Education and Health: Is More Always Better?

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Preliminary

Abstract: We study the effect of a compulsory education reform in Sweden on adult health and mortality. Using detailed individual level data on all Swedes born between 1940 and 1957, who were directly affected by the reform, we find mixed effects of exogenously subjecting pupils to educational reform on their later-life health. We find no consistent significant evidence of the protective effects of extra education on overall mortality reported by previous studies. However, aggregated general mortality rates mask significant differences in the effects of education by cause of death. We also find heterogeneous effects of the reform by gender and by residential location. As the reform was implemented as a social experiment, our analysis uncovers a causal link between achieving a higher level of education and health later in life.

I. Introduction

Large positive correlations between education and health have been reported both within and across countries. Better educated people live longer, higher-quality lives, and have healthier offspring. The large and growing health inequalities between the educated and the uneducated have become a policy concern in a number of countries, including Sweden.

Economists have been concerned with two main questions: Is the relationships causal; and if yes, then what are the channels that translate more years spent at school into better health. A sizeable literature has emerged studying these two questions (e.g. Lleras-Muney, 2005; Oreopoulos, 2006; Clark and Royer, 2010), however conclusions about the presence and magnitude of a causal effect of education on health differ. In this study we use the educational reform implemented in Sweden between 1948 and 1962, which increased the years of compulsory education from seven (eight) to nine, abolished the academic tracking system, equalized the curriculum nationwide and kept pupils of different scholastic achievement levels together longer. Clearly, this was a much more complex change than simply altering the minimum school drop-out age. The reform was designed with the intent to "bring forth the intellectual reserve" of the lower classes and foster human capital accumulation at young ages (Ahlström, 1957). A particularly appealing feature of the reform is that it was implemented as a social experiment across municipalities and time.

Previous research has shown that this social experiment had significant effects on economic and social outcomes for the affected cohorts (Meghir and Palme, 2005; Holmlund, 2006). We find mixed results for the effect of the education reform on mortality up to age 59 in the treated population. Similarly to Clark and Royer (2010), we find no significant protective effects of education on overall mortality. However, extra education affected mortality from different causes differently. We also find large differences in effects across genders and by residence in and out of major cities.

The rest of the paper proceeds as follows. First, we discuss the relevant literature studying education and mortality and previous work on the effects of the Swedish education reform. In section III we briefly summarize the reform design and the features most relevant to the question at hand. Next, we describe the data and the empirical

methods. Section VI presents the results and robustness analysis. The last section concludes.

II. Background

There exists a large literature studying the relationship between education and health. Cutler and Lleras-Muney (2006) offer an overview of the existing hypotheses, studies, and outstanding questions. The suggested mechanisms can be roughly divided into several categories. The most straightforward link is between education, income, access to good quality health care, and health. We expect this channel to be less important in the Swedish setting for at least two reasons. First, Sweden has universal health insurance whereby the state covers every resident for the entire cost of health care services. Second, physicians are salaried and do not have direct financial interest in under- or over-providing health care. At the same time, higher income facilitates access to "bads" such as alcohol and smoking. As Cutler and Lleras-Muney show, the consumption of these "bads" initially increases and subsequently falls once education increases to high school level and beyond.

Better labor conditions induced by higher education are also an unlikely channel in the Swedish setting. Strong labor unions are a trademark of the Swedish labor market, and safe and comfortable working conditions have been an integral part of the labor unions' agendas since the 1930. We present some evidence against this hypothesis by separately investigating deaths from preventable causes such as accidents.

A third possibility is that more education increases the amount of health information and improves access to information available to the individual. This has implications for health behaviors as well as the patterns of seeking and responding to care. Cutler, Deaton and Lleras-Muney (2008) and Glied and Lleras-Muney (2010) suggest that more educated people are more likely to adopt new medical technologies and to seek new medical knowledge and techniques to address treatable conditions more effectively. We directly test this hypothesis by considering the causal effect of education on mortality from treatable conditions.

A large portion of the literature attempting to evaluate the causal effect of extra schooling on health has made use of compulsory schooling reforms. Lleras-Muney (2005)

considers the case of the US in the first half of the 20th century, when many states increased the number of years children had to attend school. Her results imply that an extra year of schooling reduces the 10-year mortality rate by over 3 percentage points given a mean mortality rate of ten percent. Similarly, Oreopolous (2003), Arendt (2005) also find that increases in minimum schooling laws in England, Ireland, and Denmark respectively, improved the health of the population. Other studies provide additional quasi-experimental evidence that education improves health, see Grossman (forthcoming), but only for primary and secondary schooling.

A recent paper by Royer and Clark (2010) considers the effects of compulsory education laws in Britain in a regression discontinuity design. Using the same reform Oreopoulos (2006) finds large positive effects of extra education on health. However, Royer and Clark report negligible effects of extra schooling on own health up to age 50. After age 50 the effects completely disappear.

All previous studies (Royer and Clark, 2010; Lleras-Muney, 2005; Oreopoulos, 2006) conduct the analysis on the cohort level, and potentially omit important heterogeneities in effects between different groups. For example, extra schooling may affect people of low socio-economic backgrounds differently than their better-off peers. In this study we test for, and find, heterogeneous effects of the compulsory education reform on individuals coming from different socio-economic backgrounds. In addition to an average treatment effect, we offer estimates that can be interpreted as a treatment effect on the treated.

Previous research finds significant effects of the Swedish reform on education, roughly within 0.13-0.2 of a year increase in female education and 0.26-0.28 of a year increase in men's education (Holmlund, 2008; Meghir and Palme, 2005). Perhaps more importantly, the marginal person affected by the reform increased educational attainment from seven to nine years, but still *did not graduate from high school*. This in important in light of the evidence that the effect of an additional year of education may be non-linear. Section VI presents detailed analysis on the reform's impact on different schooling levels in Sweden. Cutler and Lleras-Muney (2006) report a linear relationship between years of education and the 5-year mortality rate, however they find non-linear effects for some

health behaviors like current smoking, which increases with each extra year of education beyond 7 and only starts decreasing after 11 completed years of education. Depending on the relative importance of two extra years of education conditional on finishing the equivalent of middle school, the results obtained in this study may or may not be directly comparable with the rest of the literature. In terms of the age at which children are affected and the (change in) years of education required, the Swedish reform is closest to the British reform of 1945, which is the subject of Clark and Royer's (2010) and Oreopoulos (2006) work.

The educational reform had effects reaching beyond increased educational attainment. For example, it significantly affected earnings and job retention among the affected individuals. Meghir and Palme (2005) find a 3.4% increase in earnings among low SES background individuals due to the educational reform and negative effects on earnings for the high SES background group. They also find stronger earnings effects among those living outside the major metropolitan areas of Stockholm, Göteborg and Malmö. One of the implications of these finding is that the earnings (or income) channel through which education affects health will be more powerful among individuals from low SES backgrounds and among those living outside the main cities.

III. The Swedish educational reform

The educational reform in Sweden has been described in detail elsewhere (see e.g. Paulston, 1968 and Marklund, 1980 for an in-depth analysis). We first describe the schooling system in the pre-reform period. Students were required to attend a basic primary school until the 6th grade, and were channeled into different academic tracks starting in the 7th grade. Those with better grades were selected for the junior secondary school, the equivalent of preparation for gymnasium or lycee in other European countries. The junior secondary school was a prerequisite for continuing on to high school. Those who were not selected were allowed to attend the basic compulsory school for another one or two years, and could continue on to vocational school after finishing[1]. Thus pre-

[1] There was also a second chance of entering Junior secondary school after completing basic compulsory education. However pupils admitted to Junior Secondary at this later point would have to begin from the start of Junior Secondary leading to a one or two year delay relative to the others of the same cohort who were admitted in the first round.

reform, most Swedish municipalities required seven years of compulsory education. Some municipalities, mostly larger cities, required eight years of compulsory schooling.

In 1948, a parliamentary committee proposed to replace the old compulsory and selective junior secondary school with a nine year compulsory comprehensive school. The students would be able to choose between three different routes after sixth grade: one with a more academic curriculum, one general level and one level which included vocational training. However, there would be no selection based on grades and all pupils would attend the same schools under the new system. Finally, all schools would have the same national curriculum.

After a nation-wide evaluation experiment it was decided to implement to the reformed school system everywhere. The experiment introduced the proposed comprehensive school on the basis of geographic areas, where entire municipalities or parts of city communities, rather than by separate schools or classes. By the time the experiment started, Sweden was divided into about 2,500 city communities and rural municipalities. The number of municipalities was, however, reduced to 1,037 in a reform of the municipality system in 1952, which is the municipality division that we use in the empirical analysis.

The municipalities that would implement the new system were not chosen randomly; the National School Board, which administered the experiment, chose them out of a group of applicants, so as to form a representative set in the board's judgement, based on municipality characteristics (see Chapter 2 in Sixten Marklund, 1981). The final decision on reform assignment was made by the municipality council. A means tested stipend was also introduced in 1953 in the reform areas to ease the perceived financial burden of extending the years of schooling.

New municipalities were added every year to the reform. Once a cohort started in the old system it was not switched over subsequently. As figure 2 shows, around five percent of the cohort born in 1945 were treated to the new reformed system. The percentage increases to full compliance among the 1957 cohort, who were entering first grade at the time the new schooling system was mandated nation-wide. Although national implementation of the reform took place in 1962, the cohort from which the new school system was implemented varied between municipalities; some municipalities

implemented the new system starting from the cohort of pupils who were in 1st grade in 1962 while others implemented it for older cohorts as well up to those which were attending 5th grade in 1962.

In figure 1 we show the proportion of children from birth cohorts 1940-1955 who attained the lowest pre-reform educational level (solid line) and the lowest compulsory post-reform educational level (dashed line). The figure shows that in 15 years the proportion of children with only seven years of compulsory education declined from 35 % in the 1942 birth cohort to 0 in the 1955 birth cohort. Not all of this decline was picked up by the new compulsory level of education. The proportion of children completing the new compulsory level increases from 8% to only around 18%, suggesting that a significant proportion of children continued past the new compulsory level in later cohorts.

An important difference between the Swedish educational reform and the state compulsory education legislation in the US is that the reform did not postulate new regulations on child labor. Physical labor was prohibited for children under the age of 9 in 1833 and a law in 1872 regulated child labor in manufacturing. Very few children under the age of 16 were involved in mostly agricultural activities by the time the education reform was adopted. Some of the protective effect of compulsory education that operates through eliminating child labor is not going to be relevant in our setting.

IV. Data

We use data encompassing the entire population born in Sweden between 1940 (1945) and 1957. To avoid unmeasurable possible confounders in children born during the Second World War, in most of the analysis we exclude individuals born before 1946. Even though Sweden maintained neutrality during the war and was not as adversely affected as other European countries, we cannot entirely rule out in-utero and early-life conditions induced by the war and affecting long-term health outcomes. While we do not observe people who were born in Sweden but subsequently immigrated, the analysis is not contaminated by immigrants who came to Sweden after the reform took place as the sample comprises individuals who were born and resided in Sweden at least until 1960.

Parents have been matched to children through the population registers. Whenever available, information on parents' educational achievement was retrieved as well. We code individuals' socio-economic background according to the highest completed school grade by the index person's father. Children of unskilled fathers (with only a statutory (minimal) level of education) are considered born in low socio-economic conditions. Everyone whose father had more than the minimal level of education required by law at the time is considered of high socio-economic status. Unskilled fathers account for CCC% of the sample. Completed years of own education were obtained from the Swedish education register. The register reports *levels* of completed education, rather than years of completed education. The translation into years of schooling was done using the officially required amount of schooling one needed to obtain to qualify for completing the next level of schooling[2]. The 1960, 1990, and 2003 Censuses were used to obtain information on municipality of residence at the time. The residence information from the 1960 census was used to assign reform status to each individual. For those born before 1943, this information may be misleading as they may have left their municipality of childhood residence at age 18. This is the second reason why we conduct the analysis on cohorts born after 1945, as they are less likely to have moved[3]. A similar method of assigning reform status was used also by Holmlund (2008). We differ in using the 1960 municipality of residence throughout the entire sample, and in manually assigning the reform status to the few remaining individuals who were not adequately matched using her algorithm[4]. The two samples are very comparable and show consistent average characteristics. The sample using here is larger and we have a slightly more accurate measure of reform status. Table 1 reports summary statistics of the main outcomes and control variables.

[2] The mapping works as follows: 7 years for old minimum level of schooling (level 1), 9 for new compulsory level of schooling (level 2), 9.5 for old post-middle school (realskola, level 3), 12.2 for high school (level 4), 16 for college/university (level5) and 19.6 for post-graduate studies (level 6).
[3] Using the municipality of birth is another possibility, however, for cohort born before 1947 the registers contain the municipality of the hospital where the child was born, rather than the mother's municipality of residence.
[4] The algorithm is described in Holmlund (2008). The authors thank Helena Holmlund for sharing her code with us.

The mortality data come from the National Bureau of Health and Welfare (Socialstyrelsen) and cover all deaths that happened in Sweden and lists and International Classification of Diseases (ICD) 9 or ICD10 primary cause of death. First, we look separately on the three main causes: Cancer (ICD 10 Chapter C00-C97 and ICD 9: 140-208), Circulatory diseases (ICD 10 Chapter I00-I99 and ICD 9: 390-459) and Diseases in the respiratory organs (ICD 10 Chapter J00-J99 and ICD 9: 460-519). Second, following the definitions suggested by Rutstein et al. (1976) we look separately on "Avoidable" and "Preventable" causes of death. Table A1 in the appendix lists the mapping between ICD codes and different mortality groups.

A significant advantage of these data is that we know the exact date and cause of death for each individual in our sample, in addition to their reform treatment status. Previous work on the subject has had to rely on counts of missing individual between different census waves (Lleras-Muney, 2006) or mortality databases that only record aggregate mortality statistics by age (Clark and Royer, 2010). The cause of death allows us to further investigate whether the reform affected different mortality rates differently. For example, while cancers are equally likely to affect any individual in the population, some chronic conditions such as diabetes and heart disease have been linked to bad health habits.

V. Empirical Methods

In Tables 2A and 2B we show the first stage estimates of the effect of the Swedish educational reform on completed years of education for females (table 2A) and males (Table 2B). The first column shows the estimated coefficient using the whole sample of females and males. The reform increased completed education among women by 13% of an academic year or a month and a half on average. The increase in men's completed years of education is slightly higher, at 2.2 months on average. In pre-reform municipalities, approximately 20% of the population have the minimum level of education. Post-reform this group comprises only 2.4% of the population and the proportion of people in the second-lowest education group doubles from 7% pre-reform to 15% post-reform. Thus, the reform potentially affected close to 20% of the population.

Lleras-Muney estimates that the schooling reforms from the early 20th century US she uses affected less than 5% of the population. The British reforms used by Oreopoulos (2006) and Clark and Royer (2010) affected 50% (in 1947) and 25% (in 1972) of the relevant population. In terms of the size of the affected population our study is closer to research on the British reform.

We use the reform as a social experiment and evaluate its impact in a framework similar to the basic difference-in-differences methodology employed in Palme and Meghir (2005)[5]. The identification relies on the fact that individuals from the same birth cohort residing in different municipalities varied in reform eligibility status. Similarly, individuals residing in the same municipality but born in consecutive years vary in reform status.

This estimation approach could lead to biased results if there existed municipality-specific trends that affect both education and health. Including explicit controls for these trends has been shown to not significantly change the estimates of the effect of educational reform on schooling (see Holmlund, 2008). As further evidence, we show that after controlling for cohort and municipality fixed effects, parental socio-economic status does not predict reform treatment (see table 2).

We investigate the effects on mortality using stratified Cox proportional hazard models of mortality. The data are stratified on the individual's municipality of residence in 1960. The baseline stratified model is

$$(1)\quad h_i(t) = h_{0i}(t)\exp(\beta_1 * reform + \lambda_i * birthcohort_i)$$

where $h_{0i}(t)$ is the group-specific (stratum-specific) baseline hazard, assumed to be the same across individual sharing the same group but differing across strata. The coefficients β_1 and λ_i are assumed to be the same across individuals from different

[5] An alternative would be to use the reform as an instrumental variable for education, which is one of the approaches taken in Holmlund (2008) and Holmlund, Lindahl and Plug (2010). However, the reform did not only change the number of compulsory schooling years. It also affected the distribution of peer groups; the tracking system of students into different scholastic bins; and the curriculum. As such, the reform likely affected much more than just education, and we cannot guarantee that the omitted variable(s) affecting both health and education were not also affected by the reform. Holmlund (2008) also discusses the issue.

strata. Individuals who do not die are right-censored in 2005, the last year for which we have mortality data[6].

VI. Results

We start by reporting general trends in the association between education and health in Sweden. Table 3 shows the coefficients broken down by gender and into mortality by cause and age groups. Among individuals below forty years of age, education makes little difference for mortality. Women appear to be benefiting more than men (who don't benefit), but even that hazard ratio has a confidence interval implying at most an 8 percent reduction in the probability of death by 40. These results could either be due to the underlying fact that the protective effects of education show up later in life or due to the combined effect of positive and negative biases, where the negative bias is stronger among younger people. For example, if more education induced worse habits such as smoking and drinking in young age, and those habits led to the quick expiration of the affected individuals, the protective effects of education in earlier ages could be masked because of their opposing effects.

Breaking down mortality by cause, the effects in the bellow-40 age group show up in mortality due to respiratory, circulatory and treatable diseases. Women who attain higher education are less likely to die of respiratory or treatable causes by age 40, but more educated men face elevated risks from circulatory diseases.

There are strong positive effects of education on survival among both men and women and across age groups. Both the overall mortality and mortality broken down by different causes are significantly affected by higher education. However, we do not find consistent evidence of a monotonic protective effect as the population ages.

Compulsory school changes and education

The first evidence that educational reform increased educational attainment is graphical. Figure 3 is a plot of the average proportion of children born between 5 years pre-reform and 4 years post-reform who received the old and the new compulsory

[6] Individuals who were born in Sweden and immigrated are not recorded in the mortality register. For these people we do not have information on the date of death.

schooling levels. A clear downward trend in the proportion with least schooling is visible in the last years pre-reform, but also a significant jump down in the average proportion in the first cohort affected by the reform. The percentage of people attaining the lowest educational level drops from a little over 10% in the last cohort pre-reform to less than 5 percent in the first affected cohort, and tapers off to 2% 3 years after the reform was started in the municipality of residence. The graph on the right shows the average proportion of children attaining the second lowest level of education around the reform implementation year. In pre-reform cohorts on average 10% of children get the second lowest (new compulsory) level of education pre-reform. Here again we see a sizeable jump up of around 5% between the last pre-reform cohort and the first affected cohort. The proportion immediately stabilizes at the new level of around 17%.

In the lower two panels of Figure 3 shows the completion rates of vocational and high school education grades among birth cohorts around the reform implementation. The reform's effect spilled also into the vocational school attainment category, where we see an increase of around 2 percent. High school as a terminal degree does not appear to have been affected by the reform beyond the general upward trend associated with the secular increase in educational attainment at the time. To summarize, the educational reform was effective in reducing the proportion of individuals with seven years of education to minimum, increasing the proportion of those with 9 years of education and vocational schooling. The reform had negligible effect on college and post-graduate completion rates.

Previous research shows that the main thrust of the reform was among children coming from low SES family backgrounds, as measured by father's education. In Appendix figures 1A and 2A we graphically show the reform effect among children of low (1A) and high (2A) socio-economic backgrounds. The first notable fact is that in the last cohort before reform implementation, on average 13% of low SES children had the old compulsory level of education, while only 3% of high SES children attained only that level of schooling. Similarly, 15% of low SES children had 9 years of education, compared to a little over 8% of their richer peers. The percentage of individuals with vocational education was similarly higher among those coming from low SES families, but high school and college graduates were much more common among high SES

background children. Clearly, the main effects of the educational reform were felt among low-SES children, as has already been reported by others. Up to 20% of all low SES children were affected, compared to less than 5% among the high SES kids. Figures A1 and A2 show that the reform had a delayed effect on high school attainment among low SES children, however it was much smaller than what appears to be a direct immediate effect on high school completion among high SES children. Four years after the reform, poorer pupils are 0.5% more likely to graduate from high school compared with the year of reform implementation. Over the same period richer children experience an effect which is 3 times larger in absolute magnitude – an increase of 1.5 %. Thus, the reform effects on educational attainment are large and significant among low SES background individuals, and much smaller among high SES individuals.

The reform increases the probability that any woman has completed 8 years of education by 7.7 percentage points, and by 9.5 percentage points in women from lower SES backgrounds. The probability of having completed vocational school also increases, but the estimate in the large sample is not statistically significant. Among low SES females, the reform increases the probability of having graduated from vocational school by 3.6 percentage points. The estimates for high school, college, and graduate school are economically smaller and statistically insignificant in both samples. Overall, in the restricted sample the reform increases the years of education by more than one third of a year, while the effect in the full sample is small and statistically insignificant. The estimates reported in table 2 confirm that the effect of the reform was restricted among women coming from lower socio-economic background and those who did not exhibit high propensity to continue at higher levels of education.

Next we turn to regression analysis. In table 2 we report the estimated reform effect on all individuals, and then we show two additional sets of results – the effect of the reform by paternal SES and by gender. Based on the findings of previous research and the targeted population group, we expect children whose parents had lower SES to benefit more from the reform.

The estimates reported in table 2 support this assumption. The average treatment effect of the reform was an increase in attained years of education of 14%, which is a little over one and a half months. Both high and low SES background children are

affected by the reform, but the magnitude of the effects are very different. High SES children's educational attainment increases by 6% of a school year, while low SES children's education increases by 17%, or two calendar months.

Breaking down the effect further, we see that the group that benefited most from the reform were boys coming from low SES families. Among members of this group the reform resulted in an increase of two and a half months of education on average. The corresponding change for girls is a month less. Overall, the reform affected boys more strongly than girls. Both high SES boys and girls experience small gains from the reform, the coefficients being marginally significant in both groups and economically small. Thus we expect the bulk of the reform effect to be demonstrated among individuals with low SES background, and in particular low SES males.

It is notable that the education effects obtained here are similar in magnitude to those reported by Meghir and Palme (2005) who study a random subset of the population of around 12000 individuals born in 1948 and 1953 and by Holmlund (2008) who uses a 35% random sample of the population. Compared to Meghir and Palme (2005) who use survey responses to assign reform status to individuals, here we have a noisier measure of reform participation, which likely attenuates our estimates somewhat.

The effects of extra education on mortality

In table 4 we report the estimates from model (1) broken down by gender. We first show the reform effects on overall mortality. Then we examine in detail mortality by different causes. Overall mortality up to age 59 is not affected by reform participation for either men or women. The 95 percent confidence intervals are fairly tight, ruling out effects larger than a 5% reduction in mortality for men and 4.2% reduction in mortality for women. Examining general mortality at different ages also does not yield significant estimates. The only marginally significant hazard rate is among men between the ages 40-50, who appear to benefit from extra education. However, even among those individual the 95% confidence interval is fairly tight and excludes gains larger than a 9% reduction in the mortality rate.

All deaths are not created equal, and it is possible that the overall non-results for mortality from all causes grouped together mask important differences between mortality

by cause. To explore this possibility we break down the overall mortality into mortality due to different causes and examine the effects of educational reform on each of those separately. The empirical results are presented in tables 4A and 4B for males and females respectively. Cancer mortality is not significantly affected by more education. The positive hazard rate among men above 50 is not precisely estimated, the confidence band including values as low as a decrease of 10% and as high as an increase of 30%. Interestingly, circulatory and respiratory mortality appear to increase with education, but only among women. Even though the statistical significance is only at the 10% level, elevated circulatory and respiratory mortality rates among the reform group over the age of 40 are significant enough to drive the overall mortality from these causes upwards. We do not see these effects among men. However, males experience higher mortality from preventable causes such as motor vehicle and other accidents, lung cancer and liver cirrhosis. To further separate the effects of accidents from preventable causes that are conceivably in the hands of the individual, we break out mortality from liver cirrhosis and lung cancer. Rather than accidents, it's these two causes of death driving up the preventable mortality among men. Note that the estimated hazard rates are greater than one among all age groups and the overall mortality is also significantly higher than among less educated men. Finally, the lower mortality hazard due to the reform among men 40-50 years of age appears to be driven by a significant reduction in mortality from treatable diseases in that group. Note the confidence band around that estimate is almost completely under the unit hazard rate. This suggests that one of the effects of extra education is to induce individuals to seek care earlier in the development of the health condition and to respond better to administered care. Unfortunately women do not appear to have the same effects.

To investigate the source of elevated hazards among the more educated, we first split the sample by residence in 1990. In tables 5A and 5B we show the estimated effects of education on mortality by cause among men and women living in the three main metropolitan centers – Stockholm, Malmö, and Göteborg – and the rest. In the interest of space, we limit the exposition only to mortality from causes that were statistically significant at least at that 10% level in tables 4A and 4B. We find only marginally significant effects of extra education on preventable mortality among men living in large

cities. The coefficients are both economically and statistically smaller than the estimates for the other group. Men living outside the three major metropolitan centers experience a significantly higher preventable mortality rate if they are affected by the education reform. Among individuals ages 40 and below living outside major cities, mortality rates from any cause are elevated, and the overall mortality rate is significantly higher than those who were not affected by the reform. On the other hand, the protective effect of extra education on mortality from treatable causes appears to be driven by men residing in major cities. Overall, men in major cities experience small gains from education reform, while those living outside the main metropolitan centers lose. .

Women living in major cities benefit from having more education. The protective effect of extra education appears after age 50 among females. Those living in large metropolitan areas have lower average mortality rates, however the estimates on circulatory and respiratory disease mortality suggest that the elevated hazards among women affected by the reform are driven by the subsample of women residing in large metropolitan centers. As we only observe individuals up to age 59, it is possible that the lower mortality rates (in particular cancer mortality) in the 50-59 years old group reflects increased life expectancy due to better disease management. Deaths due to these causes may be shifted to a later period because of better medical interventions.

SES background, educational reform, and mortality

Our data allow us to test for the effect of high socio-economic background while growing up on mortality later in life. In table 6 we include a dummy for father's education (high or low) in the main specification. It is reassuring that including the SES background variable does not affect the coefficient on the reform variable, once again confirming that the reform adoption was not affected by individual pupils' SES. Being born in a relatively more educated family has a strong protective effect on all-cause mortality at all age groups up to age 60. The strongest effect for both men and women is on mortality up to age 40. This is expected since the protective effects of the family are more strongly expressed at younger ages. The effect diminishes over time, but remains economically and statistically significant in the oldest age group. It is notable that SES background is more protective for men than women, across all age groups. The effect of

SES dwarfs the reform effect, even when the latter is significant, such as among men 40-50 years of age. Being born in a more educated, and likely better-off family has a much stronger positive effect on long-term health than acquiring more education.

In tables 7A and 7B we test for potentially heterogeneous reform effects by SES background. Again, we focus on overall mortality but also on causes of death that showed elevated hazards due to educational reform. Based on the results reported in table 2, we expect the health impact of the reform to be concentrated among individuals from low socio-economic backgrounds, as they were the ones affected by the reform. The results do not support this hypothesis. Women from low SES families are not showing any significant mortality effects of the reform. Among low SES men, we see a decrease in mortality among 40-50 year-olds, but then an increase among 50-59 year olds. Thus, among low SES background men, extra education may offset the time of death from the person's 40s to his 50s, however the overall mortality effect is not different from zero. The overall mortality rate for this group is lower among those affected by the reform, however the estimate is not statistically precise. Still, we can exclude *increases* in mortality larger than 5% attributable to the reform.

Both high SES women and men appear to have benefited from the reform. They show lower mortality over the age of 50, and the overall mortality hazard is lower. Even though the coefficients are not precisely estimated, we can exclude effects larger than a 5% increase in mortality due to the educational reform among high SES men and women. This suggests that the health-relevant effects of the reform operated through channels different from educational attainment.

Again, we turn to mortality by cause to test for differential reform effects across different disease classes. The results are presented in tables 7A and 7B. First, it is clear that none of the elevated (or reduced) mortality hazards reported in tables 4A and 4B are due to the high SES background group. The effects of men and women's mortality are isolated among the low SES background population, implying that what we are picking up is indeed related to educational reform. Among men, those with low SES family background experience an slight decrease in mortality from treatable causes between 40 and 50 years of age, but the same group shows an elevated liver cirrhosis and lung cancer

mortality after age 50. It is notable that the latter is elevated also among high SES males, however the estimate is very imprecise.

The elevated female mortality from circulatory and respiratory diseases is entirely isolated among women of low SES. The reform coefficient for respiratory mortality is above one across all age categories, and in all but one age category for circulatory mortality. Both the gains and losses from extra education are concentrated among the group most likely to have been directly affected by educational reform.

Robustness checks

A potential concern is that the reform is picking up underlying trends in the municipalities that affect health and coincide in time with the introduction of the reform. To gauge the significance of this identification threat, we first consider only birth cohorts within a year of the reform implementation. As these children were exposed to more similar conditions while growing up, we should capture a cleaner estimate of the reform impact. First, we consider all-cause mortality, then we focus on the mortality causes that appear to react significantly to the reform in the full-sample estimations. For men, we present results for lung cancer and liver cirrhosis. For women, we investigate circulatory and respiratory cause mortality. Table A2 in the Appendix presents the results.

The estimates from the constrained sample show a consistent picture with the full sample results. All signs are consistent, and most of the magnitudes of the hazard rates are also similar. The one exception is respiratory mortality among females, which is strongly positively affected by the reform in the constrained sample. This suggests that any secular processes that were affecting municipalities at the time of the reform implementation work in favor of reduced mortality among the better educated. The most obvious explanation is the trend in smoking among females born in the 50s, which increased with every cohort between 1950 and 1957 (REF).

As figure 7 shows, the incidence of deaths was not always "smooth" over time depending on reform status and gender. Spikes in death incidence indicate that there was a disproportionately high number of individuals belonging to the group who died in a year. Form example, in 1994 there was a disproportionately high number of reform men who died, and in 1991 there was a spike in deaths among women affected by the reform.

To avoid possible contamination of estimates spanning from random events that affected one of the groups disproportionately, we repeat the mortality analysis after discarding all deaths from 1991, 1994, 1995, and after 2002. The results are presented in Table A3 in the Appendix. There are no significant differences with the main estimates.

VII. Concluding remarks

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TABLES

Table 1: Summary statistics of main outcomes and controls

	No reform		Reform	
Variable	Obs	Mean	Obs	Mean
Female	1250247	0,489	735557	0,488
Married in 2003	1112997	0,645	691992	0,570
Never married in 2003	1112997	0,149	691992	0,250
N children	1250247	1,940	735557	1,908
		[1,246]		[1,262]
Any children	1250247	0,846	735557	0,823
Years of education	1148662	10,988	698765	11,741
		[3,108]		[2,828]
High father's education	575994	0,303	572123	0,390
Died	1250247	0,064	735557	0,034
Average age in 2003	1250247	57	735557	50
		[3,782]		[3,333]
Living in major city in 1990				
Education mean level	1250247	2.983	698765	3.39
Education level1	1250247	.1988	735557	.024
Education level2	1250247	.0771	735557	.157
Education level3	1250247	.4028	735557	.459
Education level4	1250247	.0298	735557	.0507
Education level5	1250247	.201	735557	.249
Education level6	1250247	.0091	735557	.009

Table 2: The effect of the educational reform on years of completed schooling

Years of education	*(1)*	*(2)*	*(3)*
	All	High father's education	Low Father's education
Reform	0.144*	0.062**	0.172*
	(0.021)	(0.024)	(0.019)
Observations	1246937	313150	933787
R-squared	0.032	0.017	0.020
Robust standard errors in parentheses			
+ significant at 10%; ** significant at 5%; * significant at 1%			

Table 2A: The effect of the educational reform on years of completed schooling for women

Years of education	*(1)*	*(2)*	*(3)*
	All	High father's education	Low Father's education
Reform	0.113*	0.053+	0.132*
	(0.028)	(0.028)	(0.026)
Observations	619051	153653	465398
R-squared	0.031	0.019	0.020
Robust standard errors in parentheses			
+ significant at 10%; ** significant at 5%; * significant at 1%			

Table 2B: The effect of the educational reform on years of completed schooling for men

Years of education	*(1)*	*(2)*	*(3)*
	All	High father's education	Low Father's education
Reform	0.176*	0.072+	0.212*
	(0.025)	(0.037)	(0.028)
Observations	627886	159497	468389
R-squared	0.040	0.024	0.027
Robust standard errors in parentheses			
+ significant at 10%; ** significant at 5%; * significant at 1%			

Table 3: The relationship between years of education and mortality at different ages. Cox stratified proportional hazard models.

Men	*(1)*	*(2)*	*(3)*	*(4)*
Age	*50-60*	*40-50*	*40 and below*	*all*
Years of completed education	0.894* (0.882 - 0.907)	0.862* (0.836 - 0.888)	1.011 (0.975 - 1.048)	0.894* (0.881 - 0.906)
Observations	527574	691620	691756	691756
Women	*(1)*	*(2)*	*(3)*	*(4)*
Age	*50-60*	*40-50*	*40 and below*	*all*
Years of completed education	0.909* (0.895 - 0.922)	0.907* (0.878 - 0.936)	0.958+ (0.913 - 1.005)	0.910* (0.897 - 0.923)
Observations	519874	680300	680416	680416

Robust 95% confidence intervals in parentheses

+ significant at 10%; ** significant at 5%; * significant at 1%

Table 3A: Mortality and education correlations by cause of mortality - men

Men		Circulatory		
Years of completed education	0.887* (0.869 - 0.906)	0.842* (0.796 - 0.891)	1.186+ (0.990 - 1.421)	0.884* (0.867 - 0.902)
		Respiratory		
Years of completed education	0.857* (0.787 - 0.934)	0.905 (0.723 - 1.132)	1.006 (0.881 - 1.150)	0.877* (0.802 - 0.959)
		Preventable		
Years of completed education	0.836* (0.802 - 0.872)	0.808* (0.733 - 0.890)	0.984 (0.933 - 1.037)	0.847* (0.807 - 0.890)
		Treatable causes of death		
Years of completed education	0.819* (0.786 - 0.853)	0.762* (0.673 - 0.864)	0.991 (0.867 - 1.132)	0.813* (0.782 - 0.845)
Observations	527574	691620	691756	691756
Robust 95% confidence intervals in parentheses				
+ significant at 10%; ** significant at 5%; * significant at 1%				

Table 3B: Mortality and education correlations by cause of mortality - women

Women		Circulatory		
Years of completed education	0.842* (0.812 - 0.874)	0.803* (0.741 - 0.871)	1.002 (0.877 - 1.145)	0.842* (0.815 - 0.869)
		Respiratory		
Years of completed education	0.806* (0.742 - 0.875)	0.591+ (0.319 - 1.095)	0.811* (0.699 - 0.942)	0.793* (0.737 - 0.852)
		Preventable		
Years of completed education	0.874* (0.843 - 0.905)	0.819* (0.742 - 0.904)	1.096 (0.975 - 1.233)	0.874* (0.847 - 0.901)
		Treatable causes of death		
Years of completed education	0.872* (0.830 - 0.916)	0.813** (0.688 - 0.961)	0.841* (0.768 - 0.921)	0.861* (0.818 - 0.906)
Observations	519874	680300	680416	680416
Robust 95% confidence intervals in parentheses				
+ significant at 10%; ** significant at 5%; * significant at 1%				

All specifications include birth year cohort dummies and are stratified according to residence in different Swedish municipalities in 1960 (~1000 municipalities in total). Standard errors are clustered on the municipality level.

Table 4: Mortality and educational reform

Men	*(1)*	*(2)*	*(3)*	*(4)*
Age	*50-60*	*40-50*	*40 and below*	*all*
Reform	1.063	0.954+	0.980	0.986
	(0.979 - 1.153)	(0.906 - 1.004)	(0.907 - 1.060)	(0.950 - 1.024)
Deaths	11,024	14,786	6,220	32,030
Observations	502056	671398	677479	677479

Women	*(1)*	*(2)*	*(3)*	*(4)*
Age	*50-60*	*40-50*	*40 and below*	*all*
Reform	0.954	1.059	0.991	1.006
	(0.874 - 1.042)	(0.981 - 1.143)	(0.878 - 1.118)	(0.958 - 1.056)
Deaths	7,203	9,081	3,255	19,539
Observations	488463	647739	650856	650856

Robust 95% confidence intervals in parentheses

+ significant at 10%; ** significant at 5%; * significant at 1%

Table 4A: Mortality and educational reform by cause of death - men

Men		Cancer		
Reform	1.135+	0.948	0.934	1.012
	(0.990 - 1.301)	(0.852 - 1.055)	(0.738 - 1.182)	(0.933 - 1.097)
Deaths	3,250	3,063	730	7,043
		Circulatory		
Reform	0.992	0.939	1.069	0.970
	(0.824 - 1.193)	(0.821 - 1.074)	(0.865 - 1.322)	(0.883 - 1.065)
Deaths	3,362	3,602	842	7,806
		Respiratory		
Reform	1.310	0.772	0.988	0.949
	(0.868 - 1.976)	(0.533 - 1.120)	(0.607 - 1.606)	(0.755 - 1.193)
Deaths	317	377	188	882
		Preventable		
Reform	1.427*	0.962	1.047	1.047
	(1.140 - 1.786)	(0.866 - 1.068)	(0.939 - 1.168)	(0.972 - 1.127)
Deaths	1,203	3,510	3,118	7,831
		Lung cancer and liver cirrhosis		
Reform	1.450*	1.026	1.050	1.160**
	(1.135 - 1.852)	(0.842 - 1.250)	(0.663 - 1.663)	(1.009 - 1.334)
		Treatable causes of death		
Reform	1.028	0.820+	1.062	0.914
	(0.803 - 1.314)	(0.668 - 1.006)	(0.761 - 1.481)	(0.787 - 1.061)
Observations	502056	671398	677479	677479

Robust 95% confidence intervals in parentheses

+ significant at 10%; ** significant at 5%; * significant at 1%

Table 4B: Mortality and educational reform by cause of death - women

Women		Cancer		
Reform	1.054 (0.858 - 1.295)	1.036 (0.930 - 1.154)	0.926 (0.823 - 1.041)	0.998 (0.934 - 1.067)
Deaths	4,048	4,452	1,074	9,574
		Circulatory		
Reform	1.067 (0.868 - 1.313)	1.166+ (0.978 - 1.390)	1.151 (0.780 - 1.697)	1.132+ (0.977 - 1.312)
Deaths	1,115	1,314	371	2,800
		Respiratory		
Reform	1.430+ (0.934 - 2.188)	1.260 (0.875 - 1.815)	1.248 (0.652 - 2.387)	1.278+ (0.983 - 1.662)
Deaths	266	251	128	645
		Preventable		
Reform	0.879 (0.714 - 1.081)	0.938 (0.772 - 1.138)	0.950 (0.784 - 1.151)	0.910 (0.803 - 1.032)
Deaths	1,057	1,629	1,062	3,748
		Lung cancer and liver cirrhosis		
Reform	0.878 (0.706 - 1.092)	0.852 (0.665 - 1.092)	1.356 (0.783 - 2.349)	0.887 (0.758 - 1.038)
		Treatable causes of death		
Reform	1.206 (0.949 - 1.533)	0.936 (0.751 - 1.166)	1.069 (0.768 - 1.487)	1.011 (0.874 - 1.168)
Observations	488463	647739	650856	650856
Robust 95% confidence intervals in parentheses				
+ significant at 10%; ** significant at 5%; * significant at 1%				

All specifications include birth year cohort dummies and are stratified according to residence in different Swedish municipalities in 1960 (~1000 municipalities in total). Standard errors are clustered on the municipality level.

Table 5: Reform and mortality – by residence in 1990.

Men

Major cities	*(1)*	*(2)*	*(3)*	*(4)*
Age	*50-60*	*40-50*	*40 and below*	*all*
Reform	1.052	0.939	1.050	0.986
	(0.926 - 1.196)	(0.854 - 1.032)	(0.780 - 1.414)	(0.910 - 1.068)
Observations	189156	253153	253967	253967

Outside major cities	*(1)*	*(2)*	*(3)*	*(4)*
Age	*50-60*	*40-50*	*40 and below*	*all*
Reform	1.068	0.953	1.204+	1.000
	(0.961 - 1.187)	(0.891 - 1.018)	(0.998 - 1.453)	(0.951 - 1.051)

Robust 95% confidence intervals in parentheses

+ significant at 10%; ** significant at 5%; * significant at 1%

Women

Major cities	*(1)*	*(2)*	*(3)*	*(4)*
Age	*50-60*	*40-50*	*40 and below*	*all*
Reform	0.864**	1.105	0.855	0.986
	(0.766 - 0.975)	(0.980 - 1.247)	(0.604 - 1.211)	(0.916 - 1.062)
Obs	174986	231395	231780	231780

Outside cities	*(1)*	*(2)*	*(3)*	*(4)*
Age	*50-60*	*40-50*	*40 and below*	*all*
Reform	1.031	1.053	1.000	1.033
	(0.901 - 1.181)	(0.964 - 1.151)	(0.761 - 1.313)	(0.961 - 1.111)
Observations	298822	396561	397511	397511

Robust 95% confidence intervals in parentheses

+ significant at 10%; ** significant at 5%; * significant at 1%

All specifications include birth year cohort dummies and are stratified according to residence in different Swedish municipalities in 1960 (~1000 municipalities in total). Standard errors are clustered on the municipality level.

Table 5A: Reform and mortality – by cause of mortality and residence in 1990. Men in major cities (Stockholm, Göteborg and Malmö).

Men city	*(1)*	*(2)*	*(3)*	*(4)*
Age	*50-60*	*40-50*	*40 and below*	*all*
Men city		Cancer		
Reform	1.027 (0.854 - 1.235)	0.933 (0.800 - 1.087)	0.864 (0.461 - 1.618)	0.986 (0.882 - 1.102)
		Lung cancer and liver cirrhosis		
Reform	1.334 (0.946 - 1.882)	0.959 (0.694 - 1.326)	0.401 (0.110 - 1.463)	1.067 (0.845 - 1.348)
		Treatable causes of death		
Reform	1.023 (0.724 - 1.446)	0.642** (0.451 - 0.916)	0.746 (0.279 - 1.999)	.814+ (.6463311- 1.027197)
Observations	189156	253153	253967	253967

Men out city	*(1)*	*(2)*	*(3)*	*(4)*
Age	*50-60*	*40-50*	*40 and below*	*all*
		Cancer		
Reform	1.188 (0.952 - 1.482)	0.973 (0.836 - 1.132)	1.009 (0.623 - 1.634)	1.041 (0.921 - 1.176)
		Lung cancer and liver cirrhosis		
Reform	1.565* (1.122 - 2.184)	1.047 (0.794 - 1.381)	3.628* (1.376 - 9.569)	1.289** (1.053 - 1.578)
		Treatable causes of death		
Reform	1.009 (0.707 - 1.440)	0.945 (0.715 - 1.250)	1.370 (0.653 - 2.872)	0.970 (0.786 - 1.198)
Observations	312900	417466	419275	419275
Robust 95% confidence intervals in parentheses				
+ significant at 10%; ** significant at 5%; * significant at 1%				

All specifications include birth year cohort dummies and are stratified according to residence in different Swedish municipalities in 1960 (~1000 municipalities in total). Standard errors are clustered on the municipality level.

Table 5B: Reform and mortality – by cause of mortality and residence in 1990. Women in major cities (Stockholm, Göteborg and Malmö).

Women	*(1)*	*(2)*	*(3)*	*(4)*
Age	*50-60*	*40-50*	*40 and below*	*all*
		Circulatory		
Reform	1.171	1.250	3.205+	1.261**
	(0.833 - 1.644)	(0.951 - 1.643)	(0.894 - 11.486)	(1.011 - 1.574)
		Respiratory		
Reform	1.264	1.709	1.033	1.551+
	(0.670 - 2.385)	(0.850 - 3.436)	(0.086 - 12.459)	(0.952 - 2.526)
Observations	174986	231395	231780	231780
Robust 95% confidence intervals in parentheses				
+ significant at 10%; ** significant at 5%; * significant at 1%				

Women out city	*(1)*	*(2)*	*(3)*	*(4)*
Age	*50-60*	*40-50*	*40 and below*	*all*
		Circulatory		
Reform	1.044	1.094	1.336	1.097
	(0.740 - 1.474)	(0.879 - 1.362)	(0.698 - 2.558)	(0.912 - 1.320)
		Respiratory		
Reform	1.431	1.120	0.960	1.163
	(0.808 - 2.535)	(0.698 - 1.795)	(0.216 - 4.265)	(0.811 - 1.667)
Observations	298822	396561	397511	397511
Robust 95% confidence intervals in parentheses				
+ significant at 10%; ** significant at 5%; * significant at 1%				

All specifications include birth year cohort dummies and are stratified according to residence in different Swedish municipalities in 1960 (~1000 municipalities in total). Standard errors are clustered on the municipality level.

Table 6: Reform, SES background and mortality

Men	*(1)*	*(2)*	*(3)*	*(4)*
Age	*50-60*	*40-50*	*40 and below*	*all*
Reform	1.066	0.954+	0.974	0.987
	(0.981 - 1.158)	(0.908 - 1.003)	(0.900 - 1.053)	(0.951 - 1.024)
High SES	0.805*	0.735*	0.576*	0.722*
	(0.762 - 0.851)	(0.700 - 0.773)	(0.512 - 0.648)	(0.687 - 0.758)
		Lung cancer and liver cirrhosis		
Reform	1.456*	1.030	1.035	1.164**
	(1.139 - 1.861)	(0.845 - 1.256)	(0.654 - 1.636)	(1.011 - 1.340)
High SES	0.750*	0.602*	0.334*	0.633*
	(0.640 - 0.879)	(0.502 - 0.722)	(0.206 - 0.542)	(0.566 - 0.709)
Women		*Women*		
Age	*50-60*	*40-50*	*40 and below*	*all*
Reform	0.957	1.059	0.985	1.007
	(0.876 - 1.045)	(0.981 - 1.143)	(0.872 - 1.112)	(0.958 - 1.057)
High SES	0.874*	0.804*	0.637*	0.796*
	(0.823 - 0.929)	(0.766 - 0.844)	(0.559 - 0.726)	(0.762 - 0.832)
		Lung cancer and liver cirrhosis		
Reform	0.883	0.854	1.330	0.890
	(0.711 - 1.097)	(0.666 - 1.095)	(0.768 - 2.303)	(0.760 - 1.043)
High SES	0.742*	0.757*	0.427*	0.721*
	(0.630 - 0.873)	(0.625 - 0.917)	(0.232 - 0.785)	(0.628 - 0.828)

Robust 95% confidence intervals in parentheses

+ significant at 10%; ** significant at 5%; * significant at 1%

All specifications include birth year cohort dummies and are stratified according to residence in different Swedish municipalities in 1960 (~1000 municipalities in total). Standard errors are clustered on the municipality level.

Table 7: Reform and mortality, by SES background

Men	*(1)*	*(2)*	*(3)*	*(4)*
Age	*50-60*	*40-50*	*40 and below*	*all*
		Low SES background		
Reform	1.129*	0.944**	0.968	0.994
	(1.035 - 1.232)	(0.893 - 0.999)	(0.890 - 1.053)	(0.954 - 1.037)
		High SES background		
Reform	0.881+	0.964	1.098	0.961
	(0.762 - 1.020)	(0.853 - 1.090)	(0.867 - 1.392)	(0.881 - 1.048)
Robust 95% confidence intervals in parentheses				
+ significant at 10%; ** significant at 5%; * significant at 1%				

Women	*(1)*	*(2)*	*(3)*	*(4)*
Age	*50-60*	*40-50*	*40 and below*	*all*
		Low SES background		
Reform	1.003	1.062	1.047	1.033
	(0.904 - 1.114)	(0.970 - 1.163)	(0.915 - 1.198)	(0.972 - 1.098)
		High SES background		
Reform	0.848+	1.053	0.822	0.944
	(0.707 - 1.016)	(0.904 - 1.226)	(0.624 - 1.081)	(0.851 - 1.048)
Robust 95% confidence intervals in parentheses				
+ significant at 10%; ** significant at 5%; * significant at 1%				

Table 7A: Reform and mortality by cause and SES background - men

Men	(1)	(2)	(3)	(4)
Age	50-60	40-50	40 and below	all
		Low SES background		
		Treatable		
Reform	1.041	0.824+	1.018	0.914
	(0.806 - 1.346)	(0.655 - 1.037)	(0.704 - 1.473)	(0.780 - 1.072)
		Lung cancer/liver cirrhosis		
Reform	1.439**	0.973	1.062	1.131
	(1.057 - 1.959)	(0.795 - 1.191)	(0.662 - 1.702)	(0.971 - 1.316)
		High SES background		
		Treatable		
Reform	0.984	0.743	2.153	0.933
	(0.515 - 1.880)	(0.463 - 1.193)	(0.788 - 5.880)	(0.650 - 1.339)
		Lung cancer/liver cirrhosis		
Reform	1.572	1.186	0.855	1.233
	(0.888 - 2.784)	(0.712 - 1.976)	(0.193 - 3.793)	(0.879 - 1.730)
Robust 95% confidence intervals in parentheses				
+ significant at 10%; ** significant at 5%; * significant at 1%				

All specifications include birth year cohort dummies and are stratified according to residence in different Swedish municipalities in 1960 (~1000 municipalities in total). Standard errors are clustered on the municipality level.

Table 7B: Reform and mortality by cause and SES background - women

Women	(1)	(2)	(3)	(4)
Age	*50-60*	*40-50*	*40 and below*	*all*
		Low SES background		
		Respiratory		
Reform	1.337 (0.829 - 2.158)	1.409 (0.924 - 2.148)	1.498 (0.791 - 2.836)	1.362** (1.047 - 1.773)
		Circulatory		
Reform	1.208 (0.926 - 1.575)	1.147 (0.933 - 1.411)	1.198 (0.790 - 1.819)	1.173+ (0.980 - 1.403)
		High SES background		
		Respiratory		
Reform	2.152 (0.676 - 6.850)	0.908 (0.381 - 2.163)	0.448 (0.047 - 4.247)	1.054 (0.500 - 2.224)
		Circulatory		
Reform	0.690 (0.412 - 1.156)	1.236 (0.765 - 1.998)	0.958 (0.366 - 2.504)	0.982 (0.671 - 1.437)
Robust 95% confidence intervals in parentheses				
+ significant at 10%; ** significant at 5%; * significant at 1%				

All specifications include birth year cohort dummies and are stratified according to residence in different Swedish municipalities in 1960 (~1000 municipalities in total). Standard errors are clustered on the municipality level.

FIGURES

Figure 1: Proportions of birth cohorts with pre- and post-reform levels of compulsory education

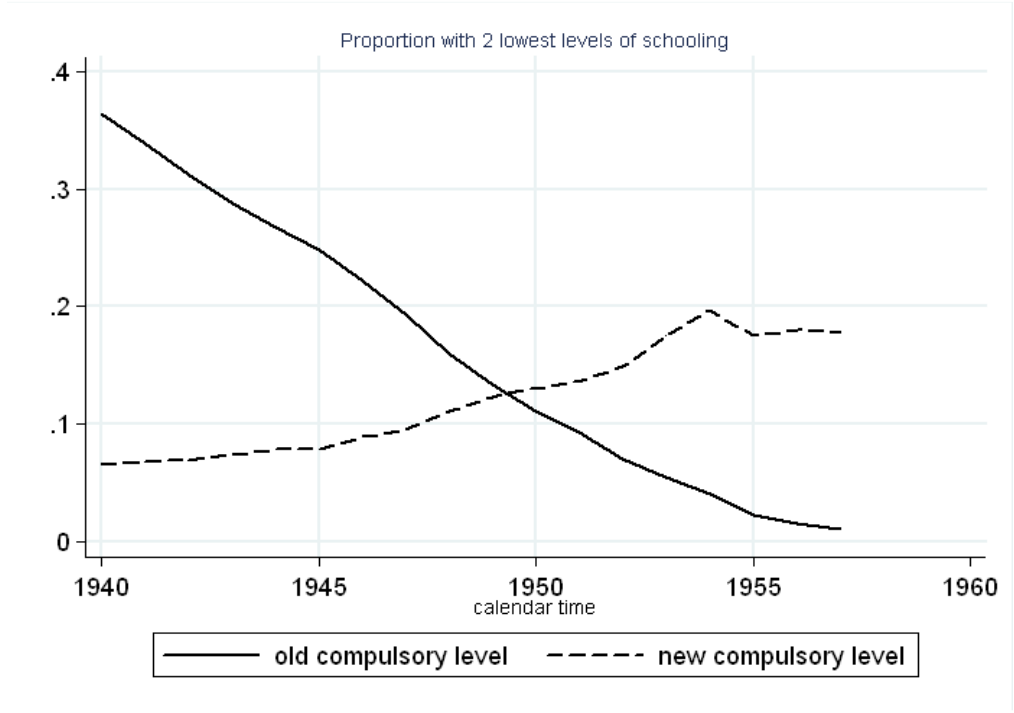


Figure 2: Percent of birth cohort affected by reform

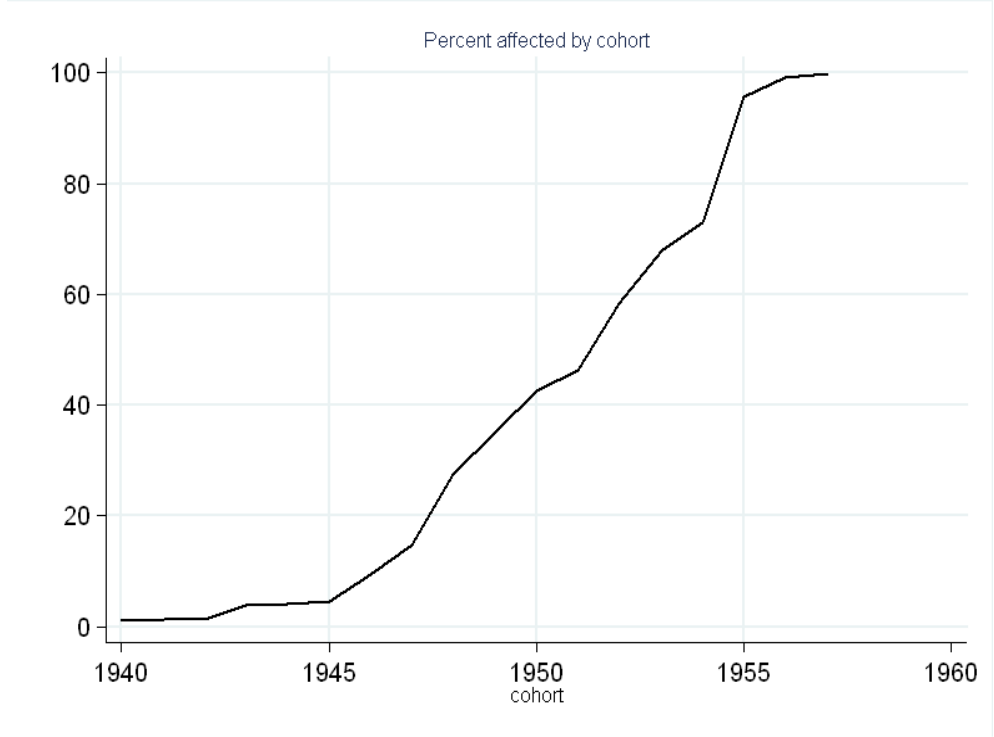
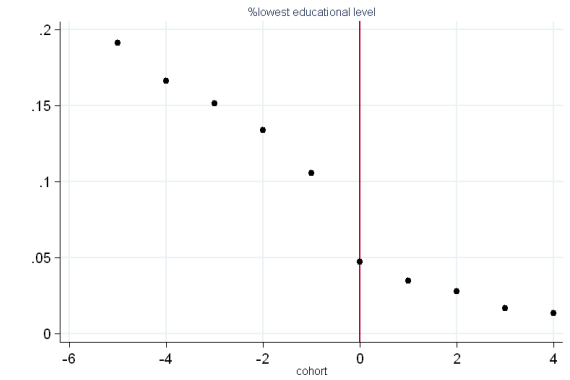
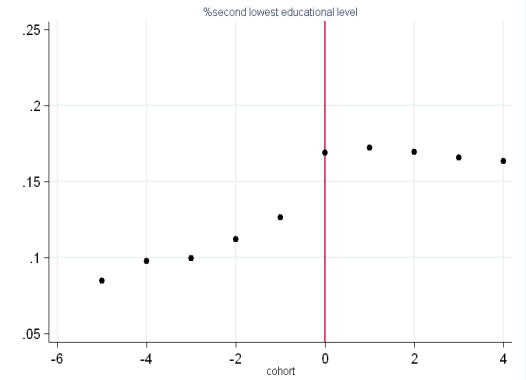


Figure 3: Changes in proportion of individuals with different levels of education around the time of reform implementation in the municipality

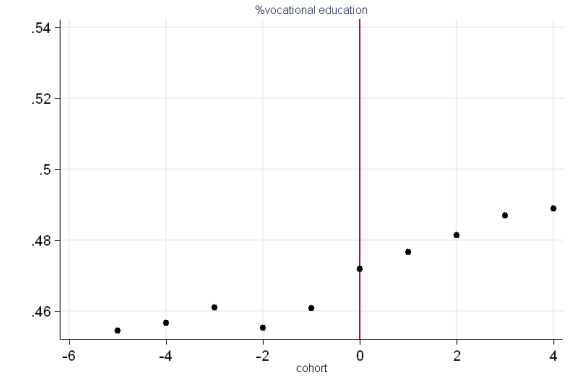
Proportion with old compulsory level



Proportion with new compulsory level



Proportion with vocational education



Proportion with high school

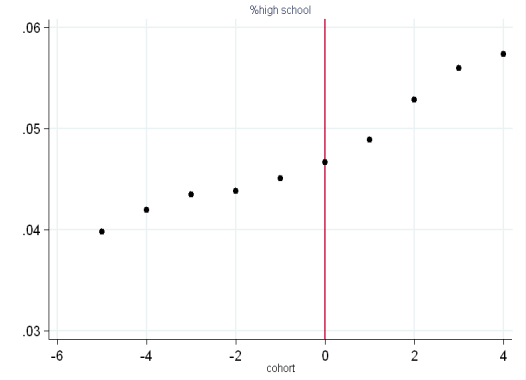


Figure 4: Crude mortality rates by cohort and age

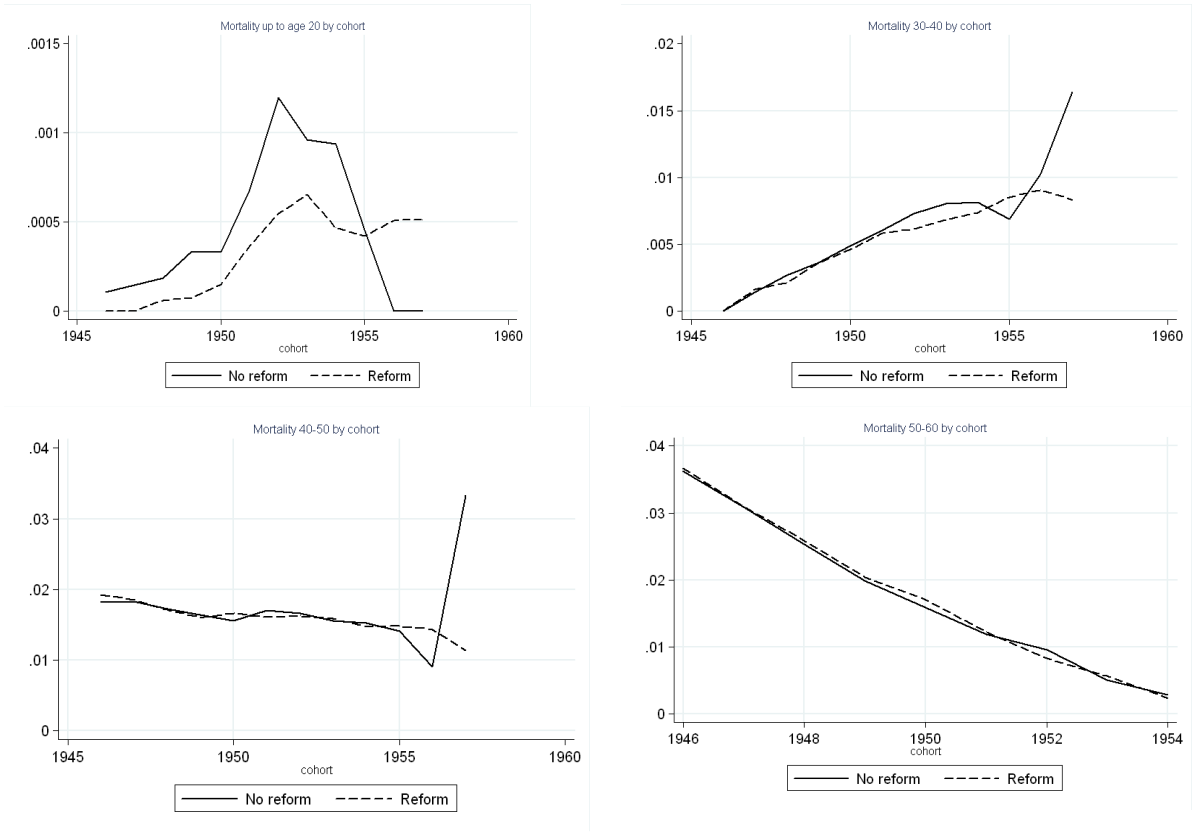
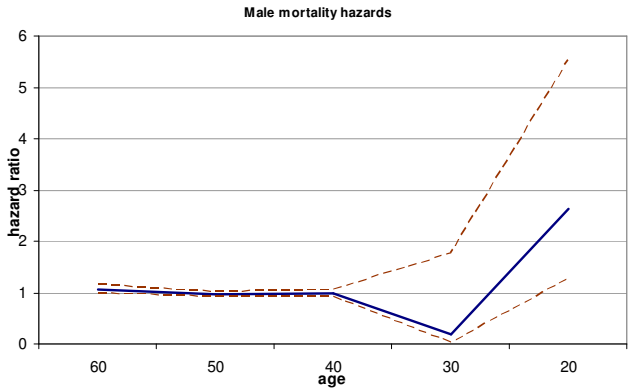


Figure 5: Estimated reform mortality hazard ratios at different ages

Men



Women

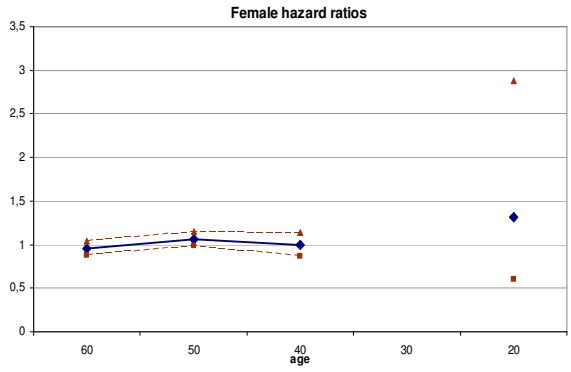
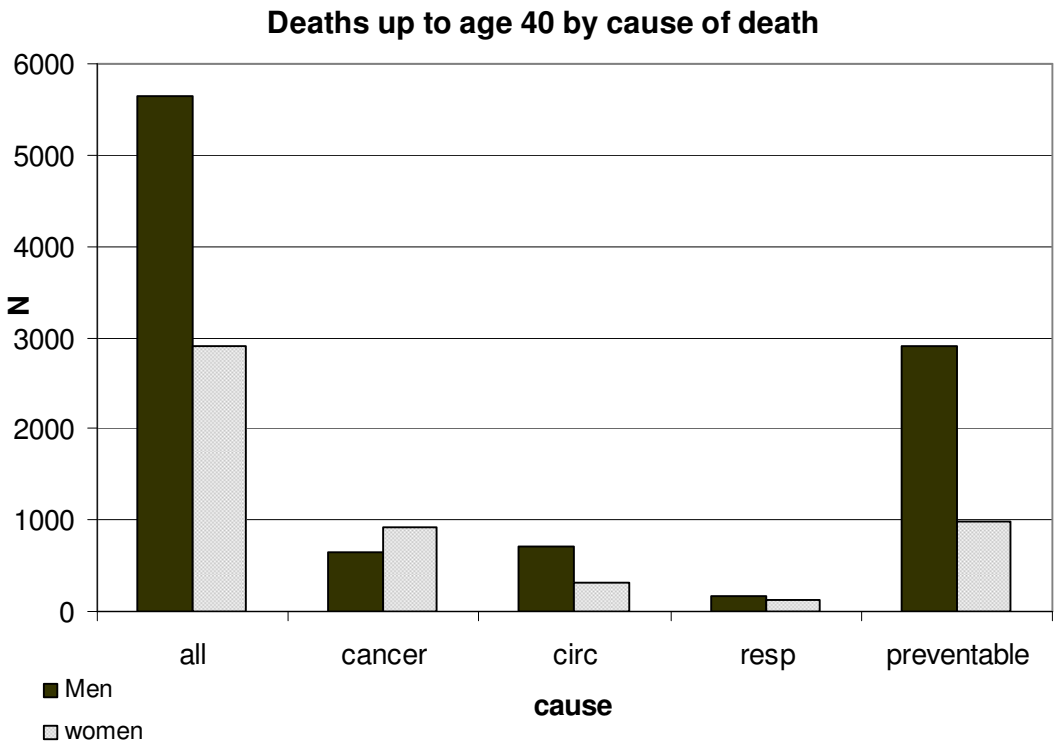
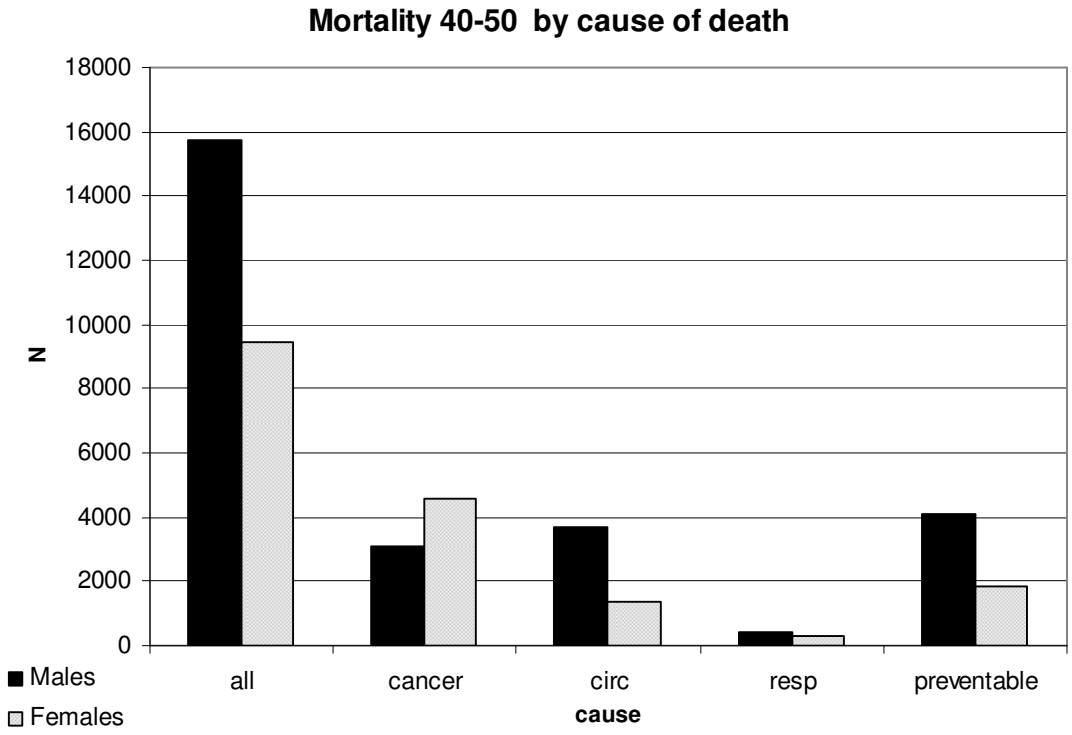


Figure 6: Crude mortality by cause of death at different ages




Mortality 40-50 by cause of death
18000
16000
14000
12000
10000
8000
6000
4000
2000
0
N
Males
Females
all
cancer
circ
resp
preventable
cause

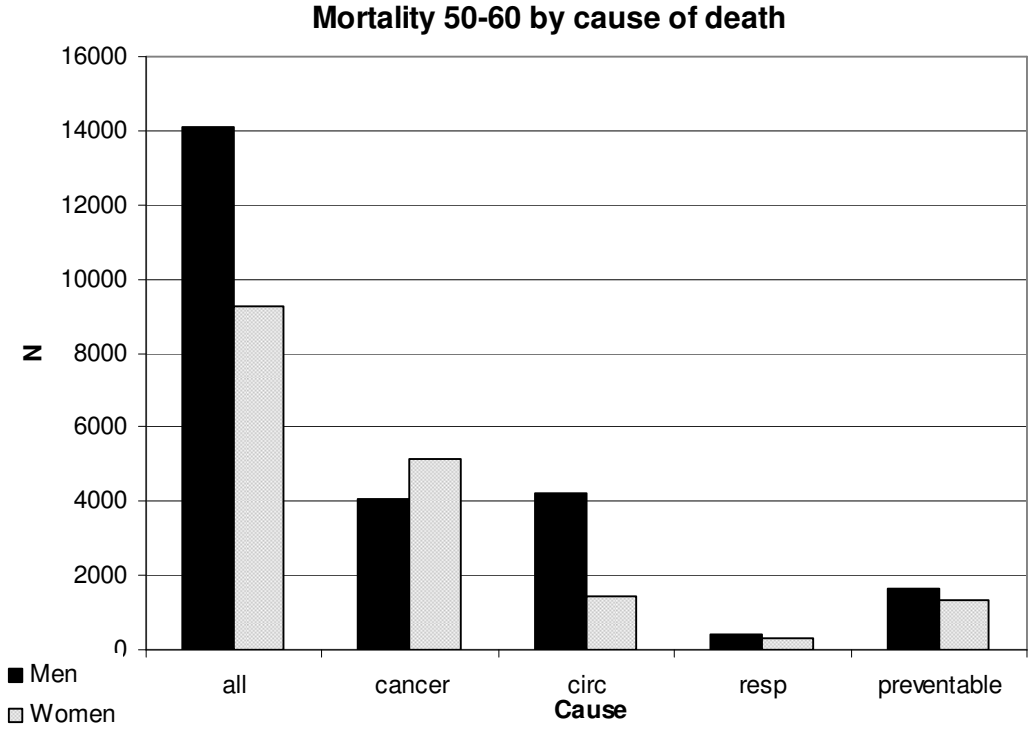
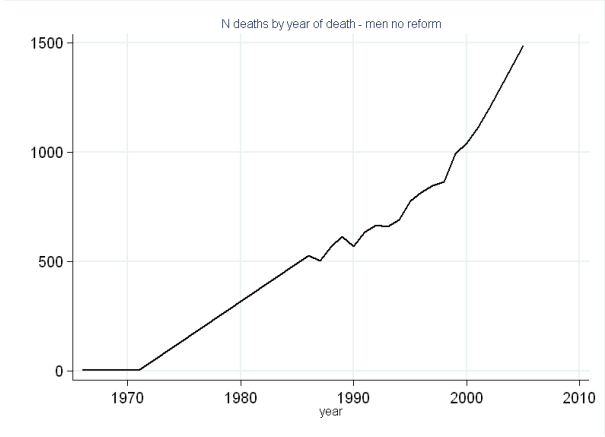
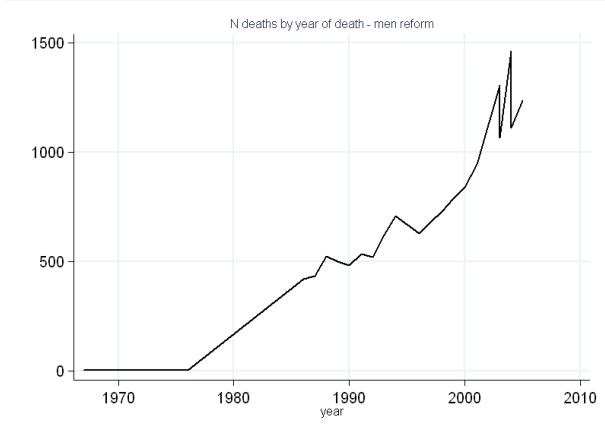

Mortality 50-60 by cause of death
16000
14000
12000
10000
8000
6000
4000
2000
0
N
Men
Women
all
cancer
circ
resp
preventable
Cause

Figure 7: Number of deaths by year and reform status

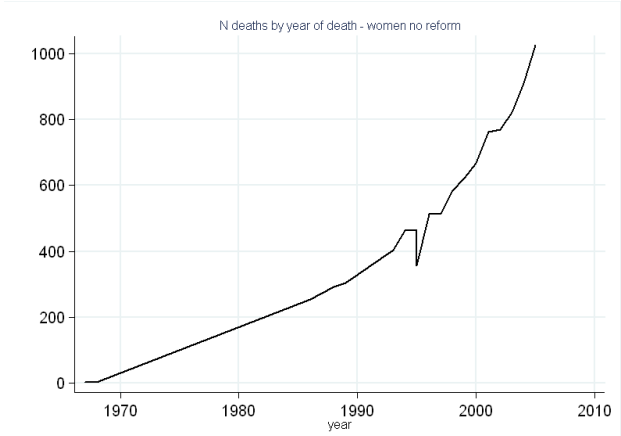
Men no reform



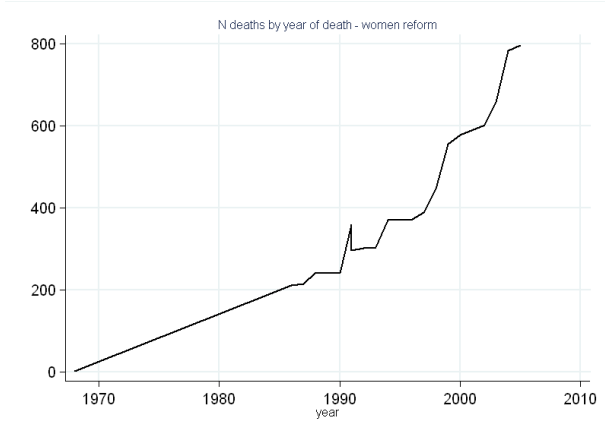
Men reform



Women no reform



Women reform



APPENDIX TABLES

Table A1: ICD 9 and ICD 10 codes groups used to establish different causes of death

Cause	Age group	ICD9	ICD10
Treatable causes of death			
Tuberculosis	5-64	010-018, 137	A15-A19. B90
Malignant neoplasm of cervix uteri	5-64	180	C53
Chromic rheumatic heart disease	5-44	393-398	I05-I09
All respiratory diseases	1-64	460-519	J00-J99
Asthma	5-44	493	J45, J46
Appendicitis	5-64	540-543	K35-K38
Abdominal hernia	5-64	550-553	K40-K46
Hypertensive and cerebrovascular disease	35-64	401-405,430-438	I10-I15, I60-I69
Chollelthiasis and cholecystitis	5-64	574, 575.0, 575.1	K80-K81
Maternal deaths	All ages	630-676	O00-O99
Cancers	All ages	140-239	C00-C99; D00-D48
Diseases of the circulatory system	15-64	390-459	I00-I99
Diseases of the respiratory system	All ages	460-519	J00-J99
Preventable causes of death			
Lung cancer	5-64	162	C33-C34
Cirrhosis of liver	15-64	571.0-571.3, 571.5-571.6	K70, K74.3-K74.6
Motor vehicle accidents	5-64	800-999 (death due to external injury)	V02-V89

Table A2: Cohorts within a year of the reform: men

Men	*(1)*	*(2)*	*(3)*	*(4)*
Age	*50-60*	*40-50*	*40 and below*	*all*
Reform	0.992	0.901+	0.932	0.939
	(0.870 -	(0.807 -	(0.767 -	(0.866 -
	1.132)	1.006)	1.132)	1.017)
		Treatable		
Reform	0.907	0.663+	0.783	0.775+
	(0.615 -	(0.422 -	(0.346 -	(0.591 -
	1.337)	1.041)	1.771)	1.017)
		Lung cancer/cirrhosis		
Reform	1.379+	0.780	0.832	1.035
	(0.943 -	(0.533 -	(0.312 -	(0.803 -
	2.015)	1.142)	2.215)	1.334)
Observations	217124	254382	256502	256502
Robust 95% confidence intervals in parentheses				
+ significant at 10%; ** significant at 5%; * significant at 1%				

Cohorts within a year of the reform: women

Women	*(1)*	*(2)*	*(3)*	*(4)*
Age	*50-60*	*40-50*	*40 and below*	*all*
Reform	1.050	1.157	1.231	1.121+
	(0.875 -	(0.963 -	(0.848 -	(0.996 -
	1.260)	1.390)	1.786)	1.261)
		Circulatory		
Reform	1.157	1.127	0.555	1.072
	(0.782 -	(0.778 -	(0.235 -	(0.837 -
	1.712)	1.633)	1.311)	1.372)
		Respiratory		
Reform	2.634**	1.195	1.297	1.858**
	(1.076 -	(0.405 -	(0.245 -	(1.007 -
	6.450)	3.531)	6.873)	3.427)
Observations	171404	205378	206318	206318
Robust 95% confidence intervals in parentheses				
+ significant at 10%; ** significant at 5%; * significant at 1%				

Table A3: Analysis excluding 1994 and post 2002 for men

	(1)	*(2)*	*(3)*	*(4)*
Age	*50-60*	*40-50*	*40 and below*	*all*
Reform	1.063 (0.979 - 1.153)	0.954+ (0.906 - 1.004)	0.990 (0.914 - 1.072)	0.989 (0.952 - 1.026)
		Cancer		
Reform	1.135+ (0.990 - 1.302)	0.948 (0.852 - 1.055)	0.928 (0.732 - 1.178)	1.011 (0.933 - 1.097)
		Circulatory		
Reform	0.992 (0.824 - 1.194)	0.939 (0.821 - 1.074)	1.067 (0.863 - 1.320)	0.970 (0.883 - 1.065)
		Respiratory		
Reform	1.310 (0.868 - 1.976)	0.773 (0.533 - 1.121)	1.110 (0.657 - 1.874)	0.982 (0.777 - 1.241)
		Preventable		
Reform	1.427* (1.140 - 1.786)	0.962 (0.866 - 1.069)	1.057 (0.947 - 1.180)	1.051 (0.975 - 1.133)
		Lung cancer/cirrhosis		
Reform	1.450* (1.135 - 1.852)	1.026 (0.842 - 1.251)	1.062 (0.668 - 1.687)	1.162** (1.010 - 1.336)
Observations	501777	671090	677075	677075
Robust 95% confidence intervals in parentheses				
+ significant at 10%; ** significant at 5%; * significant at 1%				

Excluding 1991 and 1995 for women

	(1)	*(2)*	*(3)*	*(4)*
Age	*50-60*	*40-50*	*40 and below*	*all*
Reform	0.954	1.059	0.991	1.006
	(0.874 -	(0.981 -	(0.878 -	(0.958 -
	1.042)	1.143)	1.118)	1.056)
		Cancer		
Reform	0.926	1.036	1.054	0.998
	(0.823 -	(0.930 -	(0.858 -	(0.934 -
	1.041)	1.154)	1.295)	1.067)
		Circulatory		
Reform	1.067	1.166+	1.151	1.132+
	(0.868 -	(0.978 -	(0.780 -	(0.977 -
	1.313)	1.390)	1.697)	1.312)
		Respiratory		
Reform	1.430+	1.260	1.248	1.278+
	(0.934 -	(0.875 -	(0.652 -	(0.983 -
	2.188)	1.815)	2.387)	1.662)
		Preventable		
Reform	0.879	0.938	0.950	0.910
	(0.714 -	(0.772 -	(0.784 -	(0.803 -
	1.081)	1.138)	1.151)	1.032)
		Lung cancer/cirrhosis		
Reform	0.878	0.852	1.356	0.887
	(0.706 -	(0.665 -	(0.783 -	(0.758 -
	1.092)	1.092)	2.349)	1.038)
Observations	488463	647739	650856	650856

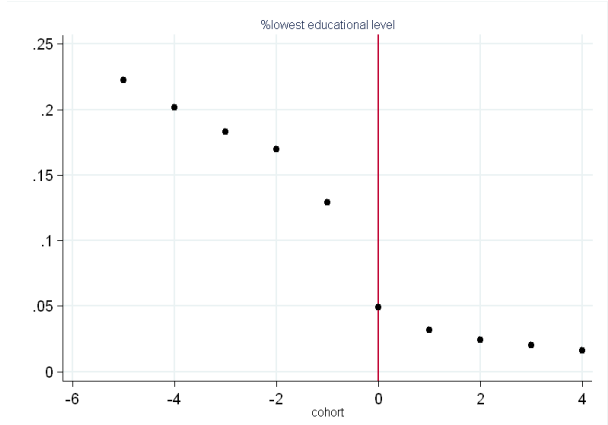
Robust 95% confidence intervals in parentheses

+ significant at 10%; ** significant at 5%; * significant at 1%

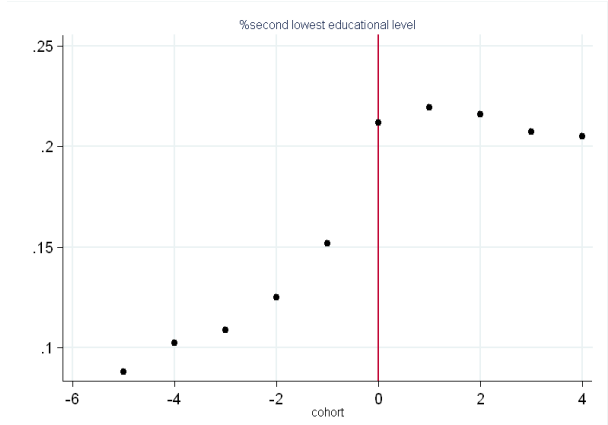
APPENDIX FIGURES

Figure A1: Changes in proportion of individuals with different levels of education around the time of reform implementation in the municipality – low SES background

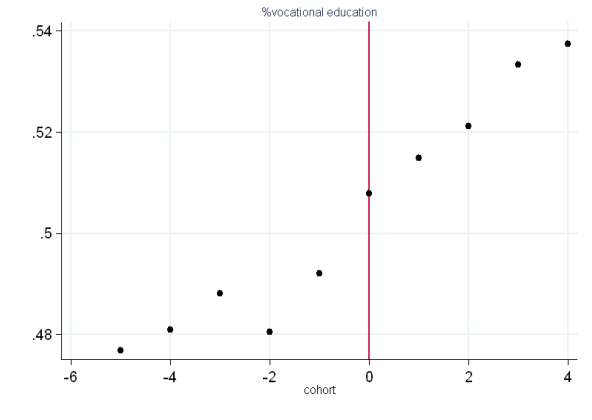
Proportion with old compulsory level



Proportion with new compulsory level



Proportion with vocational education



Proportion with high school

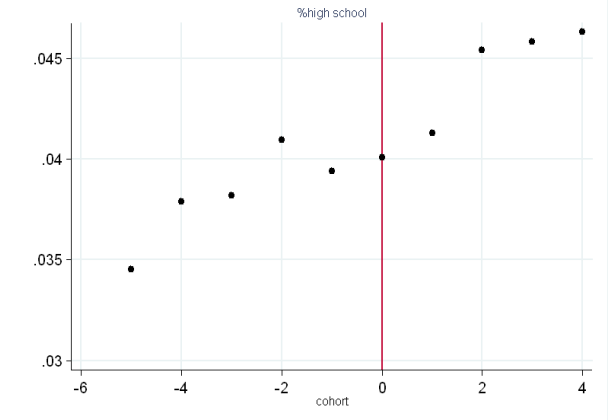
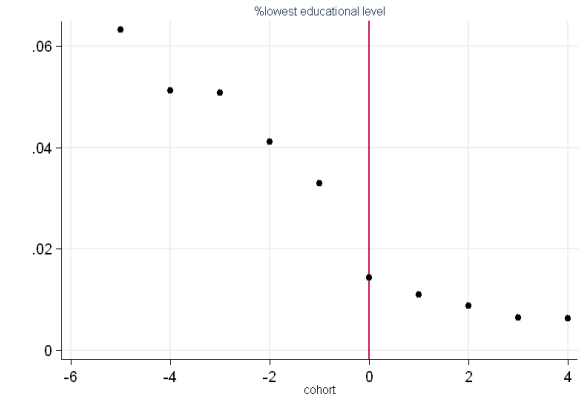
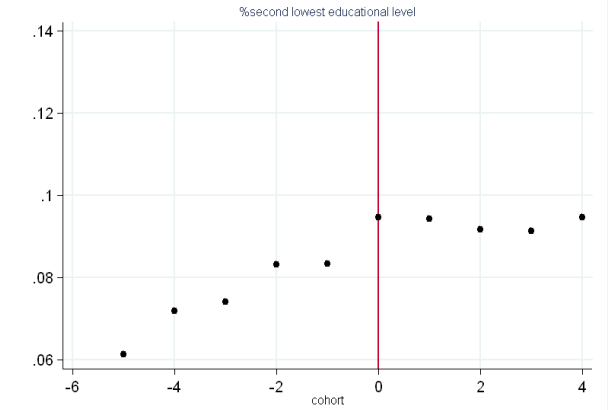


Figure A2: Changes in proportion of individuals with different levels of education around the time of reform implementation in the municipality – high SES background

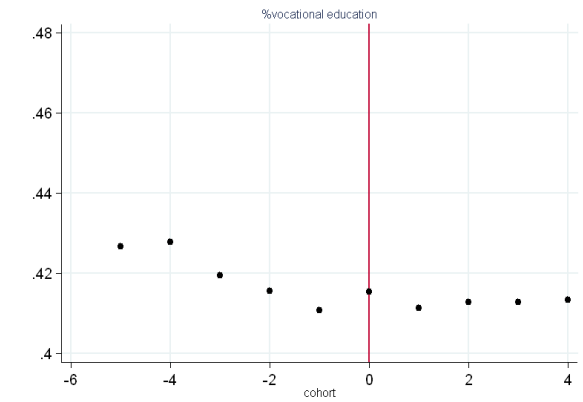
Proportion with old compulsory level



Proportion with new compulsory level



Proportion with vocational education



Proportion with high school