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Compulsory schooling reforms, education and mortality in twentieth century Europe

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ABSTRACT

Education yields substantial non-monetary benefits, but the size of these gains is still debated. Previous studies report causal effects of education and compulsory schooling on mortality ranging anywhere from zero to large and negative. Using data from 18 compulsory schooling reforms implemented in Europe during the twentieth century, we quantify the average mortality gain and explore its dispersion across gender, time and countries. We find that more education yields small mortality reductions in the short- and long-run for men. In contrast, women seem to experience no mortality reductions from compulsory schooling reforms.

Introduction

There is a strong statistical relationship between health, health behavior, and education, or socio-economic status more broadly which is known since at least 170 years (e.g., Cutler & Lleras-Muney, 2010; Engels, 1845; Grossman, 2006; Virchow, 1862). Various theoretical mechanisms have been suggested to explain this link. First, education may not only increase productivity in the labor market but also in non-market activities which include the "production of health". Education then makes an individual more efficient in producing health, for example, by raising the benefits of health inputs like doctor visits (Grossman, 1972). Second, education may change the inputs into health production, for example by affecting health-relevant behaviors such as smoking, drinking, diet, exercising or using safety precautions (Cutler & Lleras-Muney, 2010; Rosenzweig & Schulz, 1981). Such changes might occur because schools teach what is healthy or because educated people are better able to obtain and evaluate health information (Kenkel, 1991; Nayga, 2000; de Walque, 2007).

Education can affect health and health behaviors through other channels (Lochner, 2012; Oreopoulos & Salvanes, 2011). For instance, education raises income which allows buying healthier food, living in healthier environments, or purchasing better health insurance or health care. Moreover, the better educated usually work in physically and psycho-socially less demanding occupations. Recent research shows no education gradient among nuns and monks (Luy, 2003), suggesting that education-related differences in environment and occupation play important roles outside cloisters. Education might also affect psychological predispositions that directly or indirectly improve health and health behaviors, such as sense of control or time preferences (Becker & Mulligan, 1997; Fuchs, 1982).

However, a strong and stable correlation does not imply a causal link from education to health. Unobserved (social and genetic) background might drive both good health and better education. The correlation could also be explained by reverse causation if unhealthy people obtain less education.

To identify causal effects of education on health, exogenous variation in education is crucial. Recent studies exploit compulsory schooling reforms, which generate exogenous variation at the lower end of the educational distribution. Using data from different countries and time periods the literature reports, however, conflicting results. Lleras-Muney (2005) finds that one additional year of compulsory schooling reduces 10-year mortality in the United States by as much as 6 percentage points. Cipollone and Rosolia (2011), van Kippersluis, O'Donnell, and van Doorslaer (2011) and Fischer, Karlsson, and Nilsson (2013) report much smaller effects for Italy, the Netherlands and Sweden, respectively. Other studies find no reductions in mortality in France (Albouy & Lequien, 2009), the United Kingdom (Clark & Royer, 2013), or the United States (Mazumder, 2008). Recent analyses for Sweden report small, only marginally significant effects of compulsory schooling on all-cause mortality (Lager & Torssander, 2012; Meghir et al., 2012).

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Are such diverse results the consequence of differences in data sources and statistical modeling decisions? Or are the differences systematically linked to economic or institutional context (Cutler & Lleras-Muney, 2010)? For instance, education might have nonlinear effects on mortality because more education affects mortality at low but not high baseline educational levels (Clark & Royer, 2013; Meghir et al., 2012). It is difficult to separate these explanations using a single reform in a single country.

In this paper, we use 18 European reforms to estimate the average mortality effect of compulsory schooling and education over the course of the twentieth century. Our multi-country setting has several advantages over single country analyses. First, we compare mortality effects of compulsory schooling legislation for different time periods and countries using harmonized data. This comparison may explain why previous studies, which rely on different reforms, data sources and mortality measures, report divergent results. Second, our analysis reveals whether compulsory schooling reforms have an effect on mortality across a range of settings. Finally, we explore if treatment effects of reforms are systematically related to a country's characteristics.

Methods

Data sources

We base our analysis on 18 national reforms of compulsory schooling implemented in 11 European countries and regions between 1903 and 1976: Austria, Belgium, Denmark, England and Wales, Scotland, Northern Ireland, France, Ireland, Italy, the Netherlands and Spain. Table 1 lists key features of each reform: the year of implementation and minimum schooling levels, which birth cohort was first affected by the reform and their mortality rates. The online appendix provides more detailed information on each reform in our analysis.

To analyze the effect of these reforms on educational attainment (measured in years of schooling) we use data from the European Social Survey (ESS), the International Social Survey Programme (ISSP) and the Survey of Health, Ageing and Retirement in Europe (SHARE). The ESS and ISSP are repeated cross-sectional surveys based on nationally representative samples of the population aged 18 and older (15 and older in the ESS) in over 20 countries (ESS) and over 30 countries (ISSP) respectively. SHARE is based on nationally representative samples of the population aged 50 and older in 15 European countries.

We use all five waves of the ESS with sample sizes of 1500–3000 individuals in each country and wave. For the ISSP, we use all 25 waves with sample sizes of 1000–3000 individuals in each country and wave. For SHARE, we use three waves with sample sizes of 1000–4000 individuals in each country and wave.

We pool data across surveys and all available waves and calculate average educational attainment for all cohorts around the reform date, separately by gender and country. Overall, we have information on educational attainment for 27,237 women and 21,979 men once we restrict the sample to the 10 cohorts before and 10 years after each reform. We measure education only for respondents who have lived long enough to become sample members. Given the educational gradient in mortality (and survey participation), we might therefore overstate average educational attainment for older cohorts. As long as this selection bias is a smooth function of age and education, however, it is absorbed by age polynomials in our estimations.

Our mortality data come from the Human Mortality Database (2011). This database uses a common methodology to convert death and birth counts from vital statistics into mortality rates by year, age and birth cohort for over 30 countries. For our analysis, we use the cohort-specific mortality rates by age and year, computed as the probability that an 18 year-old cohort member dies over the next 20, 30, 40 or 50 years. We choose 18 as our starting age because compulsory schooling is completed at that age in all countries. As an example, consider the 1947 reform in England and Wales, which has increased the minimum school leaving age from 14 to 15. The first cohort affected was born in 1933 and turned 18 in 1951. The 20-year mortality rate for this cohort is computed as one minus the probability that an individual survives until 1971, conditional on being alive in 1951. We calculate mortality rates over longer time spans, for other cohorts and other countries accordingly.

Reduced form estimation

We first explore whether there is an effect of compulsory schooling reforms on mortality comparing mortality rates of cohorts just affected by the reforms to mortality rates of cohorts that are unaffected by the reform. The reduced-form approach mirrors

Table 1
Compulsory schooling reforms in Europe.[a]

Country	Year reform was implemented	First cohort affected	Change in minimum schooling level	Change in the number of years	Male 20-year mortality for first cohort affected	Female 20-year mortality for first cohort affected
Denmark	1903	1890	7–9	2	0.083	0.085
Belgium	1919	1910	3–8	5	0.085	0.063
Netherlands	1928	1917	6–7	1	0.052	0.033
France	1937	1923	7–8	1	0.095	0.054
Scotland	1946	1932	8–9	1	0.028	0.019
England and Wales	1947	1933	8–9	1	0.022	0.014
Netherlands	1950	1938	7–9	2	0.020	0.011
Northern Ireland	1957	1943	8–9	1	0.028	0.013
Italy	1963	1949	5–8	3	0.022	0.011
Austria	1966	1953	8–9	1	0.034	0.015
France	1967	1953	8–10	2	0.035	0.014
Spain	1970	1957	6–8	2	0.029	0.010
Ireland	1972	1958	8–9	1	0.022	0.009
Northern Ireland	1972	1958	9–10	1	0.028	0.011
Denmark	1971	1957	7–9	2	0.028	0.012
England and Wales	1973	1958	9–10	1	0.019	0.009
Netherlands	1973	1959	9–10	1	0.019	0.009
Scotland	1976	1958	9–10	1	0.023	0.011

[a] See the Online Appendix for a description of each compulsory schooling reform. Mortality rates in the last two columns are based on data from the Human Mortality Database (2011).

specifications used in regression discontinuity designs (Imbens & Lemieux, 2008; Lee & Lemieux, 2010). Specifically, we estimated Equation (1) separately by country and gender:

$$\ln\left(\frac{m_c^x}{1-m_c^x}\right) = \alpha + \beta_{\mathrm{RD}} D_c + \gamma_1 Y_c + \gamma_2 Y_c^2 + \gamma_3 (D_c \times Y_c) + \gamma_4\left(D_c \times Y_c^2\right) + u_c \quad (1)$$

where m_c^x is the x-year mortality rate of cohort c. D_c is a dummy variable that indicates if a cohort is affected by the reform, and Y_c is the year of birth. Our baseline estimates use ten cohorts before and after the reform. We exclude the first cohort potentially affected as some members of that cohort might not have been treated by the reform.

To adjust flexibly for cohort trends in mortality, we use local quadratic polynomials in the year of birth Y_c (relative to the first cohort affected). We estimate effects for each country separately, which accounts flexibly for any country-specific cohort trends as well as for heterogeneous treatment effects across countries. To allow for differences in compliance with the reform and gender-specific mortality trends, we run separate analyses for male and female mortality.

As long as there is some compliance with the reform and monotonicity, the reduced-form shows whether there is *any* effect of schooling on mortality. Below, we report odds ratios, $exp(\beta_{\mathrm{RD}})$ which (for values close to one) approximate relative annual mortality risks. Our identifying assumption is that cohorts born just above and just below the threshold face the same potential no-reform mortality risks conditional on flexible cohort trends.

Two sample 2SLS estimation

Using data on educational attainment from the ESS, ISSP and SHARE, we implement a 2SLS estimator on the subset of reforms with education data. In the first stage, we estimate a variant of Equation (1). The dependent variable is now average educational attainment for cohort c and the estimation is performed separately by gender and country as before. Like the reduced-form estimates, we estimate the first stage with data aggregated by birth cohort, gender and country.

The second stage in turn identifies the causal effect of one additional year of compulsory schooling on mortality among those affected by the reform (assuming some compliance and monotonicity). To implement the 2SLS estimator, we estimate the following model:

$$\ln\left(\frac{m_c^x}{1-m_c^x}\right) = \beta_0 + \beta_{2\mathrm{SLS}}\mathrm{Educ}_c + \tilde{\gamma}_1 Y_c + \tilde{\gamma}_2 Y^2 + \tilde{\gamma}_3 (D \times Y) + \tilde{\gamma}_4\left(D \times Y^2\right) + u_c \quad (2)$$

where we use the indicator for compulsory schooling reforms D_c as an instrument for years of schooling Educ_c in the first stage. Controls include a local quadratic polynomial to capture secular trends in educational attainment. As before, the estimation is run separately by country and gender to account for country- and gender-specific mortality differences.

Because the 2SLS estimator is based on separate data sources, we implement a variant of the two-sample 2SLS estimator (Angrist & Krueger, 1992; Inoue & Solon, 2010). Note that the 2SLS and IV estimator no longer coincide when two different data samples are used. Inoue and Solon (2010) recommend the 2SLS estimator because it corrects for differences in the distribution of the instrument across the two samples.

The identifying assumption of the 2SLS estimator is that compulsory schooling reforms affect mortality only through the years of education obtained. This assumption would be problematic if countries reform the quality of education as well. In that case, the 2SLS estimates reflect mortality reductions associated with changes in both quality and quantity of schooling.

Meta-analysis of reform effects

In the final step, we estimate the mean and dispersion of reform effects in Europe, independent of the particular circumstances of a compulsory schooling reform, the time period or the implementing country. We perform separate meta-analyses for the reduced-form and the 2SLS estimates.

To pool country-specific results, individual reform estimates are weighted using random or fixed effects (e.g., Borenstein, Hedges, Higgins & Rothstein, 2009). The fixed effects approach assumes no heterogeneity of mortality effects in the underlying population and assigns a weight equal to the inverse of the country's sample variance to each reform. The random effects approach assumes that mortality effects are heterogeneous and drawn from a normal distribution. The goal of the random effects approach is to estimate the mean of the distribution of effects, not a single "true" effect. It also gives lower weights to outliers. To determine the degree of heterogeneity of effects across countries, we use I-squared:

$$I - \text{squared} = \max\left(0, \frac{Q-\mathrm{df}}{Q} * 100\right) \quad (3)$$

where $Q = \sum_{i=1}^{k} w_i(\beta_i - \overline{\beta}_i)^2$. The weights w_i are the inverse of the sampling variance of study i, β_i is the treatment effect in study i, $\overline{\beta}_i$ is the mean effect across all studies, and df denotes the degrees of freedom. I-squared varies from 0% to 100%, with higher values indicating more treatment effect heterogeneity and thus favoring a random effects approach. Values of 50% (75%) are considered thresholds for moderate (large) treatment effect heterogeneity (Higgins, Thompson, Deeks, & Altman, 2003).

Results

Reduced form effects on mortality

Odds ratios and 95% confidence intervals of the mortality effects of compulsory schooling reforms in the short run (ages 18–38 and ages 18–48) and in the long run (ages 18–58 and ages 18–68) are shown in Tables 2 and 3. For 20-year mortality of men, 13 out of the 18 odds ratios are smaller than one indicating a reduction in mortality. For six reforms, we find statistically significant reform effects (at the 5% level). For 30-year mortality rates, nine out of the 13 odds ratios indicate a reduction in mortality, and for 40-year mortality rates, we find that five out of eight odds ratios show a reduction in mortality.

Overall, the most consistent mortality reductions for men are observed for the 1919 reform in Belgium and the 1928 reform in the Netherlands (as the odds ratio is smaller than one). The Belgian reform has reduced 30-year mortality by (1/0.956–1)*100 = 5.6%, 40-year mortality by 4.6%, and 50-year mortality by 4.3%. The Dutch reform has reduced male 30- and 40-year mortality by 4.4% and 6.6%, respectively.

To explore whether our data do not have sufficient power to uncover statistically significant effects in most countries, we use meta-analysis to estimate a pooled effect across reforms. Figs. 1 and 2 show forest plots of reform-specific effects and the pooled

Table 2
Effect of compulsory schooling reforms on male mortality.[a]

Country	Year	OR (20-year)	95%-CI	OR (30-year)	95%-CI	OR (40-year)	95%-CI	OR (50-year)	95%-CI
Denmark	1903	0.982	(0.905, 1.065)	0.991	(0.949, 1.035)	1.007	(0.957, 1.060)	0.992	(0.956, 1.029)
Belgium	1919	**0.903**	(0.848, 0.961)	**0.947**	(0.913, 0.983)	**0.956**	(0.933, 0.980)	**0.959**	(0.939, 0.981)
Netherlands	1928	0.949	(0.824, 1.094)	0.950	(0.880, 1.024)	0.973	(0.933, 1.015)	0.992	(0.965, 1.020)
France	1937	1.348	(1.048, 1.734)	1.216	(1.023, 1.444)	1.129	(1.015, 1.255)	1.081	(1.002, 1.166)
Scotland	1946	1.016	(0.942, 1.096)	0.957	(0.907, 1.009)	0.974	(0.947, 1.003)	1.016	(0.990, 1.042)
England and Wales	1947	0.935	(0.783, 1.116)	0.943	(0.867, 1.025)	0.969	(0.924, 1.015)	1.013	(0.984, 1.043)
Netherlands	1950	0.981	(0.913, 1.054)	**0.958**	(0.919, 0.999)	**0.938**	(0.900, 0.978)		
Northern Ireland	1957	**0.926**	(0.871, 0.983)	1.047	(0.946, 1.158)	1.037	(0.920, 1.170)		
Italy	1963	1.025	(0.969, 1.085)	1.016	(0.957, 1.078)				
Austria	1966	**0.935**	(0.876, 0.998)	0.940	(0.875, 1.010)				
France	1967	**0.951**	(0.927, 0.976)	0.979	(0.939, 1.021)				
Spain	1970	0.984	(0.939, 1.032)	0.979	(0.934, 1.025)				
Denmark	1971	1.064	(0.920, 1.229)	1.003	(0.887, 1.134)				
Ireland	1972	0.973	(0.909, 1.042)						
Northern Ireland	1972	0.943	(0.864, 1.029)						
England and Wales	1973	**0.984**	(0.971, 0.998)						
Netherlands	1973	1.027	(0.959, 1.100)						
Scotland	1976	**0.938**	(0.885, 0.994)						
Pooled Estimates		**0.971**	(0.952, 0.990)	**0.973**	(0.955, 0.990)	**0.974**	(0.952, 0.997)	0.999	(0.999, 1.024)

[a] The table shows reduced-form estimates of the 19 compulsory schooling reforms on 20-year, 30-year, 40-year and 50-year mortality for men. The numbers (in columns labeled "OR") are the coefficients on the reform dummy (from estimating Equation (1) separately for each country) converted to exp(β); hence, they represent the effect on the odds of dying (and not the log odds ratio as in Equation (1)). The upper and lower bound of the corresponding 95% confidence intervals are also reported in brackets next to the estimate. The pooled estimate is from a meta-analytic model using random effects to construct weights for each reform (see Section Two-Sample 2SLS Estimates of Education on Mortality for further details). Statistically significant effects are marked in bold.

estimate for 20-year mortality rates (based on a random effects approach as we can reject the null hypothesis of no heterogeneity with $p < 0.05$). The gray square represents the weight of each reform in the pooled estimation. The pooled estimate (shown as a dashed line) suggests an overall reduction in male mortality by 2.8%. We find similar and statistically significant effects for 30- and 40-year mortality rates, but not for 50-year mortality rates (see the bottom of Table 2). In contrast, we find basically no significant effect for women (shown in Table 3). For 20-year mortality, the point estimates are often negative but not statistically significant. We also find no evidence for a reduction in longer-run mortality. Only the 1928 reform in the Netherlands reduces female mortality (by 12.2% for 20-year mortality, 6.3% for 30-year mortality and 5.6% for 40-year mortality). The meta-analysis (in Fig. 2 and the bottom of Table 3) shows insignificant pooled effects close to one. Hence, compulsory schooling reforms as implemented in Europe during the twentieth century seem to have had substantial health benefits in terms of lower mortality rates for men, but hardly any mortality effects for women.

The impact of the reforms on educational attainment

Table 4 reports the results of reform-by-reform regressions of educational attainment on compulsory schooling reform dummy variables, controlling for polynomials of age or cohort, respectively. Estimates of the effect of the reforms on the number of years of schooling are shown in the column labeled "First stage for 2SLS".

On average, the compulsory schooling reforms in Europe increase years of education by 0.50 years for men and 0.54 years for women (see bottom of Table 4). Individually, most reforms have no statistically significant effects on schooling in our data and would not pass the usual diagnostic checks for weak instruments. A few reforms show a negative effect on educational attainment though none of them are statistically significant.

Table 3
Effect of compulsory schooling reforms on female mortality.[a]

Country	Year	OR (20-year)	95%-CI	OR (30-year)	95%-CI	OR (40-year)	95%-CI	OR (50-year)	95%-CI
Denmark	1903	1.05	(0.995, 1.108)	1.017	(0.974, 1.063)	0.996	(0.969, 1.025)	1.030	(1.005, 1.056)
Belgium	1919	1.005	(0.943, 1.072)	1.026	(0.975, 1.079)	1.040	(0.997, 1.084)	1.000	(0.978, 1.023)
Netherlands	1928	**0.891**	(0.841, 0.945)	**0.941**	(0.900, 0.983)	**0.947**	(0.921, 0.974)	0.987	(0.972, 1.002)
France	1937	1.082	(0.954, 1.226)	1.041	(0.955, 1.134)	1.016	(0.956, 1.081)	1.005	(0.965, 1.047)
Scotland	1946	0.999	(0.959, 1.040)	1.033	(0.994, 1.074)	0.986	(0.943, 1.031)	0.985	(0.955, 1.016)
England and Wales	1947	1.002	(0.974, 1.031)	1.006	(0.971, 1.042)	1.001	(0.969, 1.034)	1.032	(1.017, 1.047)
Netherlands	1950	0.935	(0.839, 1.041)	0.964	(0.925, 1.005)	0.989	(0.934, 1.047)		
Northern Ireland	1957	0.957	(0.799, 1.146)	0.966	(0.918, 1.016)	0.937	(0.851, 1.032)		
Italy	1963	0.983	(0.931, 1.038)	0.998	(0.958, 1.041)				
Austria	1966	1.026	(0.953, 1.104)	1.001	(0.953, 1.052)				
France	1967	0.967	(0.918, 1.018)	**0.959**	(0.925, 0.994)				
Spain	1970	1.018	(0.934, 1.108)	1.023	(0.984, 1.064)				
Denmark	1971	0.968	(0.801, 1.170)	1.044	(0.927, 1.177)				
Ireland	1972	0.934	(0.845, 1.032)						
Northern Ireland	1972	**0.878**	(0.791, 0.975)						
England Wales	1973	0.994	(0.956, 1.034)						
Netherlands	1973	0.995	(0.940, 1.054)						
Scotland	1976	0.986	(0.912, 1.065)						
Pooled Estimates		0.986	(0.966, 1.006)	0.996	(0.978, 1.014)	0.991	(0.968, 1.014)	1.007	(0.988, 1.027)

[a] See Table 2.

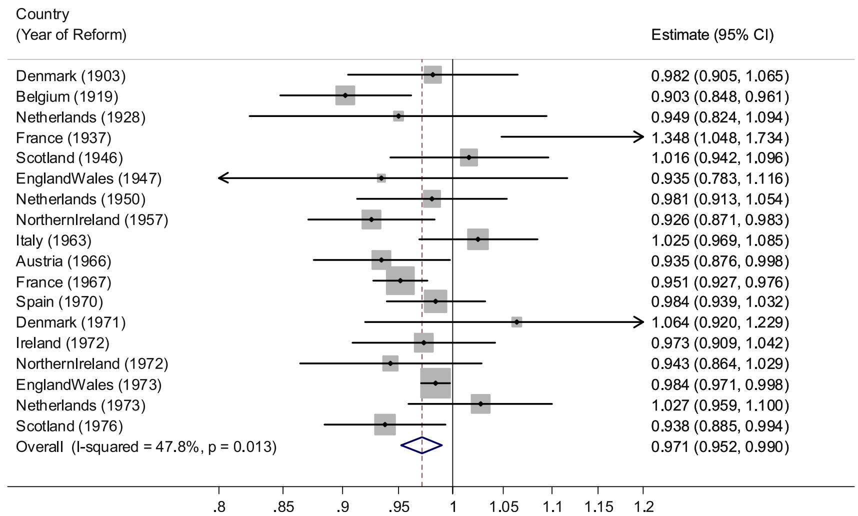

Fig. 1. Reduced-form Estimates of Compulsory Schooling Reforms on 20-year Mortality Rates (Men). The figure summarizes the reduced-form effects of 18 compulsory schooling reforms on the odds of men dying between the ages 18 and 38 (conditional on surviving to age 18). The point estimates (represented in the figure as a gray square) are constructed by estimating Equation (1) and converting the resulting coefficients β to $\exp(\beta)$. The corresponding confidence intervals are represented as a horizontal line. The size of each square indicates the weight of each reform in the pooled estimation (with larger squares indicating a higher weight). The I2-statistic (in percentages) reflects the degree of heterogeneity of estimates. According to the *p* value, we can reject the null that there is no heterogeneity across studies. The pooled estimate (using random reform effects to construct the weights) is shown as the dashed line.

The lack of precision of the first-stage estimates is not surprising. First, the number of observations within each cohort-year-gender cell is relatively small (76 on average). Second, we use data aggregated to the cohort-year-gender level to be consistent with our mortality data, and we restrict the first stage to only 10 birth cohorts above and below the reform threshold. As such, the weak first-stage estimates are likely the consequence of small samples and possible measurement error in education. Given that other studies have successfully used multiple European compulsory schooling reforms (Brunello, Fort, & Weber, 2009; Fort, Schneeweis, & Winter-Ebmer, 2011) the variability of our first-stage estimates does not necessarily indicate that compulsory schooling reforms had no effect on educational attainment. Indeed, our estimates are similar in magnitude, though considerably less precise than existing micro estimates (shown on the right of Table 4 labeled "Estimates in the literature"). Yet, the average effect on educational attainment in the literature is about 0.4 years and hence similar to our pooled estimates of 0.50 for men and 0.54 for women.

Two-sample 2SLS estimates of education on mortality

The two-sample 2SLS estimates are based on the subset of reforms for which we have cohort-specific data on both years of education and mortality. We further drop reforms with a negative first-stage. A negative first-stage could arise because of measurement error in education in some countries or to a real negative effect of the reform on educational attainment. Given our large-scale international surveys, the first explanation seems unlikely. More plausibly, we think that some unobserved feature of the reform's implementation or compliance produced a negative first stage. To avoid confounding effects, we therefore drop those cases.

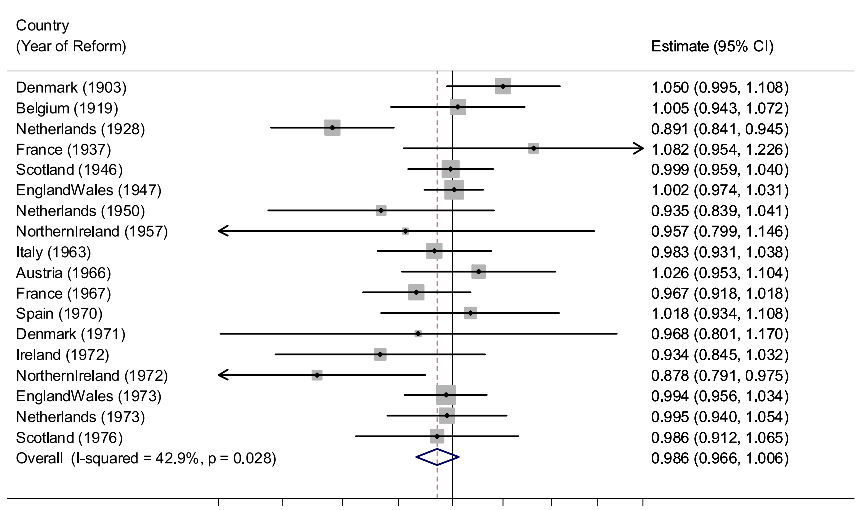

Fig. 2. Reduced-form Estimates of Compulsory Schooling Reforms on 20-year Mortality Rates (Women). See notes to Fig. 1.

Table 4
Effect of compulsory schooling reforms on years of schooling.[a]

Country	Reform year	Gender	Our first-stage estimates		Estimates in earlier studies		
			First-stage coefficient	Standard error	First stage coefficient	Standard error	Source
Netherlands	1928	Men	1.745	(3.480)	0.669	(0.109)	van Kippersluis et al. (2011)
Netherlands	1928	Women	1.273	(0.707)	No effect	NA	van Kippersluis et al. (2011)
France	1937	All			0.110	(0.064)	Albouy and Lequien (2009)
France	1937	Men	−1.788	(1.642)			
France	1937	Women	0.161	(0.880)			
England and Wales	1947	Men	0.421	(0.645)	0.420	(0.062)	Clark and Royer (2013)
England and Wales	1947	Women	0.425	(0.460)	0.527	(0.054)	Clark and Royer (2013)
Great Britain	1947	All			0.436	(0.071)	Oreopoulos (2006)
Netherlands	1950	Men	−0.617	(0.795)	−0.399	(0.564)	Levin and Plug (1999)
Netherlands	1950	Women	0.342	(0.634)			
Northern Ireland	1957	All			0.391	(0.073)	Oreopoulos (2006)
Italy	1963	Men	0.581	(0.854)			
Italy	1963	Women	−0.095	(0.710)	0.190	(0.035)	Brandolini and Cippolone (2002)
Austria	1966	Men	1.551	(0.769)			
Austria	1966	Women	0.943	(0.615)			
France	1967	All			0.280	(0.063)	Albouy and Lequien (2009)
France	1967	Men	0.348	(0.692)	0.263	(0.025)	Grenet (2013)
France	1967	Women	1.271	(0.617)	0.274	(0.035)	Grenet (2013)
Spain	1970	Men	1.292	(0.843)	0.720	(0.274)	Pons and Gonzalo (2002)
Spain	1970	Women	0.824	(0.821)			
Denmark	1971	Men	0.450	(0.681)	0.262	(0.285)	Arendt (2005)
Denmark	1971	Women	−0.161	(0.667)	0.166	(0.302)	Arendt (2005)
Ireland	1972	Men	0.069	(0.597)			
Ireland	1972	Women	0.477	(0.520)			
England and Wales	1973	Men	1.078	(0.599)	0.267	(0.028)	Grenet (2013)
England and Wales	1973	Men	1.078	(0.599)	0.318	(0.049)	Clark and Royer (2013)
England and Wales	1973	Women	0.427	(0.473)	0.310	(0.022)	Grenet (2013)
England and Wales	1973	Women	0.427	(0.473)	0.252	(0.050)	Clark and Royer (2013)
Netherlands	1973	Men	0.324	(0.830)			
Netherlands	1973	Women	1.173	(0.707)			
Pooled Estimates		Men	0.501	(0.222)			
		Women	0.541	(0.181)			

[a] The table shows the first-stage of compulsory schooling laws on education. The coefficients are from estimating Equation (1) separately by country and gender where the dependent variable is the average years of schooling completed in birth cohort c. The sample is restricted to the 10 cohorts above and below the reform threshold. The right-hand side of the table shows first-stage estimates reported in the literature for individual reforms and the respective sources.

We also drop the earliest reforms because we have no data available to estimate the first stage. The sample now includes the following nine reforms for men and ten reforms for women: Netherlands 1928, France 1937 (women only), England and Wales 1947, the Netherlands 1950 (women only), Austria 1966, France 1967, Spain 1970, Ireland 1972, England and Wales 1973, the Netherlands 1973 and Denmark 1971 (men only).

The pooled 2SLS estimates for men show a 2.04% reduction in 20-year mortality and a 3.84% reduction in 30-year mortality for an additional year of schooling (see Table 5). Note also that we now

Table 5
Effect of compulsory schooling reforms on 20- and 30-year mortality[a] (two-sample 2SLS).

Country	Year	Men				Women			
		20 Year mortality		30 Year mortality		20 Year mortality		30 Year mortality	
		OR	95%-CI	OR	95%-CI	OR	95%-CI	OR	95%-CI
Netherlands	1928	0.985	(0.929, 1.045)	0.988	(0.949, 1.029)	0.937	(0.808, 1.087)	0.965	(0.883, 1.055)
France	1937					0.871	(0.572, 1.325)	0.932	(0.741, 1.172)
England and Wales	1947	0.846	(0.523, 1.368)	0.864	(0.599, 1.246)	1.004	(0.959, 1.050)	1.012	(0.959, 1.067)
Netherlands	1950					0.847	(0.594, 1.207)	0.913	(0.746, 1.119)
Austria	1966	**0.960**	(0.924, 0.998)	**0.950**	(0.926, 0.975)	1.029	(0.966, 1.096)	1.002	(0.920, 1.092)
France	1967	0.897	(0.609, 1.323)	1.083	(0.786, 1.491)	0.973	(0.938, 1.010)	0.979	(0.958, 1.000)
Spain	1970	0.989	(0.958, 1.020)			1.019	(0.950, 1.093)		
Denmark	1971	1.152	(0.857, 1.547)						
Ireland	1972	0.788	(0.230, 2.701)			0.864	(0.716, 1.043)		
England and Wales	1973	0.986	(0.966, 1.007)			0.986	(0.900, 1.080)		
Netherlands	1973	1.061	(0.842, 1.338)			0.995	(0.952, 1.041)		
Pooled Estimates		**0.983**	(0.968, 0.998)	**0.961**	(0.940, 0.982)	0.991	(0.971, 1.012)	0.982	(0.964, 1.001)
I-Squared		0.0%		13.5%		0.0%		0.0%	
p-value		0.871		0.325		0.637		0.803	

[a] The table shows two sample 2SLS estimates of compulsory schooling on 20- and 30-year mortality for men (left-hand side) and women (right-hand side). The set of compulsory schooling reforms is restricted to reforms for which we have data on both education and 30-year mortality rates and reforms with a positive first-stage (see Table 4). The numbers (in columns labeled "OR") are the coefficients on the reform dummy β (from estimating Equation (1) separately for each country and gender) converted to $\exp(\beta)$. The coefficients thus represent the effect on the odds of dying (and not the log odds ratio as in Equation (1)). The upper and lower bound of the corresponding 95% confidence intervals are also reported in brackets next to the estimate. The pooled estimate is from a meta-analytic model using fixed effects to construct weights for each reform (see Section Two-Sample 2SLS Estimates of Education on Mortality for further details). Statistically significant effects are marked in bold.

cannot reject the hypothesis effect homogeneity. Most likely, this is due to the fact that the 2SLS estimates are “normalized” with respect to the number of years of schooling, whereas the reduced form effects were heterogeneous with respect to the number of years compulsory schooling was increased.

In contrast to men, we find no significant mortality effects of education for women. This is not surprising, given that the reduced form effects were already not significant.

Unfortunately, we cannot implement the 2SLS estimator for 40- and 50-year mortality rates because data on educational attainment are not available for the very early reforms.

Robustness checks

Our identification relies on the assumption that cohorts left and right of the threshold are similar to each other in every aspect other than being subject to the compulsory schooling reform (including unobserved characteristics). This assumption is more likely to hold if the sample is restricted to cohorts very close to the threshold. Yet, a narrow time window also reduces statistical power. We re-estimated our reduced form models using both a shorter time window (5 cohorts on each side of the threshold) and a longer time window (20 cohorts on each side). As expected, the effects were less precisely estimated using only 5 cohorts, and the pooled estimates were no longer statistically significant for men. Using 20 cohorts, we found a statistically significant reduction both for male mortality (for 7 reforms) and female mortality (for 6 reforms). Moreover, the pooled estimate using $\pm$ 20 cohorts now indicated a significant mortality reduction for women (see Tables A2 and A3 in the Online Appendix).

Another concern might be that a second-order polynomial in birth cohort does not accurately capture secular trends in mortality. To check the robustness of our results, we re-estimated the reduced form models using a local linear trend or global cubic function in birth cohort instead. The resulting estimates using different cohort-specific trends generated very similar results to the baseline using a local quadratic polynomial.

Since our mortality data are available at an annual frequency, we employ a hazard rate approach to trace differences in mortality over the life-cycle. The dependent variable is the log cohort- and age-specific mortality rate and control variables include a local linear (or quadratic) trend of the birth cohort as well as age fixed effects. The duration estimates yields similar results to our baseline (see Table A4 in the Online Appendix). We use the hazard approach also to control for the mortality effects of World War II. In most countries of our study, cohorts born before or during the war are likely to be affected by war-related destruction, hunger, occupation or displacement. To account for increased mortality during war years (1939–1945), we add an indicator variable to our baseline specification. A comparison of the estimates with and without World War II reveals few differences (see Tables A4 and A5 in the Online Appendix). Overall, the hazard approach suggests that our focus on decennial mortality intervals captures the effect of compulsory schooling sufficiently well.

International migration flows, in particular emigration from Italy to Germany or from Ireland to England during our study period, are another challenge to our identification. Data on migration flows by education level since 1975 (Defoort, 2006; Docquier & Marfouk, 2006) suggest that the majority of Italian and Irish emigrants are low-skilled. How might this affect our results? Consider two scenarios: First suppose that migrants are educated in their home country, but then leave and never return to their country. In this case, the mortality and education of migrants are measured in the destination country. Education in the country of origin is then overstated; whether mortality is over- or understated depends on whether migrants are healthier than stayers in the country of origin. Now assume that migrants return to their home country. Then the migrants show up in the survey and mortality statistics of the country of origin. Our approach remains valid in the second scenario because education attainment and mortality are not mismeasured. On the whole, we think the possible bias from emigration is not a major concern for our analysis (see also Clark & Royer, 2013).

Discussion

Our study is motivated by the fact that the growing literature on the causal effects of formal education on mortality, or health in general, has found mixed results. Even researchers using seemingly identical identification strategies – compulsory schooling reforms – arrive at different conclusions depending on the country, the time period or gender studied. However, it is unclear whether these differences can be explained by variation in the quality of data, operationalization of key variables, or other methodological choices. The aim of our paper is to study the contextual factors of the causal link between education and health holding these factors constant. Using a common methodology and strictly comparable data, we analyze the effect of 18 compulsory schooling reforms implemented across Europe between 1903 and 1976 on mortality.

Gender differences

Overall, we find that lengthening compulsory schooling led to small reductions in mortality among men, but we find no significant reductions for women. For men, the risk of dying between the ages 18–38 (the 20-year mortality rate) is reduced by 2.9%. Very similar effects are found for 30- to 50-year mortality. Women's mortality is however not affected by the schooling reforms at any age. Similar gender asymmetries of compulsory schooling reforms are documented for health behaviors in Germany (Kemptner, Jürges, & Reinhold, 2011) and for mortality in Sweden (Meghir et al., 2012). The two-sample 2SLS estimates show a picture similar to that of the reduced-form: men benefit from additional education, but women do not. For men, an additional year of education reduces the 20-year mortality rate by around 2.0%. Note that the difference between the effect for men and women is, however, not statistically significant.

It is interesting to contrast our findings with (descriptive) evidence from the U.S. For instance, Zajacova and Hummer (2009) find no significant gender differences in the association between education and mortality at lower levels of education but some differences favoring men at education levels past high-school.

The asymmetries in our study cannot be explained by differences in compliance with the new schooling laws. On average, the compulsory schooling reforms have increased years in school by about the same amount: 0.501 for men and 0.541 for women.

Another explanation for the gender asymmetry is that health benefits of compulsory schooling are transmitted via labor market participation, e.g., through higher earnings or higher occupational status. In that case, we expect later reforms to have stronger effects on female mortality than earlier reforms, because female labor market participation rates increase towards the end of our study period. We test this hypothesis but find no relationship between female labor market participation rates (around the reform dates) and our estimated reduced-form effects (details are not reported).

The differential effect could also be the consequence of gender-specific occupational choices and associated differences in exposure to occupational health hazards. Men are traditionally overrepresented in blue-collar jobs. After the compulsory schooling reforms, they might have found white-collar jobs instead and are therefore less exposed to hazardous working conditions. Unfortunately, we do not have the data to test this relationship empirically.

Gender differences in mortality reductions from education are important because of secular trends in mortality differences across gender. During most of the 19th and 20th century, the mortality gap between men and women widens (Lopez, 1983). Over the last two or three decades, the pattern reversed. The mortality gap might narrow because men and women engage similarly in (un-)healthy behavior; or because medical progress favors the treatment of conditions that traditionally kill more men than women (Glei & Horiuchi, 2007). Our results suggest that the widespread educational expansion in developed countries over the course of the 20th century benefits men more than women – contributing to the decline of the gender mortality gap.

Effect heterogeneity across time and space

Our reduced-form estimates suggest that there is substantial heterogeneity in mortality effects across time. Across alternative specifications, the effects on male mortality are larger if the education reform is implemented prior to 1930. On average, these reforms reduce male mortality between the ages 18 and 38 by 6.4%. In contrast, reforms implemented after 1970 reduce male mortality between the ages 18 and 38 by just 1.2%.

One explanation for this pattern is that early reforms increase the average school leaving age by more years than later reforms. Fig. 3 plots the reduced-form odds ratio against the first-stage estimates for men (top panel) and women (bottom panel). The figure indicates that mortality reductions (an odds ratio smaller than one) are positively correlated with the effect of the compulsory schooling reform on educational attainment (a large first-stage estimate), although the correlation is much weaker for women.

Reductions in mortality could be larger for early reforms because compulsory schooling is especially effective when baseline educational attainment is low – as was the case in Europe in the early twentieth century. However, the relationship between the minimum schooling requirements just prior to the reform and the reduced-form estimate of the reform effects is very weak for men and essentially zero for women (see Figure A3 in the Online Appendix). These findings suggest that compulsory schooling does not have stronger effects at low education levels.

Yet another explanation for larger effects of early compulsory schooling reforms is that health benefits are larger when baseline mortality rates are comparatively high – as they were prior to World War II in many European countries. We find that a larger baseline mortality rate (just prior to the reform) is associated with a larger reform effect among men but not among women (see Figure A4 in the Online Appendix).

Low income or high poverty rates may be other reasons why countries with lower life expectancy experience higher benefits of additional education. If average income levels are low, additional education and hence higher income might allow more health-improving investments. To proxy average income, we use data on GDP per capita in the years around the reform (using data from Broadberry & Klein, 2012 reported in 1990 US Dollars). Reductions in mortality among men are somewhat larger in countries with lower GDP per capita (but there is no relationship for women) (see Figure A5 in the Online Appendix). These results are consistent with evidence from a Swedish compulsory schooling reform showing stronger mortality reductions for men (but not women) coming from more disadvantaged socio-economic backgrounds (Meghir et al., 2012).

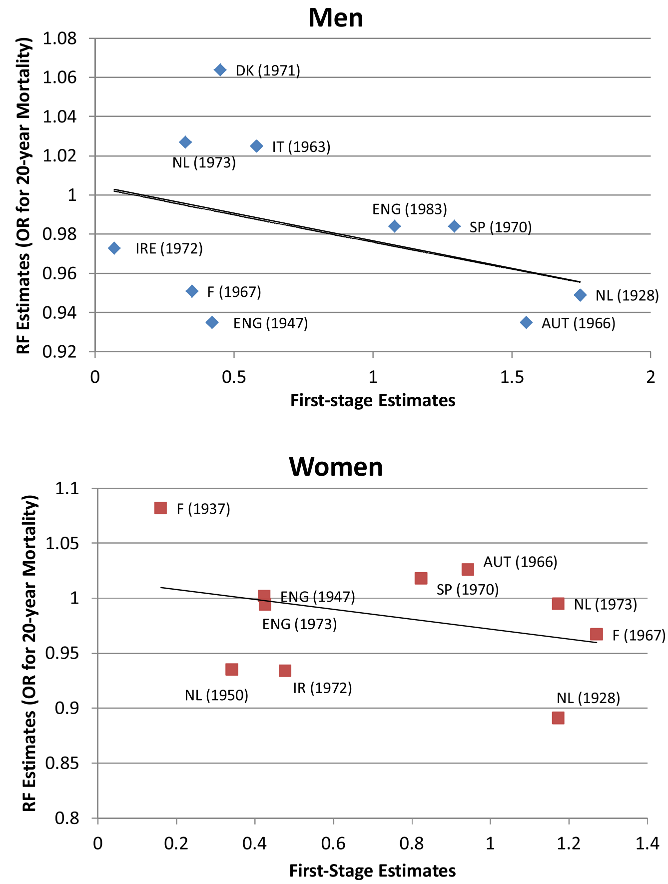

Fig. 3. Reduced-form Mortality and First-Stage Estimates. The figures plot the reduced-form estimate the effect of compulsory schooling reform on the odds ratio of dying between age 18 and 38 (20-year mortality rate) against the first-stage estimate of compulsory schooling reforms on educational attainment. The top panel reports the results for men, the bottom panel for women. The sample is restricted to reforms for which information on educational attainment is available and which have a positive first-stage estimate.

Concluding remarks

Our research contributes to a quickly growing literature on the causal effect of education on health. To shed light on the context of compulsory schooling reforms, we compare effects across a large number of European countries and reforms spanning more than seven decades. As such, our analysis is complementary to the detailed studies based on individual data for the Netherlands (van Kippersluis et al., 2011), Sweden (Meghir et al., 2012) or the United Kingdom (Clark & Royer, 2013). A limitation of our study is that the estimates of the reforms in individual countries are likely to be of low statistical power. We address this concern by meta-analysis. An advantage of this study design is that we were able to explore why the effects found in the literature are heterogeneous. Two consistent findings emerge. First, throughout the 20th century, the effect of compulsory education on health is stronger among men than among women. Thus the effect of compulsory schooling reforms may have varied by the relative proportion of men and women complying with the reform. Second, early twentieth century reforms are more effective than later reforms. This may explain why Lleras-Muney (2005) or van Kippersluis et al. (2011) find fairly large effects for the early twentieth century reforms in the US and the Netherlands. In contrast, reforms enacted in Britain (Clark & Royer, 2013) or France (Albouy & Lequien, 2009) after the Second World War have no causal effects on mortality. Hence our evidence strongly suggests that conflicting results reported in the previous literature are not a coincidence but rather a systematic feature of different reform settings.

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Appendix A. Supplementary data

Supplementary data related to this article can be found at http://dx.doi.org/10.1016/j.socscimed.2014.01.037.

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