.003)	(0.011)	(0.020)	(0.012)	(0.033)
N	7,935	11,514	11,496	11,514	11,514
Mean	0.13	0.09	0.09	0.09	0.09
Anxiety, concern etc	-0.001	-0.015	-0.026	0.009	0.026
	(0.004)	(0.014)	(0.023)	(0.018)	(0.054)
N	8,468	12,487	12,472	12,487	12,487
Mean	0.16	0.15	0.15	0.15	0.15

Notes: This table presents the impact of compulsory school reforms on self-reported health and health behaviours. See notes for table 5. Note that a full set of dummy variables for survey year are included in all regressions and DiD regressions are modelled without municipality trends.
Source: ULF-Survey. Own calculations.

6 Discussion

Our findings show that across two major school reforms that led to clear and substantial increases in years of education we observe only small and generally insignificant changes in mortality and other measures of health. Our IV point estimates for the 8 year reform find that an additional year of education yields a -0.01 and 1.2 percentage point change in mortality using DiD and RD respectively and the lower bound of our confidence intervals allows for a 1.7 percentage point reduction in mortality.

The research that most closely aligns to that of ours is that of Lleras-Muney (2005), Mazumder (2008) and Clark and Royer (2013). Our IV estimates are much smaller than those of Lleras-Muney (2005) and are more in line with the findings of Clark and Royer (2013) for Britain, and Mazumder (2008) for the USA. The findings of Clark and Royer (2013) are potentially the most convincing evidence gathered so far. However, there are concerns that the results of Clark and Royer (2013) are based upon two reforms in Britain that impacted very different cohorts (1947 and 1972) and both reforms were implemented overnight nationwide. The cohorts are likely to have been very different, both in terms of their own characteristics but also in terms of the health and labour market structures they were exposed to and there may have been large general equilibrium effects of a nationwide roll out of increased compulsory schooling that potentially reduce the earnings effect of these reforms. Our results from Sweden are based on two reforms, different in design, that were rolled out over time and with overlapping cohorts and evaluated using two different identification strategies and we still find only small or zero impacts on health. Our findings together with those of Clark and Royer (2013) suggest that the timing of the reforms and the nature of their roll out has little bearing on the results. Our own findings are also consistent across the two reforms and suggest it is not specific features of the Swedish compulsory school setting that explain the Swedish results because the two reforms were in fact quite different.

Whilst our findings for the 9 year reform are similar to those of Meghir et al. (2017), who also study the impact of the 9 year reform on mortality in Sweden, our results add a large sense of robustness to their findings. We have shown that previous estimates of the impact of schooling have been downward biased because of the measure of years of education used and in fact the 9 year comprehensive school reform had a much greater effect on years of education than previous estimates suggest. We have also extended the analysis of the 9 year reform to include self-reported measures of health and health related behaviours. The final and leading contribution of our paper is that we introduce another school reform that allows us to instrument the effect of education on health. The 9 year reform has been argued to not be a pure years of schooling reform (Meghir and Palme, 2005) and therefore analysis using the 9 year reform has to be kept to considering the reduced form impacts. This is not to say that analysis of the 9 year reform is not of interest, it is just that the theories that we are testing often relate to years of education (Grossman, 2015).

The results of this paper and of Meghir et al. (2017) stand in contrast with previous Swedish research of Lager and Torssander (2012) and Spasojevic (2010) who find small health improving impacts of the 9 year reform. In the appendix C we have attempted to replicate the results from Lager and Torssander (2012) who found a small but significant reduction in mortality due to the 9 year reform. Unfortunately we are unable to replicate their results exactly, but the analysis highlights how our conclusion is robust to their reform assignment and a different observation sample. We have also shown that we can repeat the significant finding of a causal impact of the 9 year reform on self-reported health outcomes of Spasojevic (2010) using a different and larger dataset. However, this result is not robust to a DiD or RD identification strategy.

The nature of our study is that we have been able to pin down quite a few variables that potentially explain the differences in impacts of compulsory school reforms found in the literature. We start with two reforms that both have a substantial impact on years of schooling. The reforms themselves were also different to each other and were rolled out in a way that concerns about resource shocks such as lack of teachers and schools and even general equilibrium effects that may be the case with the reforms in Britain, for example, do not apply in the same way to the Swedish reforms. That both reforms were rolled out during similar time periods further implies that the cohorts exposed to the two reforms later acted under the same welfare and labour market institutions. The clean comparisons of the two reforms further strengthen the internal validity of our findings. We have used a large dataset that includes a whole range of health outcomes. Different econometric methods have been used to estimate the impact on mortality. Two identification strategies have been used to assess the sensitivity of estimating across different sub-groups and under different identifying assumptions. Our finding of no or small mortality impacts of education are robust to modelling strategy, both LPM and Cox proportional hazard regression, identification strategy, either DiD or RD design, and school reform type.

The results presented here have strong internal validity. But how relevant are the findings outside of Sweden? Like the National Health Service (NHS) in Britain, the Swedish health care system has universal coverage and is publicly provided with access at the point of need paid for through taxation. Whilst universal provision removes the direct role of financial resources in determining health care quality, a channel which improved education could influence, there is still plenty of scope for the more educated to achieve better health. Any publicly provided health care system has to prioritise resources and more educated individuals are potentially more likely to be able to manipulate the system to their advantage, either through knowing how the system works, being more aware of the services that are available or through the ability to convince doctors of the need for treatment. Medical services also only in part determine health outcomes. Health behaviours and investments are also very important in determining health outcomes and these are also potentially impacted by education through better understanding and knowledge. Financial resources may also play a role in determining our health behaviours and health investments. To understand how education may impact health we need to understand which of these potential channels are channels that matter and this means the results of this paper, due to their high internal validity, are of importance.

The reform cohorts we have considered in this paper were born across the span 1938-1954. Like Clark and Royer (2013) who considered the school reform of 1947, some of these cohorts were impacted by the Second World War (WWII). Children in Britain during the war were moved out of the big cities and lived with members of the extended family or even volunteers in the countryside. Sweden was neutral during WWII and was much less affected in general and there were no specific policies to move children out of the large cities.[20] So whilst WWII was an unusual time, it is unlikely to have impacted the external validity of the results drawn here because life for children in Sweden during the war was largely unaffected.

We argue therefore that our results are not specific to the cohorts we consider. The results also help us understand which economic channels, if any, education has an impact on health. Our result, that we are not able to identify an effect of education on mortality,

[20]During WWII it was possible for schools to cancel classes in case of a threat. However, any time lost had to be caught up later on. Also if a teacher was called for military service a substitute teacher had to be called in. Historical sources suggest no educational disruptions in Sweden during the period of WWII (see Bhalotra et al. (2016)).

hospital admissions or self-reported health, results that are internally very robust, is therefore an important finding and of relevance beyond Sweden.

7 Conclusion

The literature documenting the education gradient in health is vast yet the causal effect literature using compulsory school reforms as instruments for education has produced results that have been difficult to summarise. In this paper we have been able to pin down many of the potential explanations for differences in results across studies and have shown using a large dataset with a long follow-up period that the causal impact of education on mortality is small. This is also true for hospital admissions and other health measures. For mortality we can rule out impacts of years of education larger than -1.7 percentage points. These results hold across econometric technique and identification strategy. We argue that the conclusions we draw from the results of this paper are not specific to the Swedish context, rather they have a more general relevance.

Compulsory school reforms have provided a powerful way to assess the causal impact of education on health. They often impact a large population and can provide exogenous variation in years of schooling under certain parametric assumptions. They also impact a sub-population that is often a public health policy focus: the lowly educated who live short lives and have poorer health outcomes generally. However, the mounting evidence suggests compulsory schooling laws as policy levers for public health improvements may not be very effective. It is important to note that we do not conclude that education has no impact on health outcomes. Evidence has been found that compulsory schooling reforms can have an impact on health across generations (Lundborg et al., 2014). There is also hope found further up the education ladder, where evidence using the Vietnam draft as an instrument to incentivise college attendance in the USA (Buckles et al., 2016; Grimard and Parent, 2007; De Walque, 2007) has found a mortality reducing impact and an improvement in health related behaviours. Experience from the compulsory schooling literature suggests the results from the Vietnam draft induced college attendance need to be replicated for other populations, health outcomes and identification strategies before any firm conclusions can be drawn however.

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Appendix A - Background to the Swedish school reforms

In this appendix we provide some background information on the Swedish school system, the two school reforms and their interrelation used in this paper.

A.1 The Swedish School System

Long before compulsory schooling was introduced by law on a nationwide level in Sweden, a large fraction of the population had basic reading and writing capabilities, and a notable share of all parishes had introduced some kind of primary school on a voluntary basis (Lindmark, 2015). Regulations announced in the mid to late 19th century came to imply both the right to cost-free primary schooling and an obligation to take part in the schooling offered (Fredriksson, 1971). Specifically the 1882 legal statue of *Folkskolan* stated that every parish had to offer primary schooling by an approved teacher, that school attendance was compulsory for all children, and that children should start primary school the year they turn seven years old (Edgren, 2011).

The country was divided into school districts (generally corresponding to a parish, and later a municipality) and the local school board was responsible for the organization of elementary education. To overcome differences in content and format across school districts, a national central education plan was introduced in 1919 (Paulsson, 1946). These guidelines were published by the Ministry of Ecclesiastical Affairs and included time tables and syllabuses for compulsory schooling. The ministry also appointed school inspectors responsible for yearly evaluations of a number of school districts (Fredriksson, 1971). Completion rates were high and more than 90 per cent of all pupils finished compulsory schooling with full curriculum (Fredriksson, 1950).

In the 1920s elementary schooling in Sweden was compulsory for six years, but the central education plan provided curricula also for seven years of schooling and in 1920 a clause was introduced in the primary school code (paragraph 47 mom. 4) that stated a seventh school year could be made compulsory in a school district (Fredriksson, 1950). At the time Sweden applied a tracking system, where good performing students (defined by an assessment) could select to switch to an academic educational track and study at a four or three year long junior secondary school (*Realskola*) after the fourth or the sixth year of elementary education, respectively. The alternative was to continue and finish basic compulsory schooling. Attending junior secondary school allowed students to continue to higher secondary school (*Gymnasium*) which was a prerequisite for University. During the first half of the twentieth century, the Swedish school system was highly selective and the vast majority of people only completed compulsory education Björklund et al. (2004).[21]

In 1936 the national Government decided that a seventh year of schooling should be compulsory. The law came into force on July 1, and the decision to extend compulsory schooling by an extra year was taken by the school board of the school district. It was stipulated that seven years of schooling had to be implemented across the whole country over a twelve-year period, before 1949 (Fischer et al., 2013). The reform was seen as a new epoch, especially among teachers, because previously Folkskola had remained the same since 1882,

[21] See e.g.Centralbyrån (1977) for yearly numbers of students matriculating to lower and higher secondary schooling. For the cohorts of interest in this study matriculation to lower secondary schooling increased over time (16 percent of cohort 1938 and 30 percent of cohort 1951).

offering six years of compulsory education (Folkskollärarförbund, 1949).[22] With the bill of 1936, school districts were also allowed to introduce an eight year of compulsory schooling, but for this they needed to send in a formal application and to be given the king's consent.

A.2 The 8 Year Reform

With the start of the World War II, the Swedish political debate came to place a large focus on how to best foster democratic members of society. More education was seen as one of the main components for fulfilling this goal (Edgren, 2011). Thus, despite the on-going national implementation of the seventh year of compulsory schooling, a reform work was initiated and assigned to a new expert commission (*Skolutredningen* later replaced by *Skolkommissionen*) in 1940. This was the first governmental commission with a real mission to investigate primary and secondary education together, and with an aim to replace the tracking system with a unified comprehensive school system tying compulsory schooling closer to secondary education.[23]

Between 1940-1948 the commission continuously released reports evaluating the current school system and developing proposals and guiding principles for the future compulsory school (Marklund, 1982). Although the main focus of the commission's work was to postpone tracking decisions to higher grades, and by that improve equality of opportunity, and despite the on-going implementation of the seven-year compulsory schooling, there were also continued efforts to further extend compulsory schooling within the old system. The commission's work discussed an eight year extension of *Folkskola*, and in 1945 the Minister of Ecclesiastical Affairs proposed a bill introducing compulsory eight year schooling (without changing tracking options), but no action was taken by the Government (Fredriksson, 1971).

As stated above one of the main arguments for extending compulsory education was *democratic fostering*. This motive was not new. The 7 year reform was also motivated by fostering democracy including universal suffrage which was argued to place great demands on members of society, wherefore a solid education is necessary (Ecklesiastikdepartementet, 1935). The on-going war made this argument even more important in the debate. Specifically an eight-year extension was believed to improve student performance with respect to elementary skills in reading, writing and math, but also other subjects. An extension would also allow for the introduction of foreign languages (English) as a subject. In addition to theoretical arguments an extension was further justified by social and ethical arguments and that an eight-year could fill a supportive and nurturing role for young people that have not established on the labour market (SOU, 1945).

A second argument for extending compulsory education was induced by international *benchmarking* -- that Sweden was lagging behind (Waldow, 2013). Compared to other countries few students matriculated to higher levels of education, and the time spent in compulsory education was still quite modest. For example, compulsory education in the US endured at least until age 16, in Germany there were Volkschule or Hapuptschule until the age of 15 and in the UK students generally had nine years of compulsory schooling in the late 1930s.

[22]For a detailed review of the background and the implementation of the seventh compulsory year, see Fischer et al. (2013).

[23]Since the 1890's there had been a quite heated debate about the rational of the the so-called parallel system where student took different tracks. The main argument in the debate for that all students should have to complete the very same basic education before continuing to secondary schooling, was that it created inequalities (Morawski, 2010).

A third argument for extending compulsory schooling was the *increasing specialization of the labour market* and the *increased complexity* of society and societal life, implying a need to significantly increase educational goals of Folkskolan. Finally, the economic and societal *duality* that existed between urban and rural areas was brought forward to motivate a general 8 year reform or a compulsory school reform. Specifically with respect to education, the rural areas of the country was falling behind, e.g. smaller shares of students matriculating to junior secondary education in rural compared to urban areas (Centralbyrån, 1977). With a general implementation of the 8 year reform such differences could decrease.

The main arguments of the proponents of the 8 year reform to why the realization of an eight-year extension was seen as preferred compared to a comprehensive school reform was that there was (i) no large demand from students nor from the parties of the labour market for a 9 year comprehensive reform, and that (ii) the supply of teachers was too limited for a comprehensive reform, but also that the teachers generally had too limited education (SOU, 1945).

Likely spurred by the political debate some municipalities applied and got consent from the king and took the opportunity to implement a mandatory eight-year of Folkskola (Folkskollärarförbund, 1943).[24] The first two municipalities to implement an eight mandatory year were Kävlinge and Mariestad in the school year of 1941/42. The number of municipalities offering an eighth year gradually increases in the next-coming decade: In 1946/47 there were 33 and in 1958/59 207 municipalities, respectively. A characteristic of the municipalities introducing a mandatory eight-year in this time period is that they were urban and most of the larger cities of Sweden were early birds in this development. Consequently a quite large share of all students in the country had eight years of compulsory schooling: in the school year 1948/49 this was 16 per cent and in the school year 1951/52 this was 25 per cent (Folkskollärarförbund, 1952)

All municipalities introducing the eighth year followed the *main form* curriculum requiring full time reading and a teacher with an appropriate teacher degree.[25] Normative and binding curricula regarding the eight-year were missing in the early period, but the curriculum and hourly plan presented in the proposal of Skolutredningen in 1946 generally became the norm for the school districts that implemented an eight-year of Folkskola. The mandatory subjects in the eight grade were the same as in seventh grade, but local preferences could to some extent be met (Fredriksson, 1971).

A.3 The 9 Year Reform

In 1948, the expert commission proposed to replace the compulsory primary and the junior secondary school with a nine-year compulsory comprehensive school. The expert commission however wanted to evaluate the new school form before introducing it to all schools across the country. The reform was therefore introduced during an assessment period where the 9 year comprehensive school was introduced in different locations at different points in time.

[24] Only in a few cases a municipality did not get the permission to implement the extension. The reason was that the district asked to do an isolated change and only introduce the change in a separate school in a municipality (Fredriksson, 1971).

[25] The alternative to the main form were *exception forms*, characterised by half time reading or that the teacher did not have an appropriate teachers degree. In the early 1940's more than 90 percent of all pupils in Sweden went to a school that were assigned to the main forms (Fredriksson, 1950).

Starting from 1949/1950 the 9 year reform was rolled out at the municipality level.[26] For the first year of the roll-out of the reform 14 municipalities are selected to participate in the assessment. [27] The evaluation period was not run as a random experiment, but the National School Board chose the areas from a group of applicants to form a representative set based on observable municipality characteristics. Municipalities participating in the early assessment period were compensated with earmarked money from the central government for the increased costs following the expansion of mandatory education (Holmlund, 2008). After the assessment period, the national parliament decides to permanently introduce the 9 year reform to all schools the country in 1962. Seven years later, by 1969, all municipalities were obliged to have the new comprehensive school running (Marklund, 1982) and *Folkskolan* was fully disconuated.

The reform reshaped the entire school system and compared to the old tracking system students were kept in the same school type for nine years. Besides extending compulsory education from seven or eight to nine years and postponing tracking, the educational reform also came with a change in the national curriculum implying English and civics became a compulsory subject, but there were no major changes to the total number of hours or the distribution of hours taught in different subjects (Richardson, 1992).

The educational reform was also pedagogical. The commission proposal of of 1948 was very clear on that the traditional school and its working methods were obsolete. Specifically whole-group teaching and questions-response methods should be replaced by more individualized and activating elements, pandering students drive and independence (Marklund, 1982).[28]

Based on the principles of the final report of *Skolkommissionen* a new educational plan for schools to follow is released 1962 (Lgr 62). The pedagogical key concepts of the plan are individualization and activity learning (Larsson, 2011). The pedagogical fundament on the special position of the individual and that the school should foster independent individuals did not meet any major objections (Marklund, 1989). However the first reform municipalities experienced difficulties in getting accurate work material and text books (Marklund, 1982).

A.4 Comparing the Two reforms

Based on the above it is evident that Sweden experienced a continuous roll-out of extending the compulsory amount of schooling from 6 to 9 years over a period of 40 years, and that the 8 year reform and the 9 year reform were implemented across overlapping cohorts. On average the 8 year and the 9 year reform was 7 years apart in a municipality.

Both reforms introduced change in the extent of compulsory schooling. As regards the exact definition of treatment it however seems that the two reforms differ somewhat. Treated

[26]The comprehensive school system is introduced throughout the whole municipality, or in certain schools within a municipality. At the time there were 1037 municipalities in Sweden.

[27]Municipalities had to show interest in the reform and also report on various issues, such as e.g. population growth, local demand for education, tax revenues and school situation, and all municipalities that took part in the first year of assessment were required to have eight year comprehensive schooling. The 14 first-movers were selected out of 144 municipalities.

[28]The emphasis on the importance of the need of new working methods can also be assigned to the aim that education should foster democratic societal members. As discussed by Richardson (1978) there is also a change regarding the view of the individual in the late 1940's. The development of the individual now matters more than the societal development. An essential feature of the report by the commission is that that the school should be more pupil centred and less subject-matter oriented. Another novel perspective is the view that parents, not the school, are responsible for the pupil.

students of the 8 year reform faced no significant school system changes, nor any changes in working methods in class. Thus, any effects from the 8 year reform should mainly be driven by changes in the amount of time spent in education.[29] With the abolishment of the tracking system the 9 year reform implied a fundamental change of the complete school system and the reform also came with a new curriculum program and methods. Any effects from the 9 year reform can thus be driven by changes in the amount of time spent in education, by that the new system kept students together in the same school until the ninth grade, and/or changes in curricula, working methods and pedagogics.

As discussed above schools and teachers initially faced some problems in that they lacked appropriate teaching materials corresponding to the new curricula and teaching methods of the comprehensive school. According to Marklund (1982) teachers degrees of freedom with respect to novel and open-ended activities were also limited by that students and parents that wanted *Realskola* but instead had to undergo compulsory schooling in the comprehensive system, translated their ambitions and goals for the former school type to the latter. Together this suggest that the first part of the 9 year reform likely was more similar to the 8 year reform. Also the first period of the 9 year reform was more similar to the previous school system in the sense that most schools still streamed students into different classes according to their choices regarding languages or vocational training and harder and easier courses in some subjects (Marklund, 1982).

The two reforms were gradually implemented across municipalities. The timing of implementation in individual municipality was based on a mixture of local and national decisions. As regards the wider institutional context, we are unaware of any reforms that might have coincided with the 8 year or the 9 year school reform at the local level. During the assessment period of the 9 year reform it was only municipalities that showed interest in the reform that could be selected implying reform implementation was not random. Previous studies suggest that 9 year reform was implemented earlier in municipalities with higher incomes and with higher average education, see e.g. Lundborg et al. (2014). Regarding the 8 year reform the early-birds tended to be more urban and most of the larger cities implemented a mandatory eight year. Smaller municipalities followed and in the end more than half of all municipalities had introduced a mandatory 8th grade before implementing the comprehensive 9 year school reform.

A.5 Reform data and Validation

The reform data for the 9 year reform was generously shared by Helena Holmlund and we rely on a dataset as used in Hjalmarsson et al. (2015), of which an earlier version is described in detail in Holmlund (2008). The dataset encompasses information on the year a specific school district introduced the new comprehensive school.

While the 9 year reform has previously been used in several economic applications, this paper is the first to use the 8 year reform and the reform data on the timing of the year of introduction of the eight year in each municipality was purposively collected from archives and digitized by the authors. Various official sources provide aggregate information on the development of the implementation on the 8 year reform. To check the accuracy of the gathered reform data we perform checks to confirm that the collected information conform with aggregate official statistics. For example, information on the share of school districts in the

[29]See e.g. discussion by Orring et al. (1962) on that all earlier reforms than the 9 year reform more or less left the fundamental work of schools unaffected.

country that had eight years of compulsory schooling in certain years from Skolöverstyrelsen (1955) and from Centralbyrån (1977), respectively, suggest our data conform with aggregate statistics.

The decision to introduce eight years of compulsory schooling was made on the municipal level, and the assumption is that schools within the same district implemented the reform in the same year. Theoretically there could however be discrepancies between schools within municipalities. We believe the assumption is valid since official sources state that the change generally applied to a whole district (see e.g. Fredriksson (1971) and Skolöverstyrelsen (1955)). Moreover, aggregate figures on the share of all students taking on the extra year of compulsory education in certain years (Centralbyrån, 1977) suggest that there should be no major deviations from this rule.

Appendix B Online tables and figures

Table B.1: ICD codes used to define causes of death and hospitalisation

Diagnosis	ICD 10 code	ICD 9 code	ICD 8 code	ICD 7 code
Cancer	C00-D48	140-239	140-239	140-239
Circulatory disease	I	390-459	390-458	400-468
External causes	S,T,V,W,X,Y	800-999,E800-999	800-999,E800-999	800-999,E800-999

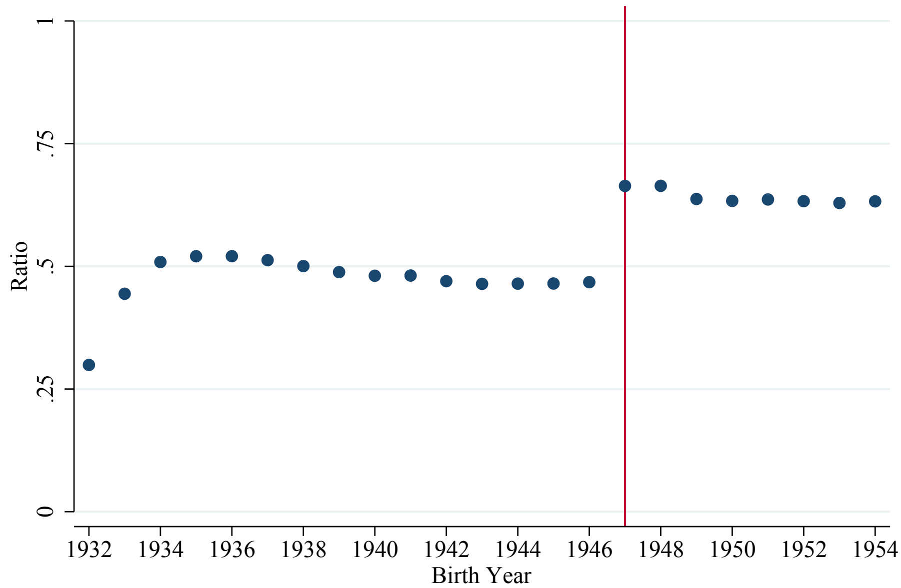

Fig. B.1: Correlation between place of birth and place of residence over time

Notes: Scatter plots of correlation between place of birth and place of residence as recorded in the 1960 census over time. The vertical line at 1947 indicates when place of birth was changed from being recorded as municipality of the hospital to being recorded as place of residence of the mother.
Source: SIP. Own calculations.

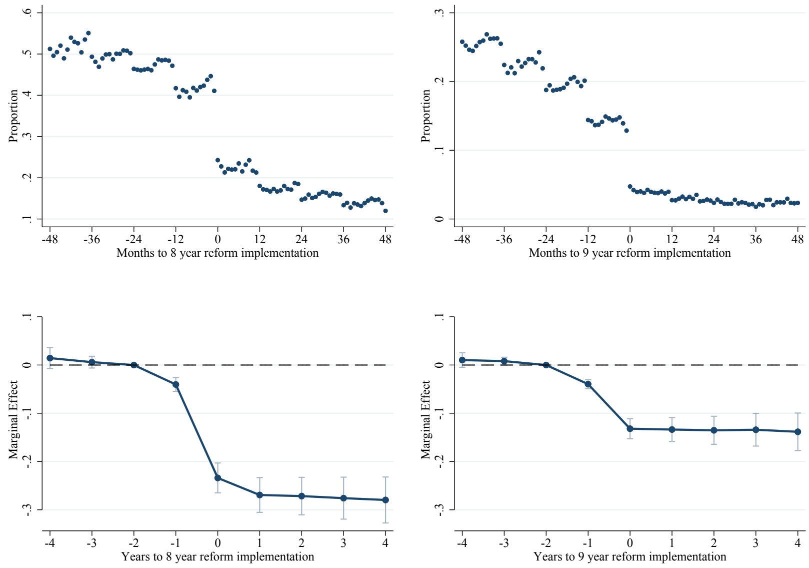

Fig. B.2: Impact of the reforms on leaving school with 7 years of old primary school

Notes: Top two panels: Scatter plots of proportion with 7 years of old primary schooling by age in months measured as months to reform implementation in their municipality where the first cohort impacted is at zero. The bottom two panels: plot regression coefficients of an individual's birth year relative to the first reform cohort in their municipality on proportion with 7 years of old primary schooling (spikes represent the 95% confidence interval for each coefficient estimate). Municipality and birth year fixed effects and municipality level time trends are controlled for, a bandwidth of 10 years is used and clustered standard errors are estimated at the municipality level. Category 4 is four or more years after the first reform cohort. The reference category is "two years before the first reform cohort" (t-2).
Source: SIP. Own calculations.

Table B.2: Compulsory schooling reforms' impact on education (ULF survey)

	(1)	(2)
	8 YEAR REFORM	9 YEAR REFORM
Difference in Difference	0.333***	0.530***
	(0.124)	(0.074)
F-stat	7.20	50.90
N	8,200	19,153
Regression Discontinuity	0.627***	0.355***
	(0.160)	(0.105)
F-stat	15.45	11.56
N	8,200	19,153

Notes: This table shows the impact of the 8 year and 9 year school reforms on years of education. Each coefficient is from a separate regression by reform, method and group. The DiD specification includes birth cohort, survey year and municipality fixed effects and an observation window of up to 10 years before and after the first cohort impacted by the reform. Regression discontinuity estimates have separate second polynomials in the running variable either side of the cut-off and a full set of dummy variables for month of birth, survey year and gender. Robust standard errors clustered by municipality level (for DiD) and by the running variable (for RD) are in parentheses. Testing the null of the coefficient: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$
Source: ULF-Survey. Own calculations.

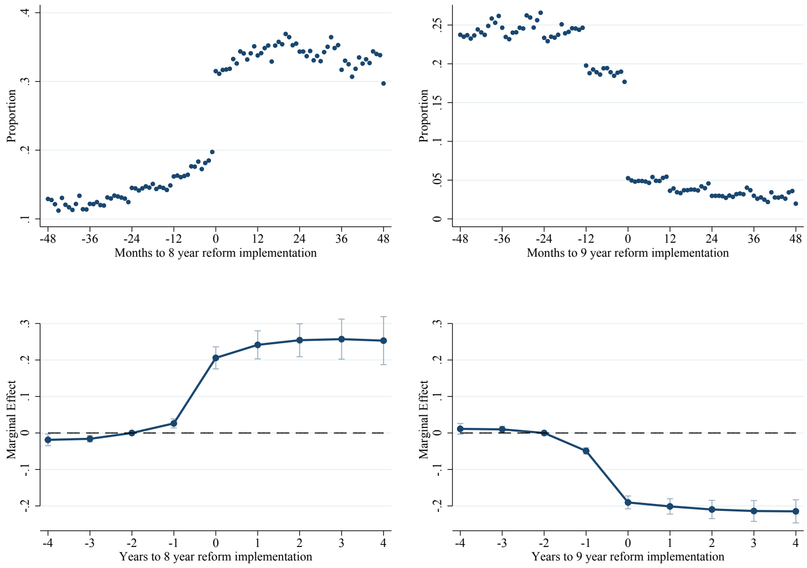

Fig. B.3: Impact of the reforms on leaving school with 8 years of old primary school

Notes: Top two panels: Scatter plots of proportion with 8 years of old primary schooling by age in months measured as months to reform implementation in their municipality where the first cohort impacted is at zero. The bottom two panels: plot regression coefficients of an individual's birth year relative to the first reform cohort in their municipality proportion with 8 years of old primary schooling. See notes for table B.2
Source: SIP. Own calculations.

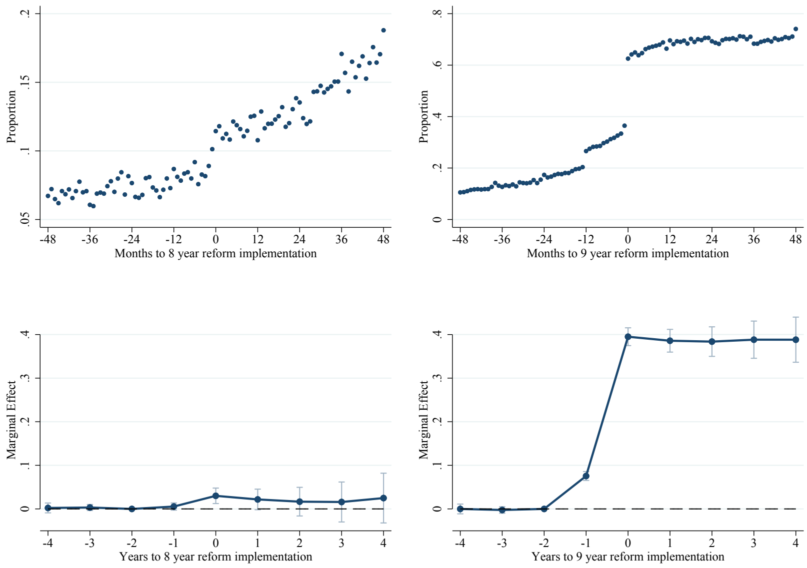

Fig. B.4: Impact of the reforms on leaving school with 9 years of old primary school or 9 years of comprehensive school

Notes: Top two panels: Scatter plots of proportion with 9 years of old primary schooling or new comprehensive schooling by age in months measured as months to reform implementation in their municipality where the first cohort impacted is at zero. The bottom two panels: plot regression coefficients of an individual's birth year relative to the first reform cohort in their municipality on 9 years of old primary schooling or new comprehensive schooling. See notes for table B.2
Source: SIP. Own calculations.

Table B.3: Sensitivity analysis: Reforms' impact on education

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
PANEL A: 8 YEAR REFORM								
DiD	0.223***	0.221***	0.245***	0.271***	0.281***	0.292***	0.033	0.310***
	(0.031)	(0.027)	(0.023)	(0.024)	(0.024)	(0.025)	(0.021)	(0.027)
DiD with municipality trends	0.159***	0.218***	0.242***	0.264***	0.272***	0.281***	0.031	0.294***
	(0.033)	(0.025)	(0.022)	(0.023)	(0.023)	(0.024)	(0.020)	(0.025)
RD	0.264***	0.241***	0.240***	0.225***	0.230***	0.204***	0.017	0.285***
	(0.047)	(0.036)	(0.028)	(0.025)	(0.023)	(0.022)	(0.025)	(0.030)
N	143553	251958	352728	447649	534403	608642	534403	506676
PANEL B: 9 YEAR REFORM								
DiD	0.353***	0.438***	0.485***	0.511***	0.533***	0.546***	0.306***	0.630***
	(0.022)	(0.018)	(0.018)	(0.017)	(0.018)	(0.019)	(0.014)	(0.020)
DiD with municipality trends	0.259***	0.402***	0.460***	0.500***	0.523***	0.538***	0.299***	0.615***
	(0.024)	(0.018)	(0.019)	(0.018)	(0.019)	(0.019)	(0.014)	(0.022)
RD	0.194***	0.231***	0.298***	0.357***	0.392***	0.435***	0.249***	0.529***
	(0.031)	(0.020)	(0.021)	(0.022)	(0.023)	(0.024)	(0.015)	(0.021)
N	332517	599339	856337	1070486	1247808	1384702	1247808	1180536
Bandwidth (years)	2	4	6	8	10	12	10	10
Admin. data based educ. measure							✓	
Drop $t-1$ cohorts								✓

Notes: This table presents sensitivity analysis of the impact of the reforms on years of education. Model (5) is as in table 3 and Models 1 to 6 are as model (5) but vary the bandwidth. Model (7) is as (5) but uses the administrative data based measure of years of schooling as used in prior research. Model (8) is as model (5) but is a sandwich/donut estimator, removing individuals up to one year too old to be eligible for the reform. Testing the null of the coefficient: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$
Source: SIP. Own calculations.

Table B.4: Sensitivity analysis: LPM estimates of impact of the reforms on mortality by 2013

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
PANEL A: 8 YEAR REFORM								
DiD	0.0016	0.0017	0.0018	0.0012	0.0005	-0.0001	-0.0010	0.0001
	(0.0035)	(0.0031)	(0.0027)	(0.0024)	(0.0021)	(0.0021)	(0.0020)	(0.0024)
DiD with municipality trends	0.0024	0.0004	0.0012	0.0002	-0.0000	-0.0003	-0.0015	-0.0001
	(0.0043)	(0.0034)	(0.0028)	(0.0025)	(0.0022)	(0.0022)	(0.0022)	(0.0027)
RD	-0.0078*	-0.0005	0.0000	0.0018	0.0012	0.0027	0.0067***	0.0042
	(0.0040)	(0.0040)	(0.0034)	(0.0032)	(0.0028)	(0.0025)	(0.0026)	(0.0037)
N	143553	198598	251958	352728	447649	534403	608642	534403
PANEL B: 9 YEAR REFORM								
DiD	0.0020	0.0022	0.0013	0.0001	-0.0006	-0.0002	0.0001	-0.0002
	(0.0019)	(0.0016)	(0.0015)	(0.0013)	(0.0012)	(0.0011)	(0.0010)	(0.0012)
DiD with municipality trends	0.0021	0.0024	0.0013	-0.0004	-0.0010	-0.0003	0.0001	-0.0005
	(0.0023)	(0.0018)	(0.0016)	(0.0014)	(0.0012)	(0.0011)	(0.0011)	(0.0012)
RD	-0.0014	-0.0007	0.0016	0.0031*	0.0018	-0.0016	-0.0041***	-0.0057***
	(0.0020)	(0.0019)	(0.0019)	(0.0016)	(0.0015)	(0.0014)	(0.0014)	(0.0020)
N	332517	466287	599339	856337	1070486	1247808	1384702	1247808
Bandwidth (years)	2	3	4	6	8	10	12	10
Dummy for t-1 cohorts								✓

Notes: This table presents sensitivity analysis of the LPM estimates of the reforms on death by 2013. Model (5) is as in table 5. See notes for table B.3. Testing the null of the coefficient: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Source: SIP. Own calculations.

Table B.5: Sensitivity analysis: Cox proportional hazard regression results of the reforms' impact on mortality

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: 8 Year Reform								
DiD	1.0155	1.0104	1.0114	1.0020	0.9942	0.9918	0.9856	0.9882
	(0.0296)	(0.0256)	(0.0225)	(0.0196)	(0.0172)	(0.0169)	(0.0163)	(0.0194)
DiD with municipality trends	1.0077	0.9995	1.0064	1.0003	0.9937	0.9905	0.9851	0.9894
	(0.0338)	(0.0264)	(0.0222)	(0.0197)	(0.0171)	(0.0169)	(0.0163)	(0.0194)
RD	0.9497*	1.0012	1.0074	1.0159	1.0119	1.0136	1.0210	1.0093
	(0.0297)	(0.0305)	(0.0261)	(0.0245)	(0.0210)	(0.0190)	(0.0185)	(0.0253)
N	143553	198598	251958	352728	447649	534403	608642	534403
Panel B: 9 Year Reform								
DiD	1.0266	1.0195	1.0061	0.9890	0.9839	0.9922	0.9982	0.9864
	(0.0245)	(0.0207)	(0.0182)	(0.0160)	(0.0143)	(0.0130)	(0.0126)	(0.0139)
DiD with municipality trends	1.0130	1.0148	1.0030	0.9872	0.9827	0.9888	0.9887	0.9844
	(0.0279)	(0.0216)	(0.0187)	(0.0160)	(0.0142)	(0.0130)	(0.0127)	(0.0138)
RD	0.9769	0.9906	1.0138	1.0269	1.0043	0.9925	0.9905	0.9754
	(0.0244)	(0.0234)	(0.0229)	(0.0199)	(0.0182)	(0.0160)	(0.0154)	(0.0192)
N	332517	466287	599339	856337	1070486	1247808	1384702	1247808
Bandwidth (years)	2	3	4	6	8	10	12	10
Dummy for t-1 cohorts								✓

Notes: This table presents sensitivity analysis of the Cox PH estimates of the reforms on survival till 2013. Model (5) is as in table 7. See notes for table B.3. Testing the null of the coefficient: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Source: SIP. Own calculations.

Table B.6: Sensitivity analysis: Reforms' impact on inpatient days admitted to hospital

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
PANEL A: 8 YEAR REFORM								
DiD	-1.0262	-0.5604	0.4691	0.7164	0.3372	0.2160	0.5121	0.4649
	(1.2257)	(0.9418)	(0.6379)	(0.5575)	(0.5086)	(0.4776)	(0.4835)	(0.6359)
DiD with municipality trends	-1.0365	-0.9562	-0.0454	0.4792	0.3770	0.2745	0.5651	0.6246
	(1.5446)	(0.9809)	(0.6723)	(0.5860)	(0.5329)	(0.4855)	(0.4839)	(0.6596)
RD	1.0865	0.2052	-0.4837	-0.2628	0.4911	0.8469	0.2609	1.9432*
	(1.1899)	(0.9675)	(0.9021)	(0.8472)	(0.8229)	(0.7659)	(0.7066)	(0.9906)
N	143553	198598	251958	352728	447649	534403	608642	534403
PANEL B: 9 YEAR REFORM								
DiD	0.4357	0.6149	0.0387	-0.0503	0.0622	-0.0561	-0.0273	0.0784
	(0.7895)	(0.7215)	(0.6081)	(0.5374)	(0.4671)	(0.4192)	(0.3890)	(0.4658)
DiD with municipality trends	-0.3990	0.6067	0.1252	0.0280	0.0887	-0.0707	-0.0244	0.0958
	(0.8890)	(0.7354)	(0.6252)	(0.5397)	(0.4661)	(0.4180)	(0.3877)	(0.4594)
RD	1.4492*	0.4467	0.1299	0.1559	0.1706	0.2070	-0.0244	1.1018*
	(0.7926)	(0.9088)	(0.7857)	(0.6385)	(0.5507)	(0.4980)	(0.4647)	(0.6199)
N	332517	466287	599339	856337	1070486	1247808	1384702	1247808
Bandwidth (years)	2	3	4	6	8	10	12	10
Dummy for t-1 cohorts								x

Notes: This table presents sensitivity analysis of the regressions estimates of the reforms on hospital admissions by 2013. Model (5) is as in table 8. See notes for table B.3. Testing the null of the coefficient: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$
Source: SIP. Own calculations.

Table B.7: Sensitivity analysis: Education effects on self-reported health and health behaviours for different bandwidth choice (5 year bandwidth)

	OLS (1)	RF-DiD (2)	IV-DiD (3)	RF-RD (4)	IV-RD (5)
Panel A: 8 Year Reform					
Fair or bad health	-0.028**	-0.017	-0.049	0.002	0.003
	(0.013)	(0.022)	(0.063)	(0.039)	(0.047)
N	2,857	5,047	5,045	5,047	5,047
Mean	0.31	0.22	0.22	0.22	0.22
Smoke daily	-0.012	0.006	0.017	-0.051	-0.060
	(0.010)	(0.023)	(0.061)	(0.031)	(0.037)
N	2,827	5,024	5,022	5,024	5,024
Mean	0.25	0.26	0.26	0.26	0.26
Obese	-0.025***	-0.011	-0.027	0.013	0.012
	(0.009)	(0.023)	(0.058)	(0.038)	(0.034)
N	1,799	3,092	3,070	3,092	3,092
Mean	0.14	0.11	0.11	0.11	0.11
Anxiety, concern etc	-0.004	-0.008	-0.021	-0.029	-0.025
	(0.010)	(0.028)	(0.067)	(0.036)	(0.033)
N	1,894	3,322	3,307	3,322	3,322
Mean	0.17	0.15	0.15	0.15	0.15
Panel B: 9 Year Reform					
Fair or bad health	-0.020***	0.006	0.011	-0.021	-0.079
	(0.004)	(0.014)	(0.027)	(0.016)	(0.074)
N	13,868	13,049	13,036	13,049	13,049
Mean	0.27	0.18	0.18	0.18	0.18
Smoke daily	-0.005	-0.025	-0.047	0.003	0.012
	(0.004)	(0.018)	(0.034)	(0.022)	(0.079)
N	13,731	12,988	12,975	12,988	12,988
Mean	0.27	0.28	0.28	0.28	0.28
Obese	-0.010***	0.006	0.014	0.014	0.071
	(0.003)	(0.015)	(0.033)	(0.017)	(0.090)
N	8,553	7,697	7,639	7,697	7,697
Mean	0.13	0.09	0.09	0.09	0.09
Anxiety, concern etc	-0.001	-0.006	-0.013	0.010	0.044
	(0.003)	(0.017)	(0.036)	(0.021)	(0.095)
N	9,126	8,417	8,363	8,417	8,417
Mean	0.16	0.15	0.15	0.15	0.15

Notes: This table presents the impact of compulsory school reforms on self-reported health and health behaviours. A 5 year bandwidth is used in DiD and RD regressions instead of 10 years as used in 9 to assess sensitivity to bandwidth choice. See notes for table 5.
Source: ULF-Survey. Own calculations.

Appendix C Reconciliation with Lager and Torssander (2012)

Our results differ to those of Lager and Torssander (2012) who also look at the impact of the 9 year reform on mortality in Sweden. They found a small and statistically significant mortality reducing effect of the 9 year comprehensive school reform for the population aged 40 plus. There are two obvious differences between our paper and theirs. In their paper they make some adjustments to reform assignment based on the observation that a large proportion of individuals in certain municipalities had the minimum years of schooling one or two years before the reform was officially implemented and adjust the reform assignment accordingly. The other major difference is that they have data only up to 2007 and make some slightly different sample restrictions. In table C.1 rows 1 to 4 we move slowly between our main results towards their specification, reform assignment and sample selection. Column (1) is for the sample aged 40 plus and column (2) is for all ages. In row 2 we, like them, model the hazard without trends. This has next to no impact on the coefficients or the standard errors. In row 3 we use their reform assignment code that they kindly shared with us. This increases the impact slightly. In row 4 we attempt to replicate the sample restrictions of Lager and Torssander (2012) (cohorts born between 1943-1955, observed in 1960 and 1965 censuses, followed up to 2007, not emigrated). The results are essentially the same as for row 3 but the standard errors have increased. Column 5 are the actual results from Lager and Torssander (2012) where they find a small but significant reduction in mortality due to the school reform. Their result is not substantially different from our row 4 replication, but we are unable to repeat their significant finding exactly or their sample size. What we can conclude is that the results of this current paper are robust to reform assignment methodology and sample selection and suggest that the impact of education on health is really quite small.

Table C.1: Replication of Lager and Torssander (2012): Cox proportional hazard estimates

	9 Year Reform	
	From aged 40	All ages
	(1)	(2)
Row 1*: own sample, own reform assignment, trends*		
9 year reform	0.9826	0.9888
	(0.0132)	(0.0130)
Row 2*: As per Row 1, no trends*		
9 year reform	0.9862	0.9922
	(0.0134)	(0.0130)
Row 3*: As per Row 2, LT (2012) reform assignment*		
9 year reform	0.9794	0.9859
	(0.0136)	(0.0133)
N	1,242,843	1,247,808
No. Deaths	115,417	120,382
Row 4*: As per Row 3, LT (2012) sample restrictions*		
9 year reform	0.9769	0.9896
	(0.0194)	(0.0160)
N	1,280,550	1,304,807
No. Deaths	73,909	98,166
Row 5*: Results from LT (2012)*		
9 year reform	0.96	0.98
	[0.93-0.99]	[0.95-1.01]
N	1,200,519	1,247,867
No. Deaths	65,329	92,351

Notes: This table presents the impact of the 9 year compulsory school reform on mortality. See text for details. Robust standard errors clustered by municipality level in parentheses. Testing the null of the coefficient: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. 95% confidence intervals in brackets.