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Who dies early? Education, mortality and causes of death in Norway

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ABSTRACT

We estimated the effects of education on mortality and causes of death in Norway. We identified causal effects by exploiting the staggered implementation of a school reform that increased the length of compulsory education from seven to nine years. The municipality-level education data were combined with complete records of all deaths from 1960 to 2015 from the Norwegian Cause of Death Registry. These data covered the entire life span of persons aged 16–64.

One additional year of education caused a reduction in mortality of about 10% for men. The effect was negligible for women. For men, a large part of the effect was due to fewer accidental deaths. We suggest two explanations for this finding. First, there are differences in risk-taking behaviour between people with a high level of education and those with a low level. Second, more education leads to upward occupational mobility. This mobility is mainly from occupations for which the risk of accidents is high to occupations for which the risk is low. Our results supported the fundamental cause theory. This is because education had a stronger effect on mortality for causes of death that are preventable than for causes of death that are not preventable. More education had no effect on the probability of dying of diseases that were amenable to medical intervention only. This gives some support to our results that patients are treated equally, independent of their level of education. This may be due to the large public involvement in financing and provision of health services.

1. Introduction

Differences in longevity correlate strongly with a host of social and economic indicators such as education, income and occupation (Cutler et al., 2006). Systematic differences in lifespan remain a persistent non-egalitarian feature of most societies, including the relatively wealthy and well-organized Nordic welfare states. Less well-educated people tend to die earlier (for a review see: Galama et al., 2018; Glymour and Manly, 2018; Hamad et al., 2018; Montez and Friedman, 2015). This is supported by the results from our study: Education has a sizable causal effect on mortality in Norway. For men aged 16–64, one additional year of education caused a reduction in mortality of about 10%. There was little or no effect for women. Accidents were the major cause of death for men with lower education, i.e. men with additional education were more likely to avoid hazardous situations.

The paper's key contribution is a comprehensive analysis of causes of death, which allowed us to disentangle several underlying mechanisms that link education and mortality. In particular, we were able to test some of the key predictions of the fundamental cause theory, which is commonly used to explain social inequalities in mortality (Link and Phelan, 1995; Mackenbach et al., 2015; Masters et al., 2015; Phelan et al., 2004). Following Phelan et al. (2004), we distinguished between preventable and non-preventable causes of death. Inequalities in mortality according to education were largest for causes of death that could be prevented; i.e. our results supported the fundamental cause theory. Recently, Mackenbach et al. (2015) introduced the distinction between preventable causes of death that were amenable to behavioural change only, as opposed to preventable causes of death that were amenable to medical intervention only. Using this classification we found that the main effect of education was on causes of death that are amenable to behavioural change only. There was no effect on the probability of dying as a result of diseases that are amenable to medical intervention only.

Our results are derived from a unique combination of Norwegian register data at the individual level. We have complete records of all deaths from 1960 to 2015 from the Norwegian Cause of Death Registry. We combined these data with municipality-level data on the staggered introduction of a school reform that extended compulsory education from 7 to 9 years during the period 1960–1972. Some municipalities implemented the reform early and others later. The control group

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comprised children born too early to have been exposed to the reform. The treatment group comprised children in the same municipality born late enough to have been exposed to the reform. We used the school reform as an instrumental variable for years of education, and estimated causal effects of education on mortality using a model with municipality and year fixed effects. Extensive balancing tests supported the assumption of random assignment of the introduction of the reform, and several robustness tests substantiated the study's key findings.

In the next section, we briefly describe the fundamental cause theory and discuss its implications within the context of the Norwegian health care system. We then describe the Norwegian school reform, our sample and present key descriptive statistics on education and mortality. In the subsequent sections, we outline the research design, and present the results. We then proceed by presenting results from tests of the identifying assumptions. Finally, we examine the relationship between education and causes of death and discuss our findings.

2. The fundamental cause theory - possible effects within a country with a large public health care sector

The key focus of the fundamental cause theory is to explain the persisting association between socioeconomic status and mortality (Link and Phelan, 1995; Mackenbach et al., 2015; Masters et al., 2015; Phelan et al., 2004). According to the theory, an individual's socioeconomic status provides him or her with several types of resources which can be used to avoid the risk of disease and/or to minimize the consequences of disease once it occurs. The following resources are important: money, knowledge, power, prestige and social connections. The availability of these resources is believed to be the key explanation why socioeconomic differences in mortality persist over time and place. Despite the popularity of the theory, there are few empirical studies in which the central claims have been tested.

According to Phelan et al. (Phelan et al., 2004; Phelan et al., 2010), the socioeconomic gradient in mortality is expected to be larger for causes of death that are under human control. This is because personal resources such as education and income can be used to obtain knowledge about health risks, preventive services and access to curative services. Conversely, causes of death for which little is known about prevention and the effectiveness of treatment are expected to be less sensitive to social influences. These causes of death cannot be avoided, even for individuals who belong to the upper socioeconomic groups. If this is correct, we would expect to find a stronger socioeconomic gradient in mortality for causes of death that are preventable than for causes of death that are not preventable. The few studies that exist give support to this claim (Phelan et al., 2004; Masters et al., 2012; Masters et al., 2015; Miech et al., 2011). Most of them are from the United States.

Phelan et al. (2004, 2010) do not distinguish between preventable causes of death that are amenable to behaviour change only, as opposed to preventable causes of death that are amenable to medical intervention only. This distinction was made by Mackenbach et al. (2015). He tested the fundamental cause theory in 16 European countries. An important finding was that, for the Nordic countries, the mortality rates for causes of death that are amenable to medical intervention only, were small in comparison to causes of death that are amenable to behaviour change only. In comparison to most of the other countries included in the study, the Nordic countries have a large public involvement in financing and provision of health services. For example, in Norway, public financing accounts for nearly 90% of total health care expenditure (Ringard et al., 2013). Nearly all health services are financed through taxes, and virtually everyone has free health care and equal access given equal need. Hospitals are publically owned and financed, with doctors who receive a salary. Primary physicians have a key role as gatekeepers for patients with regard to access to specialist services. Only primary physicians can refer patients, and patients do not get access to specialist services or for admission to hospital without a referral. All Norwegians have a statutory right to be on the list of a primary physician (Ringard et al., 2013; Grytten and Sørensen, 2007). Through a patient list system primary physicians assume medical responsibility for a well-defined population of patients. This system is meant to secure access to services and continuity of care. Nearly all inhabitants (99%) are on a list (Ringard et al., 2013). Several studies have shown that there are virtually no differences in access to primary physician and specialist services according to patients' level of education in Norway (Finnvold and Paulsen, 2002; Finnvold et al., 2005; Kaarboe and Carlsen, 2014; Statistics Norway, 2018a). Due to the large public involvement in health care in Norway, there may be less inequalities in causes of death for diseases that are amenable to medical intervention only than in countries with less public involvement. We tested this by using the classification criteria developed by Mackenbach et al. (2015).

3. The Norwegian school reform

In 1960, Norway started to implement a nationwide school reform to increase the length of compulsory education from seven to nine years. Municipalities decided when to implement the reform, with a deadline at the end of 1972. The gradual implementation of the reform meant that Norway, during a 12-year period, had two parallel school systems. Whether a particular child underwent seven or nine years of compulsory education depended on the municipality where he/she grew up and his/her year of birth. The first birth cohort for which a nine-year compulsory education was possible was that of 1947; the children in the last cohort to complete the old system were born in 1958. All children started school in the year they became seven, i.e. they were aged between six and a half and seven and a half when they started school. School entry occurs once a year in the middle of August and children are entitled to attend the nearest school in the municipality where they live.

Children finished compulsory education at the age of 14 in the old system, and 16 in the new system. The main effect of the reform was to increase the number of years of education (Lund, 1999). For further details about the reform, see Aakvik et al. (2010), Lie (1973) and Telhaug (1969).

We used the 1960 census to identify the municipality in which the child grew up (Statistics Norway, 1987). Statistics Norway provided this information as individual level data. Sources for the timing of the introduction of the reform in each municipality were the annual school year-books (Ness, 1971). We identified the timing of the reform in 706 of the 735 municipalities that existed in 1960 (Ness, 1971). The geographic variation in implementation is shown in Fig. 1. Many rural municipalities adopted the reform early. Municipalities with major cities implemented the reform later.

4. Sample and descriptive statistics

4.1. Sample

Our sample comprised individuals born between 1944 and 1951. We used the following criteria for deciding on these birth cohorts:

4.1.1. The lower age limit

The oldest people in our sample were born in 1944. These people were aged 16 in 1960. At the age of 16, they had all had the opportunity to complete compulsory education (7 or 9 years), depending on when the municipality had introduced the school reform.

4.1.2. The upper age limit

The youngest people in our sample were born in 1951. These people were aged 64 in 2015, if they were still alive. Our data file with The Norwegian Cause of Death Registry covers the years 1960–2015 (Norwegian Institute of Public Health, 2016). Those born in 1951 or

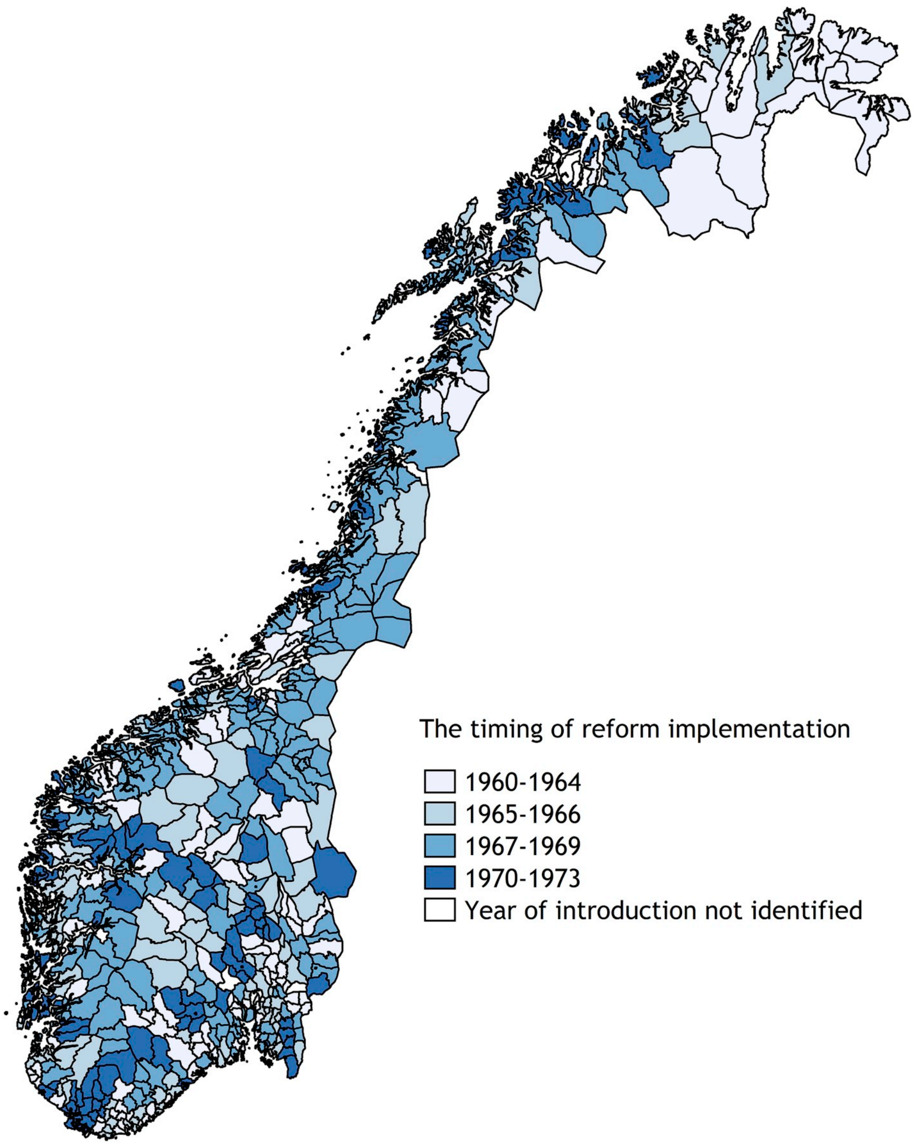

Fig. 1. Year of introduction of the Norwegian school reform.

earlier were registered alive at age 64, or dead if they had not lived to the age of 64.

The proportion of individuals exposed to the reform by year of birth is given in Appendix 1. For those born in 1951, nearly 30% were exposed to the reform. For those born earlier, the percentage of exposed individuals was lower.

4.2. *The proportion who died*

In Appendix 2, we show the proportion of deaths for men and women according to year of birth. The figure provides the basis for two comments: First, the proportion who died was markedly higher for men than for women. Second, the proportion of deaths was slightly higher for those born in the mid-1940s compared to those born in the early 1950s. In particular, this was the case for men.

The geographic variation in the proportion of deaths of individuals aged between 16 and 64 by municipality is shown in Appendix 3. The proportion is highest in rural municipalities in northern Norway and in municipalities with major cities in south-eastern Norway, and lowest in rural municipalities in western Norway.

5. Research design

We used the introduction of the reform as an instrumental variable to estimate a local average treatment effect of education on mortality. This reform variable has been used in several papers to study causal effects of education on the following outcomes: intergenerational transmission of education, family size, teenage births, mobility in the labour market, IQ and earnings, birth weight, periodontal treatment and cancer incidence (Aakvik et al., 2010; Black et al., 2005, 2007; 2008, 2010; Grytten et al., 2014; Grytten and Skau, 2017, 2018; Leuven et al., 2016; Machin et al., 2012). The reform dummy variable was specific for municipality and year of birth. The municipalities implemented the reform at different times. Thus, we were able to compare individuals in the same municipality who had 9 years compulsory education with those who had 7 years.

Let the subscript *imt* denote child *i* who grew up in municipality *m* and was born in year *t*. $R_{m(t)}$ equals 1 for children born late enough to be exposed to the reform, and 0 for children born too early to be exposed to the reform. Let E_{imt} be number of years of education for an individual *i* who grew up in municipality *m*, and was born in year *t*. The model includes municipality fixed effects (θ_m), year of birth fixed effects

(ϑ_t), and a random variable capturing other influences (ε_{imt}). The first-stage model is specified as:

$$E_{imt} = \alpha_0 R_{m(t)} + \theta_m + \vartheta_t + \varepsilon_{imt} \ ; \ t \le t^* \quad (1)$$

Let $\hat{E}_{mt}$ be the predicted number of years of education from the first-stage regression, and let D_{imt} be a binary variable indicating death of individual i who grew up in municipality m and was born in year t.

The second stage regression, where municipality fixed effects are denoted μ_m and year of birth is denoted ρ_t, is:

$$D_{imt} = \beta_0 \hat{E}_{mt} + \mu_m + \rho_t + \varepsilon_{imt} \ ; \ t \le t^* \quad (2)$$

Our results are also presented as reduced form estimates where the probability of death was regressed directly on the reform variable:

$$D_{imt} = \alpha_0 R_{m(t)} + \theta_m + \vartheta_t + \varepsilon_{imt} \ ; \ t \le t^* \quad (3)$$

The reform was introduced at the municipality level and encompassed all children and adolescents in the municipality. Therefore, we invoked the "Stable Unit Treatment Value Assumption" (SUTVA) at the municipality level. This assumes that there was no interference between municipalities, i.e. the reform did not induce families to move to another municipality. This issue has been examined by Lie (1973) and Telhaug (1969). They found no evidence that inhabitants moved to other municipalities when the reform was implemented. Peer group effects might generate within-municipality correlation. Therefore, we estimated the model with robust standard errors clustered at the municipality level.

6. Results

6.1. OLS estimates

Education had a negative coefficient on the probability of death (Table 1). The coefficient was larger for men than for women. For men, the probability of death decreased by 1.4 percentage points per additional year of education ($p < 0.001$). For women, the corresponding decrease was 0.8 percentage points ($p < 0.001$).

6.2. Reduced form estimates

The school reform caused a marked reduction in the probability of death. For men and women, the reform led to a decrease in the probability of dying by 0.9 percentage points (Table 1). In Norway, in the 1960s, each birth cohort comprised about 60 000 people. An implication of our reduced form estimates is that about 500 more of each birth cohort would have died between the ages of 16 and 64 had the reform not been introduced.

Similar to the OLS estimate, the reduced form estimate was nearly three times stronger for men than for women. For men, the probability of dying decreased by 1.3 percentage points for those who had 9 years of compulsory education compared to those who had not ($p < 0.001$).

Table 1
The effect of the school reform on the number of years of education and on the probability of dying between the ages of 16 and 64. Individuals born between 1944 and 1951. First and second stage regressions. Regression coefficients with standard errors clustered by municipality (in brackets).

Variables	Men and women	Men	Women
Ordinary least square			
Education (in years)	−0.0110 ***	−0.0144 ***	−0.0085 ***
	(0.0003)	(0.0004)	(0.0003)
Reduced form			
Reform = 1	−0.0092 ***	−0.0130 ***	−0.0051*
	(0.0024)	(0.0035)	(0.0026)
First stage estimates			
Reform = 1	0.42***	0.47***	0.37***
	(0.039)	(0.052)	(0.463)
F- value	112.4	81.9	63.8
Second stage estimates			
Education (in years)	−0.0133**	−0.0165**	−0.0081
	(0.0055)	(0.0069)	(0.0072)
Number of deaths	47 433	29 974	17 459
Total	451 920	232 852	219 068

***$p \le 0.001$; **$p < 0.05$; *$p < 0.10$.

6.3. First stage estimates

The first-stage estimates show that the reform led to an increase in education of less than half a year, marginally more for men and less for women (Table 1). The estimates were quite precise for all subsamples. The F-test statistics are well above the standard weak-instrument threshold (Stock et al., 2002). Further, the first stage coefficients are well within the range that Salvanes and co-workers report from their studies. Their estimates vary from 0.19 to 0.50, depending on the sample used for the analyses and model specification (Aakvik et al., 2010; Black et al., 2005; Monstad et al., 2008).

6.4. Second stage estimates

For men, the probability of death decreased by 1.6 percentage points for each additional year of education ($p < 0.001$) (Table 1). The proportion of men who died before the reform was introduced was nearly 15%. An implication of the second stage results is that one additional year of education led to a reduction in the proportion of deaths for men by about 10%. For women, the estimate is negative, but not statistically significant at conventional levels.

7. Test of identifying assumptions

An underlying assumption of our instrumental variable estimation is that the reform is independent of the potential outcomes of education and mortality. This assumption is met if assignment to the reform is as good as random. We address this conjecture in four ways.

7.1. Balancing tests

With random assignment, the control and treatment group should be balanced in terms of pre-treatment observables. We present differences in predetermined variables for children conditional on reform exposure. Since municipality of residence and year of birth determined whether children were educated in the old or the new school system, we hypothesize that whether children were educated in the reformed or non-reformed system was random. If selection is random, the two samples should be balanced in terms of observable and unobservable predetermined characteristics. We present the results of balancing tests using before and after data on parents' age when the reform was introduced, whether the parents died at the age of 64 or younger and the number of siblings for each child.

We regressed the reform variable (=1 if the child was exposed to the reform) against each of the predetermined variables described above. At conventional levels of significance, the reform had no statistically significant association with any of the predetermined variables (Table 2). Note as well that the regression coefficients are small in value, in particular in relation to the standard errors.

7.2. Different samples

Cohorts of children and adolescents that were close to the time when the reform was introduced may be more similar in terms of unobservable characteristics compared to cohorts of children and adolescents that were far from the time when the reform was introduced. We examined this by estimating reduced form models on different

Table 2
Balancing tests. Results from fifteen regressions. Individuals born between 1944 and 1951. Regression coefficients with standard errors clustered by municipality (in brackets).

Independent variable	Dependent variable: Reform = 1					
	Men and women		Men		Women	
	Regression coefficient	Mean (N)	Regression coefficient	Mean (N)	Regression coefficient	Mean (N)
Father's age when the reform was introduced	−0.0094	51.4	0.0348	51.5	−0.0741	51.1
	(0.0772)	(315 494)	(0.1015)	(180 420)	(0.0939)	(135 074)
Mother's age when the reform was introduced	0.0576	48.0	0.1033	48.1	0.0059	47.9
	(0.0740)	(336 068)	(0.0844)	(190 765)	(0.1018)	(145 303)
Whether the father died at the age of 64 or younger = 1	−0.0012	0.149	0.0037	0.150	−0.0072	0.148
	(0.0044)	(307 725)	(0.0055)	(176 177)	(0.0058)	(131 548)
Whether the mother died at the age of 64 or younger = 1	−0.0037	0.081	−0.0058	0.082	0.0013	0.080
	(0.0025)	(297 525)	(0.0035)	(169 320)	(0.0036)	(128 205)
Number of siblings	−0.0100	1.79	−0.0165	1.78	−0.0020	1.80
	(0.0113)	(350 895)	(0.0154)	(197 922)	(0.0162)	(152 973)

Note: Municipality fixed effects and child's year of birth included in all analyses.

samples, i.e. with different numbers of years on each side of the reform.

In the analyses with the larger samples, the estimates are slightly more precise than the estimates with the smaller samples (Appendix 4). However, the sizes of the estimates are similar, i.e. our results are robust across samples. Furthermore, the results support our findings shown in Table 1.

7.3. The timing of the introduction of the reform

Another assumption of our analyses is that the timing of the introduction of the reform was as good as random with respect to our response and exposure variables. In Appendix 5, we show results from two regressions in which individual-level data were aggregated at the municipal level. In the first regression, the response variable was defined as the proportion of deaths between the ages of 16 and 64. In the second regression, the response variable was defined as the mean number of years of education. The key explanatory variable was the year the reform was introduced.

The year the reform was introduced had no statistically significant ($p < 0.05$) effect on our two response variables and the regression coefficients were small. This indicates that the results reported in Table 1 are not biased due to correlation between the timing of the introduction of the reform and the response and exposure variables.

7.4. Unobservable variables – a placebo test

An advantage with our data is that it was possible to check whether our main results were biased due to unobservable variables. We carried out a placebo test in which we redefined the reduced form regression to capture pre- and post-reform effects. The pre-reform effects were measured using lead variables, and the post-reform effects were measured using lag variables (for definitions see Appendix 6). We did not expect the lead variable to have any significant positive effect on the outcome. This is supported by the results. The size of the regression coefficients was small (Appendix 6). These results were in clear contrast to the effects of the lag variable. The coefficients for the lag variables were of a reasonable size, they had the correct sign (positive), and the value 0 was not contained in the 95% confidence interval. The estimate for the variable measuring the contemporaneous effect was about the same size as the reduced form estimate in Table 1.

8. The causes of death

Having established a relationship between education and mortality, we turn to the question of what accounts for this relationship. Following the criteria developed of Mackenbach et al. (2015) and Phelan et al. (2004), we classified causes of death into the following groups:

1. Deaths that were amenable to behavioural change only
2. Deaths that were amenable to medical intervention only
3. Deaths that were amenable to both behavioural change and medical intervention
4. Deaths that could not be prevented either by behavioural change or medical intervention
5. Deaths caused by accidents
6. Deaths that could not be classified according to whether they could be prevented.

The classification was carried out using ICD-7 to ICD-10 from the Norwegian Cause of Death Registry (Norwegian Institute of Public Health, 2016). We were able to classify 72% of all causes of death using the criteria of Mackenbach et al. (2015). The criteria they used for classification are described in detail in Appendix 7. An additional 9% of causes of death were classified according to the criteria of Phelan et al. (2004) (for details see Appendix 7). Thus altogether 81% of all causes of death were classified.

We applied a multinomial logit model to estimate reduced form effects of the school reform on the probability of dying in each of the groups. Individuals who were alive were defined as the reference group. The response variable D_{imt} has seven values (x), the reference group *alive* and the six causes of death (x = 1, 2 … 6). This leads to the multinomial regression model.

$$ln\,\frac{P(D_{imt}=x)}{P(D_{imt}=Alive)} = \phi_0^x R_{m(t)} + \theta_m^x + \vartheta_t^x \;;\quad t \le t^* \tag{4}$$

The reduced form estimate is largest for deaths caused by accidents (Table 3). Deaths from accidents include accidental falls, accidental poisoning and transport accidents. The regression coefficient indicates that the reform led to a reduction of 0.41 percentage points in such deaths. The effect is nearly 6 times larger for men than for women.

For the other causes of death that were amenable to behavioural change only, and to deaths that were amenable to both behavioural change and medical intervention, the reduced form estimates were negative and of reasonable size. However, they were not statistically significant at conventional levels (Table 3). In order to test the fundamental cause theory, and using similar classification criteria as Phelan et al. (2004) and Mackenbach et al. (2015), we carried out two types of analysis.

In the first analysis, we collapsed groups 1, 2 and 3 into one group. This new group included all causes of death that according to Phelan et al. (2004) could be prevented. The reduced form estimate is of a

Table 3
The effect of the school reform on causes of death. Individuals born between 1944 and 1951. Marginal effects with standard errors clustered by municipality (in brackets). Independent variables kept at their mean values.

Cause of death	Men and women		Men		Women	
	Marginal effects (standard error)	N	Marginal effects (standard error)	N	Marginal effects (standard error)	N
Alive	0.0087*** (0.0024)	411 550	0.0134*** (0.0033)	205 181	0.0032 (0.0025)	206 369
Deaths that were amenable to behavioural change only = 1	−0.0010 (0.0008)	9355	−0.0012 (0.0013)	5992	−0.0006 (0.0009)	3363
Deaths that were amenable to medical intervention only = 1	0.00004 (0.0006)	5007	−0.0002 (0.0005)	1989	0.0002 (0.0008)	3018
Deaths that were amenable to both behavioural change and medical intervention = 1	−0.0016 (0.0012)	12 916	−0.0025 (0.0019)	9147	−0.0005 (0.0010)	3769
Deaths that could not be prevented either by behavioural change or medical intervention = 1	0.0003 (0.0006)	7876	0.0002 (0.0010)	4105	0.0004 (0.0012)	3771
Deaths that could not be classified according to whether they could be prevented = 1	−0.0023** (0.0009)	9808	−0.0030** (0.0013)	6278	−0.0014 (0.0011)	3530
Accidents = 1	−0.0041*** (0.0068)	5781	−0.0066*** (0.0014)	4796	−0.0013** (0.0008)	985
Total		462 293		237 488		224 805

***p ≤ 0.001; **p < 0.05.

reasonable size (Table 4). The regression coefficient indicates that the reform led to a reduction of 0.29 percentage points for preventable deaths (p = 0.07). For causes of death that could not be prevented, the estimate was small, and far from being statistically significant at conventional levels. These results give support to the fundamental cause theory.

In the second analyses, we collapsed groups 1 and 3 into one group. Causes of death that were amenable to medical intervention only (group 2) were kept in a separate group (Mackenbach et al., 2015). For these causes of death the estimate was small, and far from being statistically significant at conventional levels (Table 4). Further, the estimate was fairly precise with a 95% confidence interval in the range 0.0012 to −0.0011. These results give support to the finding of Mackenbach et al. (2015), that in countries with a large public involvement in health care, there are less inequalities in causes of death for diseases that are amenable to medical intervention than in countries with less public involvement. Partly, this may be because with public involvement, health services are more equally accessible. We tested this conjecture, using data from the Survey of Living Conditions 2002 (Hougen and Gløboden, 2004).

The survey was a cross-sectional study carried out by Statistics Norway in 2002. The sample was drawn from a population consisting of

Table 4
The effect of the school reform on causes of death. Testing the fundamental cause theory. Individuals born between 1944 and 1951. Men and women. Marginal effects with standard errors clustered by municipality (in brackets). Independent variables kept at their mean values.

Cause of death	I		II	
	Marginal effects (standard error)	N	Marginal effects (standard error)	N
Alive	0.0090*** (0.0025)	411 550	0.0090*** (0.0025)	411 550
Deaths that were amenable to behavioural change only, to medical intervention only or to both behavioural change and medical intervention = 1	−0.0029* (0.0017)	27 278		
Deaths that were amenable to behavioural change only or to both behavioural change and medical intervention = 1			−0.0030* (0.0017)	22 271
Deaths that were amenable to medical intervention only = 1			0.00004	5007
Deaths that could not be prevented either by behavioural change or medical intervention = 1	0.0003 (0.0007)	7876	0.0003 (0.0007)	7876
Deaths that could not be classified according to whether they could be prevented = 1	−0.0023** (0.0009)	9808	−0.0023** (0.0009)	9808
Accidents = 1	−0.0041*** (0.0068)	5781	−0.0041*** (0.0068)	5781
Total		462 293		462 293

***p ≤ 0.001; **p < 0.05, *p < 0.10.

all people living at home aged 16 and older. The survey contained data about number of visits during the last 12 months to primary physicians and to specialists working outside the hospital. For hospitals, there was data about the number of outpatient visits during the last 12 months, and whether the individual had been admitted to inpatient care during the last year. We ran several regression analyses where these variables were used as output measures with level of education as the independent variable. Several control variables were included in the regressions (for details see Appendix 8). The results are presented for the whole sample (birth cohort: 1901–1986) and for individuals born during the period 1994 to 1951. A consistent finding in all the analyses was that level of education had no statistically significant effect on any of our outcome measures. These results support previous studies, which have shown that there are virtually no differences in access to primary physician and specialist services according to patients' level of education in Norway (Finnvold and Paulsen, 2002; Finnvold et al., 2005; Kaarboe and Carlsen, 2014; Statistics Norway, 2018a).

The analyses we carried out on the data from the Survey of Living Conditions 2002 were descriptive. Thus they may not reflect a causal relationship. Bias may arise because the estimation does not take account of unobserved variables that are correlated with both education and access to care. A common cited unobserved variable is ability (Grossman, 2006; Oreopoulos and Salvanes, 2011). People with a high level of ability will most likely have a high level of education, and also seek medical care when necessary. As ability is positively correlated with both education and medical treatment, omission of ability from the estimation would lead to an upward bias of the OLS estimate. Thus the coefficients reported in Appendix 8, might have been even smaller if we had been able to take unobservable variables into account in the estimation. This was not possible, partly because the data were cross-sectional, and partly because we were not able to identify any valid instrumental variables.

9. Discussion

9.1. Education and mortality – discrepancies across studies

During the last decade, the use of compulsory school reforms has become a usual method for estimating the causal effects of education on health and health-related behaviour. This effect has been identified using either an instrumental variable framework or a regression discontinuity design. The studies show conflicting results, even when the same identification strategy has been used (for a review see: Galama et al., 2018; Glymour and Manly, 2018; Grossman, 2015; Hamad et al., 2018; Montez and Friedman, 2015).

Conflicting results have been found in studies performed in the USA, the UK and Sweden, both between and within countries. In some studies from these countries, a causal effect of education on mortality has been found (Cao et al., 2014; Davies, Dickson, Smith, Van den Berg and Windmeijer, 2018; Fischer et al., 2013; Fletcher, 2015; Glied and Lleras-Muney, 2008; Lleras-Muney, 2005). On the other hand, there are also often quoted studies from the USA, the UK and Sweden in which no causal effect has been found (Black et al., 2015; Clark and Royer, 2013; Lager and Torssander, 2012; Mazumder, 2008; Meghir et al., 2018). In a large study encompassing compulsory school reforms in 18 European countries, Gathmann et al. (2015) found that more education led to a reduction in mortality for men, but not for women. This finding is supported by our results. In some studies, health outcome measures such as self-reported health, body mass index, long-term illness, dementia, hypertension and diabetes have been used. Typically, the results are mixed (Arendt, 2005; Braakmann, 2011; Fletcher, 2015; Kemptner et al., 2011; Li and Powdthavee, 2015; Nguyen et al., 2016; Silles, 2009; Zhong, 2015).

In reviews, results have been compared across studies (Galama et al., 2018; Glymour and Manly, 2018; Grossman, 2015; Hamad et al., 2018; Montez and Friedman, 2015). It is not possible from these reviews to fully explain why results vary between and within countries. This is partly because the published papers often contain too little information or lack the relevant type of information for comparisons to be made. In their review, Galama et al. (2018), conclude: "the lack of uniformity makes it very challenging to summarize and compare findings, particularly the magnitudes of their effects". We have experienced the same, and have not succeeded in explaining in a meaningful way why the results from our study are different from the results in the studies in which a causal effect of education on mortality has not been found.

Our outcome variable is a measure of early mortality, and thus may represent a selected set of causes of death. The estimates in Table 1 may have been different given a longer observation period. There are few studies in which the effect of education on mortality has been estimated up to old age. The studies that exist show conflicting results. In one study from the Netherlands and in one from Sweden significant effects were found up to an age well over 80 years (Fischer et al., 2013; Van Kippersluis, O'Donnell and Van Doorslaer, 2011). On the other hand, another study from Sweden found no effects (Meghir et al., 2018). This was also the case in the study by Albouy and Lequien (2009) from France. Gathmann et al. (2015) concluded that the effect of education on mortality is largest for the older cohorts. Based on the results from these previous studies, it is not possible to predict what the results would have been in our study if the follow-up period had been longer.

9.2. External validity

The IV-results presented in Table 1 yield local average treatment effects (LATE), and they exploit only a subset of the cohorts affected by the school reform. The estimated causal effects are valid for compliers, and one might wonder whether they can be generalized to non-compliers. Applying the LATE-theorem, we assume that there are no defiers; i.e. the monotonicity assumption is likely to be fulfilled. Since all children were obliged to take 9 years of education, and practically none dropped out, never-takers are essentially non-existent.

Always-takers are relevant; they are those children who would have taken at least 9 years or more education even in the absence of the reform. Oreopoulos (2006) shows that the LATE-estimates and the ATE-estimates (ATE - average treatment effects) will converge when the share of children affected by a school reform increases. The cohorts included in our sample were mostly living in municipalities that offered compulsory education only, and a relatively large fraction of children were therefore affected by the reform. This would imply that LATE is more similar to ATE. Furthermore, note that the OLS-estimates in Table 1 are fairly similar to the IV-estimates. Since the OLS-estimates exploit differences in years of education for the entire sample, this indicates that the IV-estimates may not be that different from the ATE-estimates.

9.3. The methodology used in our study – some strengths

Compared to some of the other studies within this field, our study has several advantages.

First, we have individual data on outcomes for the entire period after the reform and all the way up to 2015; i.e. for the entire life span of people aged 16 to 64. In most other studies, data on outcome has only been available many years after the introduction of the reform. For example, in the French study the school reform was introduced in 1923 while data on outcomes were available for the period 1968 to 2005 (Albouy and Lequien, 2009). The outcome was whether the individual was alive at the age of 80. Similarly, in the study from the Netherlands the reform was introduced in 1928, while data on outcomes were available from 1998 to 2005 (Van Kippersluis et al., 2011). The outcome was the probability of dying between the ages of 81 and 88. Van Kippersluis et al. (2011) argue that the lack of data on deaths for several decades after the reform was introduced, led to a downward bias of the causal estimates.

Second, we have data about a school reform that was introduced in 706 municipalities at different times over a 12-year period. In most other studies, such reforms were introduced concurrently throughout the countries in question (Albouy and Lequien, 2009; Arendt, 2005; Braakmann, 2011; Clark and Royer, 2013; Davies et al., 2018; Van Kippersluis et al., 2011). This makes the design sensitive to “compound treatment”, i.e. the possibility that the timing of the reform coincided with other policy changes at the same point in time (Eggers et al., 2018; Gerber et al., 2013). Our research strategy allowed us to test whether the timing of the introduction of the reform in the 706 municipalities was uncorrelated with the response and exposure variables (Appendix 5). The results showed that there was no correlation. Further, children who were exposed and children who were not exposed to the reform were similar with respect to relevant predetermined variables (Table 2). Both these results indicate that bias due to “compound treatment” is less likely with our study design.

9.4. Education and causes of deaths

The main effect of the reform was to lower the number of deaths from accidents. This finding is consistent with descriptive studies that have shown deaths caused by accidents to be highest among less well educated people (Erikson and Torssander, 2008; Gill et al., 2005; Khang et al., 2004; Malmivaara et al., 1993). We suggest two explanations for this finding.

First, there are differences in risk-taking behaviours, such as in alcohol and substance use, between people with a high level of education and those with a low level (Cutler and Lleras-Muney, 2010; Jung, 2015). Alternatively, preferences for certain types of behaviour, for example motorcycle riding, may vary according to level of education. Both differences in risk-taking behaviours and differences in preferences are likely to be correlated with accidental death.

Second, more education leads to upward occupational mobility (Kambourov and Manovskii, 2008; Sicherman, 1990) This mobility is mainly from occupations for which the risk of accidents is high to occupations in which the risk is low. This risk is particularly high for people who work in agriculture, industry, building and construction, and transport (National Institute of Occupational Health, 2017; Statistics Norway, 2017; 2018b). The percentage of people employed in these occupations fell by 15% from 1970 to 2015 (Hasås, 2017). Since these are mainly male occupations (Statistics Norway, 1994, 2018c), upward mobility resulting from more education has primarily benefitted men. Therefore, the risk of dying from occupational accidents has fallen more for men than for women.

It has been shown that additional education leads to a healthier lifestyle; for example, less alcohol consumption and smoking, a healthier diet and more exercise (Cutler and Lleras-Muney, 2010). According to the fundamental cause theory, education provides the individual with different types of resources that can be used to avoid the risk of disease. Such resources are knowledge about the benefits of a healthy lifestyle and how to make healthy choices. Studies have shown that people with less education have poorer health than people with more education (for a review see: Cutler et al., 2006). Our results partly support this evidence (Table 3). The estimate for deaths that were amenable to behavioural change only, were negative and of a reasonable size, but failed to reach statistical significance at the conventional level. Most likely, the lack of statistical power is because deaths from these causes usually occur after the age of 70 (Norwegian Institute of Public Health, 2012). Our study includes individuals aged 64 and younger; i.e. most of the individuals may not have reached an age when the risk of dying from preventable diseases is high.

The reform had no effect on deaths that were amenable to medical intervention only (Table 4). From an egalitarian point of view, this finding is encouraging, as it indicates that access to medical treatment and quality of care are not determined by individual resources, such as level of education. This is also supported by our analyses of the data from the Survey of Living Conditions. A similar finding has been reported from Sweden, another country with a large public health care sector (Westerling et al., 1996). In countries with less public funding of health care, there are marked differences in the number of deaths that are amenable to medical intervention according to level of education (Glied and Lleras-Muney, 2008; Mackenbach et al., 2015; Stirbu et al., 2010).

10. Conclusions

In conclusion, our results indicate that education is important for survival until the age of 64, in a country with a strong public involvement in health care. The effect was particularly strong for men. For men, a large part of the effect was due to fewer accidental deaths. Part of this effect might be explained by an upward occupational mobility from accident-prone occupations to low risk occupations. These are occupations where mainly men are employed. Therefore, men are more likely to benefit from upward occupational mobility than women. Part of the effect may also be explained by differences in risk-taking behaviours and differences in preferences for certain behaviours, between educational groups – all things that are correlated with accidental death. For causes of death that could not be prevented, the effect of education was small and non-significant. This result gives support to the fundamental cause theory. More education had no causal effect on the probability of dying of diseases that are amenable to medical intervention. This gives some support to our results that patients are treated equally, independent of their level of education. in Norway.

Declaration of competing interest

There are no conflicts interest.

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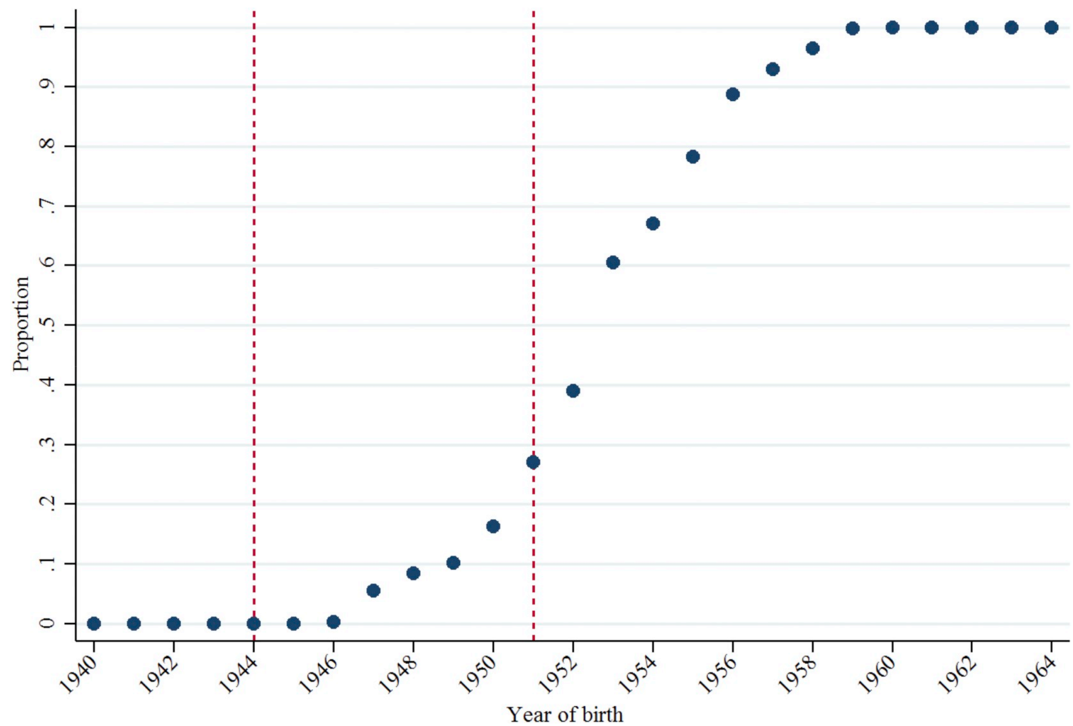
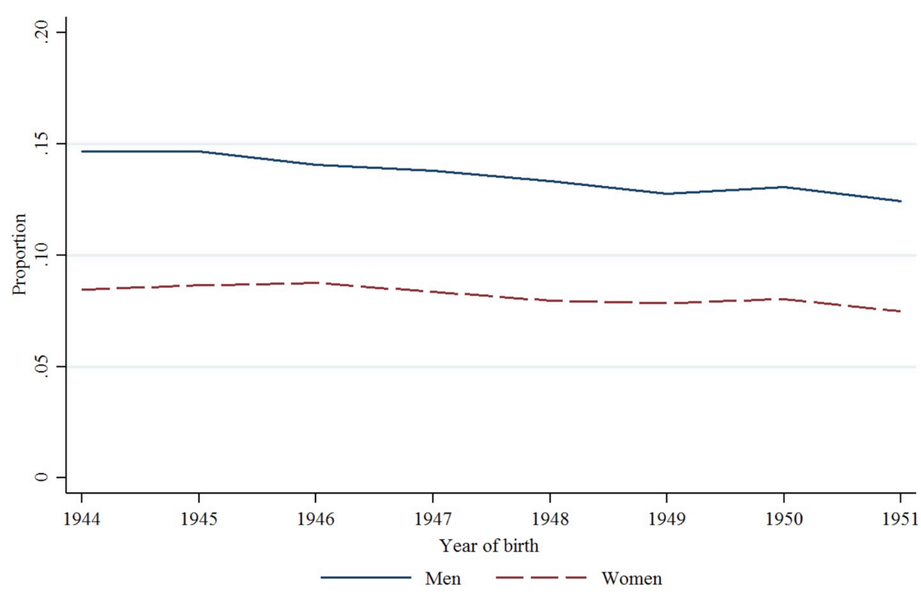
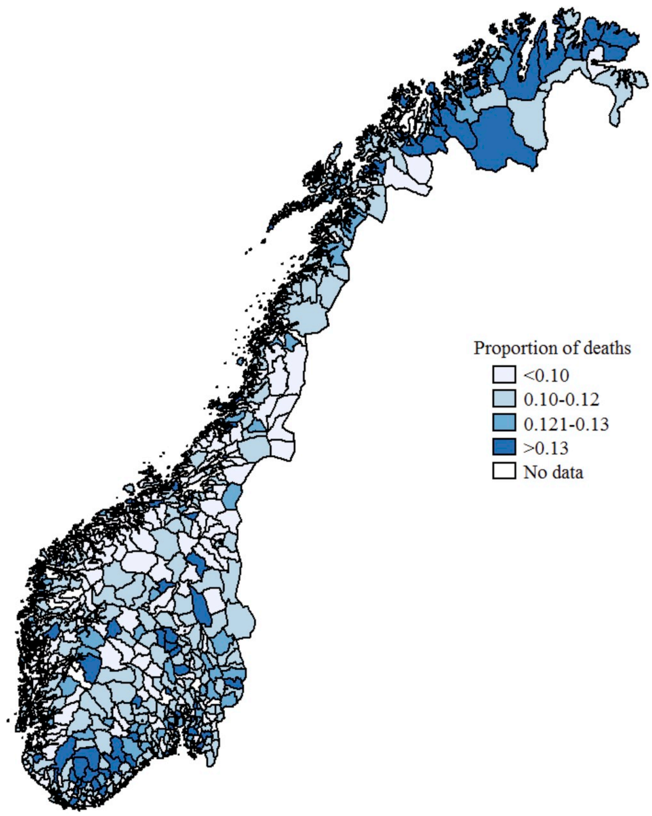
Appendix 1. The proportion of individuals exposed to the school reform by year of birth



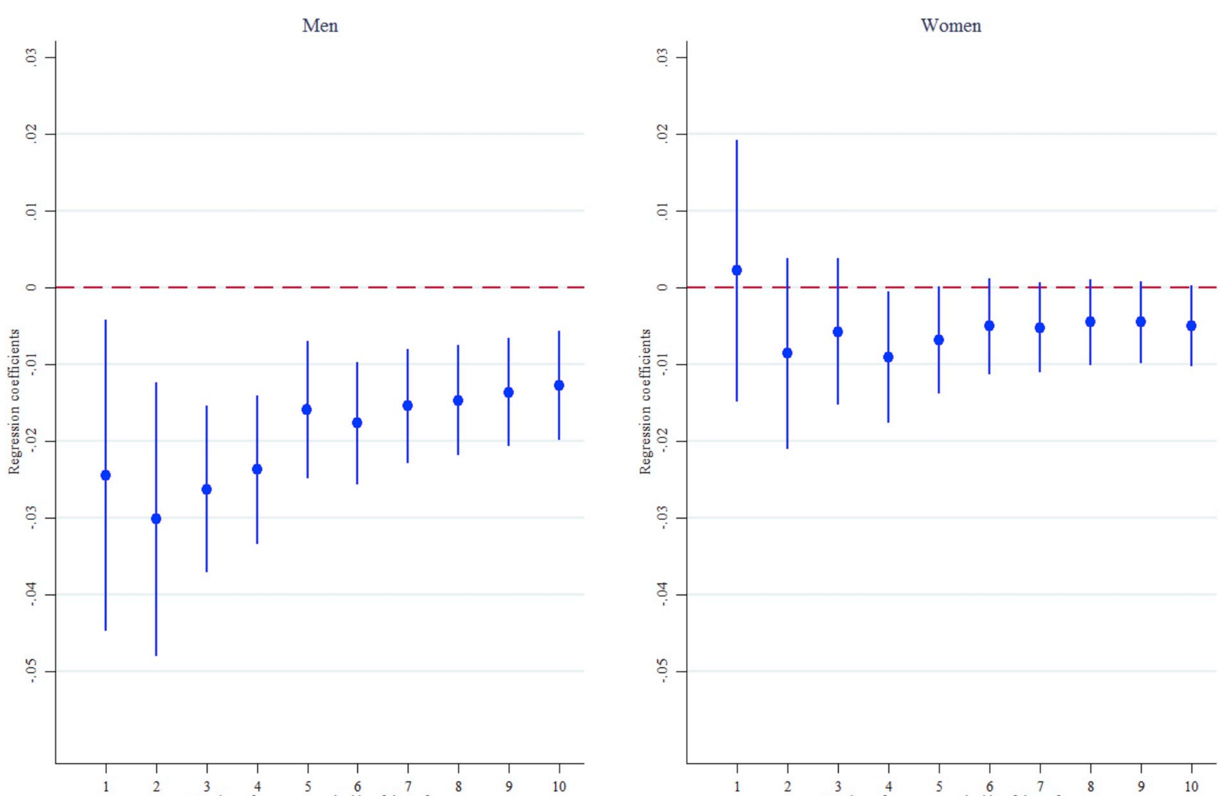
Appendix 2. Proportion of deaths for individuals aged between 16-64 years according to year of birth. Individuals born between 1944 and 1951



Appendix 3. Proportion of deaths for individuals aged between 16-64 years by municipality. Individuals born between 1944 and 1951



Appendix 4. The effect of the school reform on the probability of dying between the ages of 16–64. Individuals born between 1944 and 1951. Estimates with different samples. Reduced form regression coefficients with 95% confidence intervals

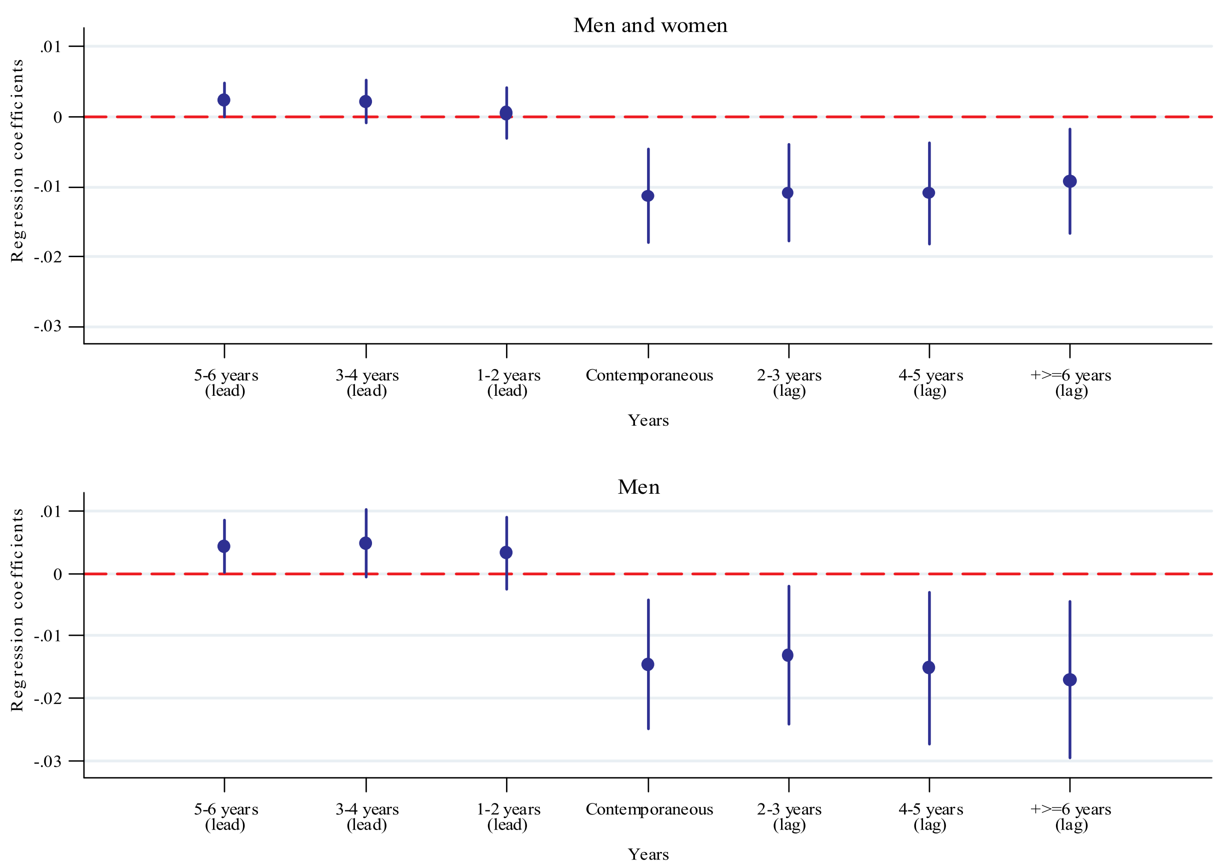


Appendix 5. The effect of the year the reform was introduced on the proportion of deaths for people aged 16–64 and on the mean years of education. Individuals born between 1944 and 1951. Municipality level data. Regression coefficients with standard errors clustered by municipality (in brackets)

Independent variable	Dependent variables					
	Proportion of deaths for people aged 16–64			Mean years of education		
	Men and women	Men	Women	Men and women	Men	Women
Year the reform was introduced	−0.0002	−0.00004	−0.0006	−0.0060	−0.0064	−0.0044
	(0.0004)	(0.0006)	(0.0005)	(0.0075)	(0.0091)	(0.0080)
Number of observations (municipality-years)	4,500	4,496	4,494	4,500	4,495	4,494

Note: Municipality fixed effects and year of birth included in all the analyses.

Appendix 6. Lead-lag effects of the school reform on the probability of deaths for individuals aged between 16-64 years. Individuals born between 1944 and 1951. Reduced form regression coefficients with 95% confidence intervals



Notes: We defined the following independent variables: The contemporaneous effect was defined as 1 in the year the reform was introduced, and 0 in all other years. The first lead dummy variable was equal to 1 in the two years preceding the introduction of the reform, and 0 otherwise. The second lead dummy variable was equal to 1 three and four years before the introduction of the reform, and 0 otherwise. The lagged dummy was equal to 1 two years after introduction of the reform, and 0 otherwise. Our outcome was the probability of death between the ages of 16 and 64.

Appendix 7. Causes of death, classified according to the criteria described by Mackenbach et al. (2015) and Phelan et al. (2004). Number of deaths for individuals born between 1944 and 1951

Diagnosis	Causes of death amendable to:			Deaths that could not be prevented either by behavioural change or medical intervention	Number of deaths
	Behavioural change only	Medical inter invention only	Both behavioural change and medical intervention		
Accidents					5,781
Alcohol abuse[1]	Yes				2,172
Appendicitis, hernia and peptic ulcer[1]		Yes			286
Cancer of brain[1]				Yes	1,108
Cancer of breast[1]		Yes			2,081
Cancer of buccal cavity, pharynx, and oesophagus[1]	Yes				749
Cancer of cervix[1]			Yes		582
Cancer of colorectum[1]				Yes	2,235
Cancer of kidney and bladder[1]				Yes	773
Cancer of larynx[1]	Yes				78
Cancer of liver[1]				Yes	239
Cancer of ovary[1]				Yes	911
Cancer of pancreas[1]				Yes	1,110
Cancer of prostate[1]		Yes			445
Cancer of skin[1]			Yes		951
Cancer of stomach[1]				Yes	692
Cancer of trachea, bronchus and lung[1]	Yes				4,334
Cardiomyopathy[2]				Yes	238
Cerebrovascular disease[1]			Yes		1,850
Chronic liver disease[2]			Yes		226
Chronic obstructive pulmonary disease[1]	Yes				1,191
Congestive heart failure[2]			Yes		229
Dementia[2]				Yes	153
Diabetes mellitus[1]	Yes				791
Hepatitis[2]			Yes		43
Hodgkins's disease and leukemia[1]		Yes			1,805
Hypertensive disease			Yes		290
Ischemic heart disease[1]			Yes		5,854
Multiple sclerosis[2]				Yes	417
Obesity[2]	Yes				40
Pneumonia influentza[1]		Yes			390
Suicide[1]			Yes		2,891

Notes: According to Mackenbach et al. (2015) the criterion for classifying causes of death as:
-amenable to behavior change was "that the combined population-attributable fraction (PAF) for smoking, alcohol abuse, overweight, low fruit and vegetable intake, physical inactivity and unsafe sex was > 50% in the Global Burden of Disease study 2000 (World Health Organization, 2002)."
-amenable to medical intervention "that (a) relative 5-year survival rates around the year 2000 exceeded 70% in Euro care (Verdecchia et al. 2008), and or (b) effective screening programs are available and had been implemented in European countries around the year 2000 (Mackenbach and McKee, 2013b), and/or (c) they are among the conditions included in most selections of "conditions amenable to medical intervention" (Nolte and McKee, 2004)." (Mackenbach et al. (2015). pp 53–54).
Phelan et al. (2004) used an expert panel of physicians and epidemiologists to classify die causes of death from the National Longitudinal Mortality Study in terms of the degree to which the cause of death was preventable due to behavioural change or medical intervention.

[1] Mackenbach et al. (2015).
[2] Phelan et al. (2004).

Appendix 8. Access to services provided in hospitals, by specialists outside hospitals, and by primary physicians. Survey of Living Conditions 2002. Regression coefficients with standard errors clustered by municipality (in brackets)

	Hospitals						Specialists outside the hospital[2]				Primary physicians			
	Probability of at least one visit during the last 12 months				Number of outpatient visits during the last 12 months		Probability of at least one visit during the last 12 months		Number of visits during the last 12 months		Probability of at least one visit during the last 14 days		Number of visits during the last 12 months	
	Inpatient care		Outpatient care											
Highest education[1]														
Upper secondary school education	−0.017	−0.019	0.007	−0.008	−0.095	−0.134	0.021	0.055	0.040	0.044	0.008	−0.010	0.084	−0.419
	(0.116)	(0.034)	(0.014)	(0.049)	(0.051)	(0.156)	(0.013)	(0.042)	(0.035)	(0.129)	(0.013)	(0.044)	(0.120)	0.384)
University/college education	−0.006	0.005	0.004	−0.036	−0.074	0.013	0.259	0.068	0.064	0.047	−0.012	−0.027	−0.162	−0.615
	(0.015)	(0.044)	(0.019)	(0.045)	(0.061)	(0.229)	(0.156)	(0.038)	(0.047)	(0.161)	(0.014)	(0.051)	(0.167)	(0.493)
Mean/proportion	0.12	0.11	0.22	0.26	0.47	0.57	0.17	0.21	0.33	0.42	0.18	0.20	2.94	3.48
Cohorts:														
1901–1986 (N = 6827)	X		X		X		X		X		X		X	
1944–1951 (N = 931)		X		X		X		X		X		X		X

*p < 0.05.
Note. Control variables: gender, age fixed effects, place of living (municipality fixed effects) and whether the respondent was chronically ill or not.

[1] Reference category: compulsory school education.

[2] These specialists have a contract with the Regional Health Authority. They are funded by an operating grant from the Regional Health Authority and by reimbursents according to a fixed fee schedule administered by the National Health Insurance Administration (Ringard et al., 2013).

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