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

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 extended compulsory education from seven to nine years. The municipality-level education data were combined with complete population records for the period 1960–2015. These data covered the entire life span of persons aged 16–64, and also included data on causes of death. One additional year of education caused a reduction in mortality of about 23 per cent for men aged 16–64. The effect was negligible for women. For men, a large part of the effect was due to lifestyle choices. More education reduced the number of deaths that were amenable to behavioural change, in particular deaths caused by alcohol use. Women had a healthier lifestyle, irrespective of their level of education. There was also an effect of education on accidental deaths. More men than women with low education are employed in occupations in which the risk of occupational accidents is high. This is also a reason why men die earlier than women.

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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 early.

Our study shows that 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 23 per cent. There was little or no effect for women. Men with lower education had a less healthy lifestyle than men with higher education, therefore they died earlier. Further, accidents were a major cause of death for men with lower education. More men than women, with low education, are employed in occupations in which the risk of occupational accidents is high. This is also a reason why men die earlier than women.

These results are derived from a unique combination of Norwegian register data. We have complete records of all deaths from1960 to 2015 from the Norwegian Cause of Death Registry. We combined these data with municipality-level data on the staggered introduction of a nationwide school reform that extended compulsory education from 7 to 9 years during the period 1960–1972. Our research design facilitated the identification of causal effects. The paper contributes to the existing literature in three ways.

First, our results contradict the findings reported in several influential papers from the United Kingdom, France, the USA and Sweden which did not find a casual effect of education on mortality (Clark and Royer, 2013; Albouy and Lequien, 2009; Mazumder, 2008; Meghir et al., 2017). Since our results clearly deviate from previous findings, we emphasize the credibility of our research design. While we used a large set of individual-level register data, most previous studies have used aggregated data (Clark and Royer, 2013; Lleras-Muney, 2005) and/or survey data (van Kippersluis et al., 2009). In our study, the experiment and treatment groups were well balanced; for example, the mortality rates for parents of children who were exposed/not exposed to the school reform were similar. We performed several additional robustness tests to demonstrate the credibility of our design.

Second, most studies relied on the introduction of a school reform as a strategy to estimate the causal effects of education. Commonly, these reforms were implemented concurrently throughout the countries in question. This feature makes the design sensitive to “compound treatment”, i.e. the possibility that the timing of the reform coincided with other policy changes (Eggers et al., 2018). We exploited an identification strategy based on a school reform which introduced compulsory education throughout Norway over a period of 12 years. That some municipalities implemented the reform early and others late made the design more robust. The control group 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 estimated local linear models on samples with varying bandwidths. As a supplementary strategy, we exploited the gradual introduction of upper secondary schools in Norway. This provided a causal estimate of the impact of education on mortality at an educational level above compulsory education.

Third, following Mackenbach et al. (2015), we classified causes of death according to whether they were amenable to medical intervention or not. An important finding was that levels of education had no causal effect on the probability of dying as a result of diseases that are amenable to medical intervention. In Norway, all health services are financed through taxes, and everyone is entitled to free health care. Our results indicate that public provision of health services has enabled equity in access to medical treatment. Education had a significant effect on deaths that were amenable to behavioural change; i.e. the education effect was mostly due to lifestyle choices.

In the next section, we present key descriptive statistics on education and mortality in Norway. In the subsequent sections, we outline the research design used to identify causal effects, and present the estimates on mortality and causes of death. Having established the key findings, we proceed by analysing heterogeneous effects and the mechanisms relating levels of education to mortality.

2. Descriptive statistics on education and mortality

2.1 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.[1] Children finished compulsory education at the age of 14 in the old system, and 16 in the new system.[2]

We used the 1960 census to identify the municipality in which the child grew up. We identified the timing of the reform in 706 of the 735 municipalities that existed in 1960. The geographic variation in implementation is shown in Figure 1. Many rural municipalities adopted the reform early. Municipalities with major cities implemented the reform late.

In Figure 2, we use binned scatterplots to illustrate the impact of the reform on years of education. The horizontal axis displays number of years before and after implementation in each municipality. For example, -1 indicates children who were not exposed to the reform, because they were born one year too early. Note that these children belong to different birth cohorts, depending on when the municipalities implemented the reform. The scatterplot indicates that the reform led to an increase in education of about half a year for both men and women.

2.2 The proportion who died

In Figure 3, we show the proportion of deaths for men and women according to year of birth and when they died in three groups. The groups were those who died when they were aged between 16 and 64; between 16 and 49; and between 50 and 64. Data from the Norwegian Cause of Death Registry for the period 1960–2015 were used to calculate the results. All individuals were born in 1951 or earlier, i.e. they had the possibility of living to the age of 64. Figure 3 provides the basis for three comments:

1. First, for all three groups, the proportion of men who died was nearly twice as large as the proportion of women who died.

[1] 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.

[2] For further details about the reform, see Aakvik et al. (2010), Lie (1973) and Telhaug (1969).

2. Second, for all three groups, the proportion of deaths was higher for those born in the 1930s and early 1940s compared to those born later. In particular, this was the case for men.
3. Third, for all birth cohorts, the proportion of people who died when they were between 16 and 49 years old was markedly lower than the proportion of people who died when they were between 50 and 64 years old.

The geographic variation in the proportion of deaths of individuals aged between 16 and 64, by municipality is shown in Appendix 1. 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.

The Norwegian Institute of Public Health has published several reports with mortality statistics by year of birth (Norwegian Institute of Public Health, 2012, 2016, 2017a). Our figures, shown in Figure 3 and Appendix 1, are in accordance to these statistics.

2.3 The school reform and mortality

In Figure 4, we display binned scatterplots to address the impact of the reform on mortality. Municipality-level data of the year the reform was introduced were merged with individual-level data from the Norwegian Cause of Death Registry for the period 1960–2015. The vertical axis shows the proportion of people who died between the ages of 16 and 64. All individuals were born in 1951 or earlier. For men, we see a substantial reduction in the proportion of deaths just after the introduction of the reform, from 0.14 to 0.11. For women, there was hardly any reduction.

3. Research design

3.1 Balancing tests

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' level of education (in years); parents' age when the reform was introduced; whether the parents died at the age of 64 or earlier; 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.[3] At conventional levels of significance, the reform had no statistically significant association with parents' age, with whether the parents died at the age of 64 or earlier, and with the number of siblings for each child (Table 1). Note as well that the regression coefficients are small in value, in particular in relation to the standard errors.

The samples were not perfectly balanced with respect to parents' level of education (Table 1). The parents of the exposed children had more education than those of the non-exposed children: 0.08 years (about 4 weeks) more for the fathers, and 0.04 years (about 2 weeks) more for the mothers ($p<0.05$). This is a minor difference, and it is unlikely to lead to any significant bias in our results.[4] Nevertheless, we included parents' level of education as a control variable in all our analyses.

3.2 Model specification

We used the introduction of the reform as an instrumental variable to estimate the causal effect of education on mortality.[5] The reform dummy variable was specific for municipality and year of birth. Let the subscript *imt* denote child *i* who has grown 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.

[3] Municipality-specific time trends, parents' year of birth, child's year of birth, and an indicator variable for missing data on each of the predetermined variables were included in the analyses.

[4] This is further supported by the work of Black et al. (2005) who used the introduction of the compulsory school reform in Norway to study intergenerational transmission of human capital. They concluded that there was "little causal relationship between parent education and child education".

[5] 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; Machin et al., 2012; Grytten et al., 2014; Grytten and Skau, 2017; Leuven et al., 2016).

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*. $T_{m(t)}$ represents a municipality-specific time trend defined as number of years before and after the reform was introduced, i.e. similar to the horizontal axis in Figures 2 and 4. ME_{imt} and FE_{imt} denote mother's and father's level of education (in years). Finally, the model includes municipality fixed effects (θ_m), year of birth fixed effects (ϑ_t), and a random variable capturing other influences (ε_{imt}). We estimated the following first-stage linear regression model:

$$E_{imt} = \alpha_0 R_{m(t)} + \alpha_1 T_{m(t)} + \alpha_2 T_{m(t)} R_{m(t)} + \gamma_M ME_{imt} + \gamma_F FE_{imt} + \theta_m + \vartheta_t + \varepsilon_{imt}, \quad -4 \leq T_{m(t)} \leq 4;\ t \leq t^* \tag{1}$$

Let $\hat{E}_{m(t)}$ 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 is then:

$$D_{imt} = \beta_0 \hat{E}_{m(t)} + \beta_1 T_{m(t)} + \beta_2 T_{m(t)} R_{m(t)} + \mu_M ME_{imt} + \mu_F FE_{imt} + \mu_m + \rho_t + \epsilon_{imt}, \quad -4 \leq T_{m(t)} \leq 4;\ t \leq t^* \tag{2}$$

We estimated the effects of number of years of education on the likelihood of death by running separate regressions on the following outcomes: those who died when they were aged between 16 and 64, between 16 and 49, and between 50 and 64. Based on the evidence presented in Figure 4, we expected the effects to be largest for men. Therefore, for each of our three outcomes D_{imt}, we ran separate regressions for men and women. Our results are also presented as reduced form estimates where the probability of death was regressed directly on the reform variable.

Our analyses were all carried out on a sample of individuals born in 1951 or earlier, i.e. with the possibility of living to the age of 64. The proportion of individuals exposed to the reform by year of birth is given in Appendix 2. For those born in 1951, 30 per cent were exposed to the reform. For those born earlier, the percentage of exposed individuals was lower.

The model was estimated using a local linear specification, meaning that we used data on individuals close to the reform cut-off. Our main analyses were carried out on a sample including four years on either side of the reform. For the reduced form estimates, we present additional analyses based on samples with several time periods (bandwidths). In all our analyses, we clustered the standard errors at the municipal level to take account of serial correlations and within-municipality correlation (Cameron and Miller, 2015).

4. Results

4.1 OLS estimates

Education had a negative effect on the probability of death (Table 2). The effect was markedly stronger for men than for women. For men, the probability of death between the ages of 16 and 64 decreased by 1.5 percentage points per additional year of education ($p<0.001$). For women, the corresponding decrease was 0.9 percentage points ($p<0.001$). For both men and women, the strongest effect of education was on the probability of death between the ages of 50 and 64.

4. 2 Reduced form estimates

The school reform caused a marked reduction in the probability of death. For men and women, the probability of dying between the ages of 16 and 64 decreased by 1.8 percentage points the year after the reform was introduced (Table 2). In Norway, in the 1960s, each birth cohort comprised about 60 000 people. An implication of our reduced form estimates is that about 1000 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 about four times stronger for men than for women. This was the case for the probability of death both between the ages of 16 and 49, and 50 and 64. For women, the regression coefficient was statistically significant at the conventional level ($p<0.05$) only for those who died between the ages of 16 and 49.

For men, the probability of dying between the ages of 16 and 64 decreased by 2.7 percentage points for those who had 9 years of compulsory education compared to those who had not. The effect of the reform was slightly stronger for the probability of dying between the ages of 16 and 49 than between the ages of 50 and 64.

4.3 First stage estimates

Similar to that shown in Figure 2, the first-stage estimates show that the reform led to an increase in education of about half a year, marginally more for men and less for women. The estimates were quite precise for all subsamples. The F-test statistics are well above the standard weak-instrument threshold (Stock et al., 2002).

4.4 Second stage estimates

For men, the probability of death between the ages of 16 and 64 decreased by 3.5 percentage points for each additional year of education ($p<0.001$). The strongest effect of education was on the probability of death between the ages of 16 and 49. The proportion of men who died before the reform was introduced was nearly 15 per cent. An implication of the second stage results in Table 2 is that one additional year of education led to a reduction in the proportion of deaths for men by about 23 per cent.

For women in all age groups, the estimates are negative, but not statistically significant at conventional levels.

5. Supplementary analyses

We carried out several supplementary analyses, mainly to test the robustness of the estimation, but also to test other relevant hypotheses.

5.1 Fewer control variables and different bandwidths

In our key analyses presented in Table 2, we included parents' level of education as a control variable. We re-estimated the model when excluding parents' level of education as a control variable. The results are shown in Appendix 3, and are almost identical to the estimates shown in Table 2. The small lack of balance observed in Table 1 does not affect our key estimates.

In Appendix 4, we show reduced form estimates for the probability of death between the ages of 16 and 64, for samples with different number of years on each side of the reform (different bandwidths). In the analyses with the broader bandwidths, the estimates are slightly more precise than the estimates with the narrower bandwidths. 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 2, where we used a bandwidth of four years. The main effect of the school reform has been to reduce the probability of death among men. There was little or no effect for women (Appendix 4).

5.2 Leads and lags

We carried out a placebo test in which we redefined the reduced form regression to capture pre- and post-reform effects. 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.

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 5). 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 per cent confidence interval. The estimate for the variable measuring the contemporaneous effect was about the same size as the reduced form estimate in Table 2.

5.3 Pre-reform trends

The identification of causal effects is based on the common trend assumption. According to this assumption, the trend in deaths would have been the same for everyone (i.e. the exposed and non-exposed individuals in our study) if the reform had not been introduced. In Appendix 6, we examine pre-reform trends in the proportion of deaths and the level of education at the municipal level.

In the figure to the left, the vertical axis displays the proportion of deaths between the ages of 16 and 64. The horizontal axis displays the number of years before the reform was implemented. The municipalities were grouped according to the year in which the reform was introduced: 1960–61 (early), 1962–64, and 1965–67 (late).

The proportion of deaths is higher in municipalities that introduced the reform early (upper curve) compared to in municipalities that introduced the reform late (lower curve). Our estimation takes this into account by including a municipality-specific time trend in Equations (1) and (2). Most importantly, the trends in the proportion of deaths in early-adopting (more deaths) and late-adopting municipalities (fewer deaths) appear to be parallel. This indicates that the common trend assumption is fulfilled.

Further support is provided in the figure to the right in Appendix 6. The vertical axis displays the mean number of years of education. Similar to the pattern shown in the previous figure (to the left), the trends in educational level in early-adopting (less education) and late-adopting municipalities (more education) appear to be parallel.

5.4 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 the table in Appendix 7, 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.[6]

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 (Appendix 7). This indicates that the results reported in Table 2 are not biased due to correlation between the timing of the introduction of the reform and the response and exposure variables.

[6] The following control variables were included in the analyses: year of birth, the proportion of women, parents' level of education (mean number of years) and municipality fixed effects.

5.5 Non-linear effects

The second stage estimates in Table 2 were larger than the OLS estimates. This is also a consistent finding in most studies in which compulsory school reforms have been used as instrumental variables for the estimation of causal effects on health outcomes and labour market outcomes (for example see Oreopoulos, 2006; Van Kippersluis et al., 2011; Card, 2001). Card (2001) offers various explanations of why this may be the case. The most plausible explanation is that the reform variable only identifies local average treatment effects. In that case, number of years of education might have a non-linear effect on the probability of death between the ages of 16 and 64. One additional year of education could have the largest negative effect on death for individuals with no or little further education after compulsory education.

We provide evidence of a non-linear effect by using the establishment of upper secondary schools in Norway as an instrumental variable for number of years of education. This gives the effect of number of years of education on death for the upper part of the educational distribution.

During the period of our study, upper secondary education was voluntary. Pupils were offered a place at an upper secondary school on the basis of their lower secondary school performance.[7] They started upper secondary school at the age of 16. These schools were previously called gymnasiums, and were the gateway to university education.

During the last century, there was a substantial increase in the number of upper secondary schools in Norway. The geographic location of secondary schools is shown on the map in Appendix 8 according to trade districts. Neighbouring municipalities are grouped together, with a trade centre as the focal point. According to the classification criteria, a trade centre has “an urban settlement of a certain size” and must be surrounded by “a commuting area” (Statistics Norway, 2001). There are 90 trade districts. Secondary schools are usually located in the trade centres.

In many rural trade districts, upper secondary schools were established late (Appendix 8). Trade districts with major cities had upper secondary schools from the beginning of the twentieth century. In districts without upper secondary schools, pupils had to travel to

[7] From 1996, all young people aged 16–19 were given the legal right to upper secondary education (Thune et al., 2015).

neighbouring districts to attend one, sometimes at a great distance from home. For many, this represented a barrier to obtaining higher education.

We followed Currie and Moretti (2003) in using the establishment of upper secondary schools in a trade district as an instrumental variable for number of years of education. Let E_{jdt} be number of years of education for individual *j* who lived in trade district *d* at the age of 16, and was born in year *t*. $USS_{d(t)}$ equals 1 for individuals born late enough to attend upper secondary school in the trade district in which they lived at the age of 16, meaning that an upper secondary school had been established in their district. Conversely, $USS_{d(t)}$ equals 0 for individuals born too early to attend upper secondary school in the trade district in which they lived at the age of 16, meaning that an upper secondary school had not been established. Similar to in Equations (1) and (2), $R_{m(t)}$ denotes the reform variable, while ME_{jdt} and FE_{jdt} denote mother's and father's level of education (in years). The model includes trade district fixed effects (θ_d), year of birth fixed effects (ϑ_t), and a random variable capturing other influences (ε_{jdt}). We estimated the following first-stage linear regression model:

$$E_{jdt} = \alpha_0 USS_{d(t)} + \alpha_0 R_{m(t)} + \gamma_M ME_{jdt} + \gamma_F FE_{jdt} + \theta_d + \vartheta_t + \varepsilon_{imt} \qquad (3)$$

Let $\hat{E}_{d(t)}$ be the predicted number of years of education from the first-stage regression, and let D_{jdt} be a binary variable indicating death of individual j who grew up in trade district *d* and was born in year *t*. The second stage regression is then:

$$D_{jdt} = \beta_0 \hat{E}_{d(t)} + \beta_2 R_{m(t)} + \mu_M ME_{jdt} + \mu_F FE_{jdt} + \mu_d + \rho_t + \epsilon_{imt} \qquad (4)$$

The analyses were carried out on a sample of individuals born in 1966 or earlier, i.e. with the possibility of living to the age of 49. Equation (4) was estimated using a linear probability model, and the standard errors were clustered at the level of the trade district.

The establishment of upper secondary schools in a trade district caused a reduction in the probability of death (Table 3). The overall reduced form estimates showed that the probability of death decreased by 0.47 percentage points after the establishment of an upper secondary school. Similar to the results presented in Table 2, the reduced form estimates were larger for men than for women.

The first-stage estimate shows that the establishment of upper secondary schools in a trade district led to an increase in education of about 0.3 years, marginally more for men and less for women (Table 3). The F-test statistics are around 9. This is lower than the critical value of 10 for the second-stage estimates to be reliable (Stock et al., 2002). Thus our instrumental variable is somewhat weak (Murray, 2006). That we used just one instrumental variable alleviates this concern (Stock et al., 2002). We have also estimated Equation (4) using the limited information maximum likelihood estimator (LIML). When the instrumental variable is weak, this estimator has better sample properties than the two-stage least square estimator (Pischke, 2016). The standard errors were slightly lower using LIML for the estimation, compared to using two-stage least squares. However, the second stage regression coefficients were almost identical.[8]

For both men and women, the second-stage estimate showed that one year of education led to a reduction in the probability of death of 1.0 percentage point (Table 3). The estimates were larger for men than for women.

Both the reduced form and the second-stage estimates in Table 3 were significantly smaller than the corresponding estimates for those who were between the ages of 16 and 49 and exposed to the reform (Table 2). This supports a non-linear effect of education on death.

6. The causes of death – unhealthy lifestyles and unequal access to health services?

Having established a relationship between education and mortality, we turn to the question of what accounts for this relationship. 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). Education provides knowledge about the benefits of a healthy lifestyle and how to make healthy choices. Numerous studies have shown that people with less education have poorer health than people with more education (for a review see: Cutler et al., 2006). Education is also related to intelligence (Brinch and Galloway, 2012).

[8] The regression coefficients and standard errors (in brackets) were for men and women -0.0096 (0.0029); for men -0.0122 (0.0038); for women -0.0060 (0.0033).

In Norway, all health services are financed through taxes. Government policy is that everyone is entitled to free health care at the point of delivery and equal access given equal need (Ministry of Health, 2002). Private health care providers account for only a small share of the health care market, particularly in the hospital sector. We would therefore not expect levels of education to affect access to health care services. Importantly, this suggests that levels of education should have little or no causal effect on the probability of dying as a result of diseases that are treatable. This was tested using data from the Norwegian Cause of Death Registry (Norwegian Institute of Public Health, 2016). Following the criteria developed by Mackenbach et al. (2015), we classified causes of death into the following groups (Appendix 9).

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 that could not be classified according to whether they could be prevented.[9]

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 six values (x), the reference group *alive* and the five causes of death (x=1, 2…5). This leads to the multinominal, local linear regression model.

$$\frac{\ln P(D_{imt} = x)}{P(D_{imt} = Alive)} = \varphi_0^x R_{m(t)} + \varphi_1^x T_{m(t)} + \varphi_2^x T_{m(t)} R_{m(t)} + \varphi_M^x ME_{imt} + \varphi_F^x FE_{imt} + \tau_m^x + \vartheta_t^x$$

$$x = 1,2,..5;\ -4 \leq T_{m(t)} \leq 4;\ t \leq t^* \qquad (5)$$

The results are shown in Table 4. The reform led to a 0.43 percentage point reduction in deaths that are amenable to behavioural change only, and to a 0.69 percentage point reduction in deaths that are amenable to both behavioural and medical intervention ($p<0.05$) (Table 4). These results are in accordance with the findings of descriptive studies in other European countries using mortality as the outcome measure (Mackenbach et al., 2015;

[9] Twenty-eight per cent of all deaths were classified as belonging to this group.

Masters et al., 2015). The estimates are markedly larger for men than for women. The reform had no effect on deaths that were amenable to medical intervention only.

7. Specific causes of death and gender differences

In supplementary analyses, we examined the effect of the school reform on specific causes of death. The response variable D_{imt} has three values (x), the reference group *alive*, a specific cause of death, and all other causes of death (z=1, 2, 3). We specified the following multinominal, local linear regression model:

$$ln \frac{P(D_{imt} = z)}{P(D_{imt} = Alive)} = \varphi_0^z R_{m(t)} + \varphi_1^z T_{m(t)} + \varphi_2^z T_{m(t)} R_{m(t)} + \varphi_M^z ME_{imt} + \varphi_F^z FE_{imt} + \tau_m^z + \vartheta_t^z$$

$$\text{z}= 1, 2, 3;\ -4 \leq T_{m(t)} \leq 4;\ t \leq t^* \qquad (6)$$

The reduced form estimate is largest for deaths caused by accidents (Appendix 10). These deaths include transport accidents (including traffic accidents), accidental falls and accidental poisoning. The regression coefficient indicates that the reform led to a 0.41 percentage point reduction in such deaths. This finding is consistent with descriptive studies that have shown deaths caused by accidents, traffic accidents in particular, to be highest among less well educated people (Khang et al., 2004; Erikson and Torssander, 2008; Gill et al., 2005; Malmivaara et al., 1993).

The reduced form estimate indicates that the reform led to a 0.19 percentage point reduction in deaths resulting from excessive alcohol consumption (Appendix 10). For men, the effect was particularly strong – a reduction of 0.0037 percentage points. For women, the reform had no effect on deaths caused by alcohol use.

For lung cancer, the regression coefficient was negative and of a reasonable size, but not statistically significant at conventional levels. For lung cancer, mortality mainly occurs after the age of 55 (Peto et al., 2000). Our study includes individuals aged 64 and younger; i.e. most of the individuals had not reached an age with the highest risk of dying from lung cancer.[10]

[10] Our finding is similar to that of Leuven et al. (2016). They analysed the effect of education on the risk of cancer using data from the Norwegian Cancer Registry. Using a research design similar to ours, they found that

For the other specific causes of death that were amenable to behavioural change only, and to deaths that were amenable to both behavioural change and medical intervention, most of the reduced form estimates were negative and of reasonable size. However, they were not statistically significant at conventional levels. Most likely, the lack of statistical power is due to the small number of deaths from these causes before the age of 65 (Appendix 9). Deaths from these causes usually occur after the age of 70 (Norwegian Institute of Public Health, 2012).

8. Discussion

8.1 Education and mortality – methodological considerations

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 with the use of the same identification strategy (for a review see Mazumder, 2008; Albouy and Lequien, 2009).

In all studies from the United Kingdom, France and Sweden and some studies from the USA, no causal effects of education on mortality have been found (Clark and Royer, 2013; Albouy and Lequien, 2009; Mazumder, 2008; Meghir et al., 2017). On the other hand, causal effects have been found in studies from the Netherlands, Canada and some studies from the USA (van Kippersluis et al., 2009; Cao et al., 2014; Fletcher, 2015; Lleras-Muney, 2005). 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. In some studies, health outcome measures such as self-reported health, body mass index, long-term illness, hypertension and diabetes have been used. Typically, the results are mixed (Arendt, 2005; Li and Powdthavee, 2015; Kemptner et al., 2011; Zhong, 2015; Braakmann, 2011).

Our study gives supports to studies in which a causal effect of education on health outcomes has been found. We believe our estimates are quite compelling. For example,

education had no causal effect on mortality from lung cancer. However, they found a causal effect on the incidence of lung cancer.

children who were exposed and children who were not exposed to the reform were similar with respect to relevant predetermined variables, such as mortality of parents. Hence, it is no surprise that the difference in the proportion of deaths between exposed and non-exposed individuals as shown in Figure 4 is similar to the causal estimate shown in Table 2. This estimate was derived using a local, linear instrumental variable specification. Our results were not sensitive to choice of bandwidths, and there was no effect before the introduction of the reform; i.e. there were no lead effects. The pre-reform trends in the proportion of deaths and in the mean number of years of education were consistent with the common trend assumption. Finally, our results support the prevailing view that the effects of education on the probability of death are non-linear. To our knowledge, there is only one other study in which the causal effect of upper secondary education on deaths among adults has been estimated (Buckles et al., 2016). Our results shown in Table 3 are in line with the findings of that study. Compared to most 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 years. 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).[11] 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., 2009)[12]. Van Kippersluis et al. (2009) 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. This is supported by our results. The second-stage estimate for the probability of death between the ages of 50 and 64 is about half the size of the estimate for the probability of death between the ages of 16 and 64 years (Table 2).

Second, we have data about a school reform that was introduced in 706 municipalities at different times over a 13-year period. In most other studies, such reforms were introduced concurrently within each country (Clark and Royer, 2013; Albouy and Lequien, 2009; van Kippersluis et al., 2009; Braakmann, 2011; Arendt, 2005). It is therefore hard to isolate the

[11] The outcome was whether the individual was alive at the age of 80.

[12] The outcome was the probability of dying between the ages of 81 to 88.

effect of the reform on mortality from other policy initiatives introduced 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 7). The results showed that there was no correlation.

8.2 Education and deaths that are amenable to behavioural change

As emphasized by Deaton (2010), empirical studies should examine why causal effects are identified. We addressed this challenge by analysing the effect of the reform on causes of death. The main effect of the reform was to lower the number of deaths that were amenable to behavioural change. This is consistent with studies finding a casual effect of education on health behaviour, such as smoking, use of alcohol, diet, exercising and body mass index (Brunello et al., 2016; Li and Powdthavee, 2015; Zhong, 2015; Arendt, 2005).

In our study, the type of health behaviour that was particularly important was alcohol use. (Appendix 10). This finding is in accordance with a large number of studies showing alcohol to be a major risk factor for chronic diseases and mortality (Rehm et al., 2003, 2009; Cutler and Lleras-Muney, 2010; Van Oers et al., 1999; Harrison and Gardiner, 1999). The effect of the reform on deaths caused by use of alcohol was particularly strong for men. This may be because, during our study period, Norwegian men had a much higher level of alcohol consumption than women; about two to three times higher (WHO, 2014; Skurtveit et al., 2001)[13]. With more education men may have become more aware of the harmful effects of use of alcohol, and changed their behaviour accordingly. For women, during the study period, the level of alcohol consumption was so low that it was most likely not a major risk factor for death. Therefore, for women, more education did not have an effect on death caused by use of alcohol. In our study, we measured death before the age of 65. There may well be an effect of education on deaths caused by alcohol use for women, but at an age later than 64.

[13] For example, during the period 1993–2000, the mean annual consumption of pure alcohol for men was 3.3 litres and for women 1.6 litres (Strand and Steiro, 2003). In 2010, the prevalence of heavy episodic drinking for men was 17.4% and for women 6.5% (WHO, 2014).

The effect of education on deaths caused by accidents is more than twice as large for men as for women; i.e. more education benefits men more than women. This is further supported by two types of descriptive statistics published by Statistics Norway.

First, for men the number of fatal traffic accidents fell from 25 per 100 000 in 1970 to 5 per 100 000 in 2010 (Norwegian Institute of Public Health, 2017b). For women the decrease was small: from 6 per 100,000 in 1970 to 3 per 100 000 in 2010. Throughout this period there has been a significant improvement in road safety and in that cars are safer. This has arguably made the most important contribution to the reduction in deaths from traffic accidents. A small but significant contribution may also have come from the marked increase in the level of education in the population. For example, the proportion of people with upper secondary education and university education increased from 47 per cent in 1970 to 71 per cent in 2010 (Statistics Norway, 2018a). Our results indicate that with higher education, men in particular have become more aware of the risks associated with this type of adverse behaviour, and have been able to take the necessary precautions to protect themselves against these risks (Cutler and Lleras-Muney, 2010).

Second, more education leads to upward occupational mobility (Sicherman,1990; Kambourov and Manovskii, 2008). This mobility is mainly from occupations in 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 (Statistics Norway, 2017, 2018b; National Institute of Occupational Health, 2017). The percentage of people employed in these occupations fell by 15 per cent 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.

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. 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 (Stirbu et al., 2010; Glied and Lleras-Muney, 2008).

9. 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. One additional year of education caused a reduction in mortality of about 23 per cent. There was little or no effect for women. For men, a large part of the effect was due to lifestyle choices. More education reduced the number of deaths that are amenable to behavioural change, in particular deaths caused by alcohol use. Education also had an effect on accidental deaths. Most likely, part of this effect is due to 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. Our results also indicate that men with higher education are more aware of the risks associated with adverse driving behaviour. Finally, more education had no causal effect on the probability of dying of diseases that are amenable to medical intervention. This finding is encouraging, as it indicates that equality in access to medical treatment has been achieved in Norway.

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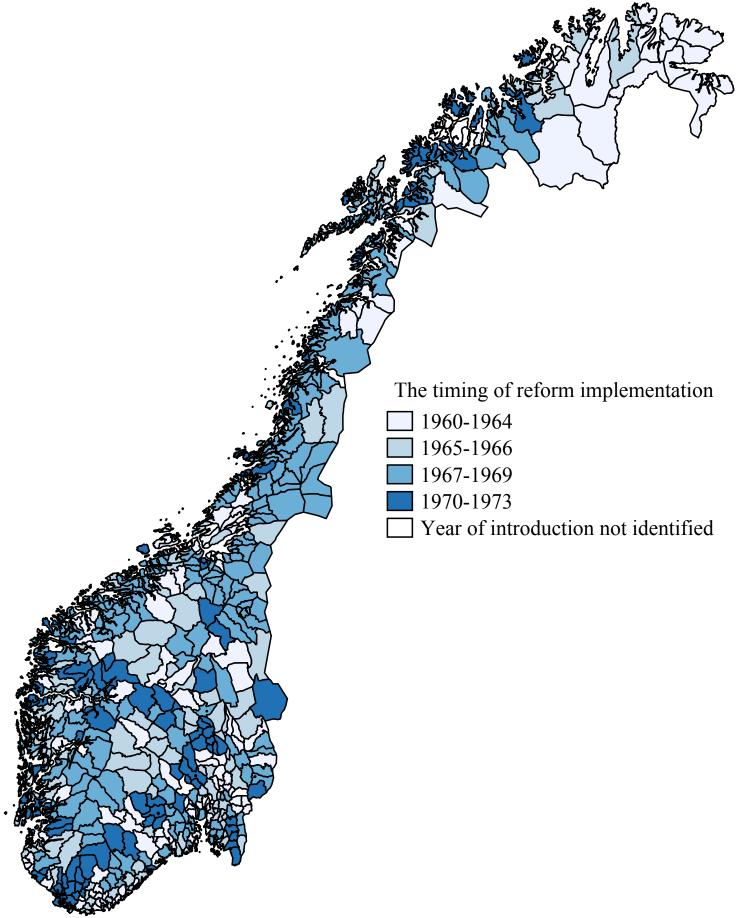


Figure 1. Year of introduction of the Norwegian school reform

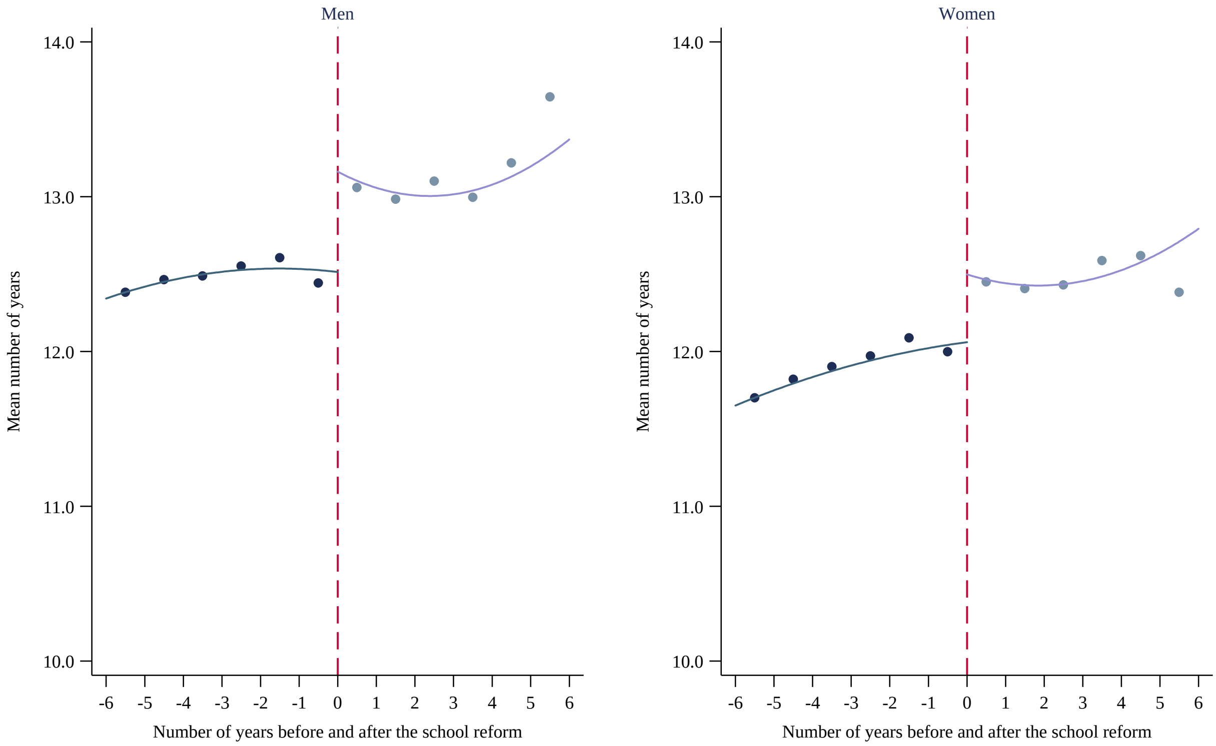


Figure 2. The effect of the Norwegian school reform on the mean number of years of education. Individuals born in 1951 or earlier

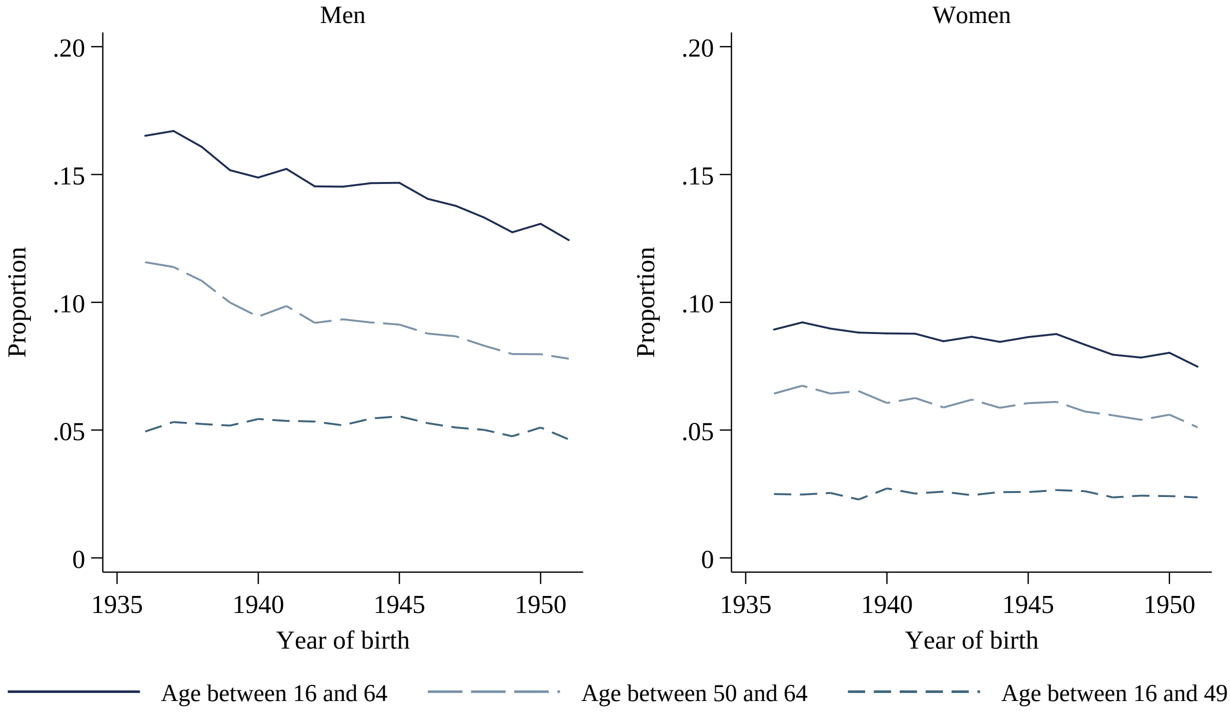


Figure 3. Proportion of deaths for men and women according to year of birth. Individuals born in 1951 or earlier

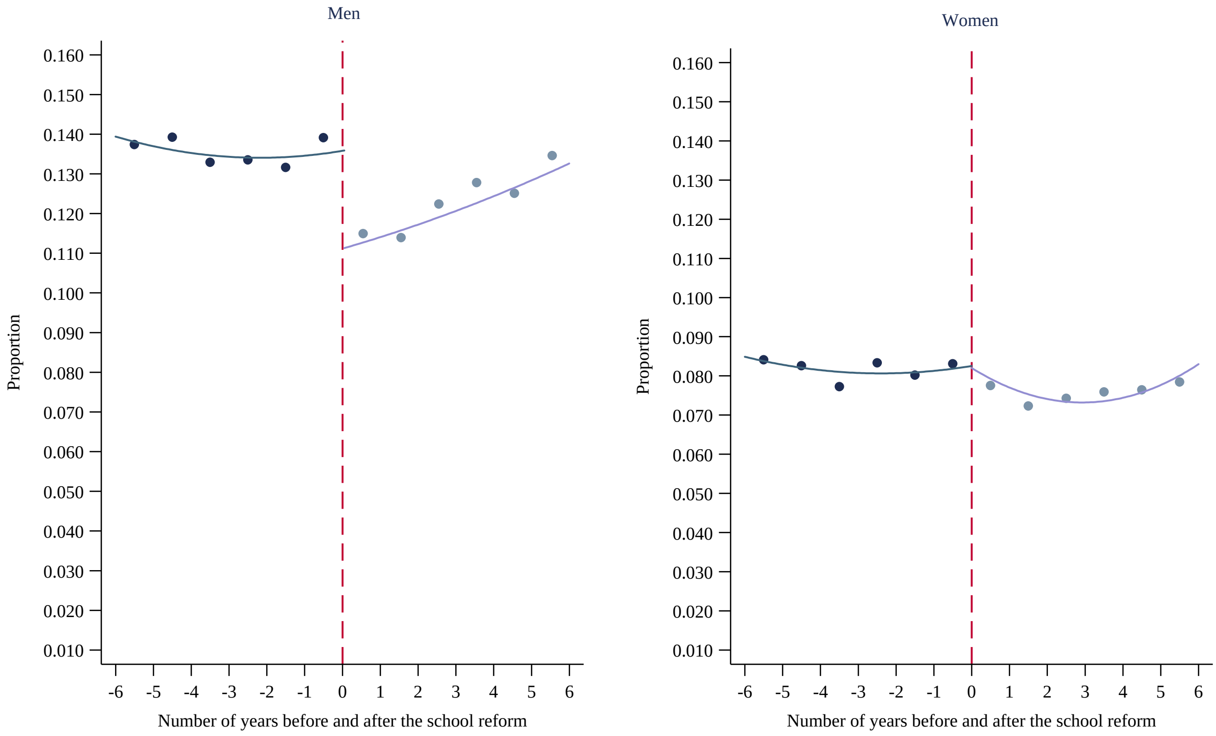


Figure 4. Proportion of deaths for individuals aged between 16-64 years, before and after the school reform.
Individuals born in 1951 or earlier

Table 1. Balancing tests. Bandwidth 4 years. Individuals born in 1951 or earlier.
Regression coefficients with standard errors clustered by municipality (in brackets)[1].

	Predetermined variables						
	Father's level of education in years	Mother's level of education in years	Father's age in years when the reform was introduced	Mother's age in years when the reform was introduced	Whether the father died at the age of 64 or earlier =1	Whether the mother died at the age of 64 or earlier =1	Number of siblings
Reform=1	0.0897 **	0.0483 *	0.0372	0.1261	0.0053	-0.0028	-0.0242
	(0.0386)	(0.0251)	(0.0915)	(0.0727)	(0.0051)	(0.0036)	(0.0188)
Total	137 682	144 872	137 682	144 872	137 682	144 872	132 625

** p<0.05
* p<0.10

[1] Municipality-specific time trends, parents' year of birth, child’s year of birth and an indicator variable for missing data for each of the predetermined variables included in all the analyses

Table 2. The effect of the school reform on the number of years of education and on the probability of dying in three different groups. Bandwidth 4 years. Individuals born in 1951 or earlier. First and second stage regressions. Regression coefficients with standard errors clustered by municipality (in brackets).

	Age between 16 and 64 years			Age between 16 and 49 years			Age between 50 and 64 years[1]		
Variables	Men and women	Men	Women	Men and women	Men	Women	Men and women	Men	Women
Ordinary least square									
Education (in years)	-0.0117 ***	-0.0150 ***	-0.0093 ***	-0.0046 ***	-0.0066 ***	-0.0030 ***	-0.0076 ***	-0.0094 ***	-0.0066 ***
	(0.0004)	(0.0005)	(0.0004)	(0.0002)	(0.0002)	(0.0002)	(0.0003)	(0.0004)	(0.0003)
Reduced form									
Reform=1	-0.0181 ***	-0.0276 ***	-0.0073	-0.0126 ***	0.0189 ***	-0.0055 **	-0.0066 *	-0.0109 **	-0.0021
	(0.0042)	(0.0054)	(0.0054)	(0.0020)	(0.0031)	(0.0027)	(0.0036)	(0.0047)	(0.0044)
First stage estimates									
Reform=1	0.52 ***	0.59 ***	0.45 ***	0.51 ***	0.58 ***	0.45 ***	0.49 ***	0.56 ***	0.43 ***
	(0.056)	(0.064)	(0.061)	(0.056)	(0.064)	(0.061)	(0.055)	(0.063)	(0.061)
F- value	84.6	83.7	53.6	84.6	83.7	53.6	80.5	79.0	49.7
Second stage estimates									
Education (in years)	-0.0266 **	-0.0358 ***	-0.0117	-0.0158 ***	-0.0206 ***	-0.0081	-0.0127 *	-0.0187 **	-0.0043
	(0.0090)	(0.0101)	(0.0122)	(0.0043)	(0.0056)	(0.0061)	(0.0075)	(0.0088)	(0.0103)
Number of deaths	19 053	12 054	6 999	6 549	4 431	2 118	12 504	7 623	4 881
Total	180 355	92 324	88 031	180 355	92 324	86 458	173 806	87 893	85 913

*** p≤ 0.001
** p<0.05
* p<0.10

[1] Born in 1951 or earlier and alive at the age of 50

Note. Mother's and father's level of education (in years) included in all the analyses

Table 3. The effect of the establishment of upper secondary schools in the trade district on the number of years of education and on the probability of dying for individuals aged between 16 and 49 years. Individuals born in 1966 or earlier.
First and second stage regressions. Regression coefficients with standard errors clustered by trade district (in brackets).

Variables	Men and women	Men	Women
Ordinary least square			
Education (in years)	-0.0051 **	-0.0072 **	-0.0032 **
	(0.0001)	(0.0001)	(0.0001)
Reduced form			
High school =1	-0.0047 **	-0.0061 *	-0.0031 *
	(0.0013)	(0.0019)	(0.0010)
First stage estimates			
Establishment of high school=1	0.32 *	0.34 *	0.31 *
	(0.107)	(0.119)	(0.103)
F- value	8.9	8.3	9.2
Second stage estimates			
Education (in years)	-0.0106 **	-0.0132 *	-0.0071 *
	(0.0031)	(0.0041)	(0.0033)
Number of deaths	58 582	40 052	18 800
Total	1 671 832	856 059	815 773

** $p \leq 0.001$
* $p<0.05$

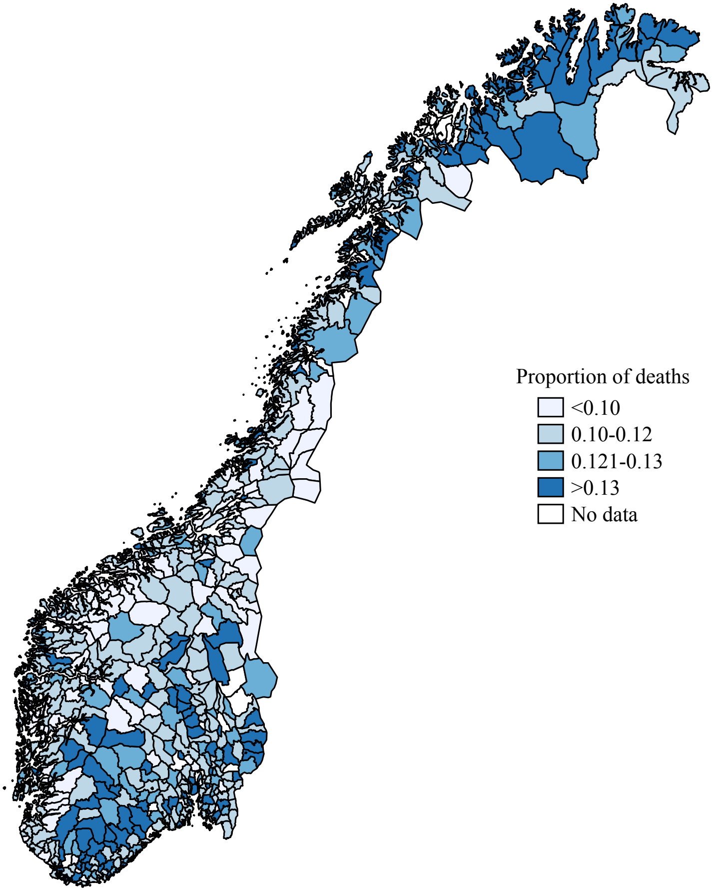
Note. Mother's and father's level of education (in years) included in all the analyses

Table 4. The effect of the school reform on causes of death. Bandwidth 4 years
Individuals born in 1951 or earlier. Marginal effects with standard errors clustered by municipality (in brackets).

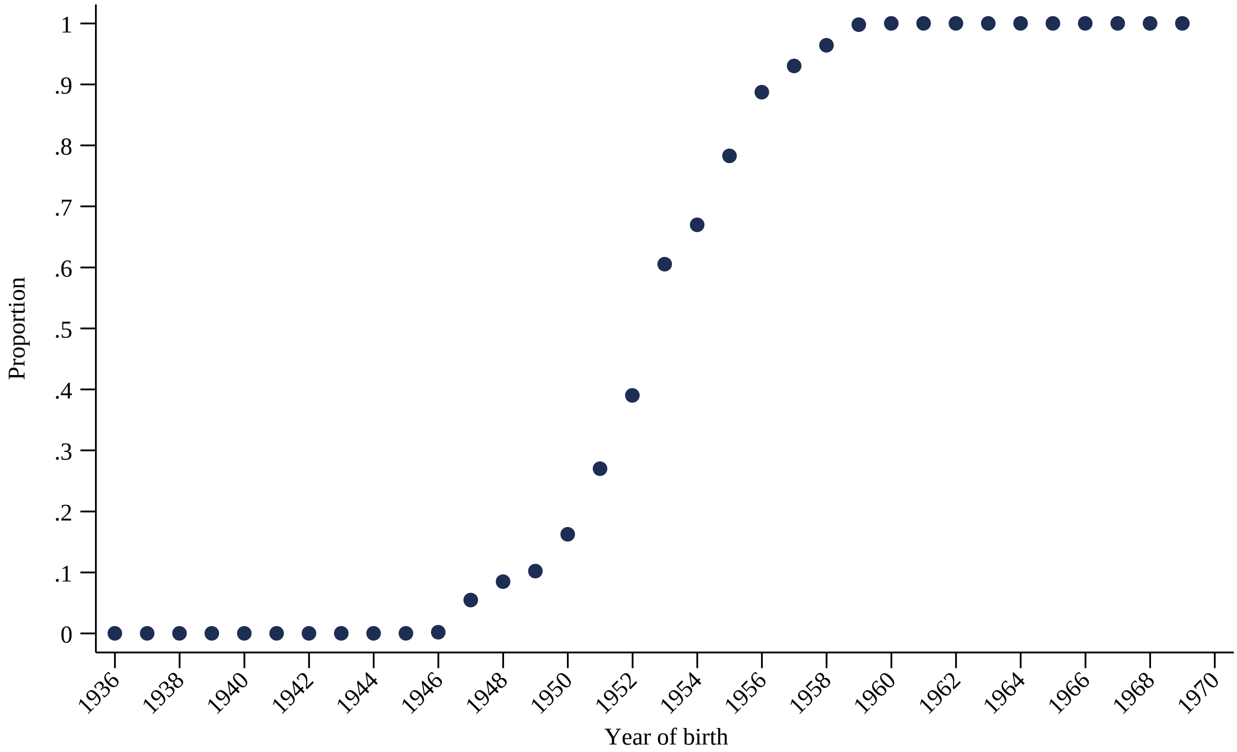
	Men and women		Men		Women	
Cause of death	Marginal effects (standard error)	N	Marginal effects (standard error)	N	Marginal effects (standard error)	N
Alive	0.0169 *** (0.0049)	161 302	0.0270 *** (0.0061)	80 270	0.0063 (0.0060)	81 032
Deaths that were amenable to behavioural change only =1	-0.0043 ** (0.0024)	3 727	-0.0069 * (0.0038)	2 420	-0.0017 (0.0020)	1 307
Deaths that were amenable to medical intervention only =1	0.0002 (0.0014)	1 827	0.0006 (0.0013)	690	-0.0001 (0.0024)	1 137
Deaths that were amenable to both behavioural change and medical intervention =1	-0.0069 *** (0.0021)	6 303	-0.0094 ** (0.0035)	4 761	-0.0043 * (0.0023)	1 542
Deaths that could not be prevented either by behavioural change or medical intervention =1	-0.0005 (0.0013)	1 911	-0.0017 (0.0016)	1 132	0.0006 (0.0016)	779
Deaths that could not be classified according to whether they could be prevented =1	-0.0053 ** (0.0021)	5 285	-0.0095 ** (0.0032)	3 051	-0.0008 (0.0030)	2 234
Total		180 355		92 324		88 031

*** $p \leq 0.001$
** $p<0.05$
* $p<0.10$

Note. Mother's and father's level of education (in years) included in all the analyses



Appendix 1. Proportion of deaths for individuals aged between 16-64 years, by municipality. Individuals born in 1951 or earlier



Appendix 2. The proportion of individuals exposed the school reform by year of birth

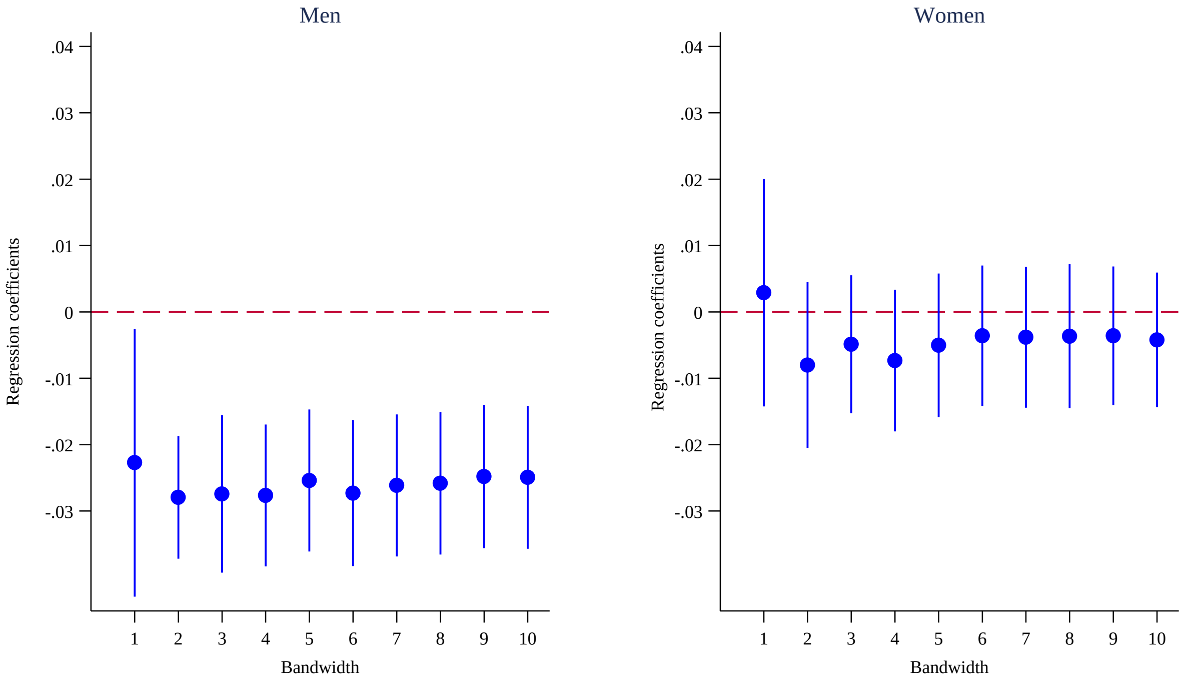
Appendix 3. The effect of the school reform on the number of years of education and on the probability of dying in three different groups. Bandwidth 4 years. Individuals born in 1951 or earlier. First and second stage regressions. Regression coefficients with standard errors clustered by municipality (in brackets).

	Age between 16 and 64 years			Age between 16 and 49 years			Age between 50 and 64 years[1]		
Variables	Men and women	Men	Women	Men and women	Men	Women	Men and women	Men	Women
Ordinary least square									
Education (in years)	-0.0117 ***	-0.0150 ***	-0.0092 ***	-0.0046 ***	-0.0066 ***	-0.0029 ***	-0.0076 ***	-0.0094 ***	-0.0066 ***
	(0.0004)	(0.0005)	(0.0004)	(0.0002)	(0.0002)	(0.0001)	(0.0003)	(0.0004)	(0.0003)
Reduced form									
Reform=1	-0.0187 ***	-0.0286 ***	-0.0078	-0.0131 ***	-0.0196 ***	-0.0060 **	-0.0067 *	-0.0112 **	-0.0021
	(0.0043)	(0.0055)	(0.0054)	(0.0020)	(0.0032)	(0.0027)	(0.0035)	(0.0047)	(0.0044)
First stage estimates									
Reform=1	0.54 ***	0.64 ***	0.45 ***	0.54 ***	0.63 ***	0.45 ***	0.52 ***	0.60 ***	0.43 ***
	(0.063)	(0.069)	(0.072)	(0.063)	(0.069)	(0.072)	(0.062)	(0.069)	(0.073)
F- value	75.2	84.5	39.6	75.2	84.5	39.6	69.0	76.4	35.9
Second stage estimates									
Education (in years)	-0.0258 **	0.0340 ***	-0.0123	-0.0154 ***	-0.0193 ***	-0.0086	-0.0124 *	-0.0180 **	-0.0044
	(0.0086)	(0.0095)	(0.0122)	(0.0040)	(0.0051)	(0.0062)	(0.0072)	(0.0082)	(0.0102)
Number of deaths	19 053	12 054	6 999	6 549	4 431	2 118	12 504	7 623	4 881
Total	180 355	92 324	88 031	180 355	92 324	86 458	173 806	87 893	85 913

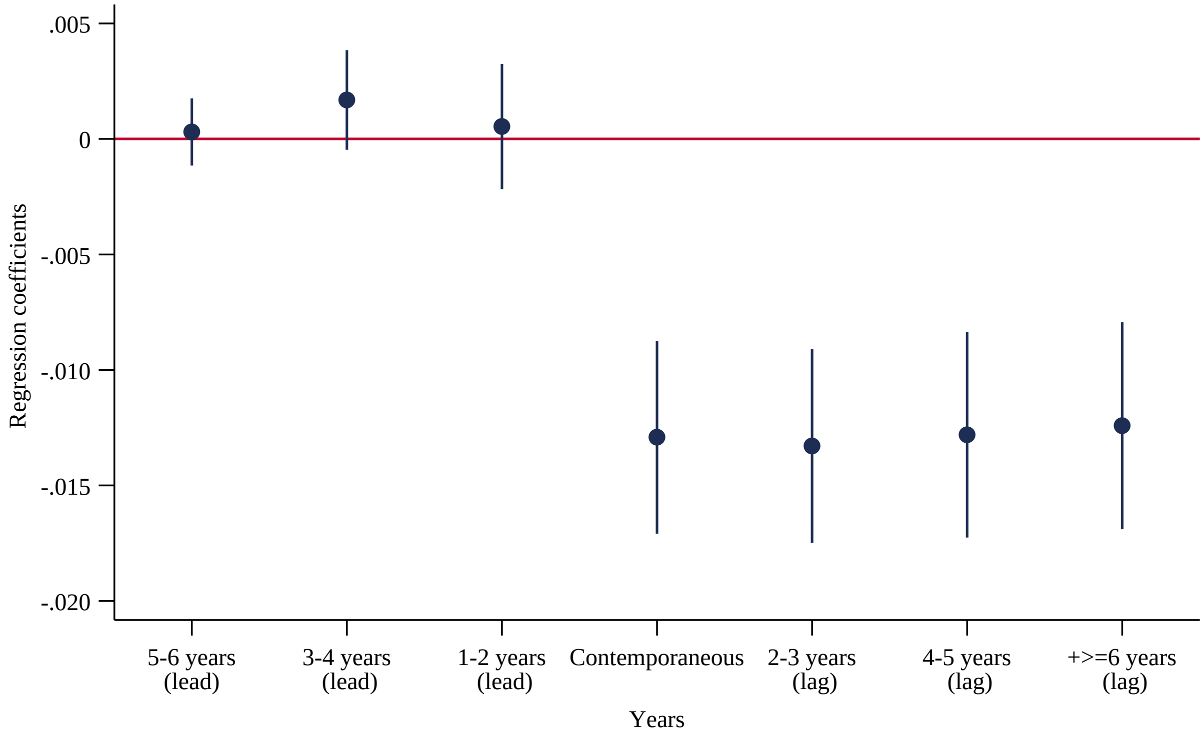
*** p≤ 0.001
** p<0.05
* p<0.10

[1] Born in 1951 or earlier and alive at the age of 50

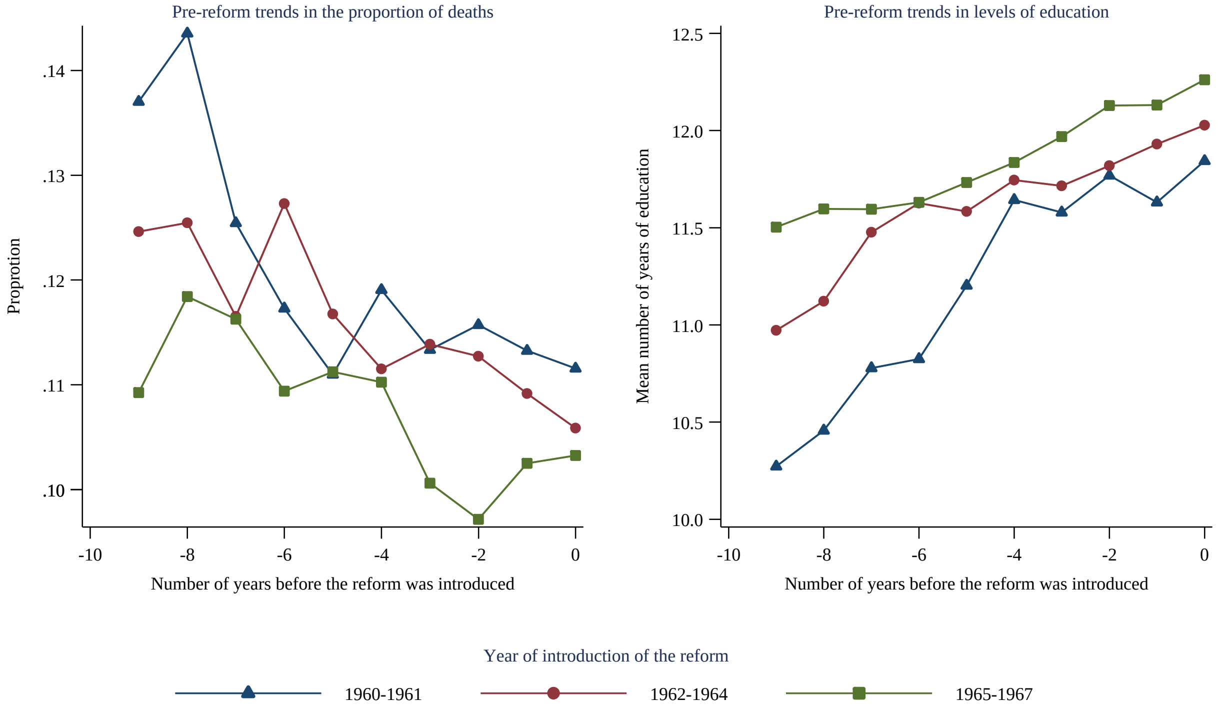
Note. Mother's and father's level of education (in years) <u>not</u> included in the analyses



Appendix 4. The effect of the school reform on the probability of death for individuals aged between 16-64 years for men and women born in 1951 or earlier. Estimates with different bandwidths. Reduced form regression coefficients with 95% confidence intervals



Appendix 5. Lead-lag effects of the school reform on the probability of deaths for individuals aged between 16-64 years. Individuals born in 1951 or earlier. Reduced form regression coefficients with 95% confidence intervals

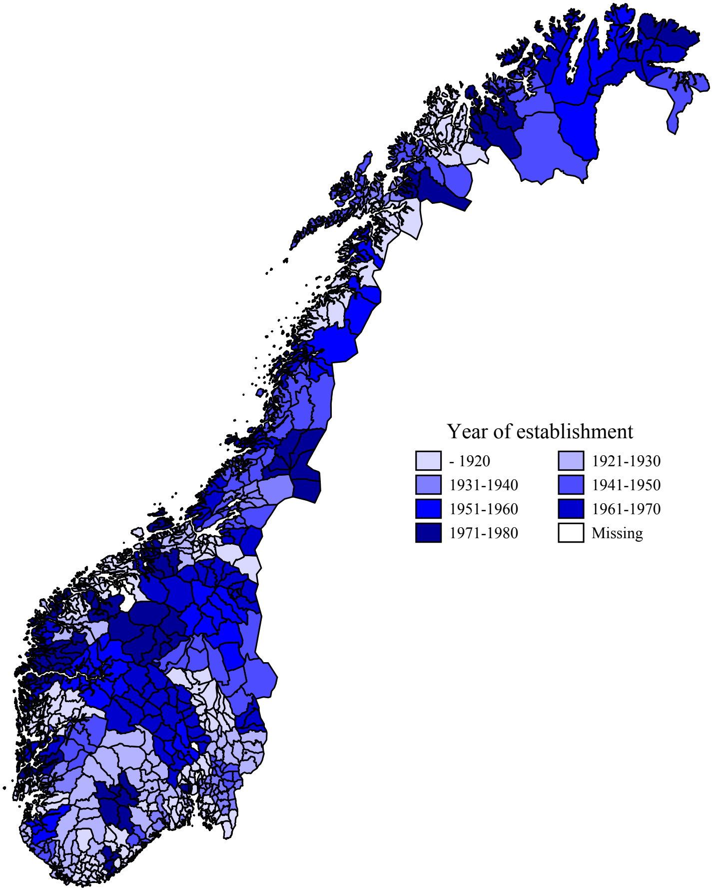


Appendix 6. Pre-reform trends in the proportion of deaths for individuals aged between 16 and 64 years, and in mean number of years of education. Municipality level data

Appendix 7. The effect of education and the proportion of deaths in the year the reform was introduced. Individuals born in 1951 or earlier. Municipality level data. Regression coefficients with standard errors clustered by municipality (in brackets).

	Proportion of deaths for individuals aged between 16 and 64 years	Mean number of years of education
Year the reform was introduced	-0.0003 (0.0004)	0.0019 (0.0061)
Number of observations (municipality-years)	5 481	5 481

Note: Year of birth, proportion of women, and parents' level of education (mean number of years) included in all the analyses



Appendix 8. Year of establishment of upper secondary schools in Norway according to trade districts

Appendix 9. Causes of death, classified according to the criteria described by Mackenbach et al. (2015) [1].
Number of deaths for individuals born 1951 or earlier. Bandwith 4 years.

Diagnosis	Causes of death amendable to: Behavioural change only	Medical intervention only	Both behavioural change and medical intervention	Deaths that could not be prevented either by behavioural change or medical intervention	Number of deaths
Accidents			Yes		2 110
Alcohol abuse	Yes				898
Appendicitis, hernia and peptic ulcer		Yes			110
Cancer of breast		Yes			806
Cancer of buccal cavity, pharynx, and oesophagus	Yes				280
Cancer of cervix			Yes		158
Cancer of colorectum				Yes	855
Cancer of kidney and bladder				Yes	297
Cancer of larynx	Yes				25
Cancer of liver				Yes	108
Cancer of pancreas				Yes	411
Cancer of prostate		Yes			163
Cancer of stomach				Yes	240
Cancer of trachea, bronchus and lung	Yes				1 656
Cerebrovascular disease			Yes		691
Chronic obstructive pulmonary disease	Yes				486
Diabetes mellitus	Yes				280
Hodgkins's disease and leukemia		Yes			607
Hypertensive disease			Yes		95
Ischemic heart disease			Yes		2 085
Pneumonia/influenza		Yes			141
Suicide			Yes		1 164

[1] Mackenbach JP, Kulhánová I, Bopp M, Deboosere P, Eikemo TA, Hoffmann R, Kulik MC, Leinsalu M, Martikainen P, Menvielle G, Regidor E, Wojtyniak B, Östergren O, Lundberg O. Variations in the relation between education and cause-specific mortality in 19 European populations: a test of the “fundamental causes” theory of social inequalities in health. Social Science & Medicine 2015; 127: 51-62.

Appendix 10. The effect of the school reform on deaths from accidents, alcohol abuse and lung cancer for individuals born in 1951 or earlier. Bandwidth 6 years.
Marginal effects with standard errors clustered by municipality (in brackets).

	Men and women		Men		Women	
Specific cause of death	Marginal effects (standard error)	N	Marginal effects (standard error)	N	Marginal effects (standard error)	N
Accidents =1	-0.0041 *** (0.0012)	3 418	-0.0057 *** (0.0023)	2 799	-0.0025 *** (0.0010)	619
Other causes =1	-0.0116 *** (0.0048)	27 175	-0.0223 *** (0.0048)	16 579	-0.0006 (0.0062)	10 596
Alive =1	0.0158 *** (0.0052)	253 658	0.0280 *** (0.0063)	126 241	0.0032 (0.0060)	127 417
Total		284 251		145 619		138 632
Alcohol abuse =1	-0.0019 ** (0.0011)	1 569	-0.0037 ** (0.0020)	1 220	-0.00002 (0.0007)	349
Other causes =1	-0.0139 *** (0.0047)	29 024	-0.0245 *** (0.0057)	18 158	-0.0028 (0.0060)	10 866
Alive =1	0.0158 *** (0.0052)	253 658	0.0282 *** (0.0064)	126 241	0.0029 (0.0061)	127 417
Total		284 251		145 619		138 632
Cancer of bronchus and lung =1	-0.0014 * (0.0011)	2 707	-0.0014 (0.0017)	1 538	-0.0013 * (0.0012)	1 169
Other causes =1	-0.0142 *** (0.0046)	27 886	-0.0263 *** (0.0056)	17 840	-0.0015 (0.0057)	10 046
Alive =1	0.0156 *** (0.0052)	253 658	0.0278 *** (0.0063)	126 241	0.0029 (0.0061)	127 417
Total		284 251		145 619		138 632

*** $p<0.05$
** $p<0.10$
* $p<0.30$

Note. Mother's and father's level of education (in years) included in all the analyses