American Economic Journal: Applied Economics 2018, 10(2): 234–256
https://doi.org/10.1257/app.20150365

Education and Mortality: Evidence from a Social Experiment†

By Costas Meghir, Mårten Palme, and Emilia Simeonova*

We examine the effects on mortality and health due to a major Swedish educational reform that increased the years of compulsory schooling. Using the gradual phase-in of the reform between 1949 and 1962 across municipalities, we estimate insignificant effects of the reform on mortality in the affected cohort. From the confidence intervals, we can rule out effects larger than 1–1.4 months of increased life expectancy. We find no significant impacts on mortality for individuals of low socioeconomic status backgrounds, on deaths that are more likely to be affected by behavior, on hospitalizations, and consumption of prescribed drugs. (*JEL* H52, I12, I21, I28)

The strong correlation between socioeconomic status (SES) and health is one of the most recognized and studied in the social sciences. Economists have pointed at differences in resources, preferences, and knowledge associated with different SES groups as possible explanations (see, e.g., Grossman 2006 for an overview). However, a causal link between any of these factors and later life health is hard to demonstrate, and the relative importance of different contributing factors is far from clear. A series of studies (e.g., Lleras-Muney 2005, Oreopoulos 2006, Clark and Royer 2013, Lager and Torssander 2012, and summaries in Mazumder 2008 and 2012), use regional differences in compulsory schooling laws or changes in national legislations on compulsory schooling as a source of exogenous variation in educational attainment in order to identify a causal effect of education on health. The results from these studies are mixed. Lleras-Muney (2005) for the United States, Oreopoulos (2006) for the United Kingdom, and van Kippersluis, O'Donnell, and van Doorslaer (2011) for the Netherlands find a strong link between attained schooling and adult health and mortality; Lager and Torssander (2012) finds some effects,

* Meghir: Department of Economics, Yale University, Box 208264, New Haven, CT 06520, and NBER, IFS, and ESRC (email: C.Meghir@yale.edu); Palme: Department of Economics, Stockholm University, SE-106 91 Stockholm, Sweden (email: Marten.Palme@ne.su.se); Simeonova: Johns Hopkins University, 100 International Drive, Baltimore, MD, and NBER (email: Emilia.Simeonova@gmail.com). We thank two anonymous referees, Douglas Almond, Anne Case, Meltem Daisal, Angus Deaton, Sergei Koulayev, Ilona Koupil, Amanda Kowalski, Ilyana Kuziemko, Per Pettersson Lidbom, Adriana Lleras-Muney, Bentley McLeod, Doug Miller, Sendhil Mullainathan, Torsten Persson, Diane Schanzenbach, and Kosali Simon, as well as participants in seminars at Tufts University, Princeton University, the University of New Hampshire, Case Western Reserve University, SOFI, CHESS, and IIES at Stockholm University, as well as at the Nordic Summer Institute in Labor Economics at the Faroe Islands and the IHEA conference in Toronto for helpful comments on earlier drafts of the paper. Financial support from the IFAU is gratefully acknowledged.

† Go to https://doi.org/10.1257/app.20150365 to visit the article page for additional materials and author disclosure statement(s) or to comment in the online discussion forum.

while Clark and Royer (2013) cannot reject the null hypothesis that extra schooling has no impact on later-life health. Analyzing outcomes of twins, Behrman et al. (2011) finds no causal impact of schooling on health in Denmark.

In this paper, we study the long-term health consequences of the introduction of a comprehensive school in Sweden, which mandated an increase in the number of years of compulsory schooling from seven or eight years (depending on municipality) to a new compulsory national level of nine years. The reform was intentionally phased in between 1949 and 1962 by being adopted early by some municipalities while others delayed its introduction. The reform had a sizeable impact on educational attainment in Sweden (Meghir and Palme 2005; Holmlund 2007; Spasojević 2010; Meghir, Palme, and Simeonova 2012). Prior work has shown that labor earnings increased later in life for those exposed to the comprehensive school, in particular for children born in homes with low educated fathers (Meghir and Palme 2005).

We use register data, including about 1.5 million individuals born between 1940 and 1957, which enables us to link assignment of type of school system to individual information from national registers on three different health outcomes. First, we study mortality using date and cause of death from the national Swedish Cause of Death Register.[1] The follow-up period stops in December 31, 2015, which means that the birth cohort born in 1940 is aged 75 when we stop observing them. Second, we look at hospitalization by cause using the Swedish in-patient register containing all hospitalization dates and International Classification of Diseases (ICD) diagnosis codes for all hospital stays in Sweden between 1987 and 2014. Finally, we use the national prescription register containing information on quantities and Anatomical Therapeutic Chemical (ATC) codes for all prescribed drugs in Sweden between 2005 and 2015.

We consider the impact of the reform on overall mortality as well as on death by cause. We first distinguish between deaths caused by circulatory diseases, shown here to be strongly associated with educational attainment, and by cancer, which is the main cause of death in the age group we study. We also consider causes of death classified by epidemiologists as "treatable" and "preventable" causes.

We use two estimation strategies. First, a difference-in-differences (DiD) approach that compares changes in mortality outcomes across cohorts in municipalities that implemented the reform compared to those that did not. Since we use 14 years of gradual implementation across the (approximately) 1,000 municipalities, we have many such comparisons, leading to very high levels of precision. The second approach, a regression discontinuity (RD), exploits the cutoff date for assigning a child to a school year. In the calendar year when the reform is implemented, the children born before January 1 are assigned to the pre-reform system, while those born after that date are assigned to the school year that first implements the reform. In all cases, the econometric approach is based on a Cox proportional hazard model for lifetime duration.

One of the advantages of this research, as compared to previous studies relying on similar educational changes to identify the education gradient in life expectancy,

[1] See Socialstyrelsen (2009).

is that the Swedish reform allows us to study two groups of people, born in the same years and active in the same labor markets, but having been educated by two different education systems at the same time. This allows us to use the econometric techniques described above rather than comparing across different birth cohorts or across groups of people brought up in different states that may differ in numerous ways. Compared to previous Swedish studies (such as Lager and Torssander 2012 and Spasojević 2010) we use both regression discontinuity, and difference-in-differences, a much larger sample, a longer follow-up period, and a larger set of outcome variables.

Our results show that, although the reform significantly elevated the educational attainment of the least skilled and increased the average years of schooling by more than a quarter of a year, it did not affect the life expectancy of those assigned to the new school system compared to the old one. Neither do we find an impact on hospitalization or drug use. This is despite a strong association between schooling and better health outcomes that we establish in our descriptive analysis.

I. The Comprehensive School Reform

A. *The Swedish School System before and after the Reform*

Prior to the implementation of the comprehensive school reform, pupils attended a common basic compulsory school (*folkskolan*) until grade six. After the sixth grade, pupils were selected to continue either for one or, in mainly urban areas, two years in the basic compulsory school, or to attend the three year junior secondary school (*realskolan*). The selection of pupils into the two different school tracks was based on their past performance, measured by grades. The pre-reform compulsory school was in most cases administered at the municipality level. The junior secondary school was a prerequisite for the subsequent upper secondary school, which was itself required for higher education.

In 1948, a parliamentary committee proposed a school reform that implemented a new nine-year compulsory comprehensive school.[2] The comprehensive school reform had three main elements:

- *An extension of the number of years of compulsory schooling to nine years in the entire country.*

- *Abolition of early selection in different schooling tracks based on academic ability.* Although pupils in the comprehensive schools were able to choose between three tracks after the sixth grade—one track including vocational training, a general track, and an academic level preparing for later upper secondary school—they were kept in common schools and classes until the ninth grade.

[2] The school reform and its development are described in Meghir and Palme (2005); Meghir, Palme, and Simeonova (2012); and Holmlund (2007). Holmlund (2007) offers detailed analysis of the implementation of the reform and shows that, conditional on municipality fixed effects, there are no significant observable predictors of the timing of the reform. For more detailed reference on the reform, see Marklund (1981).

This is likely to have resulted in changes in the peer groups of pupils going through the reformed schooling system, leading to a broader mix of students by SES and ability over a longer period of their schooling.

- *Introduction of a national curriculum*. The pre-reform compulsory schools were administered by municipalities and the pre-reform curriculum varied between municipalities. The new national curriculum equalized academic standards across Sweden. While there is no direct evidence that the quality of schooling was affected by the reform, we cannot exclude the possibility that it changed.

B. *The Phased Introduction of the Reform*

The phased introduction of the reform, with the new comprehensive nine-year compulsory school, was viewed at the time as a social experiment, albeit not randomized. It started during an assessment period between 1949 and 1962, when the new curriculum was finalized.[3] The proposed new school system, as described above, was introduced in municipalities or parts of city communities, which in 1952 numbered 1,055 (including 18 city communities).

Municipalities could elect to implement the comprehensive school starting with first or fifth grade. Once the grade of implementation was fixed, all individuals from the cohort immediately affected and all subsequent cohorts went to comprehensive school. The older cohorts continued in the pre-reform school. Although many new schools were built as a consequence of the extension of compulsory schooling and municipalities were offered subsidized loans from the government to build schools (see Marklund 1981), most of the post-reform schooling took place in the existing school buildings, and the same teachers from the pre-reform system were used in the comprehensive school.

The phased-in introduction of the reform implied that the curricula of the pre- and post-reform school systems were taught in parallel in the same schools. However, the post-reform children were always younger than the pre-reform ones and the post-reform system was never rolled back in any municipality. The pre-reform junior secondary schools that formed the academic track in the pre-reform school system were phased out as a consequence of the reform. The school buildings were in some cases used for the last three grades in the new comprehensive school, but in many cases used for the expanding upper secondary schooling.

Figure 1 shows the take-up rate of the experiment by cohort. It is evident from Figure 1 that the cohorts included in our empirical analysis, born between 1940 and 1957, cover the entire period of implementation of the comprehensive school. In 1962 it was decided that the new comprehensive school would become the standard education in Sweden. The last class that graduated from the old schooling system did so in 1970.

The selection of municipalities was not based on random assignment. However, the decision to select the implementing areas was based on an attempt to choose

[3] The official evaluation was mainly of administrative nature. Details on this evaluation are also described in Marklund (1981).

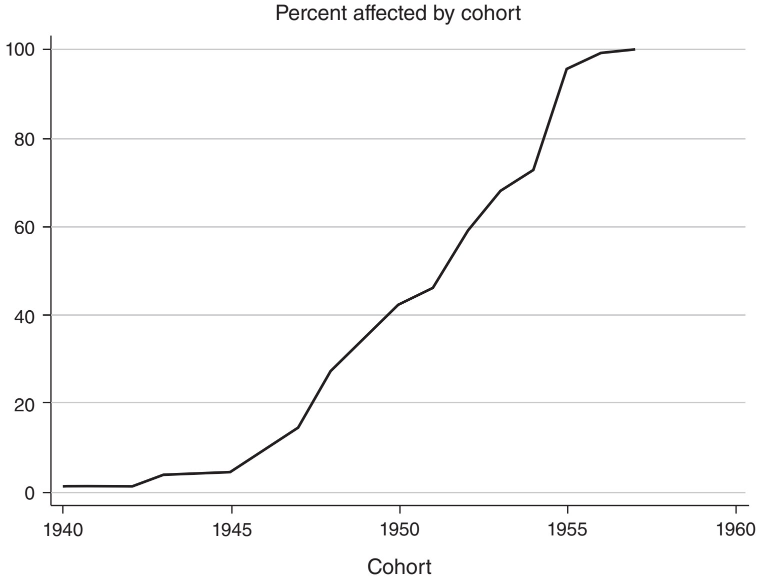

FIGURE 1. PERCENTAGE SHARE OF BIRTH COHORT ASSIGNED TO THE POST-REFORM (*comprehensive*) SCHOOL SYSTEM

locations that were representative for the entire country, both in terms of demographics as well as geographically. In the first phase of the experiment, a committee appointed by the National Board of Education chose municipalities from a pool of applicants in order to form a "representative" sample of municipalities. In later phases of the experiment, the selection process became less strict.

Meghir and Palme (2005) and Holmlund (2007) study the effect of the comprehensive school reform on educational attainments. Meghir and Palme's (2005) estimates are based on individual reform assignment recorded in school registers. For their entire sample, they find 0.252 additional years of education for males and 0.339 years for females; for low SES background persons, the estimates are 0.3 extra years for males and 0.512 for females. Holmlund (2007) reports estimates in the range 0.21–0.61 additional years of schooling for men and 0.13–0.44 for women.[4]

II. Data and the Association between Educational Attainments and Health

The original sample was obtained from Statistic Sweden's Multiple Generation Register (see Statistics Sweden 2012).[5] We include all who were born in Sweden between 1940 and 1957 and who survived until the year they turn age 16. This sample resulted in 2,184,857 observations (1,115,426 males and 1,069,431 females). We acquired data from the Population Census on church parish of birth, which was

[4] Holmlund (2007) does not have individual treatment status and imputes it from municipality of residence in 1960.

[5] All the register information described above was merged using the personal identification numbers by Statistics Sweden.

subsequently used to infer the municipality of birth and reform assignment, for 2,064,013 individuals from the original sample.

To assign date of reform assignment for each of the municipalities, i.e., the first birth cohort assigned to the reform, we use information on municipality of birth combined with an algorithm described in Holmlund (2007) and generously provided to us by Helena Holmlund.[6] The original sources for date of implementation used by Holmlund are Marklund (1981) and various reports obtained by the National Board of Education (*Skolöverstyrelsen*) and listed by Holmlund (2007). Since the reform was implemented by parts of the cities in Sweden's three largest cities—Stockholm, Gothenburg, and Malmoe—and because we have no historical records on which cohorts were first affected in each of these parts, we have excluded those born in these cities from the data. This reduced the sample to 1,562,522 observations.

Our main outcome variable is mortality, which has the strong advantage that its reporting does not depend on individual behavior (as hospitalizations might, for example). For the purpose of context, male life expectancy in 2015 in Sweden was 80.7 years and female was 84 years. As far as our sample is concerned, 25.4 percent of those not assigned to the reform were observed until death as well as 14.6 of those assigned. Of course, the latter group is on average younger. On average, individuals who went through the old schooling system have 11.4 years of schooling; those who went through the post-reform schools have 12 years. We show some basic descriptive statistics about the sample in Table 1 below.

We use two further sets of health measures as outcomes: data on number of nights in hospital care obtained from the national in-patient register and data on all prescribed drugs obtained from the national prescription register. The national in-patient register contains information on duration and ICD codes for all hospital stays in Swedish hospitals. It has national coverage since 1987, and we have data through December 31, 2014. The national prescription register includes quantities, measured in defined daily doses, and Anatomical Therapeutic Chemical (ATC) codes for all prescribed drugs in Sweden since 2005. We use data for the period until December 31, 2015.

Data on educational attainment for the father of the individual included in our sample was obtained from the 1970 census where only those aged 60 or younger were included. This restricts the sample to individuals with fathers born after 1910 when we report results by parental education (1,347,854 observations). Data on the individual's own education was obtained from the National Education Register included in the Integrated Database for Labour Market Research (LISA)(see Statistics Sweden 2011).[7] Descriptive statistics including sample size are reported in the online Appendix.

[6] The advantage of using municipality of birth as a basis of reform assignment—rather than municipality of residence, or self-reported reform status—is that it is not susceptible to parental choice of reform assignment, based on child ability. This could be achieved by moving to a municipality with a particular reform status corresponding to the year of birth of their child. There is also some evidence that parents let their children live with relatives to avoid the reform if it was implemented in their municipality of living (see Marklund 1981, or Meghir and Palme 2005, for empirical evidence).

[7] The National Education register only provides information on individual level of education. To obtain years of schooling, we use information on self-reported number of years of formal education for the relevant cohorts from the Swedish Level of Living surveys to impute average years of schooling corresponding to each level.

Table 1—Descriptive Statistics

	Non-reform sample	Reform sample
Total number of observations	990,521	572,001
Share dead	0.254	0.146
Share dead due to circulatory diseases	0.063	0.031
Share dead due to cancer	0.102	0.051
Share dead due to preventable diseases	0.021	0.009
Share dead due to treatable diseases	0.025	0.012
Average number of days in hospital care	49.4	41.5
Average number of Rx Defined Daily Doses (DDD)	9,127	6,736
Average number of years of schooling	11.4	12.0
Observations including father's education	823,947	523,907
Share of fathers with no more than compulsory schooling	0.776	0.224
Sample with father's education		
Subsample: Fathers with compulsory schooling		
Share dead	0.240	0.144
Average number of days in hospital care	48.5	41.25
Average number of Rx Defined Daily Doses (DDD)	9,246	6,920
Average number of years of schooling	10.8	11.5
Subsample: Fathers with more than compulsory schooling		
Share dead	0.195	0.116
Average number of days in hospital care	45.2	39.5
Average number of Rx Defined Daily Doses (DDD)	8,222	6,095
Average number of years of schooling	12.8	13.1

We describe the association between mortality and education using a Cox proportional hazard model (see, e.g., Cox and Oakes 1984) as well as a linear probability model (LPM). With discrete duration data, the hazard function at the heart of the Cox model is interpreted as the conditional probability of dying in the next age interval given survival to that age. This takes the form

$$I_i(r \mid educ, T) \; = \; I_0(r) \exp\{\theta \, educ_i + \gamma' T_i\}, \qquad (1)$$

where r is duration to death (age), *educ* represents years of education, and T are cohort dummies (not reported). The function $I_0(r)$ is left unrestricted. This model is convenient because it is straightforward to control for censoring (due to survival at the end of the sample period) and also naturally permits the analysis of competing risks, when considering alternative causes of death. The coefficient θ measures the change in mortality at each age associated with an extra year of education.

In column 1 of Table 2, we report $\exp(\theta)$, i.e., the ratio of the hazard for some level of education relative to the hazard with one year less of education. Thus, no impact of education corresponds to a reported coefficient of one. A number below one is equivalent to a reduction in the hazard. We also report the association between years of education and the probability of dying in our sample period, using a linear probability model and controlling for cohort effects.

In columns 2 and 3 of Table 2, we report results using independent competing risks models for the two main causes of death in the sample: circulatory diseases, cancer, and other. We repeat the exercise by reclassifying the diseases as "preventable,"

TABLE 2—MORTALITY AND EDUCATION: MALES AND FEMALES AND BY CAUSE

	All causes (1)	Circulatory diseases (2)	Cancer (3)	Preventable diseases (4)	Treatable diseases (5)
Years of schooling, Cox	0.9286	0.9044	0.9578	0.9011	0.8894
	(0.0005)	(0.0010)	(0.0008)	(0.0018)	(0.0017)
Years of schooling, LPM	−0.0125	−0.0042	−0.0029	−0.0015	−0.0019
	(0.0001)	(0.0001)	(0.0001)	(0.00003)	(0.00003)
Dead, percent	19.98	4.98	8.15	1.66	1.95
Number of deaths	364,468	90,867	148,733	30,323	35,619
Observations	1,823,901	1,823,901	1,823,901	1,823,901	1,823,901

Note: Indicator variables for gender as well as year of birth dummies included in the specification.

"treatable," and "other."[8] The preventable causes of death may reflect health behaviors and investments and the treatable ones may reflect access to health care.

The association between mortality and education is statistically very strong: the first column in Table 2 shows that an additional year of schooling is associated with a 7 percentage point reduction in the mortality rate. This translates to an increase in life expectancy associated with a year of education of 3.75 months for our observation window (16–75 years of age).[9] It also corresponds to a decline in the probability of dying of 0.0125 for each extra year of schooling within our sample as shown by the linear probability model. As a benchmark, for the cohorts under consideration, life expectancy overall has been estimated to have increased by 1.6 years and average schooling increased by 2.4 years. If the reported association reflected a causal impact, the increase in education would have accounted for 47 percent of the increase in life expectancy. The remaining columns show that the effect is largest for circulatory diseases but similar for treatable and preventable diseases.

In the online Appendix (Tables A3 and A4), we show how education is related to days of hospitalization and to the use of prescription drugs. Overall, an extra year of education is associated with fewer days in the hospital (−1.9 with standard error of 0.035). Interestingly, an extra year of schooling is associated with a 0.6 percentage point reduction in pain relief medication usage and 0.33 percentage point reduction in the use of antidepressants, an issue of relevance given the opioid epidemic in the United States.

The implication from this section is clear: there is a very strong statistical association between improved health and education, even in a high income country such as Sweden, with its strong welfare system and almost universal access to high quality health care. The key question is, of course, whether there is a causal link.

[8] Circulatory diseases are defined as ICD-10: C Chapter and cancer as ICD-10: I Chapter in the Swedish Cause of Death Registry. Online Appendix Table A1 reports how these are classified using the ICD codes available in the data.

[9] This estimate is obtained by integrating the difference between the baseline hazard and the prediction obtained by calculating $S(t|x_k) = S_0^{\exp(\beta x_k)}$, where $\exp(\beta x_k)$ is the hazard ratio estimate for years of schooling, i.e., 0.9286 (see Cleves, Gould, and Gutierrez 2004).

III. Empirical Strategy for Estimating the Effect of the Educational Reform

To estimate the impact of the reform on our various outcomes, we use two approaches. The first is based on a difference-in-differences (DiD) design and the second is based on regression discontinuity (RD).

The DiD design exploits the fact that individuals in the same birth cohort were either exposed to the educational reform or not, depending on which municipality they lived in. For some of the outcomes, namely years of education, days of hospitalization, indicators for hospitalization for various diseases and for prescription drugs, we use the standard linear difference-in-differences model. In our DiD specifications, we include a dummy variable for gender, a full set of dummies for municipality of birth, a full set of cohort dummies, separate linear trends for each municipality of birth and, finally, an indicator of whether the individual was assigned to the reform.

To examine the effects of mortality, we use both a linear probability model (as is standard in DiD specifications), where the outcome is death in any year the person is observed in the sample. We also use a Cox proportional hazard model for the duration of life again based on the DiD assumption to identify the effect. The index function is the same in both specifications. However, in the Cox proportional hazard model, we stratify on municipality of birth. This means that we allow for different baseline hazard in each municipality of birth, which is more flexible than simply including separate dummy variables for each municipality. Also, rather than including separate linear trends for each of the about 1,000 different municipalities of birth, we estimate separate linear trends for each group of municipalities, grouped by the year they implemented the reform.[10]

As Altonji and Blank (1999) and Athey and Imbens (2006) point out, the DiD approach does not require linearity. The assumptions required are that the outcome variable in the *untreated* state is related to unobservable heterogeneity in a strictly monotonic fashion; the distribution of this unobservable must be time invariant, but may depend on the group to which the individual belongs (here the municipality); and conditional on a value for this unobservable, growth of the outcome in the absence of treatment is assumed independent of group, which is the usual common growth assumption.[11] The linear model is just a special case of this.

Based on these ideas, and while a nonparametric analysis is in principle possible (as shown in Athey and Imbens 2006), we take the simpler route of using the Cox proportional hazard model.[12] This allows us to deal with censoring and with competing risks when we look at death by cause. We also show results using the standard linear probability model where the outcome is mortality over the sample period, as in the descriptive analysis. This is a standard linear DiD regression.

[10] Given the nonlinear nature of this approach, adopting 1,000 separate trends would be computationally infeasible.

[11] If the outcome variable is discrete, then point estimates require a functional form assumption—most people use the linear probability model when they have a binary outcome—however, this is just one possible *identifying* assumption.

[12] The nonparametric approach is particularly complicated by the large number of treatment and comparison groups and the numerous periods.

Similarly to the descriptive analysis, for the Cox model the hazard function at duration of life r takes the form

$$(2) \qquad I_{1,i,m,t}(r|X) = I_{0,m}(r)\exp\{\beta R_{i,m,t} + \gamma_0 F_i + \gamma_1' T_i + \gamma_2' G_i \times C_i\},$$

where i, m, and t are sub-indices for individual, municipality, and birth cohort, respectively; the function $I_{0,m}(r)$ varies freely by municipality and age; R indicates whether the individual was assigned to the reform or not based on municipality of birth and cohort; T is a vector of dummy variables for cohort of birth; F is a dummy variable for female; G is a vector of dummy variables indicating the first cohort to implement the post-reform school system in the individual's municipality of birth, and C is the individual's year of birth. Thus, the $G_i \times C_i$ term allows for differential trends by municipality groups classified by the year in which they implemented the reform. The 1,000 or so municipality fixed effects are absorbed by the baseline hazard, which is different for each municipality and is not specified parametrically.[13] The coefficient β measures the effect of the reform on mortality. By using these various approaches, we hope to improve confidence in the results.

When we consider death by different causes, we use the independent competing risks models.[14] The hazard function for each cause of death takes the same form as above.

This approach identifies the impact by comparing growth in the outcome variable across municipalities. An alternative approach is to identify the effect of the reform within municipalities based on a regression discontinuity design where we use the threshold date that determines in which year the child will start attending school—January 1 in Sweden. Anyone born on or after that date in the calendar year of the reform implementation is assigned to the reform. Before that date, they are assigned to the previous school year and, as a result, to the old schooling system. Ideally we would use a very narrow window around the discontinuity, comparing outcomes of people born just before the cutoff date and those born just after; however, this would lead to too small a sample. Instead of restricting the bandwidth, we use polynomials in the month-distance from the discontinuity (one before and one after) combined with dummy variables to control for month of birth effects. The assumption is that these polynomials control for any outcome-relevant differences for people born just before or after the break point. In addition, the discontinuity is "fuzzy" in the sense that some people may relocate their children to a different entry cohort than the one they are strictly assigned to. In an attempt to further alleviate the potential effects of selective cohort placement, we assign individuals to the reform based on their municipality of birth and their date of birth, akin to an intention-to-treat design.

The specification we use for the hazard in this case is

$$(3) \qquad I_{1,i,m,t}(r|X) = I_0(r)\exp(\beta_1 R_{i,m,t} + \gamma_1 W_i + \gamma_2 W_i^2 + \gamma_3 W_i R_{i,m,t} + \gamma_4 W_i^2 R_{i,m,t} + \delta T_i + \theta Z_i),$$

[13] This model satisfies the assumptions stated by Athey and Imbens (2006) for nonlinear DiD models. A similar parametric approach in the context of nonlinear DiD models was followed by Blundell et al. (2004).

[14] See Honoré and Lleras-Muney (2006) among others, on the identifiability of a competing risks model with dependent risks.

where W is a variable measuring the time in months to January 1, which is the cutoff date for determining the school-year for the ith individual;[15] T_i is a full set of dummy variable for cohort of birth; Z_i includes a dummy variable for gender as well as a full set of dummy variables for month of birth to control for seasonal effects in time of birth (see Dobkin and Ferreira 2010). In the estimation we included successively higher order polynomials until the additional terms became insignificant. In all cases, a second order polynomial turned out to be sufficient.[16] We can interpret the coefficient β_1 as the impact of the reform averaged across discontinuities. In the online Appendix, we also present results based on the linear probability model using this discontinuity design. The conclusions do not change.

Both empirical approaches (DiD and RD) control for time-invariant differences between the treated and the comparison groups. Both have a causal interpretation under our assumptions, but they may relate to different subpopulations, and if the effects are heterogeneous, the results may differ.

Throughout, we present results for males and females separately because they are expected to follow different underlying health processes. Since the reform had a stronger effect for those with low educated fathers (see Meghir and Palme 2005 and below), we also break down the results by father's education. We refer to those whose father had just compulsory education as low socioeconomic status (SES) and the rest as high SES.

IV. Results

A. *Effects of the Reform on Educational Attainment*

In Table 3, we start by presenting the impact of the reform on years of education.[17] The results are broken down by father's education and shown for males and females combined as well as separately. The standard errors for all results are clustered at the municipality level.

The sets of estimates from the two approaches are in general very similar. The reform led to an increase of about 0.25 of a year in education. The effect is substantially larger for the low SES group and higher for males than for females. More detailed results including impacts by level of education and the sample sizes for all comparisons are presented in online Appendix Table A2.

Finally, Figure 2 illustrates the effect graphically. Panels A and B illustrate the RD models. Panel A refers to the proportion attending the pre-reform compulsory level of education, while the panel B shows the years of education. Each dot in the figures represents the average outcome by month of birth on the basis of distance from the first month-of-birth cohort assigned to the reform in each municipality.[18]

[15] W is zero at the cutoff, negative before, and positive after.

[16] In addition, Gelman and Imbens (2014) recommend to not use higher order polynomials in order to avoid over-fitting.

[17] This has been documented elsewhere for different samples (Meghir and Palme 2005; Meghir, Palme, and Simeonova 2012), but not on this sample.

[18] That is, if the first cohort in a municipality to be assigned to the reform was those born in January 1948, those who were born in, say, April 1949, are born 15 months after the first month of birth cohort in that municipality.

Table 3—DiD and RD Estimates of the Impact of the Comprehensive School Reform on Years of Schooling

	All (1)	Low SES (2)	High SES (3)
Males and females (difference-in-differences)	0.255	0.304	0.086
	(0.015)	(0.017)	(0.024)
Males (difference-in-differences)	0.301	0.363	0.086
	(0.018)	(0.021)	(0.035)
Females (difference-in-differences)	0.205	0.238	0.086
	(0.021)	(0.024)	(0.035)
Males and females (regression discontinuity)	0.241	0.300	0.068
	(0.029)	(0.028)	(0.037)
Males (regression discontinuity)	0.313	0.375	0.081
	(0.023)	(0.033)	(0.050)
Females (regression discontinuity)	0.177	0.217	0.055
	(0.031)	(0.032)	(0.047)

Notes: Each number represents an impact from a separate regression by method and demographic group. DiD specification includes a full set of dummy variables for year of birth and municipality of birth, as well as separate linear trends for municipalities of birth. RD specification includes separate quadratic polynomials in the running variable before and after the break point, a dummy variable for gender, as well as a full set of dummy variables for month of birth. The samples of low and high SES background men and women do not add up to the aggregate sample size because of missing information on father's education in the registry data. Standard errors are in parentheses and are clustered by municipality of birth.

The regressions discontinuity estimates shown in Table 3 can be obtained using weighted least squares on the collapsed data shown in panels A and B of Figure 2.

For each outcome there is a marked jump at the first cohort assigned to the reform. However, the figures also show apparent trends pre-reform toward higher educational attainment. The main reason for this overall trend in the graph is a composition effect: groups of observations (dots) that are distant from the discontinuity and to the left will represent more municipalities that delay the reform, which is correlated with lower schooling. This feature is controlled for in the regression analysis and does not obscure the fact that there is a clear break at the time of the reform in the municipality.

Panels C and D of Figure 2 show the average residuals from a DiD model where we have excluded the reform indicator. Panel C relates to the share attending the pre-reform compulsory school, while panel D relates to years of education. The jump in schooling associated with the reform is evident here as well.

Online Appendix Figure A1 displays the same graphs as the top panels A and B of Figure 2 by father's education.[19] It is apparent from these figures that the effect of the reform is much stronger for all outcomes in the low SES group.

Correspondingly, those born in January 1947 are born 12 months before the first ones assigned to the reform. To the right of zero on the horizontal axis we have the treated group and to the left the comparison group.

[19] Individuals whose father had statutory schooling or less are labeled low SES. The rest are high SES.

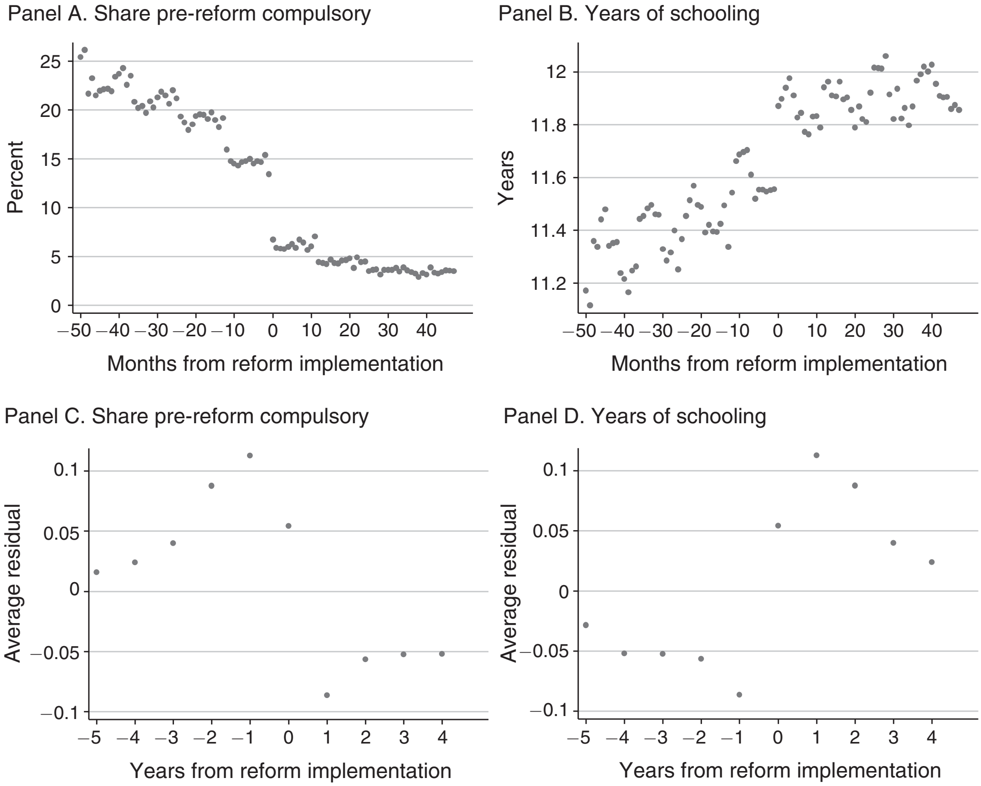

Figure 2. Effect of the Reform on Final Educational Attainments

Notes: Panel A: share with years of final education less than or equal to the pre-reform compulsory level; panel B: years of schooling; panel C: residuals from a linear probability DiD model excluding the reform indicator using an indicator for educational attainments more than the pre-reform compulsory level as a dependent variable; panel D: residuals from an OLS DiD model excluding the reform indicator using years of schooling as a dependent variable. The horizontal axis for the top (bottom) two panels is the number of birth months (years) from the first cohort for which the reform was implemented (the zero point). Negative numbers represent pre-implementation cohorts and positive ones post-implementation.

Diagnostics.—Before proceeding, we show some validation tests for the regression discontinuity. The results from these tests are shown in Figure 3. Panel A of Figure 3 shows the result from a density test suggested by McCrary (2008). The density plot shows no indication of a sudden change in the density of observations for date of birth just before or just after the discontinuity point; this illustrates that there is no significant manipulation of the date of birth—the running variable on which our classification relies.

Panels B and C show that there is no "impact" on variables that should not be affected by the reform, namely the education of the father and the municipality tax rate.[20] Similar placebo tests with other observables yield the same result (see online

[20] These were obtained from the multigenerational register and from the Yearbook of Swedish Municipalities (1960) respectively. They were linked to our data through the municipality identification number.

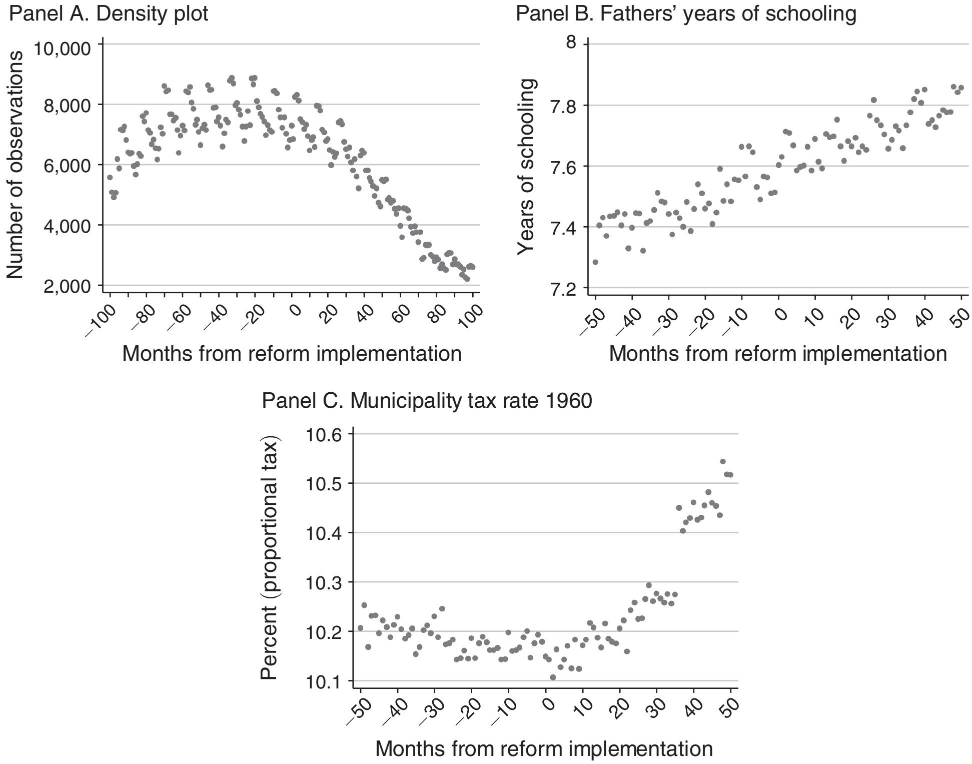

Figure 3. Diagnostic Tests

Notes: Panel A: density of observations by months before the reform. Panel B: placebo test 1—the discontinuity and father's education. Panel C: placebo test 2—the discontinuity and municipality tax rate. The horizontal axis as in Figure 2.

Appendix Figure A2). These tests illustrate the robustness of our approach. We now proceed with the central results of interest.

B. *Effects on Mortality*

Consider first the same type of graphs we just showed for education, but for mortality. Panel A of Figure 4 shows raw mortality rates by distance in months from the cutoff date of reform implementation. The two top panels A and B show the results for the entire sample and the lower panel the corresponding ones for low and high SES individuals, respectively. There is a negative slope in all graphs reflecting the fact that individuals are getting younger along the horizontal axis. However, there is no large or significant break in this trend at the cutoff date.

Panels B, D, and F of Figure 4 show the residuals from the linear probability DiD models with an indicator for having died before the end of the follow-up period as dependent variable and excluding the reform indicator from the specification. These results provide further visual evidence that the reform had no effect on mortality. We then confirm these with the regression results, using both DiD and RD approaches.

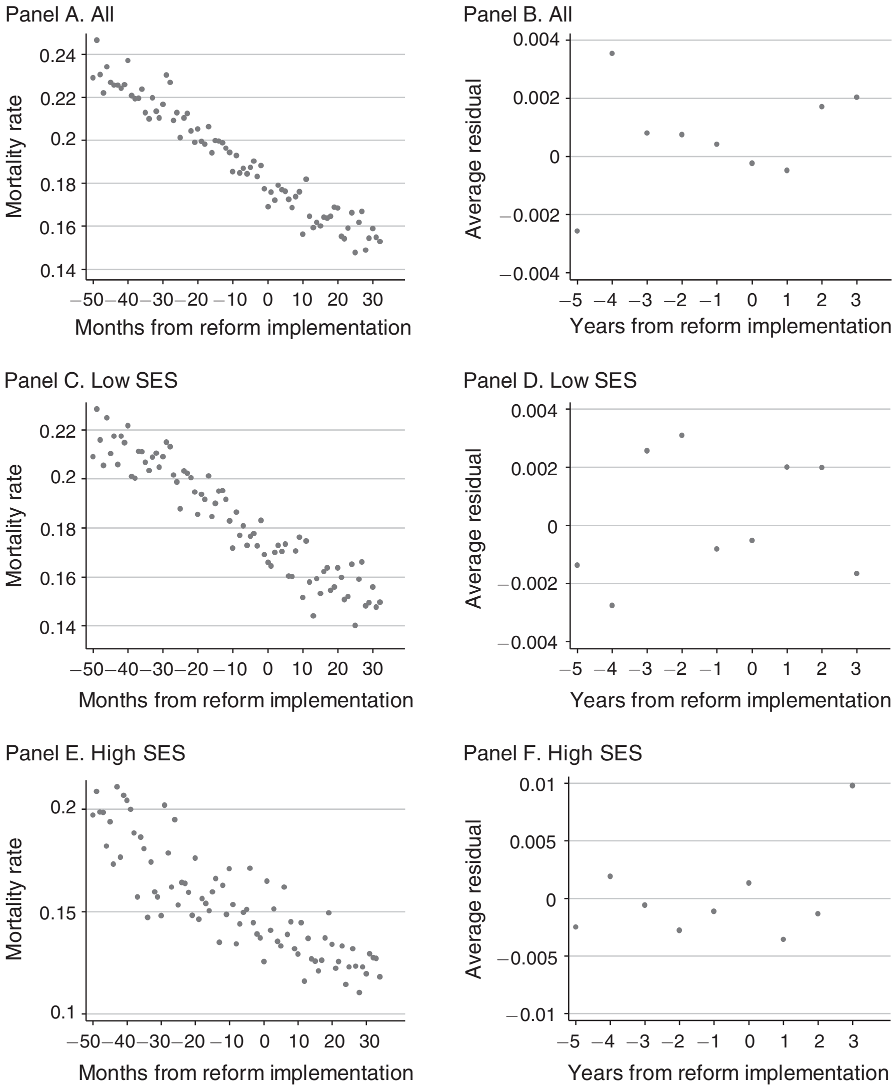

Figure 4. Reform Effect on Mortality

Notes: Left panels: average mortality rate by distance in months from reform implementation. Right panels: residuals from a linear probability DiD model excluding the reform indicator. Upper panels: population. Middle panels: low family SES. Lower panels: high family SES. y-axis measures the within-cell mortality rate (not age-adjusted) over the observed period. The horizontal axis measures the number of months between the month of birth for the individuals included in the cell and the month of birth of the first cohort of individuals affected by the reform.

Table 4 shows the results from four different specifications estimating the effect of the schooling reform on mortality. Two are based on the DiD framework and the specification shown in equation (1) and two on the RD specification in equation (2). Each of these models is estimated using both Cox regressions and the linear probability models (LPM). Panel A combines males and females and the two lower panels show results by gender.

Table 4—The Effects of Education Reform on Mortality. Cox Proportional Hazard Regressions and Regression Discontinuity Estimation Results

	All (1)	Low SES (2)	High SES (3)
Panel A. Males and females			
DiD, LPM	0.0006	0.0019	−0.0012
	(0.0017)	(0.0019)	(0.0036)
DiD, Cox	1.0005	1.0058	1.0071
	(0.0105)	(0.0128)	(0.0281)
RD, LPM	−0.00052	0.00091	0.00102
	(0.00223)	(0.00249)	(0.00456)
RD, Cox regression	0.9990	1.0088	1.0057
	(0.0135)	(0.0160)	(0.0337)
Observations	1,562,493	1,051,462	296,392
Deaths	335,085	216,861	45,406
Panel B. Males			
DiD, LPM	−0.0003	−0.0001	0.0014
	(0.0024)	(0.0028)	(0.0054)
DiD, stratified Cox	0.9841	0.9933	1.0189
	(0.0144)	(0.0161)	(0.0371)
RD, LPM	−0.0017	−0.0008	0.0040
	(0.0034)	(0.0036)	(0.0070)
RD, Cox regression	0.9937	1.0001	1.0214
	(0.0176)	(0.0201)	(0.0465)
Observations	812,719	545,523	152,782
Deaths	203,906	131,318	27,400
Panel C. Females			
DiD, LPM	0.0015	0.0040	−0.0034
	(0.0021)	(0.0024)	(0.0044)
DiD, stratified Cox	1.0125	1.0295	0.9926
	(0.0158)	(0.0203)	(0.0390)
RD, LPM	0.0007	0.0025	−0.0020
	(0.0026)	(0.0032)	(0.0053)
RD, Cox regression	1.0072	1.0224	0.9841
	(0.0184)	(0.0243)	(0.0450)
Observations	749,702	505,939	143,610
Deaths	131,179	85,543	18,006

Notes: Each number represents an impact from a separate regression by method and demographic group. Standard errors are in parentheses and are clustered by municipality of birth. DiD LPM specification includes a full set of dummy variables for year of birth and municipality of birth, as well as separate linear trends for municipalities of birth. Stratified Cox regressions include year of first implementation specific linear trends. RD specification includes separate quadratic polynomials in the running variable before and after the break point, a dummy variable for gender, as well as a full set of dummy variables for month of birth. The samples of low and high SES background men and women do not add up to the aggregate sample size because of missing information on father's education in the registry data. Standard errors are in parentheses and are clustered by municipality of birth.

The estimates shown in Table 4 imply no effect of the reform on mortality: the hazard ratio is not significantly different from one either for the DiD estimates or for the RD ones and the LPM estimates are not significantly different from zero. The general result is supported by the fact that it is robust to the choice of econometric model and that it holds within each subgroup under study—even within the group of men from low SES families, where we estimated the largest effect of the reform on educational attainment. In Section VI, we discuss the magnitude of these effects and how large they could be potentially, if one takes into account the width of the confidence interval. In online Appendix Table A5, we also show that the impact of the reform is zero at all ages in our observation window (44–70).

C. *Effects by Cause of Death*

We now turn to two alternative classifications of diseases to see whether mortality declined from causes that may be more explicitly related to behavior.[21] We first consider mortality from circulatory diseases (strongly correlated with education) and cancer (the single most important cause of death in the age group we study), with other causes of death acting as censoring.

The results are shown in Table 5, and as is evident from the first column, the effects are not significant at the 5 percent level for either circulatory diseases or cancer.[22] Some marginally significant effects for the high SES group are easily discounted, once we take into account that the *p*-values need to be adjusted for testing multiple hypotheses. These results can be confirmed visually in the various plots presented in online Appendix Figure A3.

In online Appendix Table A6, we also demonstrate that the reform did not result in significant reductions in mortality from preventable or treatable diseases. Online Appendix Figure A3 shows the corresponding plots by causes of death.

D. *Hospitalization*

As we discussed in Section III, a possible limitation of mortality as a measure of adult health is that we are not able to observe entire life histories, and effects of education on health could potentially show up later in life. Moreover, the reform could have improved health in ways that affects the quality of life but not necessarily its length. Therefore, in addition to mortality, we also use hospitalization and consumption of prescribed drugs as health outcomes. For hospitalization, we use three different measures: total number of days in hospital care, as well as binary indicators for having ever been hospitalized for cancer, circulatory, and respiratory diseases. The regressions used here are linear DiD or linear RD regressions with a quadratic polynomial in the time from the reform. The coefficient is thus interpreted as the effects of the reform on days of hospitalization.

The results are shown graphically in Figure 5. There are no apparent effects of the reform for any of the four outcomes. This assessment is further confirmed in Table 6, which shows the regression results from both DiD and RD specifications.

[21] Online Appendix Table A1 reports how these are classified using the ICD codes available in the data.

[22] We only present results for males and females combined, and we concentrate on the sample that excludes the three main cities. Results for the two gender groups separately are shown in Table A4 in the online Appendix.

Table 5—The Effects of Education Reform on Mortality by Cause of Death: Circulatory Diseases and Cancer Causes of Death Only

	All (1)	Low SES (2)	High SES (3)
Panel A. Circulatory diseases			
Reform, stratified Cox	1.0122	1.0098	0.9382
	(0.0232)	(0.028)	(0.0463)
Reform, RD	0.9992	0.9941	0.9671
	(0.0319)	(0.0356)	(0.0664)
Deaths	80,616	52,950	9,881
Panel B. Cancer			
Reform, stratified Cox	0.9882	1.0117	0.9756
	(0.0173)	(0.0209)	(0.0337)
Reform, RD	0.9744	1.0065	0.9006
	(0.0218)	(0.0290)	(0.0286)
Deaths	129,577	87,329	18,103
Observations	1,562,493	1,051,462	296,392

Notes: The table shows Cox proportional hazard competing risk estimates and RD model estimates, along with the population of Swedes born 1940–1957. Each cell presents the estimate from a separate regression by method and demographic group. Standard errors are in parentheses and are clustered by municipality of birth. DiD stratified Cox regressions include year of first implementation-specific linear trends. RD specification includes separate quadratic polynomials in the running variable before and after the break point, a dummy variable for gender, as well as a full set of dummy variables for month of birth. The samples of low and high SES background men and women do not add up to the aggregate sample size because of missing information on father's education in the registry data.

None of the 24 estimates—by model, outcome, or group—are statistically different from zero.

Finally, in online Appendix Table A7 and Figure A4, we show that the reform did not affect the use of prescription drugs either. Among the many results we show, there are two which are significant at the 5 percent level: the use of psycholeptics increases overall and the use of antidepressants among high SES individuals declines. However, one must note that we are testing many hypotheses and any adjustment for multiple hypotheses testing would imply p-values higher than 5 percent. So these results need to be discounted.

V. Discussion

What is the effect of the compulsory schooling reform on mortality? The point estimate implies a very small effect on mortality as we saw earlier. To translate the mortality estimates to implied effects on life expectancy (within the age support of the sample), we first compute the survival functions, based on the estimated hazard rates. Life expectancy is then given by the area under these functions. We see from Table 7 that the point estimates for the reform caused a change in life expectancy of between −0.026 to 0.052 months. This is consistent with the effects on

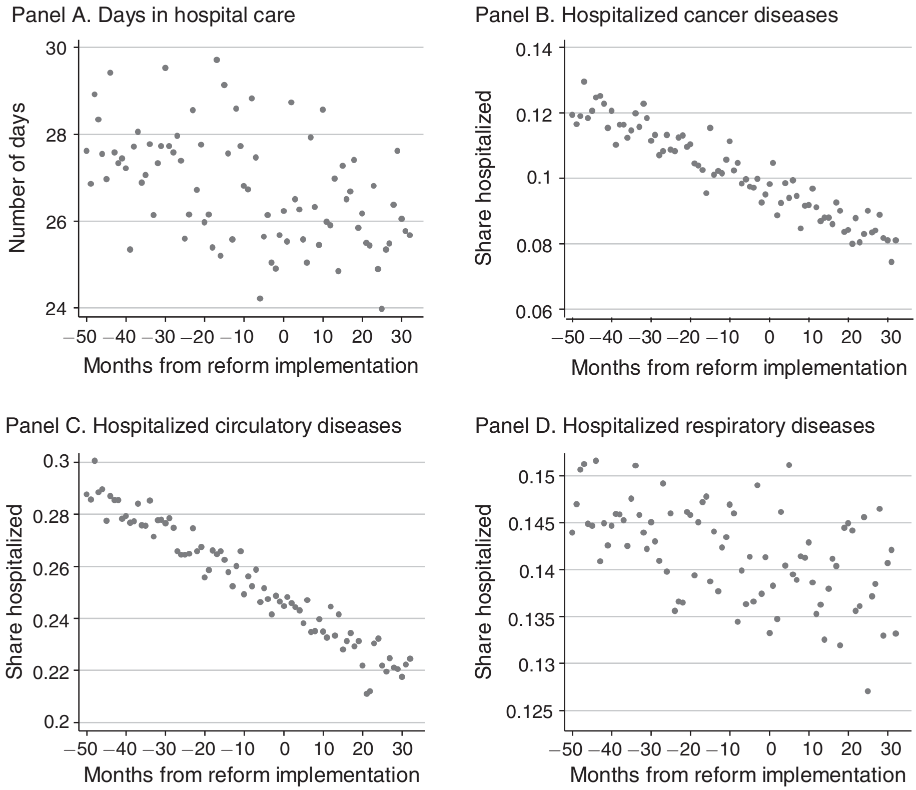

Figure 5. Reform Effect on Hospitalization: Total Number of Days in Hospital Care and Indicators for Hospital Stay by Main Diagnosis

Notes: The horizontal axis measures the number of months between the month of birth for the individuals included in the cell and the month of birth of the first cohort of individuals affected by the reform. *y*-axis measures the within-cell share (not age-adjusted) of the population hospitalized for at least one night between 1987 and 2014.

hospitalization, repeated here for completeness, which indicate a very small increase. Of course, there is a standard error around these estimates and the extreme of the confidence interval allows for a reduction of mortality as a result of the reform of 1.4 months; similarly, the edge of the confidence interval for hospitalization allows for a reduction of 0.7 days.

One way to compare our estimates to the overall associations between years of schooling and mortality would be to derive the implied effect of a year of schooling by using instrumental variables. However, a priori this is not supported by the nature of the reform that may have changed not only the quantity of schooling but also its quality. Lower socioeconomic status individuals (who are the main target) would now attend comprehensive schools, associate with peers from higher socioeconomic groups, and some would also obtain monetary support for attending extended schooling. One can argue that many of these mechanisms are beneficial to outcomes, although this may not have been so for higher SES students since their peer group may have been diluted. In any case, it is not valid to use the reform as an

TABLE 6—THE EFFECTS OF EDUCATION REFORM ON HOSPITALIZATION

	All (1)	Low SES (2)	High SES (3)
Panel A. Number of days			
Reform, DiD	0.2093	0.2618	0.6406
	(0.3117)	(0.33913)	(0.5867)
Reform, RD	0.0110	0.0579	0.2812
	(0.3726)	(0.4549)	(0.7631)
Mean dep. var.	27.10	27.34	26.00
Panel B. Cancer			
Reform, DiD	0.0008	−0.0006	0.0052
	(0.0011)	(0.0013)	(0.0023)
Reform, RD	0.0006	−0.0009	0.0056
	(0.0016)	(0.0018)	(0.0028)
Mean dep. var.	0.1104	0.1122	0.1020
Panel C. Circulatory diseases			
Reform, DiD	0.0022	−0.0004	0.0045
	(0.0016)	(0.0019)	(0.0036)
Reform, RD	0.0037	0.0008	0.0043
	(0.0022)	(0.0025)	(0.0043)
Mean dep. var.	0.2653	0.2721	0.2301
Panel D. Respiratory diseases			
Reform, DiD	−0.0013	0.0004	−0.0021
	(0.0012)	(0.0015)	(0.0029)
Reform, RD	−0.0003	0.0019	−0.0023
	(0.0017)	(0.0020)	(0.0035)
Mean dep. var.	0.1425	0.1446	0.1388

Notes: The table shows total number of days in hospital care and indicators for hospital stay by main diagnosis, along with OLS and LPM regressions for men and women. Each number represents an impact from a separate regression by method and demographic group. DiD specification includes a full set of dummy variables for year of birth and municipality of birth, as well as separate linear trends for municipalities of birth. RD specification includes separate quadratic polynomials in the running variable before and after the break point, a dummy variable for gender, as well as a full set of dummy variables for month of birth. The samples of low and high SES background men and women do not add up to the aggregate sample size because of missing information on father's education in the registry data. Standard errors are in parentheses and are clustered by municipality of birth.

TABLE 7—IMPACTS ON LIFE EXPECTANCY AND HOSPITALIZATION

	Life expectancy (months)		Hospitalization (days)	
Impact of the reform	Point estimate	Upper end of 95 percent CI	Point estimate	Lower end of 95 percent CI
Difference-in-differences	−0.026	1.054	0.209	−0.401
Regression discontinuity	0.052	1.447	0.011	−0.719

Note: Estimates obtained based on results shown in Tables 4 and 6, respectively.

excluded IV. If we do, we reject OLS strongly.[23] The p-values of this test are 0.036 and 0.006 based on DiD and RD, respectively. For hospitalization, the p-values are 0.001 and 0.029, respectively.[24]

VI. Conclusions

In this paper, we study the relation between education and health using rich administrative data. We use the introduction of Sweden's comprehensive school, which increased the amount of compulsory schooling, as a source of exogenous variation in educational attainment. We look at overall mortality and mortality by cause of death as outcome measures and consider the population of all Swedes born between 1940 and 1957 who survived until age 16. The follow-up period stops in December 2015, allowing us to observe the oldest individuals until age 75 and the youngest until age 58.

We find no significant effects of the reform on overall mortality, regardless of whether we use difference-in-differences models or a regression discontinuity approach. Indeed, our results indicate that the effect of the reform on mortality is 0 for the age window we consider with an upper limit of the 95 percent CI, suggesting an increase of life expectancy of at most 1.4 months.

The significance of the findings of no effect of the reform on health lies in the fact that we look at a very large set of outcomes, that the sample is very large, that the effect of the reform on extra schooling is substantial, and that we are able to use two alternative quasi-experimental evaluation methods that yield similar results (DiD and RD). Furthermore, there is established evidence in the literature that the reform significantly increased earnings for the low SES children (Meghir and Palme 2005). Overall, the results presented here echo others and particularly those of Clarke and Royer (2013) for the United Kingdom and Behrman et al. (2011) for Denmark, but contrast with those of Lleras-Muney (2005) for the United States and van Kippersluis, O'Donnell, and van Doorslaer (2011) for the Netherlands, who do find relatively large impacts of education on mortality.

The education reform under study in this paper did not only affect quantity of schooling. The centralized and more academic curriculum could potentially have had an effect on ability to critically process information on health-related behavior and risks (see, e.g., Cutler and Lleras-Muney 2008 for a discussion). Moreover, the abolition of tracking after sixth grade could have affected health-related behaviors through peer-group influences. To distinguish these effects from those of the quantity of schooling is not possible given our data and is our main motivation for not using reform assignment as an instrumental variable for years of schooling.

For the cohorts we are considering in this study, Sweden had an advanced public health care system providing services independently of individual income. This may limit the role of one channel through which improved education may affect health, namely that of financial resources. However, the role of education in allowing better

[23] We use the linear probability model specification where we use reform assignment as instrumental variable for years of schooling. F-statistics in first stage: 158.5 (DiD) and 157.5 (RD).

[24] Point estimates from the IV models are reported in Table A8 in the online Appendix.

access to and understanding of information and in changing one's behavior vis-à-vis investments in one's health is still potentially present. And even in the context of a public health care system, individual resources may be helpful in improving outcomes. Thus, it is important that our study does not identify an effect of improved education at the lower end of the education distribution on mortality, a result that is very robust. Ultimately, it is important to understand the roles of various channels in improving health, such as resources, access to free health care, information, and investments in health.

REFERENCES

Altonji, Joseph G., and Rebecca M. Blank. 1999. "Race and gender in the labor market." In *Handbook of Labor Economics*, Vol. 3C, edited by Orley C. Ashenfelter and David Card, 3143–3259. Amsterdam: North-Holland.

Athey, Susan, and Guido W. Imbens. 2006. "Identification and Inference in Nonlinear Difference-in-Differences Models." *Econometrica* 74 (2): 431–97.

Behrman, Jere R., Hans-Peter Kohler, Vibeke Myrup Jensen, Dorthe Pedersen, Inge Petersen, Paul Bingley, and Kaare Christensen. 2011. "Does More Schooling Reduce Hospitalization and Delay Mortality? New Evidence Based on Danish Twins." *Demography* 48 (4): 1347–75.

Blundell, Richard, Monica Costa-Dias, Costas Meghir, and John van Reenen. 2004. "Evaluating the Employment Impact of a Mandatory Job Search Program." *Journal of the European Economic Association* 2 (4): 569–606.

Clark, Damon, and Heather Royer. 2013. "The Effect of Education on Adult Mortality and Health: Evidence from Britain." *American Economic Review* 103 (6): 2087–2120.

Cleves, Mario A., William W. Gould, and G. Gutierrez. 2004. *An Introduction to Survival Analysis Using STATA*. College Station, TX: STATA Press.

Cox, D. R., and D. Oakes. 1984. *Analysis of Survival Data*. London: Chapman and Hall.

Cutler, David M., and Adriana Lleras-Muney. 2008. "Education and Health: Evaluating Theories and Evidence." In *Making Americans Healthier: Social and Economic Policy as Health Policy*, edited by Robert F. Schoeni, James S. House, George A. Kaplan, and Harold Pollack, 29–60. New York: Russell Sage Foundation.

Dobkin, Carlos, and Fernando Ferreira. 2010. "Do school entry laws affect educational attainment and labor market outcomes?" *Economics of Education Review* 29 (1): 40–54.

Gelman, Andrew, and Guido Imbens. 2014. "Why High-Order Polynomials Should Not Be Used in Regression Discontinuity Designs." National Bureau of Economic Research (NBER) Working Paper 20405.

Grossman, Michel. 2006. "Education and Nonmarket Outcomes." In *Handbook of the Economics of Education*, Vol. 1, edited by Eric Hanushek and Finis Welch, 577–633. Amsterdam: North-Holland.

Holmlund, Helena. 2007. "A Researcher's Guide to the Swedish Compulsory School Reform." Stockholm University Swedish Institute for Social Research Working Paper 9.

Honoré, Bo E., and Adriana Lleras-Muney. 2006. "Bounds in Competing Risks Models and the War on Cancer." *Econometrica* 74 (6): 1675–98.

Integrated Database for Labour Market Research. 2011. Statistics Sweden. http://www.jpi-dataproject.eu/Home/Database/162?topicId=3 (accessed January 10, 2018).

Lager, Anton Carl Jonas, and Jenny Torssander. 2012. "Causal effect of education on mortality in a quasi-experiment on 1.2 million Swedes." *Proceedings of the National Academy of Sciences* 109 (22): 8461–66.

Lleras-Muney, Adriana. 2005. "The Relationship Between Education and Adult Mortality in the United States." *Review of Economic Studies* 72 (1): 189–221.

Marklund, Sixten. 1981. *Skolsverige 1950–1975: Försöksverksamheten*. Stockholm: Liber Utbildningsförlaget.

Mazumder, Bhashkar. 2008. "Does Education Improve Health?: A Reexamination of the Evidence from Compulsory Schooling Laws." *Economic Perspectives* 33 (2): 2–16.

Mazumder, Bhashkar. 2012. "The effects of education on health and mortality." *Nordic Economic Policy Review* 1: 261–301.

McCrary, Justin 2008. "Manipulation of the running variable in the regression discontinuity design: A density test." *Journal of Econometrics* 142 (2): 698–714.

Meghir, Costas, and Mårten Palme. 2005. "Educational Reform, Ability, and Family Background." *American Economic Review* 95 (1): 414–24.

Meghir, Costas, Mårten Palme, and Emilia Simeonova. 2012. "Education, Health and Mortality: Evidence from a Social Experiment." National Bureau of Economic Research Working Paper 17932.

Meghir, Costas, Mårten Palme, and Emilia Simeonova. 2018. "Education and Mortality: Evidence from a Social Experiment: Dataset." *American Economic Journal: Applied Economics*. https://doi.org/10.1257/app.20150365.

Oreopoulos, Philip. 2006. "Estimating Average and Local Average Treatment Effects of Education when Compulsory School Laws Really Matter." *American Economic Review* 96 (1): 152–75.

Socialstyrelsen. 2009. "The Swedish Cause of Death Registry." http://www.socialstyrelsen.se/statistics/statisticaldatabase/help/causeofdeath (accessed January 10, 2018).

Spasojević, Jasmina. 2010. "Effects of Education on Adult Health in Sweden: Results from a Natural Experiment." In *Current Issues in Health Economics* (*Contributions to Economic Analysis*), Vol. 290, edited by Daniel Slottje and Rusty Tchernis, 179–99. West Yorkshire, UK: Emerald Group Publishing.

Statistics Sweden. 2012. *Flergenerationsregistret*. Örebro: Statistics Sweden.

van Kippersluis, Hans, Owen O'Donnell, and Eddy van Doorslaer. 2011. "Long-Run Returns to Education: Does Schooling Lead to an Extended Old Age?" *Journal of Human Resources* 46 (4): 695–721.

This article has been cited by:

1. Jostein Grytten, Irene Skau, Rune Sørensen. 2020. Who dies early? Education, mortality and causes of death in Norway. *Social Science & Medicine* **245**, 112601. [Crossref]
2. Govert E. Bijwaard, Per Tynelius, Mikko Myrskylä. 2019. Education, cognitive ability, and cause-specific mortality: A structural approach. *Population Studies* **73**:2, 217-232. [Crossref]
3. José M. Alonso, Rhys Andrews, Vanesa Jorda. 2019. Do neighbourhood renewal programs reduce crime rates? Evidence from England. *Journal of Urban Economics* **110**, 51-69. [Crossref]
4. Atticus Bolyard, Peter A. Savelyev. 2019. Understanding the Education Polygenic Score and Its Interactions with SES in Determining Health in Young Adulthood. *SSRN Electronic Journal* . [Crossref]
5. Bahadır Dursun, Resul Cesur, Naci Mocan. 2018. The Impact of Education on Health Outcomes and Behaviors in a Middle-Income, Low-Education Country. *Economics & Human Biology* **31**, 94-114. [Crossref]
6. Guy Lacroix, Lalibertée-Auger Francois, Pierre-Carl Michaud, Daniel Parent. 2018. The Effect of College Education on Health and Mortality: Evidence from Canada. *SSRN Electronic Journal* . [Crossref]
7. Peter A. Savelyev. 2012. Conscientiousness, Education, and Longevity of High-Ability Individuals. *SSRN Electronic Journal* . [Crossref]