Causal effect of education on mortality in a quasi-experiment on 1.2 million Swedes

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In 1949–1962, Sweden implemented a 1-y increase in compulsory schooling as a quasi-experiment. Each year, children in a number of municipalities were exposed to the reform and others were kept as controls, allowing us to test the hypothesis that education is causally related to mortality. We studied all children born between 1943 and 1955, in 900 Swedish municipalities, with control for birth-cohort and area differences. Primary outcome measures are all-cause and cause-specific mortality until the end of 2007. The analyses include 1,247,867 individuals, of whom 92,351 died. We found lower all-cause mortality risk in the experimental group after age 40 [hazard ratio (HR) = 0.96, 95% confidence interval (CI) 0.93–0.99] but not before (HR = 1.03, 95% CI 0.98–1.07) or during the whole follow-up (HR = 0.98, 95% CI 0.95–1.01). After age 40, the experimental group had lower mortality from overall cancer, lung cancer, and accidents. In addition, exposed women had lower mortality from ischemic heart disease, and exposed men lower mortality from overall external causes. In analyses stratified for final educational level, we found lower mortality in the experimental group within the strata that settled for compulsory schooling only (HR = 0.94, 95% CI 0.89–0.99) and compulsory schooling plus vocational training (HR = 0.92, 95% CI 0.88–0.97). Thus, the experimental group had lower mortality from causes known to be related to education. Lower mortality in the experimental group was also found among the least educated, a group that clearly benefited from the reform in terms of educational length. However, all estimates are small and there was no evident impact of the reform on all-cause mortality in all ages.

epidemiology | natural experiment

Understanding what determines mortality at the population level can strengthen our efforts to promote health and reduce health inequalities. Previous research has suggested that education may be such a determinant, because it has been linked to mortality risks both between and within countries, including in natural experiments (1–4).

However, it has not yet been established whether education and health are indeed causally related; neither is the mechanism clear. A causal link between education and health could, for instance, be the result of health literacy learned at school, such as the ability to make use of health messages (5). Alternatively, the link could be an indirect consequence of having a better job and a higher income (i.e., other circumstances related to good health) (6).

It is hard to distinguish possible causal effects of education on health from confounders, such as parental background and cognitive ability (7). At the national level, it has proved difficult to disentangle the effect of educational policies from other progressive policies possibly implemented around the same time. Furthermore, it has been difficult to rule out reverse causality (i.e., that health precedes education, rather than vice versa).

We were able to study the mortality effects of a nationwide Swedish quasi-experiment that was explicitly designed to evaluate the new 9-y comprehensive school before eventually introducing it for all pupils. The reform had three major consequences: (i) 1 further year of compulsory schooling (from 8 to 9 y); (ii) the end of

early tracking into junior secondary schools that before the reform typically took place after the fourth grade of elementary school (with 5 subsequent years at junior secondary school) or after grade six (with 4 subsequent years at junior secondary school); and (iii) that more children, not only those with a diploma from junior secondary school, qualified to go to senior secondary school.

The reform was carried out between 1949 and 1962. Each year, a number of new municipalities, chosen by the national authorities to represent a variety of types of municipality, were included in the experimental group; others were kept as controls. This design means that there are exposed individuals and controls in each cohort as well as each municipality, making it possible to control statistically for potential initial differences between cohorts and municipalities.

The best previous studies in the field have been based on so-called "natural experiments," where researchers must assume that exposures differ for exogenous reasons. Here, by analyzing the effects of the unique Swedish experiment, taking initial mortality differences by municipality and birth cohort into account, we can test the hypothesis that prolonged compulsory schooling reduces mortality in a large quasi-experiment with a pretest and a posttest. Short of being able to carry out a completely randomized study, this is arguably the best research design possible.

One consequence of the reform was that more children were qualified to go on to senior secondary school. Thus, if education does prove to have a causal effect on mortality, some of this effect may be mediated by the reform's positive effect on the probability of continuing to study after the 9 compulsory years. However, a proportion of children in the old school system, those who went to junior secondary school, already received basic education for 9 y (or even 10 y, see Materials and Methods) and qualified for senior secondary school. Thus, this group did not benefit from the reform in terms of number of years at school or in terms of qualifying for senior secondary school. Where the reform had been implemented, the children that otherwise would have gone to junior secondary school were now studying together with those who would otherwise have settled for compulsory schooling. There is no way of separating these groups from each other. Thus, when studying the effect of the reform that is possibly mediated by schooling beyond compulsory or basic level education, all children have to be included in the analysis. Because this means mixing a group that has benefited from the reform (those who would not previously have continued to junior secondary school) with a group for whom the reform did not make a big difference (those who would otherwise have attended junior secondary school), there might be statistical difficulties detecting any effect of the reform. On the other hand, this analysis becomes straight-forward:

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comparing all children in the experimental group to all children in the control group, making it plausible that any effects here are truly causal.

A second, possibly more substantial effect of the reform can be expected for children who in the old system would have opted either for elementary school only or elementary school plus vocational education. These children gained 1 full year of extra schooling thanks to the reform, although the experimental group as a whole gained substantially less on average (8). Again, however, there is no way of knowing exactly which children in the experimental group would have settled for a basic education in the old system (the counter-factual situation here). However, one can assume that they would largely have been the same pupils who also chose the basic education in the new system. Because information about completed education from registers was available for a large proportion of our cohort, we were able to conduct analyses stratified by final educational level. It should be noted that in these analyses we would not expect the experiment to have any protective effect in the groups that had senior secondary schooling or more, given that senior secondary school has always implied (at least) 9 y of basic education.

Results

Experimental status was determined on the basis of census information about home municipality (see *Materials and Methods*). This information was missing for 6,075 (0.40%) individuals. All Swedish children born between 1943 and 1955 in 900 of the 1,029 municipalities that introduced the new type of school for one of the cohorts born between 1944 and 1955 were eligible for our study (1,247,900 individuals). We excluded 33 children (0.003%) who died during the weeks the census took place. Thus, the analytic sample consisted of 1,247,867 individuals: 491,148 (39.4%) in the experimental group and 756,719 (60.6%) in the control group (Fig. 1). Of these individuals, 31,039 (2.5%) were right-censored in the analyses because of emigration. In total, 92,351 deaths (7.4%) occurred during the time of the follow-up.

Descriptive statistics of our analytical sample are given in Table 1. The number of deaths naturally increased with age, so that 71% of all deaths occurred in the latter half of our follow-up (i.e., after age 40). In each age span, all-cause mortality was higher among men than women. Mortality was higher among the

older cohorts than the younger, with the exception of men aged 20–29, where there was no such clear trend.

Analyses of all-cause mortality comparing the entire experimental group with the entire control group during the full follow-up yielded a nonsignificant hazard ratio (HR) of 0.98, 95% confidence interval (CI) 0.95–1.01. In a corresponding Gompertz model, this hazard ratio translated into a predicted median survival time that was 3.9-mo longer for those attending the new school form (with the cohort of 1949 and Stockholm municipality as reference categories).

For men, the full follow-up model violated the assumption of proportional hazards (Table 2). The follow-up period was therefore divided into two halves (i.e., before and after age 40). These analyses suggested a 4% lower all-cause mortality risk in the experimental group after age 40 but not before. This finding was not sensitive to the choice of cutoff age; that is, analyses of mortality before and after age 30, 35, 40, and 45 all yielded significant lower mortality after—but not before—the respective ages. Using 50 as the cutoff age did not yield any significant differences. The cutoff ages of 45 and 50 both produced models that violated the proportional hazards assumption.

Cause-specific analyses after age 40 suggested lower mortality in the experimental group from overall cancer, lung cancer, and accidents (Table 3). In addition, exposed women had lower mortality from ischemic heart disease, and exposed men had lower mortality from overall external causes (also true for men and women together but with a significant sex*reform interaction). Borderline significant lower mortality was found for cancer of lymphatic and hematopoietic tissue ($P = 0.059$) and male suicide ($P = 0.072$). Tests of sex differences in effect of the reform demonstrated significant sex*reform interactions for overall external causes and suicide. Before age 40, the reform demonstrated only one significant difference: lower cerebrovascular disease mortality among women in the experimental group (HR = 0.62, 95% CI 0.41–0.94).

Analyses stratified by educational level demonstrated lower mortality in the experimental groups with compulsory or shorter vocational training; that is, individuals who definitely gained at least 1 extra year of education from the reform (compulsory) and individuals who often did so (vocational) (Table 4).
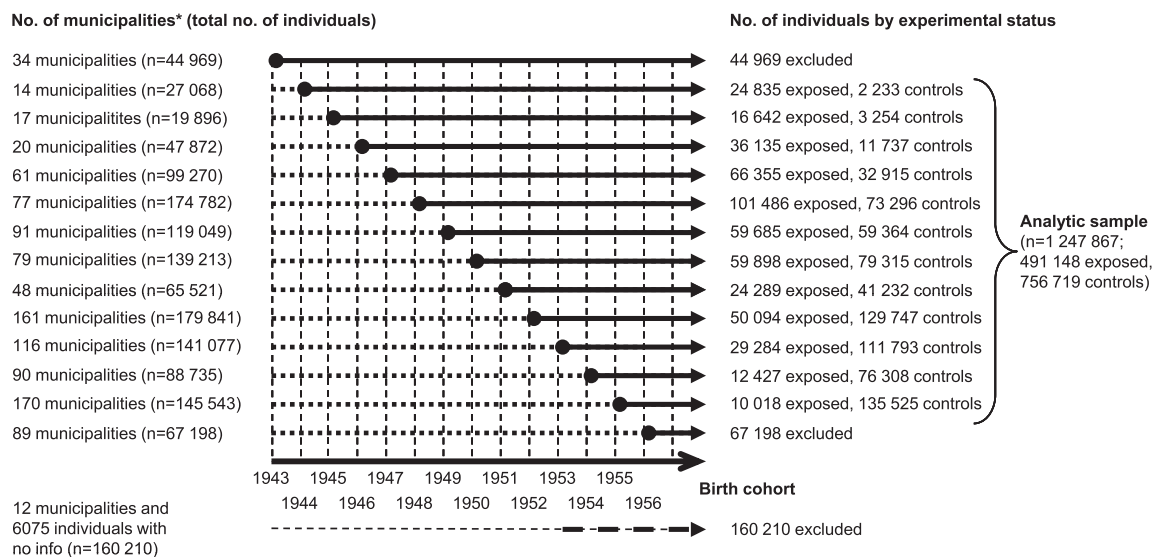
Fig. 1. Analytic sample of study.

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Table 1. Mortality rates between ages 15 and 64 (deaths per 100,000 person years) by age, sex, and birth cohort

Age and cohort	Men						Women					
Age	15–19	20–29	30–39	40–49	50–59	60–64	15–19	20–29	30–39	40–49	50–59	60–64
Birth cohort												
1943	N/A	106	147	266	561	979	N/A	51	84	165	371	631
1944	N/A	109	139	250	523	N/A	N/A	47	81	161	358	N/A
1945	98	102	142	249	499	N/A	38	50	76	169	343	N/A
1946	87	104	141	253	496	N/A	43	49	69	154	332	N/A
1947	90	104	138	246	501	N/A	40	45	71	149	321	N/A
1948	88	108	139	225	461	N/A	39	46	67	152	314	N/A
1949	88	113	133	214	N/A	N/A	43	44	68	146	N/A	N/A
1950	87	110	120	223	N/A	N/A	38	49	71	138	N/A	N/A
1951	80	111	125	221	N/A	N/A	36	47	77	144	N/A	N/A
1952	82	111	124	223	N/A	N/A	36	40	64	135	N/A	N/A
1953	97	105	124	211	N/A	N/A	42	41	72	144	N/A	N/A
1954	91	109	129	213	N/A	N/A	38	45	58	131	N/A	N/A
1955	91	114	132	201	N/A	N/A	37	46	60	129	N/A	N/A
All	89	108	134	231	507	979	39	46	71	148	340	631

N/A, not applicable.

Two instrumental variable (IV) analyses were conducted with different coding of the seven levels of education in the educational register (*Materials and Methods*). The first alternative, where the coding from a previous study was used (9), produced an estimate of the effect of the reform on average years of education of 0.17 y (95% CI 0.14–0.21), an IV-estimate corresponding to 1 y of additional education of HR = 0.80 (95% CI 0.66–0.96), and an observational estimate of 1 extra year in education of HR = 0.91 (95% CI 0.91–0.92).

The second IV analysis, where the old elementary school was assumed to be in practice only 7 y and the experimental group and control group were allowed to have different coding in the next two educational categories, produced an estimate of the effect of the reform on average years of education of 0.51 y (95% CI 0.45–0.57), an IV estimate corresponding to 1 y of additional education of HR = 0.93 (95% CI 0.87–0.99) and an observational estimate of 1 extra year in education of HR = 0.92 (95% CI 0.91–0.92).

Discussion

This quasi-experiment does not provide conclusive answers to the question about causal effects of education on mortality. All hazard ratios are close to one, and there was no evident impact of the reform on all-cause mortality in all ages. We found a small reduction in all-cause mortality corresponding to a 4% lower over-all hazard ratio after age 40 for those who were exposed to the new extended compulsory school form, which may suggest that if there are effects of education on mortality, these do not appear shortly after a person has completed his or her education but rather accumulate over time. Cause-specific analyses suggested that the reform was negatively associated with overall cancer, lung cancer, ischemic heart disease (for women), overall external causes (for men), and accident mortality, possibly indicating that tobacco (10, 11) and alcohol (12) play a role here. However, the relationship between education and alcohol consumption is complex (13–15), and an increased risk of mortality, from lung cancer and liver cirrhosis combined, was recently reported for exposed men between 1985–2005 (16).

When each sex was analyzed separately, no cause-specific association was significant in both groups at the same time. However, more formal tests of sex differences (i.e., test of the sex*reform interaction) only supported real sex differences in mortality from external causes in general, and suicide in particular. Strong associations between male suicide and socioeconomic factors in general, and education in particular, has also been reported in a recent systematic review and meta-analysis (17).

We found that the reform was negatively related to mortality in the groups that did not continue to senior secondary or tertiary education, possibly suggesting that a true effect (if any) is partly mediated by the extra year of compulsory schooling itself and not merely by increasing the probability of continuing to secondary or tertiary education. Exactly what mediated the association, even in terms of length of education, is however difficult to assess, which is illustrated here by the two instrumental variable analyses. The first analysis, where previously used coding of the education level variable was applied, produced an IV-estimate that was higher than the observational estimate for the effect of 1 y of education, even if the confidence intervals were

Table 2. Effect of the reform on all-cause mortality among men and women before age 40, in age 40 and after, and during the full follow-up, hazard ratios (95% CI)

Follow-up period (no. of deaths: men/women)	Men (n before 40 and all ages = 639,473; n after age 40 = 613,842)	Women (n before 40 and all ages = 608,394; n after age 40 = 586,677)	All (n before 40 and all ages = 1,247,867; n after age 40 = 1,200,519)
Before age 40 (18,496/8,526)	1.03 (0.97–1.08)	1.02 (0.95–1.10)	1.03 (0.98–1.07)
Age 40 and after (39,867/25,462)	0.96 (0.92–1.00)	0.95 (0.91–1.00)	**0.96 (0.93–0.99)**
All ages (58,363/33,988)	[0.98 (0.95–1.02)]*	0.97 (0.94–1.01)	0.98 (0.95–1.01)

Boldface represents significant P value (P < 0.05). All models include control for birth cohort and municipality fixed-effects. SEs are clustered at the municipal level. The pooled models (both women and men) include control for sex.
*Violation of the proportional hazard assumption.

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Table 3. Effect of the reform on cause-specific mortality after age 40, hazard ratios (95% CI)

Cause of death (no. of deaths: men/women)	Men (n = 613,842)	Women (n = 586,677)	All (n = 1,200,519)
Cancer (11,553/13,827)	0.94 (0.87–1.01)	0.94 (0.88–1.01)	**0.94 (0.90–0.99)**
Of lung/trachea/bronchus/larynx (2,221/2,471)	0.93 (0.78–1.10)	**0.83 (0.71–0.98)**	**0.88 (0.78–0.99)**
Of breast (17/3,480)	0.75 (0.04–15.10)	1.01 (0.89–1.15)	1.01 (0.89–1.15)
Of lymphatic/hematopoietic tissue (1,211/762)	0.89 (0.70–1.12)	0.75 (0.55–1.02)	0.83 (0.69–1.01)
All other cancers (8,104/7,114)	0.95 (0.86–1.04)	0.97 (0.89–1.06)	0.96 (0.90–1.02)
Circulatory diseases (11,474/3,951)	1.04 (0.96–1.13)	0.95 (0.84–1.07)	1.01 (0.95–1.09)
Ischemic heart diseases (7,016/1,612)	1.07 (0.97–1.19)	**0.80 (0.66–0.97)**	1.02 (0.92–1.12)
Cerebrovascular diseases (1,767/1,220)	1.03 (0.86–1.22)	1.04 (0.84–1.28)	1.03 (0.90–1.17)
All other circulatory diseases (2,691/1,119)	0.96 (0.84–1.11)	1.08 (0.86–1.36)	1.00 (0.88–1.13)
External causes (7,676/2,845)	**0.90 (0.83–0.98)**	0.97 (0.83–1.12)	**[0.92 (0.85–0.99)][†]**
Accidents (3,190/962)	**0.85 (0.75–0.97)**	0.91 (0.72–1.15)	**0.87 (0.77–0.98)**
Suicide and intentional self harm (3,180/1,288)	0.89 (0.78–1.01)	0.98 (0.78–1.22)	[0.91 (0.82–1.02)][†]
All other external causes (1,306/595)	1.06 (0.86–1.30)	1.06 (0.79–1.42)	1.06 (0.89–1.25)
All other causes (9,164/4,839)	0.95 (0.88–1.03)	0.99 (0.88–1.10)	0.96 (0.90–1.03)

Boldface represents significant P value (P < 0.05). All models include control for birth cohort and municipality fixed-effects. SEs are clustered at the municipal level. The pooled models (both women and men) include control for sex.
[†]Significant interaction sex*reform.

overlapping. The second analysis, with a coding of number of years of education that was slightly different but based on plausible assumptions, produced a very different estimate. Clearly then, other modifications that might be argued for here (maybe the experimental group attended longer university educations than the control group, for example) could change the estimate again. Therefore, these analyses are perhaps better done where more exact data of the length of education are available.

Two important characteristics distinguish this study from most earlier analyses of educational reforms and their health effects (reviewed in refs. 18 and 19). First, the exposure was manipulated for the express purpose of evaluating its effect (i.e., not for other reasons, as in natural experiments). This means that self-selection into the experiment was limited. Second, the units that were allocated were municipalities within a country, not whole states or countries, which tend to differ more from each other at baseline.

The allocation of the reform was not random but we take this into consideration by controlling for municipality effects. Comparing parents' educational level (available for 86% of the sample) by experimental status, an initial predicted difference in favor of the reform group was observed: 6.2 mo on average (or 0.22 SDs). However, this difference was entirely accounted for after municipality fixed effects had been introduced, with a nonsignificant difference of only 0.4 mo (0.01 SDs) left. We also restricted our analyses to those municipalities for which we have mortality data both before and after the reform. In this way, our study can be thought of as a quasi-experiment with pre- and posttests, arguably the best design possible after the completely randomized controlled trial.

We used municipality of residence in 1960 or 1965 to determine the experimental status of each individual. This means that we have some misclassification. However, most of this misclassification should be because of normal changes of residence that are not related to the school reform or by people with the same mortality risks as others. Such misclassification will be non-differential in nature and should therefore mask stronger mortality effects than those detected. According to previous research, the proportion of children moving between a reform and a non-reform municipality between birth and school age was around 4% in each direction (9, 20). One form of systematic misclassification may arise from some children from high social strata preferring the traditional junior secondary school to the new comprehensive school. However, this kind of misclassification would also bias our results toward the null hypothesis of no effect, given that individuals from high social strata tend to have better health (21).

The fact that this study covers all individuals living in Sweden means that children with major health problems from birth or early childhood are included. Some of these children were not enrolled in ordinary schools because of mental retardation or other serious health conditions. The higher mortality in this group should lead to some attenuation of the effect of the reform.

The reform resulted in a higher probability of continuing to higher studies (8). Thus, stratifying on later educational level means stratifying on a mediator, which possibly introduces systematic bias, and these analyses should therefore be interpreted with caution. We cannot be certain that those members of the control group who opted for compulsory schooling or vocational training were similar to those who made those educational choices in the experimental group. However, because the reform qualified more children for senior secondary schooling, the groups with the

Table 4. Effect of the reform on all-cause mortality for individuals with different levels of highest attained educational level at the end of 1985, hazard ratios (95% CI)

	All	Compulsory	Vocational	Senior secondary	Tertiary less than 3 y	Tertiary 3 y or more
Men	0.96 (0.92–1.00)	0.95 (0.89–1.01)	**0.93 (0.87–0.99)**	0.96 (0.86–1.08)	1.07 (0.91–1.26)	1.06 (0.94–1.20)
Dead/n	40,260/596,769	14,856/173,896	12,071/162,702	5,062/89,627	2,275/53,537	3,140/83,695
Women	0.96 (0.92–1.00)	0.92 (0.84–1.01)	**0.92 (0.85–0.99)**	1.16 (0.97–1.39)	1.01 (0.86–1.19)	0.93 (0.78–1.09)
Dead/n	25,647/574,629	7,961/133,735	9,231/205,271	1,631/47,501	2,246/74,468	2,339/73,356
All	**0.96 (0.93–0.99)**	**0.94 (0.89–0.99)**	**0.92 (0.88–0.97)**	1.01 (0.92–1.12)	1.03 (0.92–1.15)	1.00 (0.90–1.12)
Dead/n	65,907/1,171,398	22,817/307,631	21,302/367,973	6,693/137,128	4,521/128,005	5,479/157,051

Boldface represents significant P value (P < 0.05). The pooled models (both women and men) include control for sex. Follow-up between January 1986 and December 2007.

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lowest educational levels will become smaller. It seems likely that the groups from reform municipalities left at these lowest levels should, if anything, contain a higher proportion of individuals with negative health prospects.

The Swedish educational reform in the 1950s has previously been linked to higher educational attainment and earnings (8) as well as better self-rated health in later life (22). In the present study on mortality, there was no evident impact of the reform on all-cause mortality in all ages. However, the reform was linked to reduced mortality risks in later adulthood, and to causes of death that seem plausible. Lower mortality in the experimental group was also found among the least-educated, a group that clearly benefited from prolonged compulsory schooling in terms of years of education. If more education is in fact causally linked to a reduced risk of premature mortality, this is an argument in favor of longer education for the individual. Whether the same holds true at the population level is difficult to test. However, our findings are in line with both the secular increases in average life-expectancy and the higher life-expectancy in populations that enjoy a longer education.

Materials and Methods

Study Setting and Intervention. In 1949, when the school reform started, Sweden had 8 y of compulsory schooling (23). However, the municipal elementary school at this time ("*folkskola*" in Swedish) was often only 7-y long. Therefore, many children spent their eighth year at a so-called continuation school ("*fortsättningsskola*"), which had less scheduled teaching and was more practically oriented than the elementary school (24). In 1940, around 20% of children proceeded to junior secondary school ("*realskola*") or its equivalent, typically for 5 y after 4 y at elementary school, or for 4 y after 6 y of elementary schooling (23). Thus, this group fulfilled their mandatory 8 y at junior secondary school before the reform.

There was much geographical variation in the old school system; there were also social and economic barriers to secondary schooling. At that time, even some of the public schools charged fees. One purpose of the reform was to get rid of these barriers and democratize education. Another concern was that Sweden was being left behind other countries. In the United States, from where most of the inspiration for the reform came, education was compulsory until age 16 for most children. The United Kingdom decided on 10 y of compulsory schooling in 1944, and the Soviet Union had started to introduce 10-y-long compulsory schooling in the cities in 1939. Sweden, on the other hand, had only fairly recently implemented a 7-year elementary school (between 1936 and 1949), and the 8-y system was, as mentioned above, even less established (23).

The reform itself has been described as the result of a fairly technical and bureaucratic process and discussion (23). The reform was preceded by a long period of intense discussion on the part of Swedish authorities, committees, and experts. Rather than debating pedagogical approaches, they focused on such issues as the relative merits of 4 + 4 y of elementary and junior secondary schooling versus 6 + 2 y. However, works by thinkers such as John Dewey and William Kilpatrick had some impact, and the value of pleasure-filled rather than authoritarian learning was starting to be acknowledged. At least one major change in the curriculum was also implemented: English became a compulsory subject in grades four and five (23).

The new 9-y comprehensive school ("*enhetsskola*" or "*försöksskola*," later "*grundskola*") eventually replaced all three earlier junior school forms. For the majority of children, those who previously only had 7 y of elementary school and the eighth year in continuation school, the new 9-y school meant a 1-y longer basic education and an eighth year that was more academic than before. However, for those who prereform already had gone on to 5 y of academic junior secondary school after 4 y of elementary school, the reform did not mean any dramatic change. For those who prereform had gone on to 4 y of academic junior secondary school after 6 y at elementary school, the reform actually meant 1 y less education than before (23).

Determining Experimental Status. The new school was usually introduced in the first and fifth grade. Thus, a child starting first grade at the time of the reform was exposed directly, and those in second, third, or fourth grade became exposed from the fifth grade and up; all of these are considered "exposed" in our analysis. Those in sixth grade or above at the time of the reform were not exposed at all. At that time, Swedish children started school in the year of their seventh birthday. The oldest cohort in which some children were exposed to 1 extra year of schooling is therefore those who started the fifth grade in 1949 (i.e., born in 1938). The youngest cohort in which some were still not exposed to the new school form were those who started first grade in 1962 (i.e., born in 1955). In the Swedish population registers there is no individual information about whether a pupil attended the new 9-y comprehensive school. This information was therefore derived from home municipality in the censuses of 1960 (for individuals born 1943–1953) and 1965 (for individuals born 1954–1955), which were carried out around November in those years. The exposed children could then be identified through information about the timing of the reform in each municipality. This work has been described in detail earlier (20). With the help of information from the Educational Register (*Data*) on the proportion of children in each cohort receiving less than 9 y of education, we also updated values for municipalities that most probably implemented the reform 1 or 2 y later than stated. We conducted our analysis on cohorts born in 1943 or later, because too large a proportion of earlier cohorts are likely by 1960 to have moved from the municipality where they went to school, compared with later cohorts (20).

The reform variable takes the value 1 for individuals who attended the new 9-y comprehensive school and 0 for everyone else. For example, a child born in 1950 and living in a municipality that introduced the 9-y school for cohorts born in 1950 or later is considered exposed, and hence coded 1.

Data. Three population registers are linked to the censuses of 1960 and 1965: the Cause of Death Register until December 2007, the Migration Register until December 2007, and the Educational Register in 1985.

The Educational Register contains information about seven levels of education [with average number of years in parenthesis (9)]: presenior secondary education shorter than 9 y (8 y), 9-y presenior secondary education (9 y), vocational or senior secondary education shorter than 3 y (11.5 y), senior secondary education for 3 y or more (13 y), tertiary education shorter than 3 y (15 y), tertiary education for 3 y or more (17 y), and doctorate or licensiate degree (21 y). When stratifying on this variable, the compulsory category does not include junior secondary schooling only and the two highest educational categories are collapsed because of low numbers in the last one.

The Educational Register variable is also used in two different instrumental variable analyses: one with identical coding as the one mentioned in the previous paragraph, and another where the first level (presenior secondary education shorter than 9 y) is assumed to in practice correspond to only 7 y (*Study Setting and Intervention*). The second level (9-y presenior secondary education) is coded as 9 y for the experimental group but 9.5 y for the control group (assuming half of them took the old 4 + 5-y track and half the 6 + 4-y track). The third level (vocational or senior secondary education shorter than 3 y) is coded as 11 y for the experimental group (9 + 2 y) but 10 y for the control group.

Outcomes. We studied all-cause and cause-specific mortality registered by the National Board of Health and Welfare. All specific causes of death with more than 3,000 deaths in the total sample are specified: overall cancer, lung cancer, breast cancer, cancer of lymph/hematopoietic tissue, all other cancers, overall circulatory disease, ischemic heart disease, cerebrovascular disease, all other circulatory diseases, overall external causes, accidents, suicide, all other external causes, and all other causes of death together. Here, about 3,000 deaths give 80% power to detect a hazard ratio of around 0.90.

Statistical Analyses. Mortality risks are estimated with Cox proportional hazard regressions with age measured in months. Individuals enter the analyses at the census or in January 1, 1986 (right after the Educational Register data) in IV-analyses and analyses stratified by education level, and are censored at death, emigration, or at the end of follow-up (December 2007). We adjust for cohort and municipality effects by adding dummy-variables for each birth cohort and allowing different baseline hazards for each municipality, with SEs clustered at the municipal level. In formal tests of sex differences in the effect of the experiment (i.e., in analyzing whether there is a significant sex*experiment interaction), control is also added for possible sex differences in mortality trends (i.e., sex*birth cohort interactions). All instrumental variable analyses and the experimental estimates of the effect of 1 observed year in education, also include control for cohort and municipality effects, with SEs clustered at the municipal level.

Ethical Approval. The study was approved by the regional ethics committee of Stockholm, Sweden (no. 2005/556–31).

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