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Education and Health: Long-run Effects of Peers, Tracking and Years

Martin Fischer, Ulf-G Gerdtham, Gawain Heckley, Martin Karlsson, Gustav Kjellsson and Therese Nilsson

Research Institute of Industrial Economics
P.O. Box 55665
SE-102 15 Stockholm, Sweden
info@ifn.se
www.ifn.se

Education and health: long-run effects of peers, tracking and years

Martin Fischer, Ulf-G Gerdtham, Gawain Heckley, Martin Karlsson, Gustav Kjellsson and Therese Nilsson*

Abstract

We investigate two parallel school reforms in Sweden to assess the long-run health effects of education. One reform only increased years of schooling, while the other increased years of schooling but also removed tracking leading to a more mixed socioeconomic peer group. By differencing the effects of the parallel reforms we can separate the effect of de-tracking and peers from that of more schooling. We find that the pure years of schooling reform reduced mortality and improved current health. Differencing the effects of the reforms shows significant differences in the estimated impacts, suggesting that de-tracking and subsequent peer effects resulted in worse health.

Keywords: Health returns to education, school tracking, peer effects

JEL Classification: *I12, I18, I26*

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1 Introduction

Education is strongly associated with better health and longer lives, but we still have very limited knowledge as to whether this gradient captures a causal mechanism. There are several reasons to expect a relationship between education and health: education may have a causal effect through its impact on health production, or through impacting financial resources, preferences or self-empowerment, or through improved understanding or access to information that helps improve health (see e.g. Cutler and Lleras-Muney 2006; Grossman 2006). A very large empirical literature has proxied the measurement of education by years of education. The causal evidence of the impact of education measured in this dimension has relied on quasi-experimental evidence and remains inconclusive (see e.g. Cutler and Lleras-Muney 2012; Grossman 2015; Galama et al. 2018), with results being sensitive to methodology, the source of exogenous variation and to vary by time, place and gender.

Galama et al. (2018) highlight that differences in quality aspects of education are a potential explanation for the conflicting results in the literature on the causal effects of education on health. School reforms often involve reform packages where the year extension is only one of many components possibly working in different directions. Education as an input can be measured more broadly than just in quantitative terms. Quality aspects of education, such as the composition of peers, tracking and curricula changes, might also be important in determining health outcomes. Peers could share important health information or impact social norms and adoption of health behaviours, particularly in settings where perceptions are in a formative stage. Similarly curriculum changes may affect the ability to process information on health-related behaviours and related risks. Insights into the role of quantity and quality aspects education for health is a topical policy issue. They can help policy makers design policies improving health in the general population.

In this paper we use a unique quasi-experimental set-up in Sweden that allows us to tease apart years of schooling effects and compare and contrast these to the effects of the removal of tracking and peer group change on health. This setting includes two similar natural experiments—implemented during a limited time span and exposing the same target population to two treatments that both include a similar increase in quantity of education; however only one of them varied the quality of education. The comprehensive schooling reform has been analyzed in a number of previous studies,[1] whereas this is the first study to use the coincident compulsory schooling extension. Both reforms generated a sizeable increase in years of education, 0.25 and by 0.52 years respectively, which in terms of size is at the top end of the years of schooling literature. They had no spill-over effects to higher education.

Thanks to the rather unique research design with two contemporaneous reforms which both affected the schooling acquired by large parts of the population, we are able to make a number of important contributions to the literature. First, we are able to attribute the health effects of a comprehensive schooling reform including de-tracking, to different components of the reform package. Many European countries introduced

[1] The empirical work analysing the Swedish comprehensive school reform e.g. includes work on incomes (Meghir and Palme, 2005), on financial literacy (Lundborg et al., 2018), on intergenerational effects (Lundborg et al., 2014; Holmlund et al., 2011; Lundborg and Majlesi, 2018) and on crime (Hjalmarsson et al., 2015; Meghir et al., 2012).

comprehensive schooling reforms that delayed academic tracking in the 1950-1980 period (Brunello et al., 2015) with the aim to improve inequality of opportunity (Holmlund, 2008; Jones et al., 2014). The large body of research which uses such reforms to study the health effects of education can not separate out the pure years of education effects from other changes since they happened coincidentally.[2] Several studies report small overall health effects of such reforms (Spasojevic, 2010; Lager and Torssander, 2012; Palme and Simeonova, 2015; Meghir et al., 2018; Lager et al., 2016; Ravesteijn et al., 2017). If these zero effects are the sum of positive effects of schooling and harmful effects of other reform components, it is deeply troubling if the policy conclusion emerging from this body of literature is that education has no causal effect on health.

There is indeed evidence suggesting that components other than the change in years of education have affected health outcomes after such comprehesive schooling reforms. For example, Lager et al. (2016) study the Swedish reform and show emotional control loss for treated high-SES individuals with potential linkage to psychological health following peer composition changes. Bockerman et al. (2018) evaluate the mental health effects of a comprehensive school reform in Finland and find worsened mental health for some subgroups, which they suggest are due to adverse peer groups and discouragement. Moreover Meghir and Palme (2005) report that lifetime earnings for high-SES individuals decreased by 6 per cent following the reform and attribute this effect to reduced school quality. If income effects translate into longer and healthier lives (c.f. Lindahl 2005; Mazumder 2012) such economic loss could impact population health. Our paper proposes a simple method for how to disentangle the years of schooling effects and effects operating via these other channels.

A second contribution of this paper is that we present estimates of the effects of extended education on health, which challenge the emerging consensus that the effects of increasing the length of compulsory schooling are small, at least in Scandinavia. We exploit a compulsory schooling reform which is unusually suitable for answering this question: it increased the minimum years of schooling from 7 to 8, but there were no other changes to the school system or to the curriculum and it was rolled out in small geographic units over a time period of 20 years, thus reducing concerns about general equilibrium effects or bottlenecks related to input factors. The main result coming out of the analysis is that the reform had a beneficial effect on health. Our main estimate on overall mortality suggests that an 8$^{\text{th}}$ year of schooling reduced mortality by 1.2 percentage points from a baseline of 16.9 per cent. This estimate is statistically significant at the 10 per cent level only, but we find a significant (at the five per cent level) reduction in death due to circulatory disease. In addition, we are able to corroborate this finding with survey data, showing that also self-reported health and obesity improved thanks to the reform.

Third, we contribute to the growing literature analysing the role of qualitative aspects of education. Tracking changes, which often go hand in hand with quality

[2] Indeed this is the reason why some researchers have not chosen to use specific reforms as an instrument for years of education (the reform does not provide a valid instrument). The same problem evidently applies to studies using other types of reforms that involved both quantity and quality changes to examine health (see e.g. Jürges et al. (2011) that evaluate a reform involving year and compositional changes) and non-health outcomes of education, see e.g. Kirdar et al. (2015) and Grenet (2013) that evaluate reforms including both year extensions and certification system changes.

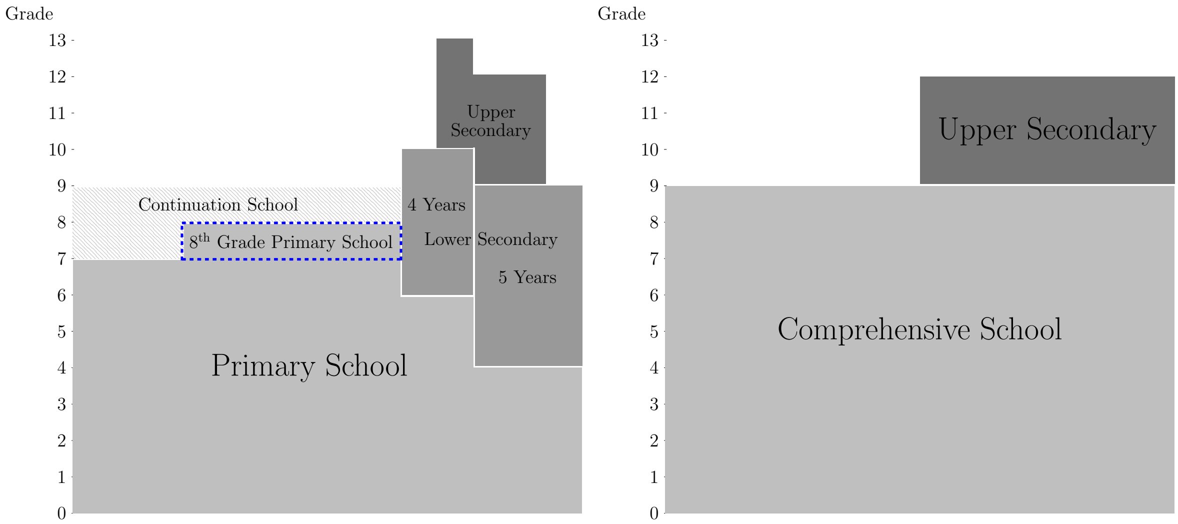
changes, have been found to impact health: Basu et al. (2018) examine exposure to two different types of secondary schooling systems in England and Wales and show the transition from early-tracking to non-selective school system increased depression and smoking. The already-mentioned studies on Finland (Bockerman et al., 2018; Ravesteijn et al., 2017) attribute findings on mental health and mortality to de-tracking. Also the literature on quality of education focusing on attendance in dissimilar types of secondary school (see e.g. Jones et al. 2011, 2012, 2014) suggests it gives rise to important differences in health and health-related outcomes, but to separate school quality from the characteristics of students that attend school is inherently very difficult. We exploit the co-existence of two different school system within the same country in order to analyse how different socio-economic groups are affected by de-tracking. Contrary to several of these previous studies, our findings suggest that de-tracking is harmful to health across the board, and not only in higher socioeconomic groups.

Since school tracking and peer effects are inherently linked, our findings also contribute to the literature on peer effects on health. That literature also suggests important interactions between school environment, peers and health. For example Robalino and Macy (2018) suggest peers to affect smoking prevalence among high-school students, and Gaviria and Raphael (2001) find strong peer effects in drug use and alcohol drinking among tenth graders. The literature has so far mainly considered short-term effects and primarily examined the impact on health behaviors (see for e.g. Sacerdote et al. (2011) for an overview). It is important to understand whether such peer effects sustain long after the peer groups have dissolved since this impacts the interpretation of the short-term effects. If health behaviours such as smoking are only affected contemporaneously and an individual's long-term health preferences remain unaffected, then smoking behavior may only be impacted in the short-term and not impact long-term health outcomes. This in turn affects policy evaluation of school policies such as the removal or introduction of tracking or school choice that affect peer-group composition.

The next section describes the two Swedish school system and the two schooling reforms. Section 3 introduces a conceptual model and illustrates what the estimated causal effects of the two reform captures. Readers not familiar with the Local Average Treatment Effect (LATE) theorem may skip section 3.2 and go straight to the conclusions of the model in section 3.3. Section 4 introduces the data, while section 5 outlines our empirical strategy. Section 6 and 7 present and discuss the empirical results. Finally, we conclude in section 8.

2 The Swedish School System and the Reforms

During the first half of the 20$^{\text{th}}$ century Sweden had a selective two-track school system. Students started school at age seven and attended a common compulsory primary school. After the 4$^{\text{th}}$ or 6$^{\text{th}}$ grade, good performing students (defined by their marks) had the option to study at lower secondary school, which allowed them to continue to upper secondary school and later university. Only a small share of students switched to the academic track and these disproportionately had parents that were professionals and administrators (Jonsson, 1991). The majority of students continued in primary school up to the 7$^{\text{th}}$ grade, but it was noted that about 30 per cent of higher ability students, that likely would complete secondary education without problems, only finished primary school (see e.g.Hammarström 1996; Husén and Härnqvist 2000).[3]



(a) Prior Comprehensive 9-Year Reform

(b) Post Comprehensive 9-Year Reform

Fig. 1: The Swedish school system

Notes: The figure illustrates the Swedish school system and the reforms considered. Figure (a) shows the one year increase in minimum years of schooling within the old school system for students not matriculating to lower secondary schooling, which we call the 8 year reform. In this system students could take an academic track after grade 4, following a five year track, or after grade 6, following a four year track. Students not enrolling in secondary education took low intensity courses six weeks per year in local continuation schools after finishing compulsory schooling. Figure (b) shows the comprehensive school system which increased minimum years of schooling and postponed tracking, which we call the 9 year reform.

Between 1941 to 1962 a large number of municipalities gradually raised the minimum years of compulsory schooling from 7 to 8 years. We call this the 8 year reform. As illustrated in figure 1(a) the reform (the first treated birth cohorts were born between 1927 to 1948) was a simple extension of the minimum years of schooling within the municipality for those students who did not go to lower secondary school. The reform was seen as an opportunity to give more time for the students to learn, without any specific changes to the curriculum. More than 1,000 municipalities had introduced a mandatory 8$^{\text{th}}$ grade before implementing the comprehensive 9 year reform (see figure A.1).[4]

From 1948 to 1969 municipalities also gradually replaced the existing school system with a new comprehensive school system illustrated by figure 1(b). We call this the 9 year reform.[5] This reform (affecting birth cohorts born between 1938 and 1959) was

[3]For the birth cohorts we consider the share of students that only finished primary school was 84% (cohort 1938) and 70% (cohort 1951).

[4]The previous literature has referred to the 8 year extension as a rare phenomenon mainly occurring in the largest cities (Holmlund, 2007), but it was in fact a large reform.

[5]Between 1949 and 1962 the reform was introduced as a *social* experiment in certain areas (Marklund, 1982). The National School Board chose the areas from a group of applicants to form a representative set based on observable municipality characteristics (Holmlund, 2007). In 1962 the Swedish parliament decided that all municipalities should be obliged to offer the new comprehensive

very different in character to the 8 year reform: it increased the minimum years of schooling, this time to 9 years, but also postponed tracking of students, with the aim of fostering greater equality of opportunity (Holmlund, 2007).[6] The removal of tracking broadened the peer group mix (social class and ability composition changes) as all students now shared the same class up to an including the 9$^{\text{th}}$ grade.

3 Conceptual Model

We now provide a simple theoretical framework for how the two schooling reforms might affect health outcomes. Our main goal is to highlight the potential differences in the impact of the reforms among different sub-populations—and in particular to provide a theoretical basis for the forthcoming empirical analysis. We will make a number of assumptions in order to make the model simple, but in section 3.3 below we will argue that many of these assumptions can be relaxed without altering the main predictions provided by the model.

We assume that the school system produces some type of human capital (such as health), H_{ij}, for individual i attending school j based on the inputs individual ability A and a school peer effect P. Each school year adds to the individual's human capital according to the production function:

$$h_{ij} = f\left(A_{ij}, P_j\right) = \eta A_{ij} + \theta P\left(\mathbf{A}_j\right) \tag{1}$$

where $\mathbf{A}_j$ is the vector of individual abilities in class j. We do not specify a particular functional form for the function $P_j\left(\cdot\right)$. Our model is consistent with the canonical "linear-in-means" model in which $P_j = \frac{1}{N_j}\sum_{i=1}^{N_j} A_{ij}$, but also other models of peer effects, such as the "rainbow" model where students benefit from heterogeneity (cf. Sacerdote et al., 2011). Also, the term "peer effects" is used here in the widest possible manner. That is, the impact of the classroom ability profile $\mathbf{A}_j$ on human capital acquisition need not be mediated only through interactions between pupils. It may arise if certain school inputs react to the composition of peers; for example, if it is easier to attract teachers to schools (or classes) with a favorable ability mix.

In the interest of simplicity, we assume that there are only two ability types in the population: high-ability individuals with ability A_1 and low-ability individuals with ability A_0. In addition, it is assumed that the tracking system imposes perfect separation of types - so that we can denote the ability profile in the academic track by $\mathbf{A}_1 = (A_1, \ldots, A_1)$ and that of the basic track by $\mathbf{A}_0 = (A_0, \ldots, A_0)$. Similarly, we denote the ability profile in a pooled classroom by $\mathbf{A}_c$.

Consistent with the historical context, we assume that the school system prescribes T_0 years of comprehensive schooling and $T_{1\tau}$ years of tracked schooling, cf. Figure 1 above. The time spent in different tracks, $T_{1\tau}$, may depend on the track τ visited. We denote the basic track by $\tau = 0$ and the academic track by $\tau = 1$. Thus, the human capital accumulated, H_{ij}, will be equal to:

$$H_{ij} = T_0\left[\eta A_{ij} + \theta P\left(\mathbf{A}_c\right)\right] + T_{1\tau}\left[\eta A_{ij} + \theta P\left(\mathbf{A}_\tau\right)\right] \tag{2}$$

school system and to have the new system in place in 1969.

[6]The 9 year reform also introduced a few minor changes to the curriculum. With the reform English was made compulsory from 5$^{\text{th}}$ grade. In 1953, English was made compulsory even for the old primary school system so all cohorts born after 1945, untreated or treated, had the same curriculum.

Equation (2) imposes the assumption that an individual's human capital is piecewise linear in years of schooling. This assumption is not necessary to derive our main results below: it is sufficient that the effects of extending the basic track are linear between 7 and 9 years of compulsory schooling.

3.1 Effects of Reforms

We now consider the impact on human capital acquisition of the two school reforms.

- **The 8 year reform**: extends the basic track from 7 to 8 years (i.e. increasing $T_0 + T_{10}$ from 7 to 8 years) while keeping the rest of the system constant.
- **The 9 year reform**: extends compulsory schooling from 8 to 9 years (i.e. increasing $T_0 + T_{10}$ from 8 to 9 years) and removes tracking.

We start with the 8-year reform. Assuming that the reform does not affect the selection into tracks, its effect on human capital is:

$$\Delta_8 H_{ij} = \begin{cases} \eta A_{ij} + \theta P(\mathbf{A}_0) = \eta A_0 + \theta P(\mathbf{A}_0) & \text{if } \tau = 0 \\ 0 & \text{if } \tau = 1 \end{cases} \tag{3}$$

Next, consider the 9 year reform that increases T_0 to 9 and abolishes tracking. According to the model, the effect of such a reform will be:

$$\Delta_9 H_{ij} = \begin{cases} 4\theta\left[P(\mathbf{A}_c) - P(\mathbf{A}_0)\right] + \eta A_0 + \theta P(\mathbf{A}_c) & \text{if } \tau = 0 \\ 5\theta\left[P(\mathbf{A}_c) - P(\mathbf{A}_1)\right] & \text{if } \tau = 1 \end{cases} \tag{4}$$

where the first term has factor 5 for high-ability individuals since their five last years are affected by de-tracking. For low-ability individuals, de-tracking affects years 5 to 8 (hence factor 4), and the additional year in school adds h_{ij} to their human capital.

Now we consider a comparison of the two reforms at the individual and at the aggregate level. Starting with the individual-level comparison and noting that we have only two types of individuals, we may compare the effects of the 9 year reform to the 8 year reform in the two groups. Introducing the notation Δ_{89}^a for $a \in \{0, 1\}$, we have

$$\Delta_{89}^a = 5\theta\left[P(\mathbf{A}_c) - P(\mathbf{A}_a)\right] \tag{5}$$

To compare the effect of the two reforms on the aggregate level, we simply summarize the individual level differences by means of a weighted sum of the effects for the high and low ability type individuals as

$$\begin{aligned} \Delta_{89} &= \alpha\Delta_{89}^1 + (1-\alpha)\Delta_{89}^0 && (6) \\ &= 5\theta\left[P(\mathbf{A}_c) - \alpha P(\mathbf{A}_1) - (1-\alpha)P(\mathbf{A}_0)\right] && (7) \end{aligned}$$

where α equals the share of high ability individuals.

A comparison of Δ_{89}^a and Δ_{89} lead to some immediate conclusions. First, whenever there are peer effects of any kind operating, $\Delta_{89}^a \neq 0$. This holds regardless of the type of peer effects operating: they may be of the linear-in-means variety or take on other forms. As long as $P(\mathbf{A}_c) - P(\mathbf{A}_a) \neq 0$ so that it matters what peer group a pupil is exposed to, the effects of the two reforms within an ability group will be different. On the other hand, a zero difference of aggregate effects ($\Delta_{89} = 0$) is compatible with peer effects operating as long as $P(\mathbf{A}_c) = \alpha P(\mathbf{A}_1) + (1-\alpha)P(\mathbf{A}_0)$. It follows that a situation with $\Delta_{89}^a \neq 0$ and $\Delta_{89} = 0$ is indicative of peer effects being linear in means.

3.2 Estimation

Next, we briefly asses what an instrumental variable estimate of returns to schooling picks up in the case of the two reforms. Our point of departure is the LATE theorem (cf. Angrist and Pischke, 2008). Readers not at all familiar with this theorem can skip this section and go straight to the conclusion of the model without a loss of understanding. The LATE theorem states that the Two Stage Least Squares (2SLS) estimator identifies a local average treatment effect for the subpopulation of compliers; i.e. individuals who increase their years of schooling when exposed to the reform. As the exposition above shows, we will need to relax the assumption that the instrument only affects the outcome via years spent in school (i.e the *exclusion restriction*) in the case of the 9 year reform, since the students who would have taken nine years of schooling regardless, the ‘‘always-takers’’, are affected by other components of the reform.

Starting with the 8 year reform and inserting the results of the above model into the Wald estimator, we get

$$\beta_{8,2SLS} = \frac{\mathbb{E}\left[H_{ij} \mid Z=1\right] - \mathbb{E}\left[H_{ij} \mid Z=0\right]}{\mathbb{E}\left[S_{ij} \mid Z=1\right] - \mathbb{E}\left[S_{ij} \mid Z=0\right]} = \frac{(1-\alpha)\left[\eta A_0 + \theta P\left(\mathbf{A}_0\right)\right]}{(1-\alpha)} = \eta A_0 + \theta P\left(\mathbf{A}_0\right) \tag{8}$$

The result in equation (8) is based on the LATE assumptions that the instrument is independent of potential outcomes and that the exclusion restriction is satisfied. This is plausible given that the reform left the school system unchanged. An additional assumption which we need to impose whenever we consider outcomes measured in adult ages is that the reform had no effect on subsequent schooling decisions. This assumption is not necessary for the 2SLS estimates having a causal interpretation. However, our result that the reform increased human capital by $\eta A_0 + \theta P\left(\mathbf{A}_0\right)$ among compliers is dependent on the assumption that these spend only one additional year in school. Our analysis of spillover effects of the reform suggests that this assumption is plausible (see appendix).

Next, we consider the introduction of the 9-year reform. In this case we have

$$\begin{aligned}\beta_{9,2SLS} &= \frac{(1-\alpha)\left[\eta A_0 + \theta P\left(\mathbf{A}_0\right)\right] + 5\theta\left[P\left(\mathbf{A}_c\right) - \alpha P\left(\mathbf{A}_1\right) - (1-\alpha) P\left(\mathbf{A}_0\right)\right]}{(1-\alpha)} \\ &= \beta_{8,2SLS} + \frac{5\theta\left[P\left(\mathbf{A}_c\right) - \alpha P\left(\mathbf{A}_1\right) - (1-\alpha) P\left(\mathbf{A}_0\right)\right]}{(1-\alpha)}\end{aligned} \tag{9}$$

Hence, the estimated effect of the 9 year reform is the sum of a pure years-of-schooling effect $\beta_{8,2SLS}$ and a peer effect represented by the second term. Also here it is clear that peer effects of the ‘‘linear-in-means’’ type cancel out, which would lead to $\beta_{8,2SLS} = \beta_{9,2SLS}$.

Finally, we consider what happens when we may identify two distinct groups in the population who differ in their ability distributions. In particular, we consider what happens when there is a high-SES group with a larger proportion of individuals of high ability. Thus, we now need to distinguish the population-level proportion of high-ability individuals α from that in the high-SES (α_u) and low-SES (α_l) groups.

Under the maintained assumptions, $\beta_{8,2SLS}$ should not change. However, for the

9-year reform, we now get[7]

$$\beta^{g}_{9,2SLS} = \beta_{8,2SLS} + \frac{5\theta \left[P\left(\mathbf{A}_c\right) - \alpha_g P\left(\mathbf{A}_1\right) - \left(1-\alpha_g\right) P\left(\mathbf{A}_0\right)\right]}{\left(1-\alpha_g\right)} \tag{10}$$

where $g \in \{u, l\}$.

A comparison of equations (9) and (10) reveals that, even if $\beta_{8,2SLS} = \beta_{9,2SLS}$, so that peer effect cancel out in the aggregate, we may still learn about those peer effects by studying subpopulations. Whenever $P\left(\mathbf{A}_1\right) \neq P\left(\mathbf{A}_0\right)$ so that the peer composition makes a difference, we will have $\beta^{g}_{9,2SLS} \neq \beta_{8,2SLS}$ even in the case where $P\left(\mathbf{A}_c\right) = \alpha P\left(\mathbf{A}_1\right) + (1-\alpha) P\left(\mathbf{A}_0\right)$.

Finally, it should be noted that in equations (9) and (10), peer effects get scaled by the first-stage effect of the reforms on schooling (the denominator with $1-\alpha$). This makes a comparison across groups, with different first-stage effects difficult. Thus, as an unscaled measure of the peer effect, we suggest the following:

$$\delta^{g} = \left(\beta^{g}_{9,2SLS} - \beta^{g}_{8,2SLS}\right)\left(1-\alpha_g\right) \tag{11}$$

We will report estimates of δ^{g} in all regression tables below.

3.3 Model conclusions

Based on the simple theoretical model sketched above, we can draw the following conclusions for the empirical analysis:

- The 2SLS estimate $\beta_{8,2SLS}$ captures the effects of a pure extension of years of schooling.
- The difference $\beta_{9,2SLS} - \beta_{8,2SLS}$ is indicative of effects of the 9 year reform not operating via additional years of schooling: such as de-tracking and peer effects.
- If peer effects are predominantly of the ‘‘linear-in-means’’ variety, we expect $\beta_{9,2SLS} \approx \beta_{8,2SLS}$.
- A comparison of $\beta^{g}_{9,2SLS} - \beta^{g}_{8,2SLS}$ across groups with different ability mix will pick up peer effects even in the case of ‘‘linear-in-means’’ peer effects.

The exposition above is based on a number of assumptions, which might not hold in practice. First, we assume that peer effects θ are homogenous across ability and socioeconomic groups. A number of studies have challenged this assumption (Burke and Sass, 2013; Brodaty and Gurgand, 2016; Bertoni et al., 2017; Booij et al., 2017). However, even though allowing the peer effect parameter θ to be dependent on ability (θ^{a}) would complicate the exposition above, it would not change any of the predictions qualitatively; the same holds for a θ which is dependent on the pupil's SES background.

Second, we assume that in the absence of tracking, each pupil is exposed to an ability profile $\mathbf{A}_c$ which applies uniformly to all pupils. In practice, there will be systematic differences across areas, and random variation in the number of high-ability students sampled in each individual class. However, our empirical analysis is conducted

[7]It should be noted that the assumption that $\beta_{8,2SLS} = \beta^{u}_{8,2SLS} = \beta^{l}_{8,2SLS}$ can be relaxed in equation (10) without changing the result.

at the municipality level with controls for municipality and time fixed effects. Thus, most of the systematic differences in $\mathbf{A}_c$ across time and space are absorbed by these fixed effects.

Third, we assume that ceteris paribus, extending compulsory schooling from 7 to 8 years has the same impact on each individual as extending it from 8 to 9 years. This is potentially a strong assumption. However, it seems reasonably to assume that at least the labor market returns to pure compulsory schooling extensions were uniformly small in Sweden (Fischer et al., 2019b).

4 Data Description

To examine the impact of the two education reforms on health outcomes we employ both population-based administrative data and survey data. This section provides a general description of the data and their sources. Table 1 presents the summary statistics of the main variables.

4.1 Reform Data

To measure the 8 year reform we purposively gathered information on the extension of the old primary school from 7 to 8 years from the Swedish National Archives. For each year we digitized information on whether a school district had seven or eight years of compulsory primary school.

To measure the 9 year reform we rely on a dataset as used in Hjalmarsson et al. (2015), of which an earlier version is described in detail in Holmlund (2007). The dataset contains information on the year a specific municipality introduced the new comprehensive school system. The Online Appendix provides detailed information on the reforms, their data sets and their validation.

4.2 Individual Administrative Data

The administrative data is drawn from the Swedish Interdisciplinary Panel (SIP).[8] We consider the universe of those born in Sweden between 1932 and 1952, who survived to 16 years of age and the year 1970, and had not emigrated from Sweden by 2012.[9]

To identify individuals as exposed or unexposed to the reforms we assign treatment status based on year of birth and municipality of residence obtained from the 1960 and 1965 censuses.[10] Both reforms were rolled out in different parts at different times

[8]The SIP is based upon Statistics Sweden's Multiple Generation dataset to which all datasets are linked using personal identifiers. The SIP is administered at the Centre for Economic Demography, Lund University, Sweden, and approved by the *Lund University Regional Ethics Committee, DNR 2013/288*.

[9]Very limited intergenerational information is available for cohorts born before 1932 explaining our chosen starting point. Our education measure comes from the 1970 census and covers up to age 18 education explaining our chosen end point of 1952. Survival up to age 16 is chosen to ensure individuals can be impacted by the reforms. Survival to 1970 is chosen because this is when years of schooling is measured.

[10]For cohorts born between 1943 and 1948, we assume that place of residence in 1960 is also the municipality where they went to school. For cohorts born on or after 1949 we follow Holmlund (2007) and use the place of residence as recorded in the 1965 census. For individuals born before 1943 we use

within the cities of Stockholm, Gothenburg and Malmö and therefore we exclude those resident in these cities. From the original sample we also drop individuals without reform assignment and those without information on years of education. Figure 2 illustrates the sample size across reform years and the birth cohorts considered for each reform year. The figure also shows the number of municipalities and how the reforms overlap in time.

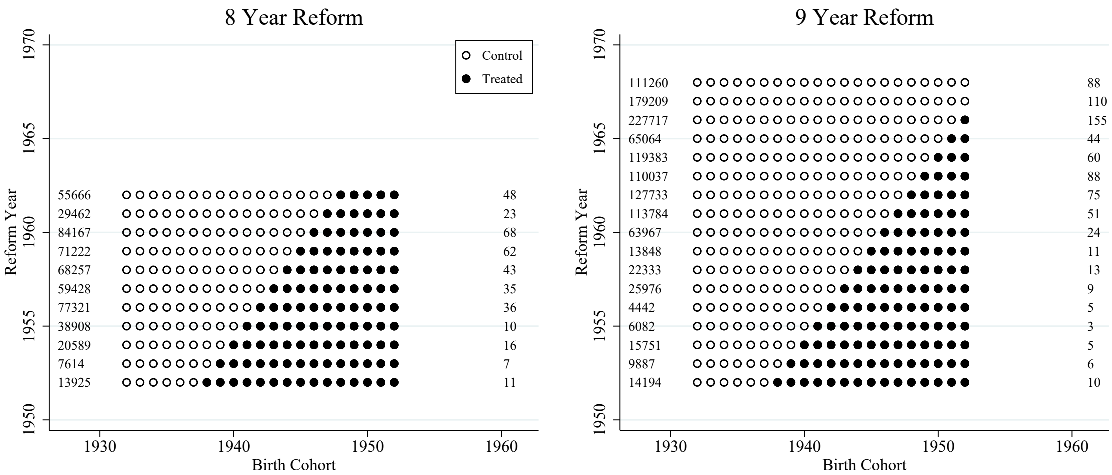


Fig. 2: Observational sample

Notes: This figure shows the sample size and number of municipalities by reform year for both the 8 year reform and 9 year reform. Total sample size is 1,501,844. The observations not listed in the figures are used as controls in our analysis.

From the administrative data we also get information on family SES, defined by father's occupation in 1960. High SES refers to fathers having white collar occupations, while low SES refers to fathers having any other occupation. Not all individuals have a father recorded in the 1960 census due to emigration or death and therefore our sample split by SES is smaller than our main sample.

4.2.1 Education

Data on schooling (primary, junior secondary and upper secondary school) is obtained from the 1970 census and combined with post schooling attainments (vocational training and tertiary education) from the Education administrative database. We derive a measure of years of education by assigning the years typically associated with different types of schooling and post-schooling qualifications and take the sum as an approximation for the total years of education.[11]

place of residence of the mother (father if information is missing for the mother and if both parents are missing we use own place of residence in 1960) as recorded in the 1960 census. For individuals without parents in the 1960 census we impute the place of residence during schooling using a cross-walk from the 1950 census. External validation of this imputation suggests it accurately identifies parish of residence in 96% of cases. Since each municipality consists of several parishes, the accuracy rate for municipalities should be even higher.

[11]Previous Swedish population administration data based studies have approximated years of education by the average length associated with the highest educational qualification (Hjalmarsson

4.2.2 Health Outcomes

Data on mortality comes from from the Swedish Cause of Death database. The data covers the years from 1964 and cause specific information is available from 1969 and runs up to 2013. We consider the whole observation period and measure the impact on death up to age 81 for the oldest cohort.

The underlying cause of death and hospitalization is recorded according to the 7^{th}, 8^{th}, 9^{th} and 10^{th} versions of the International Classification of Diseases (ICD) depending on year of death/admission. We consider the most common causes of death and hospital visits (cancer, circulatory diseases), specific diseases that are related to health behaviours and therefore potentially impacted by education and peer effects: obesity (diabetes), smoking (lung cancer) and alcohol abuse (liver disease). Panel A of table 1 presents the means of years of education and various health outcomes from our population based administrative data based outcomes variables, for the full sample and split by family SES. We observe that on average for our sample that individuals have 10.0 years of education, 17% have died by 2013 and predominantly of cancer. We also note a SES gradient in education and health outcomes with individuals with low SES on average being less educated by about two years and more often having died by 2013 as compared to the high SES group.

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et al., 2015; Lager and Torssander, 2012; Lundborg et al., 2014). We cannot use this approach for the 8 year reform because there is no information on whether an individual went to 7 year or 8 year primary school. These are clumped together in the same category in the variable capturing highest educational qualification. Importantly our method captures the impact of the 9 year reform more accurately than the traditional approach to measuring years of education as it distinguishes schooling and post schooling achievements (c.f. Fischer et al. (2019a)).

Table 1: Descriptive statistics

	Whole sample	*Low SES*	*High SES*
PANEL A: ADMINISTRATIVE DATA			
Years of Education	10.0	9.6	11.7
dead	0.169	0.161	0.125
Proportion dead due to:			
Cancer	0.063	0.062	0.051
Lung Cancer	0.010	0.010	0.008
Circulatory Disease	0.040	0.040	0.027
Diabetes	0.003	0.003	0.002
Liver Disease	0.003	0.003	0.002
N	1,501,844	1,133,343	276,853
PANEL B: SURVEY DATA			
Years of education	10.0	9.6	11.7
N	24,164	17,040	4,693
Fair or bad health	0.232	0.241	0.178
N	24,090	16,986	4,683
Smoke daily	0.237	0.239	0.221
N	23,931	16,869	4,665
Obese	0.110	0.115	0.087
N	14,037	9,931	2,705

Notes: This table shows the means for education and health outcomes for the whole sample and split by family SES based on father's occupation in 1960. High SES refers to individuals with fathers having a white collar occupations, while low SES refers to individuals with fathers having any other occupation.
Source: SIP and ULF-survey. Own calculations.

4.3 Individual Survey Data

The survey data stems from the Swedish survey on living standards (ULF) which is linked to administrative registers.[12] The survey is reported on a yearly basis 1980-2012 and includes self-reported health and health-related behaviour variables which we consider as valuable complements to our population based data. We use binary indicators for smoking behaviour, obesity, and self-reported fair or bad health (in contrast to good).[13] Panel B of table 1 presents the means of the survey data. The years of education correspond to those for the administrative data in Panel A, which suggests that the sampling frame of the survey data is representative.

[12] The ULF survey (Statistics Sweden, 2008) is a well respected survey used for a wide range of research and in recent years has formed the Swedish part of the European Union Statistics on Income and Living Conditions (EU-SILC). The survey is carried out by face-to-face interviews of a randomly selected sample of the population. The sample size is about 7,500 individuals per year.

[13] We define a binary variable *bad or fair health* equal to one if self-reported health is reported as fair or poor. Smoke Daily is a binary indicator, indicating one if smoked daily in the past 30 days prior to interview, zero otherwise. Obese is a binary indicator derived from information on height and weight creating a Body Mass Index (BMI), one indicating a BMI of 30 or more, zero otherwise.

5 Empirical Strategy

5.1 Identifying the impact of the reforms

The impacts of the reforms on years of education and health outcomes are modelled as linear functions, using either OLS or a Linear Probability Model (LPM) depending on the characteristics of the outcome variable. We identify the impact of the reforms using a DiD strategy. This exploits the fact that the education reforms were introduced at different points in time across municipalities in Sweden. Two individuals born in the same year, where one was resident in a municipality that had already implemented the reform and one in a muncipality had not yet implemented the same reform, have different exposures to compulsory schooling. This provides us with variation in reform exposure both over time and across municipalities. However, the implementation was not random as discussed in Holmlund (2007). To control for this we difference across municipalities and across birth cohorts by using dummy variables for both. As an additional control we include municipality specific trends. Our linear DiD-model takes the form:

$$H_{i,c,m} = \gamma_0 + \gamma_1 Z_{c,m} + \gamma_2 C + \gamma_3 M + \gamma_4 trend_m + \epsilon_{i,c,m}; \tag{12}$$

for individual i, birth cohort c and municipality m. $Z_{c,m}$ is an indicator variable if cohort and municipality is exposed to the school reform, C is a vector of birth year cohort dummies, M is a vector of municipality dummies, $trend$ is a vector of municipality specific trends and γ_0 is a constant term. The coefficient γ_1 measures the impact of the reforms on our outcome measures. We estimate equation (12) separately for each reform.

To obtain an estimate of the years of schooling effect that is comparable between the reform (even if the two reforms increased years of schooling by different amounts), we apply 2SLS. The first stage equals our linear equation (12) with years of education E in place of H as the dependent variable. The second stage is:

$$H_{i,c,m} = \beta_0 + \beta_1 \widehat{E}_{i,c,m} + \beta_2 C + \beta_3 M + \beta_4 trend_m + v_{i,c,m}; \tag{13}$$

where subscripts i, c, and m, and variables C and $trend$ are as for equation (12). $\widehat{E}$ is the prediction of years of schooling from the first stage. The coefficient on years of estimated education β_1 is identified by the variation in years of education that comes from the variation generated by the school reforms.

For our 2SLS coefficients to capture the causal effect of another year of education, we need to assume that the reforms affected health outcomes only via their effects on years of education. This exclusion restriction will be violated if the reforms had other impacts on students over and above their impact on years of education that then impacted on health. We argue that the 8 year reform is a a pure years of schooling reform because neither the structure of the education system nor the curriculum were altered during the implementation. In that case, the 8 year reform satisfies the exclusion restriction requirements and our 2SLS estimate has a causal interpretation.

In the conceptual model, we illustrate that the 2SLS estimate from the 9 year reform equals the sum of the schooling effect and any potential health effects of de-tracking.

This reform abolished the old school structure that tracked students from fourth/sixth grade and instead, students were kept in the same school and classes until the ninth grade.[14][15] There was also a small change to the curriculum as English was made compulsory. The removal of tracking by the 9 year reform led to a change in the peer composition, increasing the heterogeneity in the mix of ability and socio-economic status the students were exposed to. This potentially impacted health outcomes and health related behaviours as peer effects have been found to impact behaviours such as drinking, smoking, criminal behaviour and drug use in the short-term (see for e.g. Sacerdote et al. (2011) for an overview on peer effects). Such effects would violate the exclusion restriction, invalidating the 9 year reform as an instrument for years of schooling.

We estimate 2SLS model equation (13) separately for the two reforms, for both the total population and subgroups based on the SES background (father's socioeconomic status). Guided by the conceptual model, we obtain a rough estimate of the peer effects, δ, by subtracting the 8 year reform β_1 from the 9 year reform β_1, and multiply the difference by the coefficient γ_1 from the first stage of the 9 year reform (see equation (11)). We obtain such estimates for the total population, as well as for the subgroups with high and low SES background.

To perform a statistical test of the peer effects, we also estimate a combined version of equation (13) where both reforms are included as instruments for years of education. By combining the reforms we can perform a Sargan-Hansen type of over identification test testing the null hypothesis that both reforms are exogenous (Sargan, 1958; Baum et al., 2003). A rejection of this test will strongly suggest that one of the reforms fails the exclusion restriction. Under the assumption that the exposure to the 8 year reform is a valid instrument, we can use this as a statistical test of whether the removal of tracking and peer composition effects as a consequence of the 9 year reform had independent effects on health.

In summary, we are able for the first time to provide credible evidence from Sweden of the causal impact of *years* of education on health using the 8 year reform as an IV. In addition, by comparing the two reforms using the guidance of our conceptual model we can also assess whether the 9 year reform had wider effects on health other than through just years of education.

5.2 First stage results and diagnostic tests

Our identification strategy builds upon our method of treatment status assignment performing well. In addition to this and the exclusion restriction discussed above, our 2SLS estimates require our education reform impacts on years of education to be as good as random given our control strategy. In this section we establish the existence of a first stage and provide some diagnostic tests to assess the plausibility of our identification assumptions.

Figure 3 presents one event study graph for each reform. These graphs depict the

[14] There were some exceptions, where tracking was maintained within the same physical school for some subjects in the final 9^{th} grade but overall students were much more mixed.

[15] Normally peer group affects are a concern as they lead to violation of the Stable Unit Treatment Value Assumption. However, as our analysis is at the municipality level our DiD results are internally valid for the combined impact of the years of schooling and peer group composition effects.

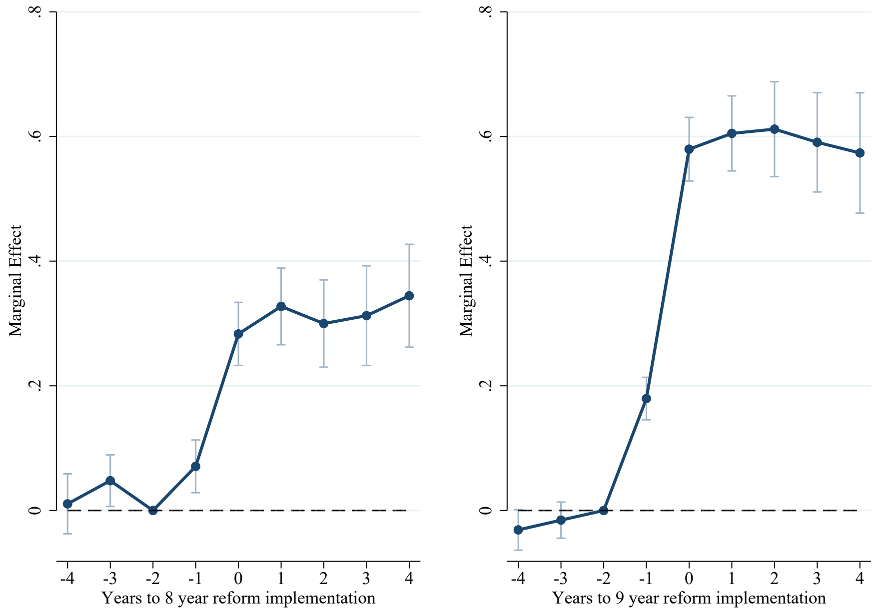

Fig. 3: Effect of the reforms on years of education

Notes: Event study scatter plots of mean years of education by birth cohort measured as difference in years of age from the first cohort impacted by the reform in their municipality. Reference cohort is t-2. Left panel is for the 8 year reform, right panel the 9 year reform. Reform implementation is at time zero. *Source:* SIP. Own calculations.

difference in average years of schooling by birth cohort relative to the birth cohort born two years prior the first cohort impacted by the reform in their municipality. The graphs show a jump in the average years of education at the reform year for both reforms, and that this jump is larger for the 9 year reform. This is expected because the 9 year reform increased years of schooling by two years for students who were in municipalities offering only 7 years of primary school and one year for those offering 8 years, whereas the 8 year reform was just a single year increase for all municipalities affected.

Note that we also see a jump in years of schooling in period t-1 and this is also much clearer for the 9 year reform. Hjalmarsson et al. (2015) suggest that the pre-reform increase in schooling is due to either measurement error in the exposure variable or due to pupils being in the wrong grade based on their age. They cite evidence that grade repetition was not a common occurrence for those in the old 7 year primary school system but was for those who were tracked into the junior secondary school. Those at junior secondary school who were born a year too early but had dropped out would have normally gone back to old primary school, but because of the reform they would have instead been caught by the 9 year school reform and would as a consequence be a year older than their peers in the same class. This story fits with what we see in the data: A small jump in t-1 for the 8 year reform where grade repetition was not very common in the old primary school system and a larger jump in t-1 for the 9 year reform where grade repetition at junior secondary school was more common. Accepting that this type of measurement error is causing the jump one year prior to the reforms, implies that the jump should be considered as a part of the reform effect. Notably, our first stage regression would in that case underestimate the effect of the reform on years of education.[16]

Table 2 presents the regression results of the impact of the reforms on years of education for all individuals. In column (1) of table 2 we present the results for the 8 year reform on years of education for all individuals and find an increase of 0.25 years of education. In column (4) we present the results for the 9 year reform for all individuals and we find an impact of 0.52 years. Note that the 9 year reform estimates presented here are much larger than previously documented (see e.g. Holmlund 2007; Lundborg et al. 2014; Meghir et al. 2018). This follows from the fact that our measure of years of education is different from what has previously been used, and this new measure better captures the impact of increased compulsory schooling on years of education (see Fischer et al., 2019a, for a detailed discussion).

Table 2: Compulsory schooling reforms' impact on education

	(1)	(2)	(3)	(4)	(5)	(6)
	8 year reform			9 year reform		
	All	Low-ses	High-ses	All	Low-ses	High-ses
Coefficient	0.248***	0.265***	0.111***	0.518***	0.594***	0.268***
	(0.020)	(0.021)	(0.035)	(0.024)	(0.024)	(0.025)
F-stat	149.97	162.39	10.23	455.93	615.84	112.18
N	1,501,844	1,133,343	276,853	1,501,844	1,133,343	276,853

Notes: This table shows the impact of the 8 year and 9 year school reforms on years of education from a DiD regression controlling for cohort and municipality fixed effects gender and municipality trends. SES is defined as whether father has a white collar job in 1960 (high SES). Robust standard errors clustered at the municipality level are in parentheses. Testing the null of the coefficient: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$
Source: SIP. Own calculations.

The results split by socio-economic background show the 9 year reform had a larger impact on years of education compared to the 8 year reform for both high- and low-SES individuals. These results also show that the impact for low-SES individuals is larger for both reforms, but in particular for the 8 year reform. The increase of mandatory schooling for the 8 year reform should only impact individuals in the basic track, which includes a larger share of low SES individuals compared to the academic track. The F-statistic results suggest that we have a strong first stage across the board, based on the the rule of thumb for weak instruments of an F-statistic above 10 (Stock et al.,

[16] In the appendix we also show figures similar to figure 3 but for the proportion with just 7 years of schooling, just 8 years of schooling and just 9 years of schooling. These all confirm the jump at the cut-off. They also confirm the pre-reform jump in t-1. The jump in t-1 for the 9 year reform coincides with a clear drop in 8 years of old primary school in t-1 suggesting it is driven by individuals dropping out of junior secondary school after one year in municipalities who had introduced the 8 year reform. We can also see that there is measurement error in the exposure variable and our treatment assignment strategy as after the 8 year reform there are still some with 7 years schooling. Similarly for the 9 year reform there is still a proportion with old primary school after reform exposure. This is partly explained by partial implementation in the municipality where exposure is given as 1 if just part of the municipality enacted the reform.

2002).[17]

We have shown that the reforms coincide with substantial increases in years of education and therefore that our method of reform assignment is working well. In addition to a strong first stage regression, our 2SLS estimates require exposure to reform to be as good as random, conditional on our control strategies. This may be violated if selective migration to and from reform municipalities occurred, either to escape or gain access to the reform. In previous work assessing the 9 year reform, both Meghir and Palme (2005) and Holmlund (2007) have tested for selective migration and have found that it was not a problem. Similarly, Fischer et al. (2019b) show that the previous 7-year reform had no impact on migration either. We are not able to test it for the 8 year reform but make the assumption that the results of these two other reforms apply to the 8 year reform as well. We view this as a plausible assumption given that the 8 year reform affected similar cohorts but was just a pure years of schooling change and would have provided much less of a reason to move compared to the comprehensive 9 year school reform.

Table 3: Diagnostics: balancing test for differences in father's predetermined characteristics by reform status

	(1)	(2)	(3)	(4)
	8 YEAR REFORM		9 YEAR REFORM	
	OLS	DID	OLS	DID
Years of Education	0.406***	-0.004	0.216***	-0.024
	(0.037)	(0.018)	(0.050)	(0.019)
No occupation	-0.006***	0.001	0.002	0.000
	(0.001)	(0.001)	(0.001)	(0.001)
Agricultural	-0.140***	-0.002	-0.068***	0.000
	(0.012)	(0.002)	(0.013)	(0.002)
Blue collar worker	0.040***	0.003	0.017**	-0.001
	(0.007)	(0.003)	(0.007)	(0.002)
White collar worker	0.100***	0.001	0.042***	0.001
	(0.010)	(0.002)	(0.011)	(0.002)

Notes: This table shows impact of reform status on various predetermined characteristics. Columns (1) and (4) are simple associations controlling for year of birth. Columns (2) and (4) are estimates from a DiD regression controlling for cohort and municipality fixed effects gender and municipality trends. Robust standard errors clustered at the municipality level are shown in parentheses. Testing the null of the coefficient: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$
Source: SIP. Own calculations.

To assess our exclusion restriction assumption we perform a batch of balancing tests of predetermined characteristics and reform assignment in table 3. The results show that when we control only for birth cohort fixed effects (columns 1 and 3) our predetermined characteristics are predicted by reform status. The correlations also go the way we might expect: the reforms were introduced earlier in areas where parents were more educated and had better jobs. In columns (2) and (4) we use our DiD specification to identify the impact of the reforms. We find that the size of the coefficients tend towards zero and become insignificant. Whilst this evidence is just

[17] In the appendix we provide the same table as 2 but calculated using the survey data and find similar coefficient estimates.

indicative that our reforms are not correlated with the error terms after applying our identification strategy, they provide credibility to our strategies. In sum, our assignment method works as implied, and there are also evidence supporting our claim that reform exposure in our DiD strategy is as good as random.

6 Results

6.1 Main results

This section presents our main analysis of the impact of both school reforms on health. In table 4 we present the reduced-form DiD regression estimates of the impact of both reforms (rows 1 and 2) and the 2SLS estimates using each reform separately and combined (rows 3 to 5). Columns (1) to (4) present the results from these separate regressions for mortality, fair or bad health, smoking and obesity respectively.

The reduced-form estimates of the 8 year reform by and large indicate a statistically significant health improving impact, except for smoking. The impact on mortality is -0.3 percentage points, while the impact on both fair/bad health and obesity is -2.9 percentage points. The corresponding reduced form estimates for the 9 year reform are smaller, of opposite sign, and insignificant. As the 2SLS estimates can be interpret as the reduced form estimates weighted by the change in years of schooling from the first stage regression in table 2, these allows us to compare the effect of the two reforms in a standardised way. As a consequence of the difference in magnitude of the first stage estimates, the difference between the 2SLS estimates of the two reforms are much larger than the difference in the reduced form estimates. The 8 year reform based 2SLS estimates indicate that an additional year of schooling reduces mortality by 0.1 percentage points, fair/bad health by 9 percentage points and obesity by 9 percentage points. The corresponding 2SLS estimates for the 9 year reform are all much smaller in magnitude (about 1/2 to 1/4), of opposite sign and insignificant. The impact on smoking is positive but small and statistical insignificant for both reforms.

Table 4 also presents our rough estimates of the (unscaled) de-tracking and peer effects on health, δ, which are computed as the difference between the 2SLS-estimates of the two reforms multiplied by the first stage of the 9 year reform (see column 4 table 2). These estimates indicate positive effects on the aggregate level for mortality, self reported bad/fair health, and obesity. In magnitude, the de-tracking and peer group component of the 9 year reform dampened the health improvements by 2/3 of the effect of one additional year of schooling (i.e. the 2SLS-estimates of the 8 year reform).

To statistically test the hypothesis of an effect of the de-tracking and peers on the aggregated level we include both reforms as instruments in the same 2SLS regression. *Overid* presents the p-value from the Hansen J-statistic testing for overidentification. We interpret this as a test for the validity of the 9 year reform as an instrument. A rejection of the null implies the exclusion restriction is unlikely to hold, suggesting that peer and de-tracking effects are different from zero —on the aggregate level. We find such evidence for mortality, fair/bad health and obesity but not for smoking. This suggests that the overall impact of de-tracking and peer group composition changes on long-term health is in fact significant and has worked against the health improving increase in years of schooling element of the 9 year school reform.

Table 4: 2SLS estimates of education effects on long-term health outcomes

	Mortality (1)	Fair/bad health (2)	Smoking (3)	Obesity (4)
WHOLE SAMPLE				
RF 8yr reform impact)	-0.003*	-0.029**	0.002	-0.032*
	(0.002)	(0.014)	(0.017)	(0.018)
RF 9yr reform impact	0.002	0.017	0.001	0.011
	(0.001)	(0.014)	(0.015)	(0.015)
2SLS (8yr)	-0.012*	-0.085*	0.007	-0.089
	(0.007)	(0.048)	(0.051)	(0.058)
2SLS (9yr)	0.003	0.033	0.002	0.021
	(0.003)	(0.029)	(0.029)	(0.029)
δ	0.008	0.061	-0.003	0.057
Overid	0.052	0.020	0.935	0.051
Mean	0.169	0.232	0.237	0.110
N	1,501,844	24,089	23,930	14,027

Notes: This table presents the impact of the reforms and years of education on mortality (column 1), and self-reported health and health behaviours (columns 2-4). The sample is cohorts born 1932-1952. All coefficients are from separate regressions (RF=Reduced form; 2SLS=Two Stage Least Square). 2SLS estimates include the relevant reform as an excluded instrument and are based on DiD with cohort, municipality fixed effects and municipality specific trends. "Overid" is the p-value from a Hansen J-statistic testing the validity of the instruments when both are used simultaneously as instruments for years of schooling in the same regression (Baum et al., 2003). δ is our measure of peer effects calculated as in equation 11. Standard errors clustered at the municipality level in parenthesis. Testing the null hypothesis: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.
Source: SIP (Mortality) and ULF-Survey (all other outcomes). Own calculations.

6.2 Cause specific results

The 2SLS results for mortality by cause are presented in figure 4. Causes considered are cancer, circulatory diseases, diabetes, lung cancer and liver disease. Cancer and circulatory diseases are the most common causes of death for the cohorts we consider and diabetes, lung cancer and liver disease are particularly amenable to changes in health related behaviours - a key channel education and peer group changes could influence. Cancer, especially lung cancer and circulatory diseases are strongly predicted by smoking. Liver disease and diabetes are very strongly linked to heavy alcohol consumption and poor diet/lack of exercise respectively, behaviours linked to short-term peer group effects (Sacerdote et al., 2011). The mortality impacts by cause are all statistically insignificant with the exception of circulatory diseases which appears to be the driver behind the reduction in mortality caused by the 8 year reform. The 9 year reform 2SLS estimates are all very small. The differences in estimated effects between the reforms support the interpretation of a health penalty from de-tracking due to the 9 year reform. Note also that the difference of the effects for lung cancer between the reforms goes in same direction as for smoking in table 4 and the significant impact of the 8 year reform on obesity corresponds to a marginally significant reduction of diabetes related mortality.

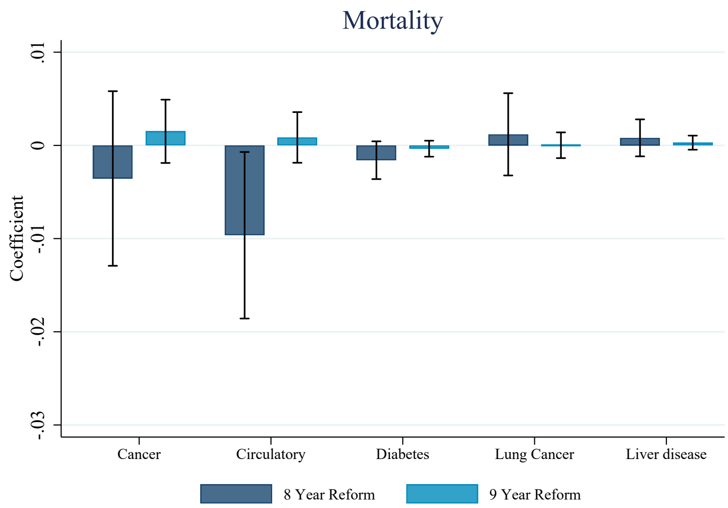

Fig. 4: Impact of the reforms on cause specific mortality and hospitalisations by 2013

Notes: This figure plots the 2SLS coefficient of years of education on mortality by main diagnosis. Lung cancer is included in cancer. 95% confidence intervals are represented by the capped vertical lines.
Source: SIP. Own calculations.

A number of sensitivity checks have been performed. We have seen that our specification is robust to inclusion of lags and leads in the event study figure for years of education (see figure 3) and that reform status is not associated with strong pre-determined predictors of treatment when using our DiD strategy. In the appendix we present a table B.3 summarising the main health 2SLS results estimated under different DiD specifications and find that the coefficients are stable to these choices.

6.3 Heterogeneity analysis

In this section we consider heterogeneity in health effects of the reforms by family SES. Specifically we stratify our analysis by occupation of the father in 1960 (white collar job vs. non-white collar job). These groups likely include different shares of individuals who were directly affected by the change in the mandatory schooling induced by the reforms (i.e. in terms of the conceptual model, the share of high ability types, α, is likely to be larger in the high-SES group). This is confirmed by the heterogeneous effect of an increase in the years of schooling. As spelled out in the conceptual model these groups may also react heterogeneously to the changes in the peer composition induced by the de-tracking.

In table 5, we see that the reduced form and 2SLS estimates of the 8 year reform for the low SES group are all of similar sign as the aggregate effect but insignificant and smaller in magnitude. The corresponding estimates for the high SES-group on mortality and self-reported fair/bad health is much larger in magnitude, although only the effect on mortality is significant (partly explained by the smaller sample size for the survey data). Note that the large difference between the reduced form and the 2SLS is due to the small share of compliers in this group. The estimates of the effect of an additional year of education on smoking and obesity for the high SES group go in the opposite direction to the effects for the low SES group, but are imprecisely measured.

Turning to peer effects, as illustrated by our conceptual model it is possible that the effect of de-tracking is small on the aggregate level while significant at the group level. Students previously selecting into one of the two tracks are now exposed to students previously selecting into the other track, which may cause opposite effects of de-tracking in the two groups. Our whole sample analysis of the effects of de-tracking may therefore be missing important insights about the effect of changes in peer group composition.

For the general health measures, de-tracking appears to affect mortality and fair/bad health negatively for both high and low SES individuals. The overid test provides support for peer effects on mortality in the high-SES group, and self-reported health in the low-SES group. For smoking, the value of δ indicates that de-tracking —or larger exposure to the other SES —increased smoking within the high SES group and reduced smoking within the low SES group. For obesity, δ indicates a reverse pattern with health worsening effects in the low SES-group and health improving effects in the high SES-group. The overid test does however only gives support to existence of an adverse peer effect for obesity amongst the low SES group.

2SLS results by cause of death split by SES presented in figure 5 show that the significant reduction in mortality amongst the high SES group due to the 8 year reform comes from a significant reduction in deaths due to circulatory diseases. For the low SES group, whilst no clear overall impact of the reforms were found on mortality, the 8 year reform reduced deaths due to diabetes (significant at the 10% level) which fits with the observed drop in obesity amongst this group.

Table 5: 2SLS estimates of education effects on long-term health outcomes, by SES

	Mortality (1)	Fair/bad health (2)	Smoking (3)	Obesity (4)
Low father SES				
RF 8yr reform impact	-0.001	-0.021	0.020	-0.037
	(0.002)	(0.019)	(0.021)	(0.023)
RF 9yr reform impact	-0.000	0.027	0.004	0.014
	(0.002)	(0.018)	(0.020)	(0.021)
2SLS (8yr)	-0.006	-0.056	0.054	-0.071
	(0.008)	(0.055)	(0.060)	(0.044)
2SLS (9yr)	-0.000	0.047	0.008	0.024
	(0.003)	(0.034)	(0.035)	(0.035)
δ	0.004	0.063	-0.042	0.072
Overid	0.547	0.078	0.476	0.070
Mean	0.161	0.241	0.239	0.115
N	1,133,343	16,982	16,865	9,903
High father SES				
RF 8yr reform impact	-0.010***	-0.022	-0.054	0.037
	(0.004)	(0.040)	(0.050)	(0.047)
RF 9yr reform impact	0.004	-0.013	-0.029	0.019
	(0.003)	(0.032)	(0.034)	(0.037)
2SLS (8yr)	-0.091**	-0.142	-0.268	0.644
	(0.042)	(0.377)	(0.473)	(6.298)
2SLS (9yr)	0.015	-0.071	-0.131	0.048
	(0.010)	(0.189)	(0.189)	(0.103)
δ	0.028	0.019	0.037	-0.160
Overid	0.003	0.837	0.731	0.472
Mean	0.125	0.177	0.221	0.089
N	276,853	4,462	4,443	2,460

Notes: This table presents the impact of the reforms and years of education on mortality (column 1), and self-reported health and health behaviours (columns 2-4). The sample is cohorts born 1932-1952. All coefficients are from separate regressions (RF=Reduced form; 2SLS=Two Stage Least Square). 2SLS estimates include the relevant reform as an excluded instrument and are based on DiD with cohort, municipality fixed effects and municipality specific trends. "Overid" is the p-value from a Hansen J-statistic testing the validity of the instruments when both are used simultaneously as instruments for years of schooling in the same regression (Baum et al., 2003). δ is our measure of peer effects calculated as in equation 11. Standard errors clustered at the municipality level in parenthesis. Testing the null hypothesis: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.
Source: SIP (Mortality) and ULF-Survey (all other outcomes). Own calculations.

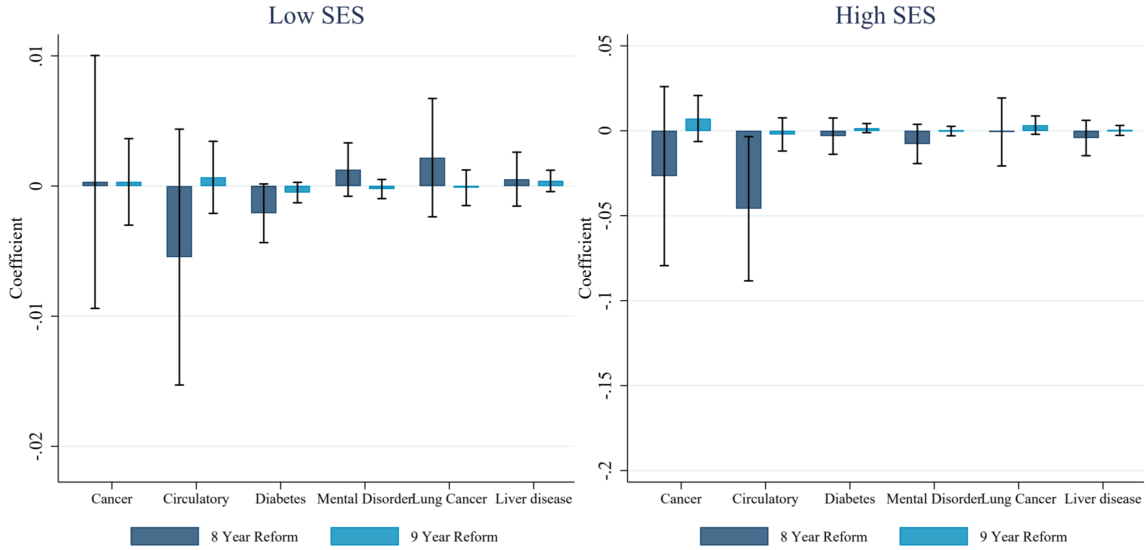

Fig. 5: Impact of the reforms on cause specific mortality by SES

Notes: This figure plots the 2SLS coefficient of years of education on mortality by main diagnosis. Low SES left panel, high SES right panel. Lung cancer is included in cancer. 95% confidence intervals are represented by the capped vertical lines.
Source: SIP. Own calculations.

7 Discussion

We have considered two major and parallel school reforms and used a unique quasi-experimental set-up that allows us to tease apart years of schooling effects and compare and contrast these to tracking and changes in peer group composition on health outcomes and health behaviours. Our analysis generates two main insights, both of high policy relevance.

First, the empirical analysis shows that there was a positive long-run health effect from the 8 year reform. Using the reform as an instrument in a two-stage least square regression, the results suggest that longevity increased following the quantitative change in education, and there are indications of health gains also when using more sensitive health measures like self-assessed health and obesity. As discussed above, the large literature using education policy reforms to isolate the causal health effects of education produce mixed evidence (Cutler and Lleras-Muney, 2012; Galama et al., 2018). However, it is rare to have an education reform that only introduce one extra year of schooling without changing other components that could affect the production and protection of health. The significance in our finding lies in the fact that we have a pure year extension (and that reform assignment is this a credible instrument for years of schooling), but also that the reform was not introduced overnight.

The existing research on the health effects of more edcuation that most closely aligns to that of ours is the population based research of Clark and Royer (2013) for Britain and that of Van Kippersluis et al. (2011) for the Netherlands. Our finding of a marginally significant reduction in mortality by 1.2 percentage points is smaller than the 2.5 percentage point decrease in mortality observed from the Dutch reform,[18] but

[18]Note that Van Kippersluis et al. (2011) observe mortality between 81 to 87 years of age, conditional

larger than the potential reduction recorded after the British reforms. Clark and Royer (2013) rule out a mortality reducing effect larger than 1% from an additional year of schooling. For both studies there may be concerns that the reforms were implemented overnight nationwide. There may have been large general equilibrium effects of these overnight nationwide reforms that potentially reduce their earnings effect; overnight national implementation may also have quality impacts on education due to resource limitations and therefore may not be capturing only the increased years of schooling impact. The cohorts impacted of these three reforms (1927 in the Netherlands, and 1947 and 1972 in Britain) are likely to have been very different, both in terms of their own characteristics but also in terms of the health and labour market structures they were exposed to. The cohorts affected by the Dutch reform and the 1947 UK reform were impacted by the World Wars which may influence the analysis and affect external validity.[19]

The second insight our analysis provides is about the role of tracking and peers to health. Several articles have examined the 9 year reform under study in this paper (Meghir et al. 2018; Palme and Simeonova 2015; Lager and Torssander 2012; Lager et al. 2016; Spasojevic 2010) but the different elements of schooling implied by the reform have previously not been possible to distinguish from each other.[20]Analysing the pure 8 year reform alongside the 9 year reform we show that de-tracking and the greater mix of peers if anything dampened the positive health impact of more years of schooling.

The work most closely aligned to our results on this point are the results from the school reform evidence literature of tracking. Basu et al. (2018) found a fraction of individuals' health and health related behaviours were negatively impacted by the removal of tracking. However, because their paper had a greater emphasis on assessing heterogeneous effects their results rely on a weaker and less convincing identification strategy compared to ours. Also, the work by Bockerman et al. (2018) and Ravesteijn et al. (2017) on the removal of tracking in Finland suggest worsened mental health and increased mortality for some subgroups.

Notably, our results suggest—at least for the more general health measures—that de-tracking and a broader set of peers have an adverse effect on health for both high and low SES groups. These effects need not represent peer effects, but it is still of interest to consider with which models of peer effects they are compatible. In his review of the literature, Sacerdote et al. (2011) lists eight different models of peer effects. Of these, our empirical results are inconistent with six. The already-mentioned ‘‘linear-in-means’’ model would predict that the refom had no noteworthy effects in the

on survival to 80, which makes the magnitudes difficult to compare

[19]The cohorts studied in this paper were born across the span 1938-1954. Like the cohorts exposed to the school reform 1947 in Clark and Royer (2013), some of the cohorts in our study were impacted by the Second World War (WWII). Using Sweden as our study case has the advantage that, although WWII was an unusual period, Sweden was a neutral part and as such much less affected than other countries. There are also historical sources supporting that the war did not cause any educational disruptions(see Bhalotra et al. 2016 on that it schools could cancel classes in case of a threat during the WWII, but any time lost had to be caught up later on and that a substitute teacher had to be called in if a teacher was called for military service), and there were no policies to move children out of the city to extended family or volunteers on the countryside, as in Britain. We argue therefore that our results should not be specific to the cohorts we consider, but instead being of relevance beyond the study period.

[20]Our reduced form results of the 9 year reform are very similar to Meghir et al. (2018).

aggregate, and that the low-SES group benefits from the new peer group composition. The ‘‘shining light’’ model predicts that all students benefit from having high-ability students around, but in our case no students benefit. According to the ‘‘invidious comparison’’ model, outcomes are harmed by the presence of better-achieving peers. This appears to indeed have happened in the low-SES group, but it is inconsistent with the adverse effects in the high-SES group. The ‘‘rainbow model’’ suggesting that heterogeneity is good for everyone is obviously inconsistent with our results, as well as the ‘‘single-crossing’’ model which would suggest that low-SES students benefit. Finally, the results are inconsistent with the ‘‘bad apple’’ model, which suggests that one disruptive student harms everyone -- since we would expect low-SES students to be exposed to fewer bad apples in the de-tracked system.

Conversely, our results are consistent with the ‘‘botique’’ and ‘‘focus’’ models, according to which classroom heterogeneity is harmful to everyone. Thus, if we are willing to accept that our estimates for the effects of de-tracking (δ) represent peer effects, then the results narrow down the possibilites for how peer effects operate quite substantially. Also, they are at odds with the interpretation provided by Bockerman et al. (2018) who argue that the removal of tracking affects the ability ranking in the class room, with low-ability students that did not take on academic tracking in the old system having a lower rank in the new system. This interpretation is well in line with Cicala et al. (2017) and Elsner and Isphording (2018) who report that the ordinal rank among peers affect academic achievement but also problem behaviour and engagement in risky behaviour. However, it is difficult to reconcile a story based on rank with high-SES students suffering health penalties when tracking is removed.

It has often been suggested that increasing the mix of high-ability and high-SES students with lower ability and lower SES students could help reduce inequalities in a number of long-term outcomes, yet the evidence from these natural experiments on health finds no evidence to support this. Our finding is novel and suggests that changes in primary school peers based on their SES background may have long-lasting impact on health behavior or actual health. Importantly, these results do not rule out positive effects of de-tracking -- either in terms of efficiency or improved equality of opportunity -- but it is difficult to argue that abolishment of de-tracking is instrumental in order to achieve health improvements.

The analysis included in this paper has considered two Swedish school reforms that were rolled out during the same time period ensuring that the cohorts exposed to the two reforms later acted under similar welfare and labour market institutions. The way the reforms were rolled out also means that concerns about resource shocks such as lack of teachers and schools and even general equilibrium effects do not apply. The clean comparisons of the two reforms and the large and detailed data are key strengths of our approach.

8 Conclusion

The literature documenting an education gradient in health is vast yet the causal effect literature using compulsory school reforms as instruments for years of education has produced varied results. Compulsory school reforms is indeed a powerful way to assess the causal impact of education on health. They often impact a large population and is often directly targeting sub-populations of specific public health interest, the low educated who live on average shorter lives and have poorer health outcomes generally. More importantly, these reforms can provide exogenous variation of exposure to education, and under certain certain parametric assumptions also variation in years of education. However, very few of these reforms actually satisfies these assumptions as the implementation mix increases in mandatory schooling with changes in school structure.

Our experimental setting provides us with one reform that only changes the quantity aspects of education, and one reform with a similar change in quantity accompanied by changes in quality aspects (de-tracking and peer exposure). Combining this set up with detailed data from surveys and registers, we are able to provide important—and policy relevant—insights on both quantity and quality aspects of education. Our findings suggest that the increasing quantity of education improves both objective and subjective health outcomes, but such health improvements may be undone by increased heterogeneity in the classroom.

Even though our study goes a long way toward unpacking the health effects of comprehensive schooling reforms, we are unable to give a detailed answer as to the mechanisms giving rise to adverse health effects from de-tracking. We are able to show that our results are inconsistent with a majority of popular approaches to model peer effects; however, it clearly has to be left as a topic for future research to determine whether long-term health penalties from such reforms arise from peer interactions or from other aspects of the learning situation.

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Appendix A Background to the Swedish school reforms

This appendix provides background information on the Swedish school system, the two school reforms used in this paper and their interrelation.

A.1 The Swedish School System

Long before compulsory schooling was introduced by law on a nationwide level in Sweden, a large fraction of the population had basic reading and writing capabilities, and a notable share of all parishes had introduced some kind of primary school on a voluntary basis (Lindmark, 2015). Regulations announced in the mid to late 19th century came to imply both the right to cost-free primary schooling and an obligation to take part in the schooling offered (Fredriksson, 1971). Specifically the 1882 legal statue of *Folkskolan* stated that every parish had to offer primary schooling by an approved teacher, that school attendance was compulsory for all children, and that children should start primary school the year they turn seven years old (Edgren, 2011).

The country was divided into school districts (generally corresponding to a parish, and later a municipality) and the local school board was responsible for the organization of elementary education. To overcome differences in content and format across school districts, a national central education plan was introduced in 1919 (Paulsson, 1946). These guidelines were published by the Ministry of Ecclesiastical Affairs and included time tables and syllabuses for compulsory schooling. The ministry also appointed school inspectors responsible for yearly evaluations of a number of school districts (Fredriksson, 1971). Completion rates were high and more than 90 per cent of all pupils finished compulsory schooling with full curriculum (Fredriksson, 1950).

In the 1920s elementary schooling in Sweden was compulsory for six years, but the central education plan provided curricula also for seven years of schooling and in 1920 a clause was introduced in the primary school code (paragraph 47 mom. 4) that stated a seventh school year could be made compulsory in a school district (Fredriksson, 1950). At the time Sweden applied a tracking system, where good performing students (defined by an assessment) could select to switch to an academic educational track and study at a four or three year long junior secondary school (*Realskola*) after the fourth or the sixth year of elementary education, respectively. The alternative was to continue and finish basic compulsory schooling. Attending junior secondary school allowed students to continue to higher secondary school (*Gymnasium*) which was a prerequisite for University. During the first half of the twentieth century, the Swedish school system was highly selective and the vast majority of people only completed compulsory education Björklund et al. (2004).[21]

In 1936 the national Government decided that a seventh year of schooling should be

[21]See e.g.Sweden (1977) for yearly numbers of students matriculating to lower and higher secondary schooling. For the cohorts of interest in this study matriculation to lower secondary schooling increased over time (16 percent of cohort 1938 and 30 percent of cohort 1951).

compulsory and seven years of schooling had to be implemented across the whole country over a twelve-year period, before 1949 (Fischer et al., 2013). The reform was seen as a new epoch, especially among teachers, because previously Folkskola had remained the same since 1882, offering six years of compulsory education (Folkskollärarförbund, 1949).[22] With the bill of 1936, school districts were also allowed to introduce an eight year of compulsory schooling, but for this they had to send in a formal application and to be given the king's consent.

A.2 The 8 Year Reform

With the start of the World War II, the Swedish political debate came to place a large focus on how to best foster democratic members of society. More education was seen as one of the main components for fulfilling this goal (Edgren, 2011). Thus, despite the on-going national implementation of the seventh year of compulsory schooling, a reform work was initiated and assigned to a new expert commission (*Skolutredningen* later replaced by *Skolkommissionen*) in 1940. This was the first governmental commission with a real mission to investigate primary and secondary education together, and with an aim to replace the tracking system with a unified comprehensive school system tying compulsory schooling closer to secondary education.[23]

Between 1940-1948 the commission continuously released reports evaluating the current school system and developing proposals and guiding principles for the future compulsory school (Marklund, 1982). Although the main focus of the commission's work was to postpone tracking decisions to higher grades, and by that improve equality of opportunity, and despite the on-going implementation of the seven-year compulsory schooling, there were also continued efforts to further extend compulsory schooling within the old system. The commission's work discussed an eight year extension of *Folkskola*, and in 1945 the Minister of Ecclesiastical Affairs proposed a bill introducing compulsory eight year schooling (without changing tracking options), but no action was taken by the Government (Fredriksson, 1971).

As stated above one of the main arguments for extending compulsory education was *democratic fostering*. This motive was not new. The 7 year reform was also motivated by fostering democracy including universal suffrage which was argued to place great demands on members of society, wherefore a solid education is necessary (Ecklesiastikdepartementet, 1935). The on-going war made this argument even more important in the debate. Specifically an eight-year extension was believed to improve student performance with respect to elementary skills in reading, writing and math, but also other subjects. An extension would also allow for the introduction of foreign languages (English) as a subject. In addition to theoretical arguments an extension was further justified by social and ethical arguments and that an eight-year could fill a

[22] For a detailed review of the background and the implementation of the seventh compulsory year, see Fischer et al. (2013).

[23] Since the 1890's there had been a quite heated debate about the rational of the the so-called parallel system where student took different tracks. The main argument in the debate for that all students should have to complete the very same basic education before continuing to secondary schooling, was that it created inequalities (Morawski, 2010).

supportive and nurturing role for young people that have not established on the labour market (SOU, 1945).

A second argument for extending compulsory education was induced by international *benchmarking* –that Sweden was lagging behind (Waldow, 2013). Compared to other countries few students matriculated to higher levels of education, and the time spent in compulsory education was still quite modest. For example, compulsory education in the US endured at least until age 16, in Germany there were Volkschule or Hapuptschule until the age of 15 and in the UK students generally had nine years of compulsory schooling in the late 1930s.

A third argument for extending compulsory schooling was the *increasing specialization of the labour market* and the *increased complexity* of society and societal life, implying a need to significantly increase educational goals of Folkskolan. Finally, the economic and societal *duality* that existed between urban and rural areas was brought forward to motivate a general 8 year reform or a compulsory school reform. Specifically with respect to education, the rural areas of the country was falling behind, e.g. smaller shares of students matriculating to junior secondary education in rural compared to urban areas (Sweden, 1977). With a general implementation of the 8 year reform such differences could decrease.

The main arguments of the proponents of the 8 year reform to why the realization of an eight-year extension was seen as preferred compared to a comprehensive school reform was that there was (i) no large demand from students nor from the parties of the labour market for a 9 year comprehensive reform, and that (ii) the supply of teachers was too limited for a comprehensive reform, but also that the teachers generally had too limited education (SOU, 1945).

Likely spurred by the political debate some municipalities applied and got consent from the king and took the opportunity to implement a mandatory eight-year of Folkskola (Folkskollärarförbund, 1943).[24] The first two municipalities to implement an eight mandatory year were Kävlinge and Mariestad in the school year of 1941/42. The number of municipalities offering an eighth year gradually increases in the next-coming decade: In 1946/47 there were 33 and in 1958/59 207 municipalities, respectively. A characteristic of the municipalities introducing a mandatory eight-year in this time period is that they were urban and most of the larger cities of Sweden were early birds in this development. Consequently a quite large share of all students in the country had eight years of compulsory schooling: in the school year 1948/49 this was 16 per cent and in the school year 1951/52 this was 25 per cent (Folkskollärarförbund, 1952).

All municipalities introducing the eighth year followed the *main form* curriculum requiring full time reading and a teacher with an appropriate teacher degree.[25] Normative and binding curricula regarding the eight-year were missing in the early period,

[24]Only in a few cases a municipality did not get the permission to implement the extension. The reason was that the district asked to do an isolated change and only introduce the change in a separate school in a municipality (Fredriksson, 1971).

[25]The alternative to the main form were *exception forms*, characterised by half time reading or that the teacher did not have an appropriate teachers degree. In the early 1940's more than 90 percent of all pupils in Sweden went to a school that were assigned to the main forms (Fredriksson, 1950).

but the curriculum and hourly plan presented in the proposal of Skolutredningen in 1946 generally became the norm for the school districts that implemented an eight-year of Folkskola. The mandatory subjects in the eight grade were the same as in seventh grade, but local preferences could to some extent be met (Fredriksson, 1971).

A.3 The 9 Year Reform

In 1948, the expert commission proposed to replace the compulsory primary and the junior secondary school with a nine-year compulsory comprehensive school. The expert commission however wanted to evaluate the new school form before introducing it to all schools across the country. The reform was therefore introduced during an assessment period where the 9 year comprehensive school was introduced in different locations at different points in time. Starting from 1949/1950 the 9 year reform was rolled out at the municipality level.[26] For the first year of the roll-out of the reform 14 municipalities are selected to participate in the assessment.[27] The evaluation period was not run as a random experiment, but the National School Board chose the areas from a group of applicants to form a representative set based on observable municipality characteristics. Municipalities participating in the early assessment period were compensated with earmarked money from the central government for the increased costs following the expansion of mandatory education (Holmlund, 2008). After the assessment period, the national parliament decides to permanently introduce the 9 year reform to all schools the country in 1962. Seven years later, by 1969, all municipalities were obliged to have the new comprehensive school running (Marklund, 1982) and *Folkskolan* was fully disconuated.

The reform reshaped the school system and compared to the old tracking system students were kept in the same school type for nine years. Besides extending compulsory education from seven or eight to nine years and postponing tracking, the educational reform also came with a change in the national curriculum implying English and civics became a compulsory subject, but there were no major changes to the total number of hours or the distribution of hours taught in different subjects (Richardson, 1992).

The educational reform was also pedagogical. The commission proposal of of 1948 was very clear on that the traditional school and its working methods were obsolete. Specifically whole-group teaching and questions-response methods should be replaced by more individualized and activating elements, pandering students drive and independence (Marklund, 1982).[28]

[26] The comprehensive school system is introduced throughout the whole municipality, or in certain schools within a municipality. At the time there were 1037 municipalities in Sweden.

[27] Municipalities had to show interest in the reform and also report on various issues, such as e.g. population growth, local demand for education, tax revenues and school situation, and all municipalities that took part in the first year of assessment were required to have eight year comprehensive schooling. The 14 first-movers were selected out of 144 municipalities.

[28] The emphasis on the importance of the need of new working methods can also be assigned to the aim that education should foster democratic societal members. As discussed by Richardson (1978) there is also a change regarding the view of the individual in the late 1940's. The development of the individual now matters more than the societal development. An essential feature of the report by the commission is that that the school should be more pupil centred and less subject-matter oriented.

Based on the principles of the final report of *Skolkommissionen* a new educational plan for schools to follow is released 1962 (Lgr 62). The pedagogical key concepts of the plan are individualization and activity learning (Larsson, 2011). The pedagogical fundament on the special position of the individual and that the school should foster independent individuals did not meet any major objections (Marklund, 1989). However the first reform municipalities experienced difficulties in getting accurate work material and text books (Marklund, 1982).

A.4 Comparing the Two reforms

Based on the above it is evident that Sweden experienced a continuous roll-out of extending the compulsory amount of schooling from 6 to 9 years over a period of 40 years, and that the 8 year reform and the 9 year reform were implemented across overlapping cohorts. The roll out of the two reforms is illustrated in figure A.1. We call this the 8 year reform. On average the 8 year and the 9 year reform was 7 years apart in a municipality.

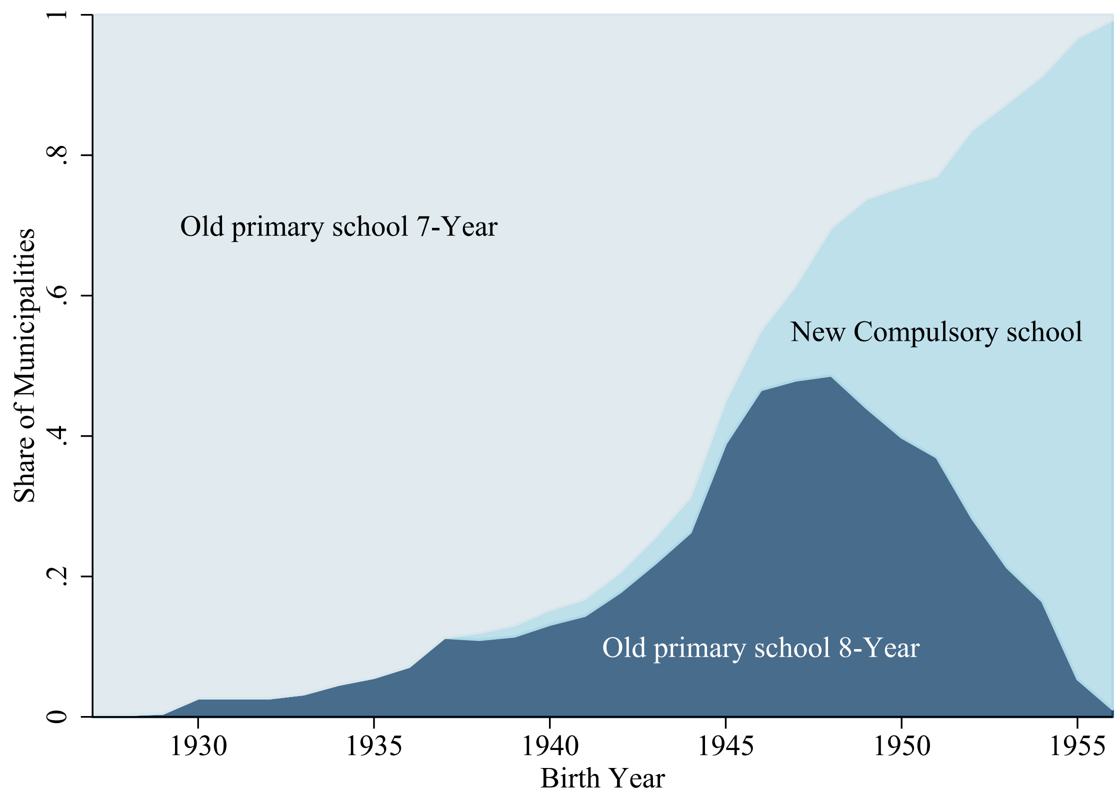

Fig. A.1: Share of municipalities by length of compulsory education

Notes: This figure shows the proportion of municipalities in Sweden who have the 7 year old primary school system, the 8 year old primary school system and the new 9 year compulsory school system by birth cohort.

Both reforms introduced change in the extent of compulsory schooling. As regards

Another novel perspective is the view that parents, not the school, are responsible for the pupil.

the exact definition of treatment it however seems that the two reforms differ somewhat. Treated students of the 8 year reform faced no significant school system changes, nor any changes in working methods in class. Thus, any effects from the 8 year reform should mainly be driven by changes in the amount of time spent in education.[29] With the abolishment of the tracking system the 9 year reform implied a fundamental change of the complete school system and the reform also came with a new curriculum program and methods. Any effects from the 9 year reform can thus be driven by changes in the amount of time spent in education, by that the new system kept students together in the same school until the ninth grade, and/or changes in curricula, working methods and pedagogics.

As discussed above schools and teachers initially faced some problems in that they lacked appropriate teaching materials corresponding to the new curricula and teaching methods of the comprehensive school. According to Marklund (1982) teachers degrees of freedom with respect to novel and open-ended activities were also limited by that students and parents that wanted *Realskola* but instead had to undergo compulsory schooling in the comprehensive system, translated their ambitions and goals for the former school type to the latter. Together this suggest that the first part of the 9 year reform likely was more similar to the 8 year reform. Also the first period of the 9 year reform was more similar to the previous school system in the sense that most schools still streamed students into different classes according to their choices regarding languages or vocational training and harder and easier courses in some subjects (Marklund, 1982).

The two reforms were gradually implemented across municipalities. The timing of implementation in individual municipality was based on a mixture of local and national decisions. As regards the wider institutional context, we are unaware of any reforms that might have coincided with the 8 year or the 9 year school reform at the local level. During the assessment period of the 9 year reform it was only municipalities that showed interest in the reform that could be selected implying reform implementation was not random. Previous studies suggest that 9 year reform was implemented earlier in municipalities with higher incomes and with higher average education, see e.g. Lundborg et al. (2014). Regarding the 8 year reform the early-birds tended to be more urban and most of the larger cities implemented a mandatory eight year. Smaller municipalities followed and in the end more than half of all municipalities had introduced a mandatory 8th grade before implementing the comprehensive 9 year school reform.

A.5 Reform data and Validation

The reform data for the 9 year reform was generously shared by Helena Holmlund and we rely on a dataset as used in Hjalmarsson et al. (2015), of which an earlier version is described in detail in Holmlund (2008). The dataset encompasses information on the year a specific school district introduced the new comprehensive school.

[29] See e.g. discussion by Orring et al. (1962) on that all earlier reforms than the 9 year reform more or less left the fundamental work of schools unaffected.

While the 9 year reform has previously been used in several economic applications, this paper is the first to use the 8 year reform and the reform data on the timing of the year of introduction of the eight year in each municipality was purposively collected from archives and digitized by the authors. Various official sources provide aggregate information on the development of the implementation on the 8 year reform. To check the accuracy of the gathered reform data we perform checks to confirm that the collected information conform with aggregate official statistics. For example, information on the share of school districts in the country that had eight years of compulsory schooling in certain years from Skolöverstyrelsen (1955) and from Sweden (1977), respectively, suggest our data conform with aggregate statistics.

The decision to introduce eight years of compulsory schooling was made on the municipal level, and the assumption is that schools within the same district implemented the reform in the same year. Theoretically there could however be discrepancies between schools within municipalities. We believe the assumption is valid since official sources state that the change generally applied to a whole district (see e.g. Fredriksson (1971) and Skolöverstyrelsen (1955)). Moreover, aggregate figures on the share of all students taking on the extra year of compulsory education in certain years (Sweden, 1977) suggest that there should be no major deviations from this rule.

Appendix B Tables and figures

Table B.1: ICD codes used to define causes of death and hospitalisation

Diagnosis	ICD 10 code	ICD 9 code	ICD 8 code
Cancer	C00-D48	140-239	140-239
Lung cancer	C33-C34	162	162
Circulatory disease	I	390-459	390-458
Diabetes	E10-E14	250	250
Liver Disease	K70,K73,K74	571	571

Notes: Lung cancer is included in cancer.

Table B.2: Compulsory schooling reforms' impact on education, survey based estimates

	(1)	(2)	(3)	(4)	(5)	(6)
	8 year reform			9 year reform		
	All	Low-ses	High-ses	All	Low-ses	High-ses
Reform impact	0.336***	0.367***	0.160	0.512***	0.571***	0.209
	(0.106)	(0.112)	(0.322)	(0.088)	(0.101)	(0.271)
F-stat	10.021	10.784	0.249	33.634	32.244	0.592
N	24,163	17,036	4,472	24,163	17,036	4,472

Notes: This table shows the impact of the 8 year and 9 year school reforms on years of education from a DiD regression controlling for cohort and municipality fixed effects gender and municipality trends. SES is defined as whether father has a white collar job in 1960 (high SES). Robust standard errors clustered at the municipality level are in parentheses. Testing the null of the coefficient: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$
Source: ULF survey. Own calculations.

Table B.3: Sensitivity results

	Mortality (1)	Fair/bad health (2)	Smoking (3)	Obesity (4)
Main specification (DiD with municipality trends)				
2SLS (8yr)	-0.012*	-0.085*	0.007	-0.089
	(0.007)	(0.048)	(0.051)	(0.058)
2SLS (9yr)	0.003	0.033	0.002	0.021
	(0.003)	(0.029)	(0.029)	(0.029)
Sensitivity 1 (DiD no municipality trends)				
2SLS (8yr)	-0.016	-0.064	0.015	-0.104
	(0.015)	(0.051)	(0.052)	(0.064)
2SLS (9yr)	-0.004*	0.008	-0.003	0.012
	(0.002)	(0.019)	(0.027)	(0.024)
Sensitivity 2 (DiD quadratic municipality trends)				
2SLS (8yr)	-0.012	-0.080	0.007	-0.127
	(0.008)	(0.066)	(0.054)	(0.091)
2SLS (9yr)	0.003	0.050*	-0.046	0.001
	(0.003)	(0.029)	(0.030)	(0.034)

Notes: This table presents the impact of years of education on mortality (column 1) and self-reported health and health behaviours (columns 2-4) estimated using different model specifications. All coefficients are from separate regressions. The sample is cohorts born 1932-1952. Standard errors clustered at the municipality level in parenthesis. Testing the null hypothesis: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.
Source: SIP (Mortality) and ULF-Survey (all other outcomes). Own calculations.

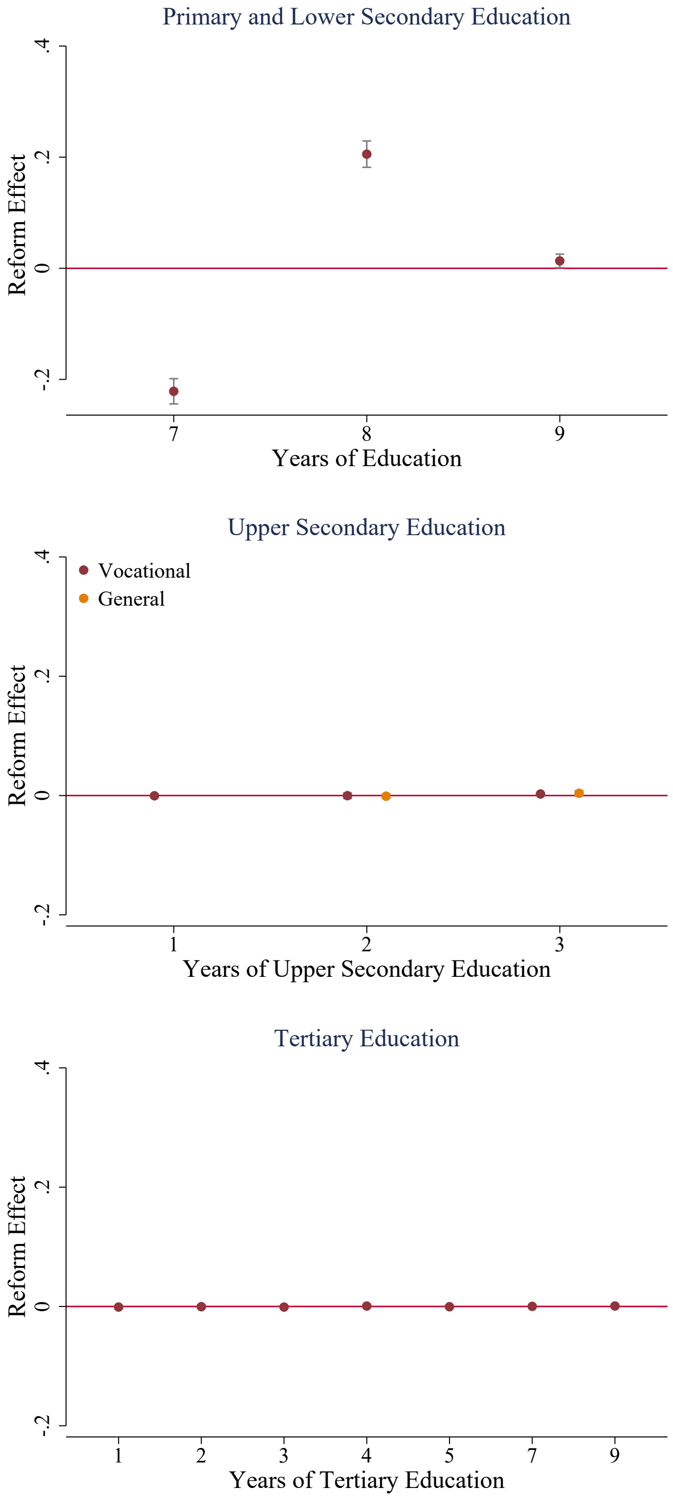

Fig. B.1: Impact of the 8 year reform on education

Notes: This figure shows analysis of potential spillovers of the reform. Regression coefficients of reform impact on binary indicators of schooling. Municipality and birth year fixed effects and municipality level time trends are controlled for.
Source: SIP. Own calculations.

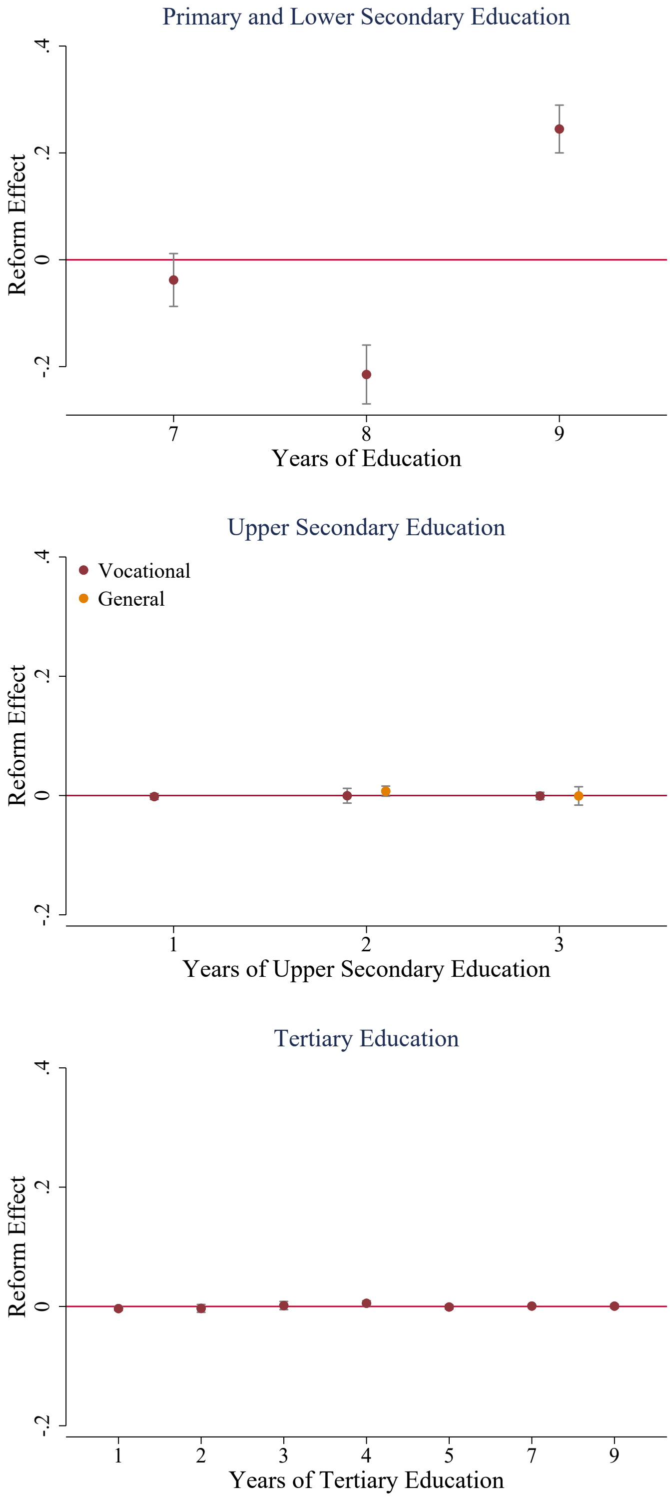

Fig. B.2: Impact of the 9 year reform on education

Notes: This figure shows analysis of potential spillovers of the reform. Regression coefficients of reform impact on binary indicators of schooling. Municipality and birth year fixed effects and municipality level time trends are controlled for.
Source: SIP. Own calculations.

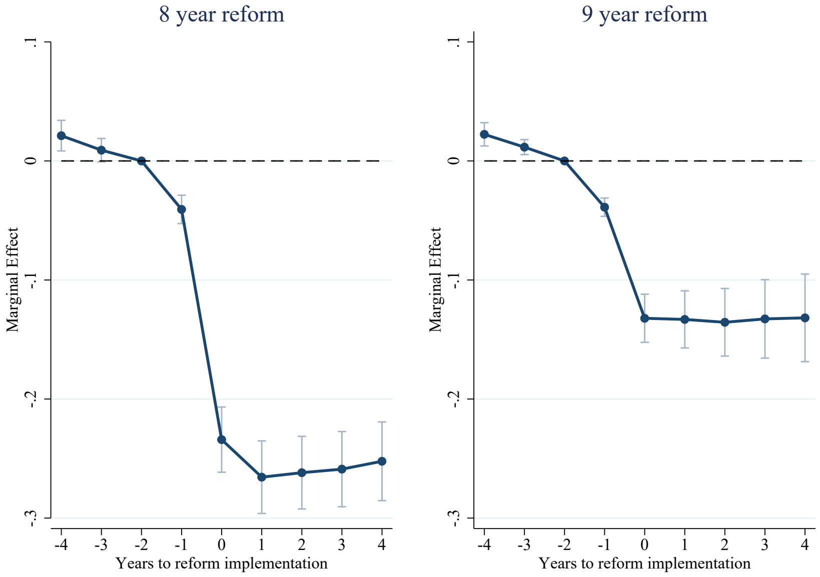

Fig. B.3: Impact of the reforms on leaving school with 7 years of old primary school

Notes: Event study figure plots regression coefficients of an individual's birth year relative to the first reform cohort in their municipality on proportion with 7 years of old primary schooling (spikes represent the 95% confidence interval for each coefficient estimate). Municipality and birth year fixed effects and municipality level time trends are controlled for and clustered standard errors are estimated at the municipality level. Category 5 is five or more years after the first reform cohort. The reference category is "two years before the first reform cohort" (t-2).
Source: SIP. Own calculations.

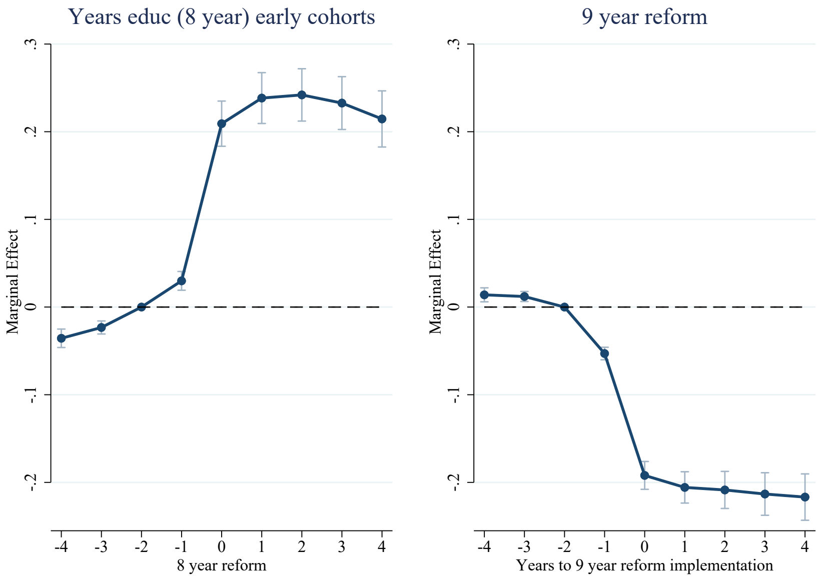

Fig. B.4: Impact of the reforms on leaving school with 8 years of old primary school

Notes: Event study figure plots regression coefficients of an individual's birth year relative to the first reform cohort in their municipality on the proportion with 8 years of old primary schooling. See notes for table B.3
Source: SIP. Own calculations.

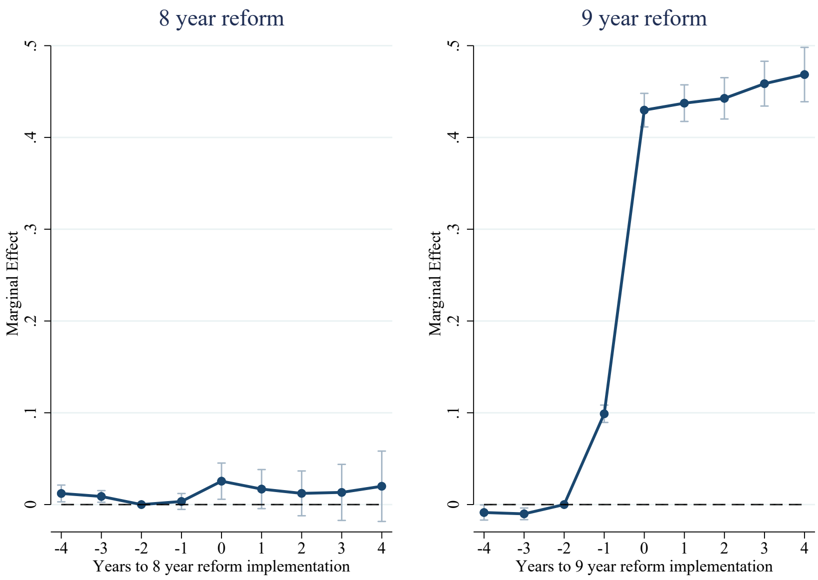

Fig. B.5: Impact of the reforms on leaving school with 9 years of old primary school or 9 years of comprehensive school

Notes: Event study figure plots regression coefficients of an individual's birth year relative to the first reform cohort in their municipality on the proportion with 9 years of old primary schooling or new comprehensive schooling. See notes for table B.3
Source: SIP. Own calculations.