Education causally affects health and mortality: Lessons to learn from quasi-experimental studies

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Abstract: Education is as a paramount public policy instrument for enhancing health outcomes, yet the literature remains inconclusive on its causal impact. It has been suggested that the mixed results may stem from considering a diverse range of policies and contexts for identification, and that the small magnitudes typically reported are due to the bulk of the literature relying on compulsory schooling extensions. In this paper we scrutinise both statements within a fixed historical context. By combining estimates from a range of educational reforms with within-family fixed-effects designs, our meta-analytic approach provides empirical support for an overall causal effect of education on premature mortality (6.4 fewer deaths per 1,000 before age 69 per year of education; 95% CI: 1.7–11.0; baseline 89) and hospitalization (3.3 percent reduction in days in hospital; 95% CI: 2.6–4.0; baseline 42 days). The average causal effect is substantially smaller than the observational association and the positive health effects are concentrated in men. Stratifying by reform type we find evidence for heterogeneity across types of educational policies, with particularly large benefits from increased compulsory schooling.

Keywords: Causal inference, educational policies, instrumental variable analysis, twin studies, health, mortality

JEL classification: I12; I18; I26

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1 Introduction

The educational gradient in health, with better educated individuals on average being healthier and living longer, is one of the most stable and well-documented empirical facts in the social and life sciences [Cutler and Lleras-Muney, 2006, 2012, Cohen and Syme, 2013]. If education indeed yields substantial and advantageous effects on health and longevity, policies promoting higher educational attainment may contribute to enhancing population health and potentially emerge as more potent instruments for promoting public health than mere increases in public health spending [Lleras-Muney, 2005, Clark and Royer, 2013]. Conversely, policies that diminish educational attainment or compromise educational quality may result in adverse health effects. Understanding the true long-term effects of education on health is essential for informed policy decisions. In this paper, we investigate the relationship between education, health and mortality, using a novel approach estimating general causality combining a multitude of different quasi-experimental estimates derived from a large and rich individual-level dataset within a single country.

The analyis of the educational gradient in health presents challenges, as initial evidence from observational studies, susceptible to biases arising from confounding or reversed causation, may not be adequate for informing policy decisions. However, an evolving body of research delves into the education-health relationship through quasi-experimental designs, including instrumental variable (IV) methods, often leveraging changes in compulsory schooling laws [Lleras-Muney, 2005, Kemptner et al., 2011, Albouy and Lequien, 2009, Lager and Torssander, 2012, Meghir et al., 2018, Grytten et al., 2020, Fischer et al., 2021], and twin designs [Fujiwara and Kawachi, 2009, Lundborg et al., 2016]. Despite much progress, the overall evidence remains mixed and inconclusive, marked by considerable variability in reported results [Cutler and Lleras-Muney, 2012, Galama et al., 2018, Gathmann et al., 2015]. Recent reviews and meta-analyses [Galama et al., 2018, Grossman, 2015, Hamad et al., 2018, Xue et al., 2021] have aimed to synthesize findings, yet they too arrive at mixed conclusions. This suggests that the intricate relationship between education and health outcomes, including mortality, may not exhibit uniform patterns across diverse contexts and demographic groups.

The underlying reasons for the wide dispersion in estimated effects thus remains elusive, but differences in methodological approaches appear to be one candidate. One recent meta-study by Xue et al. [2021] posits that variations in methods, differences in the measurement of health, and selective reporting of estimates collectively explain the observed heterogeneity across studies, leading to the conclusion that education lacks a discernible causal effect on mortality. Glymour and Manly [2018], Galama et al. [2018] and Lleras-Muney [2022] further emphasise that studies relying on single natural experiments tend to be susceptible to low statistical power and that reported instrumental variable (IV) estimates often exhibit imprecision, characterized by wide confidence intervals incapable of conclusively refuting ordinary least squares (OLS) estimates.

In this paper, we investigate the relationship between education, health and mortality relying on a combination of multiple natural experiments within a specific contextual setting. Leveraging a distinctive historical backdrop characterized by significant educational expansion in Sweden, we combine information on seven specific educational policies to gauge their causal impact on health outcomes and mortality. Between 1936 and 1968, Sweden underwent substantial alterations and expansions in its educational framework. Over time, several measures were enacted: compulsory education saw a step-wise increase from 6 to 9 years, annual term length in primary education was extended from 34.5 to 39 weeks, a notable increase in the number of lower and upper secondary schools occurred, and the establishment of new universities took place. Additionally, the educational system transitioned from a selective European tracking system to a comprehensive high school model. These reforms were implemented gradually over time across the local school district level, spanning 2,400 districts, in turn generating considerable variation in the provision and quality of education among different cohorts and geographical areas.

We evaluate all reforms using standard methods for quasi-experimental research designs and high-quality individual-level administrative data on mortality and hospitalization. Adding twin and sibling fixed effects models, we gain insights into a wide range of effects of education on a health and mortality. Combining these estimates using random-effects meta-analytic techniques, we deliver comprehensive evidence on the general causality between education and health within a singular contextual setting. At the same time we are able to single out heterogeneity by method, intervention type, and gender. For our primary outcome, mortality before age 65, pooled causal meta-estimates confirm a protective effect of education, albeit with a smaller impact than OLS estimates would suggest: accordingly, one additional year of education would reduce premature mortality by 6.4 deaths per 1,000 individuals (95% CI: 1.7–11.0; baseline 89 deaths). However, refining our causal meta-estimates to encompass only the most credible sources of identification results in attenuation and diminished precision, rendering them statistically insignificant. While this finding is anticipated due to confounding factors typically reinforcing the positive relationship between education and health, a more nuanced picture emerges when we narrow the analysis to compulsory schooling extensions, excluding policies that operate via increased opportunities. Surprisingly, in this restricted specification, the estimated effects closely align in magnitude with the OLS relationship. This noteworthy finding, though requiring cautious interpretation due to limited statistical power, challenges the prevailing assumption that school policies based on compulsion have a smaller impact on health than those enhancing opportunities.

When we consider hospitalisations, a broadly similar pattern emerges: the pooled causal meta-estimates, though statistically significant, fall short of the OLS estimates. The risk reduction per year of education, standing at 0.3 percentage points, is approximately half the magnitude of the OLS estimate. Notably, estimates linked to policy reforms instead surpass the OLS estimate, particularly evident in compulsory schooling extensions where a year of education is associated with a 1 percentage point risk reduction. Turning to the intensive margin, namely days spent in hospital, meta-estimates derived from policy-induced variation are consistently small and lack statistical significance. Consequently, we cannot dismiss the possibility that only the extensive margin of hospitalization risk is affected. Nevertheless, the findings that mortality and hospitalisation effects tend to be larger for compulsory schooling extensions than for other sources of exogenous variation is interesting. This finding becomes particularly noteworthy when contrasted with our results related to earnings: meta-estimates of causal effects on earnings consistently fall short of OLS estimates and, generally, do not exhibit greater magnitudes for compulsory schooling extensions than for other sources of exogenous variation.

We further document that these positive education effects on health and mortality, and the effect heterogeneity discussed above, are driven primarily by men. This observation aligns with several recent findings in the literature [Galama et al., 2018, Grytten et al., 2020]. In our setting the association appears to be linked to behavior, as suggested by reductions in death causes related to cardiovascular disease. For women, the evidence supporting a protective effect of education on health is weak. We find no robust effect on mortality nor hospitalizations, but there is a substantial and statistically significant increase in premature mortality from cancer. As suggested by previous research, factors such as marriage markets and fertility may significantly moderate the relationship between education and mortality among women [Nechuta et al., 2010, Palme and Simeonova, 2015]. Although maternal mortality for our cohorts was already low and close to contemporary levels in the developed world, the act of childbearing may yield enduring health consequences for women who survive past reproductive ages. Furthermore, age at first birth, nuptuality, and the use of hormone-related interventions like oral contraceptives, might bear implications for cancer risk.

This paper makes two main contributions to the casual literature on education and health. First, we address methodological challenges by drawing upon instrumental variable (IV) estimates as well as twin and sibling estimates, enabling a more efficient exploration of the causal relationship between education and health outcomes. While meta-analytic studies and cross-country comparisons based on existing evidence can increase efficiency and generally contribute to a synthesis of diverse findings, they inevitably grapple with distinguishing the impact of relevant institutional differences from confounders associated with the various contexts considered [Hamad et al., 2018]. In the previous literature, the effect heterogeneity has been attributed to (i) country and institutional differences, relating to differences in e.g. school systems and culture but also to differences derived from the pecuniary returns to schooling due to differing labour market conditions, and to (ii) time and period effects linked to changing disease environments, mortality rates, and shifts in wealth and poverty levels, subsequently influencing health and health behaviors [Gathmann et al., 2015, Galama et al., 2018]. A pivotal aspect is that all the reforms we consider were implemented within a uniform institutional framework, within the same country wherein cohorts exposed to these reforms subsequently acted under similar welfare and labour market institutions. This framework affords us increased efficiency in our analysis while it at the same time facilitates the generation of more readily understandable meta-analytical estimates.

Second, we make use of the fact that our reform dataset is rich to provide systematic empirical evidence regarding the education-health/longevity relationship. There are only a few studies that attempt to systematically understand the reason why education gradients vary across studies. We investigate the role of whether additional education is forced or opportunity increasing. This is relevant as reform conditions can be manipulated through public policy and thus informative for stakeholders, but also since different policies may have different effects. Policy interventions reducing the cost of attending school are likely to unveil different and possibly more favorable outcomes compared to policies of mandatory schooling.

From a theoretical standpoint, increased education has the potential to influence health decisions and outcomes by enhancing the efficiency of health-related activities [Grossman, 2006, Galama and Van Kippersluis, 2019] and bolstering lifetime earnings [Becker, 2007]. There is thus a potential for heterogeneous health effects, particularly if voluntary investments in education generate greater labor-market returns compared to compulsory education [Nybom, 2017]. Additionally, taking on voluntary education might alter peer groups, which may for example change preferences [Kremer and Levy, 2008, Carrell et al., 2011, Sacerdote, 2011, Fischer et al., 2021]. This is a hypothesis consistent with the findings of beneficial health effects stemming from college expansions [Buckles et al., 2016, Fletcher and Frisvold, 2014, Cowan and Tefft, 2020, Fletcher and Noghanibehambari, 2021]. In addition, compulsory reforms may have different health effects compared to voluntary, as situations where a substantial portion of the population is mandated to undergo an extra year of education may yield limited returns, diminishing the perceived value [Lleras-Muney, 2022]. All the educational policies we consider increased educational attainment, albeit varying in terms of targeted educational stages and the voluntary or mandatory nature of the reforms. In our empirical analysis we can distinguish between three reforms centered on compulsory education and four reforms that expanded educational opportunities by offering increased educational prospects.

Additionally, we capitalize on the fact that our administrative data is rich and large covering the full cohorts of both men and women born in Sweden 1930–1950. We have complete records of death from 1961 until 2013 and inpatient hospitalizations from 1964 until 2013. With data covering large parts of the period from the reforms were introduced until old age reduce problems of downward biases following pre-sample selective mortality [Van Kippersluis et al., 2011] and allow for an adequate follow-up period until age 69. A contribution is that we can make a comprehensive analysis of causes of death and hospitalization singeling out major causes which allows us to disentangle underlying mechanisms that link to education, health and mortality. We combine these full population records with data on the staggered implementation of educational reforms on the school district level or with information on the geographical distance to nearest university. Extensive balancing tests support the assumption of random assignment of the reforms given the inclusion of district fixed effects, cohort trends and a five-year pre-post observational window around the pivotal reform cohorts.

The paper is organized as follows. Section 2 provides information about the collected reform data and individual-level data drawn from administrative registers that enables us to estimate the causal impact of education on health and mortality. The empirical strategies used, including our approaches for pooling effect sizes is discussed in Section 3. The main results are presented in Section 4, while Section 5 concludes and considers explanations for our results and acknowledges limitations of the present study.

2 Data

To examine the relationship between education and health we employ historical information on educational reforms and population-based administrative data. This section provides a general description of the data and their sources. Table 2 presents the summary statistics of the main variables.

2.1 Reform Data

We gathered data concerning educational policies aimed at enhancing education across all educational tiers. We first rely on data on the year a school district introduced an extension of the annual term length, transitioning from 34/36 weeks to 39 weeks, and implemented *compulsory* 7$^{\text{th}}$ and 8$^{\text{th}}$ grades of primary education, respectively, sourced from historical archives and used in Fischer et al. [2019] and Fischer et al. [2021]. We supplement this data by collecting information on the initiation of *voluntary* 8$^{\text{th}}$ education within primary and lower secondary education in the non-academic track. Furthermore, we identified the inception of lower secondary school openings (*Realskola*) within the academic track from information preserved at the Swedish National Archive and use information on the university opening in Umeå in 1965. Finally, we incorporated external information on the introduction of the new comprehensive school system containing data on the year a specific school district introduced the new system, drawing from Holmlund [2008]. This reform extended compulsory schooling with a 9$^{\text{th}}$ grade but also involved the abolishing of tracking and other changes.

Based on the collected reform information we construct instrumental variables as binary *instrumental variable* $Z_{k,c}$ equal to one if a school district k implemented a policy affecting birth cohort c. We further generate an an instrumental variable for tertiary education by calculating the distance to the nearest university. The university opening in Umeå in 1965 introduces variation in time and space, considerably reducing the the distance to university for individuals residing in the northern counties of Sweden. Note that our instrument does not rely on distance per se – which is likely correlated with unobservables – but rather the change in distance to university. Table 1 gives an overview over the reforms and their main features while Appendix C describes all the educational policies employed in detail.

Table 1: Reforms

EDUCATION REFORM	MAIN FEATURES	F-STAT (*First stage*)
TYPE I *Compulsory Schooling Extensions*		
Term length	Extending length of term / school year in primary school, 34/36 $\rightarrow$ 39 weeks per year.	*reduced form*
Compulsory 7$^{\text{th}}$ Year	Extending compulsory education by one year (6 $\rightarrow$ 7 years).	812.52
Compulsory 8$^{\text{th}}$ Year	Extending compulsory education by one year (7 $\rightarrow$ 8 years).	79.04
TYPE II *Opportunity Increasing*		
Comprehensive School Reform†	Extending compulsory education by one or two years (7 or 8 $\rightarrow$ 9 years), abolishment of academic tracking, new curriculum, establishment of new lower secondary schools.	90.33
Voluntary 8$^{\text{th}}$ Grade	Non-compulsory 8$^{\text{th}}$ grade available in non-academic track.	19.40
Secondary School Opening‡	Expansion of lower secondary non-compulsory education (academic track), school openings increasing access and reducing distance.	19.03
Umeå University Opening‡	Expansion of tertiary education, opening of new university in northern part of country increasing access and reducing distance	9.23

NOTES: † Incorporates compulsory schooling extension as one part with multiple other aspects of the school system changing simultaneously Meghir and Palme [2005], Holmlund [2008]. We categorize the reform as opportunity increasing as this was a stated main goal of the reform. ‡. Different specification other than baseline used for instrumental variable estimations.

Interventions affecting early education (extending term length in primary school) and increased educational opportunities (the voluntary 8$^{\text{th}}$ grade, secondary school openings, and the university opening) have never been explored in relation to health effects. The effects of the introduction of Swedish comprehensive school system on health and mortality was investigated by Lager and Torssander [2012], Meghir et al. [2018], Fischer et al. [2021] and the mortality effects from the pure compulsory schooling reforms (compulsory 7$^{\text{th}}$ and 8$^{\text{th}}$ grade) were partially evaluated elsewhere Fischer et al. [2013, 2021].

2.2 Individual Level Data

The administrative data is drawn from the Swedish Interdisciplinary Panel (SIP) which links several population registers using personal identifiers: The Multigenerational Register to identify family links, the Educational Register (1985-2012), the 1950, 1960 and 1970 Swedish Censuses, the Cause of Death Register (1961-2012), the Inpatient Hospitalization Register (1964-2012) and the Income and Tax Register (1968-2012).[1]

Our baseline population consists of the universe of individuals born in Sweden 1930 – 1950, who survived until the year 1970, and who had not emigrated from the country by 2012. The SIP is cohort based with 1930 as starting cohort. As described below, our schooling measure comes from the 1970 Census which explains our chosen endpoint as cohorts born after 1950 have either incomplete or any information on education in 1970. Our sample selection criterion of survival until 1970 is thus necessary to have complete records of years of education. To identify if individuals were exposed or unexposed to the education reforms, we assign treatment status based on year of birth and municipality of residence during schooling age obtained from the 1950 and 1960 censuses. We refrain from utilizing the municipality of birth to allocate treatment status as it will introduce a substantial measurement error stemming from the registers' recording of the hospital location of birth as the birthplace cohorts born before 1947 [cf. Fischer et al., 2021].

Our study employs primary health indicators encompassing all-cause mortality, hospitalization for any cause, and the cumulative duration of hospital stays due to any cause, available for analysis until 2013. Our primary focus involves establishing a binary indicator denoting premature deaths (defined as those occurring before the age of 65) and hospitalization occurrences up to age 65 as the primary endpoints. Additionally, we explore secondary endpoints by examining specific causes of death and hospitalization according to the 7^{th}, 8^{th}, 9^{th} and 10^{th} versions of the International Classification of Diseases (ICD). Specifically we consider the three main common causes of premature deaths and hospitalization: Cancer, circulatory diseases, and external causes.

For measuring years of education we follow Fischer et al. [2021, 2022] and measure years of education by the total years in primary and secondary education from the 1970 Census and years of vocational training and tertiary education from the Swedish Educational Register.[2]

[1] 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*.

[2] This measure of years of education allows us to capture effects on each level of education separately. Previous studies based on administration data have generally approximated years of education by the average length of the overall *highest* educational qualification. This approach is insensitive to changes below the highest educational qualification. By construction the traditional approach can severely underestimate the effects of educational policies on years of education Fischer et al. [2021].

From the linked administrative data, we also get information on income and occupation. Our preferred income measure is log earnings measured at ages 38–60. We also draw information on family SES background, defined by the father's occupation in 1960. High SES refers to fathers having a high-skilled occupation defined as non-manual employees and professional self-employed using Statistics Sweden's socioeconomic index. Not all individuals have a father recorded in the 1960 census due to parental emigration or death and therefore our sample split by SES is smaller than our main sample.

Table 2: Descriptive Statistics

	Mean	SD	Min	Max	Obs.
PANEL A: DEMOGRAPHICS					
Year of Birth	1941.36	5.81	1930	1950	1,574,989
Years of Education	9.85	2.77	6	21	1,574,989
Childhood in Urban Region	0.33	0.47	0	1	1,571,030
Male	0.51	0.50	0	1	1,574,989
PANEL B: HEALTH OUTCOME					
Death $<$ Age 65	0.10	0.31	0	1	1,574,989
Ever in Hospital $<$ Age 65	0.78	0.42	0	1	1,574,989
Days in Hospital $<$ Age 65	36.81	188.02	0	17592	1,574,989
PANEL C: TREATMENT					
Term Length (Weeks)	38.21	1.01	34.5	39.0	308,568
7^{th} Year Compulsory	0.64	0.48	0	1	256,443
8^{th} Year Compulsory	0.56	0.50	0	1	368,799
Comprehensive School	0.29	0.46	0	1	491,205
8^{th} Year Voluntary	0.47	0.50	0	1	107,407
Secondary School‡	0.44	0.50	0	1	1,574,989
Distance to University (km)‡	202.46	180.31	0	1007	1,574,989

NOTES: Sample statistics for treatments are given according the baseline specification with a 5-year window around pivotal cohort for causal effect estimation. † Other specification than baseline used for instrumental variable estimations (full sample with linear year of reform trends) in order to qualify as instrument. Appendix Table A.1 provides descriptive statistics by gender.
Source: SIP. Own calculations.

3 Empirical Strategy

3.1 Identifying the Impacts of the Reforms

Instrumental variable estimation

We use linear instrumental variable estimation to estimate the causal effect of education on health outcome H_{ijk} for individual i, birth cohort j and school district k. The second stage of the 2SLS equation is given by

$$H_{ijk} = \beta_0 + \beta_1^p \widehat{E}_{ijk}^p + \nu_k + \mu_{R_k j} + u_{ijk}; \tag{1}$$

where $\widehat{E}_{ijk}^p$ is the predicted first stage for years of education from education policy p and instrumental variable $Z_{g,c}^p$. We control for school district fixed effects (ν_k) and cohort fixed effects $\mu_{R_k j}$ which we allow to differ between urban ($R_k = 0$) and rural ($R_k = 1$) districts. Following Hjalmarsson et al. [2015], we restrict the estimation sample to individuals born five years around each reform implementation in a given district, addressing potential differential trends across school districts.

For the annual term length, we estimate a reduced form specification by re-scaling the average term length to a full year. Thus estimates are comparable in the sense that they capture the effects of an additional *year* of education on H_{ijg}. The coefficient on years of estimated education β_1 is identified by the variation in years of education generated from education policies $Z_{g,c}^p$ varying across districts and over time.

To identify the causal effect of education on health the policy instruments need to fulfil several assumptions. While the exclusion restriction on that the reforms only affects health through education is generally not verifiable from the data and needs context-related assessment [Hernán and Robins, 2006], we carefully assess the relevance and independence of the reforms by examining whether treatment and control units are conditionally balanced and by assessing the reform impact on educational attainment, respectively. In the presence of heterogeneous treatments effects, the additional identification assumption of monotonocity and that the education policy did not decrease education for any individual (*exclusion of defiers*) is needed. Estimates then capture a local average treatment effect (LATE) [Imbens and Angrist, 1994]. IV estimates are policy specific and informative about the causal effect of education only for a subgroup of the population (*compliers*) which received more education due to a specific educational policy.

Sibling and Twin Fixed Effects Estimation

As an additional method to address confounding, we estimate sibling and twin fixed regressions

$$H_{ij} = \beta_0 + \beta_1 E_{ij} + \beta_2' X_{ij} + \mu_j + u_{ij}; \tag{2}$$

with μ_j sibling/twin fixed effects. β_1 captures the a causal effect of education under the assumption that the within transformation controls for all relevant confounding whereby differences in outcomes results from differences in education. It is important to recognize that twin and sibling fixed effects have faced criticism for several reasons, notably due to the exacerbation of attenuation bias through within-transformation in the presence of measurement errors in the dependent variable [Griliches, 1979]. Also, remaining differences in unobservables might drive differences in education and even worsen an omitted variable bias [Bound and Solon, 1999]. At the same time, a clear advantage with a sibling and twin fixed effects approach in our setting is the higher precision that we can get with these estimates compared to the instrumental variable estimates.

Pooling Effect Sizes

To examine general causality we generate a pooled estimate for the causal effect of education on health. We calculate several random-effects meta-analytic estimates based on the policy-derived IV and within family fixed effects estimates.

The meta-analytic average allow us to pool estimates across methods and to address the lack of power from individual quasi-experimental estimates. As within-family fixed effects and especially sibling fixed effects are prone to remaining bias while instrumental variable estimates suffer from imprecision, we calculate different meta estimates: M1 including all estimates, M2 excluding siblings but incorporating twins, MP all education policy based estimates. While M1 is most efficient, we argue that M2 and MP are more credible in terms of unbiasedness.

The random effects model allows for heterogeneity in parameter estimates. In the presence of essential parameter heterogeneity, the meta estimate gives a variance weighted average across various causal estimates. Despite being the arguably most efficient vehicle to aggregate diverse causal estimates, Such a pooled meta-estimate may not be the best guide to specific public policies, in particular if it includes very heterogeneous types of reforms. In order to safeguard against such concerns, we consider another separate meta estimate, MC, which is based on all the compulsory education reforms following their clear policy implications. Table 3 presents the components and conditions of the four different pooled meta estimates that we derive.

Table 3: Pooled Meta Estimates

	M1	M2	MP	MC
Sibling FE	✓			
Twin FE	✓	✓		
Compulsory Education Policy	✓	✓	✓	✓
Increased Opportunity	✓	✓	✓	

3.2 Independence and Relevance of the Reforms

Before examining the relationship between education and health we want to assess the independence of the educational reforms to ensure that exposure to these reform occurs in a quasi-randomly manner, considering the implemented control strategies. To achieve this, we perform covariate balancing tests for each intervention [Pei et al., 2019]. This analysis is crucial since the educational policies under scrutiny were not introduced randomly across various school districts and time periods. For example, more urban and affluent districts generally extended education earlier than rural districts. Column 1 in Appendix Tables C.1 - C.7 illustrates this selection by showing the correlation of the different reforms with pre-treatment family characteristics such as father's occupation, education and having a patronymic surname, which in a Swedish historical context is a strong predictor of lower social class [Clark, 2015]. All reforms negatively correlate with paternal occupation being a farmer as well as with fathers having a patronymic surname. The reforms also positively associate with fathers having an academic degree or a white collar job. A naïve regression with the reforms as a regressor or instrumental variable would pick up these correlations, in turn generating biased estimates.[3]

In order to mitigate the influence of confounding biases and establish a quasi-randomized design, we control for district fixed effects (FE), cohort trends and a five-year pre-post observational window around the pivotal reform cohort. Column 2 in Appendix Tables C.1 - C.7 shows that the educational policies are sufficiently balanced for our preferred regression specification. For the university opening we recover a balanced design by changing to a specification including linear cohort trends by reform year and no restricting to a five-year window.

We further want to validate the efficacy of our method in assigning reforms and illustrate their impact on educational attainment. Appendix Table A.2 presents the first stage estimates for our baseline specification. The results show that all the reforms led to a sizable and statistically significant increase in education. Each reform is precisely estimated and the F-test statistics typically suggest that the instruments attain sufficient strength according to the conventional threshold of 10 [Staiger and Stock, 1997]. The only exception lies in the distance to the nearest university, which does not fully satisfy the criterion for a strong instrument. Consequently, the findings related to distance from the nearest university should be interpreted considering this limitation.

The magnitude of the rise in years of education differs among the various educational reforms spanning from a 0.75 increase in years of education due to the implementation of a compulsory seventh-grade education in primary school to a 0.16 increment resulting from the option to voluntarily pursue an additional eighth year. The impact on education is larger for interventions forcing students to stay longer in school, while increasing education opportunities, such as introducing voluntary grades or the opening of new lower secondary schools, lead to smaller increases in education. The baseline estimates restricting the sample to a 5-year pre-post window around the pivotal reform cohort are robust to alternative specifications using county or year linear specific cohort trends.

[3]The adequacy of addressing such confounding variables constitutes a plausible origin for some of the variability observed in the estimates reported in prior studies on education and health.

4 Results

Observational Association of Education on Health

The observational association between education and the main health outcomes considered are all statistically significant and very precisely estimated. The results further suggest a sizable impact of education on population health. The OLS estimates in Column 1 of Table 4 show that one year of education associates with 10.5 fewer deaths per 1,000 before the age of 65 (95% CI: 10.4;10.7, baseline: 89 deaths per 1,000). Within our cohorts, this signifies a difference in premature mortality risk of over 50 percent between an individual completing five additional years of education by concluding upper secondary school compared to an individual completing only primary school. This magnitude is in the same ballpark as relative risk changes of premature death due to high risk factors such as smoking [Weng et al., 2019].

The OLS estimates further suggest that one year of education is associated with six fewer individuals per 1,000 ever hospitalized before the age of 65 (95% CI: 5.8;6.2, baseline: 762 per 1,000), and a decrease of 4.7 percent in the overall days spent in hospital (95% CI: 4.6;4.8, baseline 42 days in hospital). Revisiting the comparison between someone attending upper secondary school and a person completing only primary school an estimated 30 out of 1,000 individuals fewer experience hospitalization before reaching age 65, alongside an overall difference of 30 percent in cumulative hospitalization days.

Causal Effects of Education on Health

Next we examine the causal effect of education and mortality. Table 4 presents our meta causal estimates showing the effect of one year of education on our three main outcomes.[4] M1 in Column (2), which combines estimates from within family FE together with all policy instruments, suggests a decrease of 6.4 deaths per 1,000 (95% CI: 1.7;11.0). This is a substantial decline in premature mortality of roughly 30 percent compared to the OLS correlation, although the OLS estimate lies within the 95% confidence band. Excluding sibling FE estimates except those for same-sex twins in M2 in column (3) slightly attenuates the point estimate. This is possibly a

[4] Appendix Figure A.1 presents forest plots with estimates for each estimation strategy, separating between OLS, within-family FE estimates, and IV-estimates for the separate educational policies, for each outcome.

sign of remaining confounding between siblings. But this meta-estimate also becomes more noisy with a relatively large 95% CI (-3.8;12.7), neither rejecting the null hypothesis of no causal effect of education on mortality nor the corresponding OLS correlation. Homogeneity of effects across estimates is rejected (Cochran's Q-Test, p =0.006). In line with the notion that we have heterogeneity across estimates, restricting ourselves only to the policy derived estimates, MP in column (4), delivers an even smaller point estimate. On average 1.6 fewer deaths per 1,000 (95% CI: -6.8;8.9) are to be expected from an additional year of education gained through the considered education policies. Restricting the analysis to only compulsory schooling reforms, MC in column (4), effect sizes become larger but estimates remain noisy with 9.2 fewer deaths per 1,000 (95% CI: -23.8;5.4).

Table 4: Regression Results - Main Outcomes

OUTCOME	(1) OLS	(2) POOLED META ESTIMATES M1	(3) M2	(4) MP	(5) MC
Died (89)	-10.5	-6.4	-4.5	-1.1	-9.2
	[-10.7,-10.4]	[-11.0,-1.7]	[-12.7,3.8]	[-8.9,6.8]	[-23.8,5.4]
p-value: No effect	0.000	0.008	0.290	0.791	0.216
p-value: Homogeneity		0.018	0.006	0.229	0.096
Ever hospitalized (762)	-6.0	-3.2	-3.8	-7.7	-10.0
	[-6.2,-5.8]	[-3.7,-2.6]	[-10.7,3.1]	[-15.4,-0.1]	[-21.5,1.6]
p-value: No effect	0.000	0.000	0.279	0.048	0.091
p-value: Homogeneity		0.651	0.495	0.889	0.441
Days in Hospital (%) (42)	-4.7	-3.3	-2.0	-2.2	-2.6
	[-4.8,-4.6]	[-4.0,-2.6]	[-4.8,0.9]	[-7.0,2.6]	[-11.4,6.1]
p-value: No effect	0.000	0.000	0.176	0.367	0.555
p-value: Homogeneity		0.159	0.159	0.154	0.050
Sibling FE		✓			
Twin FE		✓	✓		
Compulsory Education Policy		✓	✓	✓	✓
Increased Opportunity		✓	✓	✓	

NOTES: This table shows the observational association (OLS) and pooled causal meta estimates. M1 refers to a random effects meta estimate including all school policy based instrumental variable, sibling and twin fixed effect estimates. M2 excludes sibling FE. MP is based only on all school policies excluding all within family estimates. School policies include pure compulsory schooling reforms (term length extensions, 7th and 8th grade) and opportunity increasing school policies (Comprehensive school reform, voluntary 8th grade and secondary school establishment) MC is based only on compulsory schooling reforms. Estimates show the marginal effects from one additional year of education. The units for mortality and hospitalization are reported on the absolute risk and scaled up by factor 1000. The units for days in hospital are measured as percentage points increases with each year of education. Mean of each outcome is given in parenthesis. Results refer to cohorts 1930–50. All regressions control for rural/urban birth cohort FE and a sex. Sibling and twins are identified via their biological mother. All school policy IV-estimates control for school district fixed effects. Robust standard errors clustered at school district level. Point estimate are given with 95% confidence intervals. *Source:* SIP. Own calculations.

We next examine effects education on hospitalization. Here the most comprehensive and efficient estimate M1 suggests that 3.2 individuals less will be admitted to hospital for each additional year of education (95% CI: 2.6;3.7). Once again we see that the estimate attenuates when siblings are excluded in M2. Given the large baseline risk for hospitalization the effect sizes in column 2 and 3 suggest a relatively small education effect on hospitalization. Again, the meta estimate derived from IV estimates from compulsory education reforms suggest the largest decrease. Overall, estimates are more homogeneous across methods (no rejection of

Cochran's Q-Test). When investigating the total length of hospitalization, estimates on the logarithmic days spent in hospital show a similar pattern. Meta estimate M1 suggest a 3.3 percent decrease in number of days in hospital (95% CI: 2.6;4.0) while M2 excluding siblings indicates 2.0 percent decrease (95% CI: -0.9;4.8).

Comparing the pooled meta analytical estimates with the correlation of education and health, the causal effect of education corresponds to roughly 50 percent of the OLS association. A statistical test cannot reject the equivalence of the meta analytic estimate with the corresponding observational association for mortality, but rejects the equivalence for both our hospitalization outcomes.

Effect heterogeneity by gender and cause-specific results

Several overviews of the literature on education and health highlight that many studies find significant gender disparities, often demonstrating weaker evidence for women [Galama et al., 2018, Grytten et al., 2020, Cowan and Tefft, 2020]. The reasons behind this discrepancy remain unclear, but can be theoretically attributed to both societal, cultural, and biological factors. Traditional gender norms and societal expectations often delineate distinct experiences and outcomes for men and women, potentially influencing career trajectories, decision-making autonomy, and social roles. In addition, labor market disparities, encompassing wage gaps and occupational segregation, may differently affect the economic outcomes associated with education for each gender. Differential health and risk behaviors exhibited by men and women may also contribute to disparate health outcomes linked to education, alongside the interaction between education and family structures or life course events, such as childbearing.

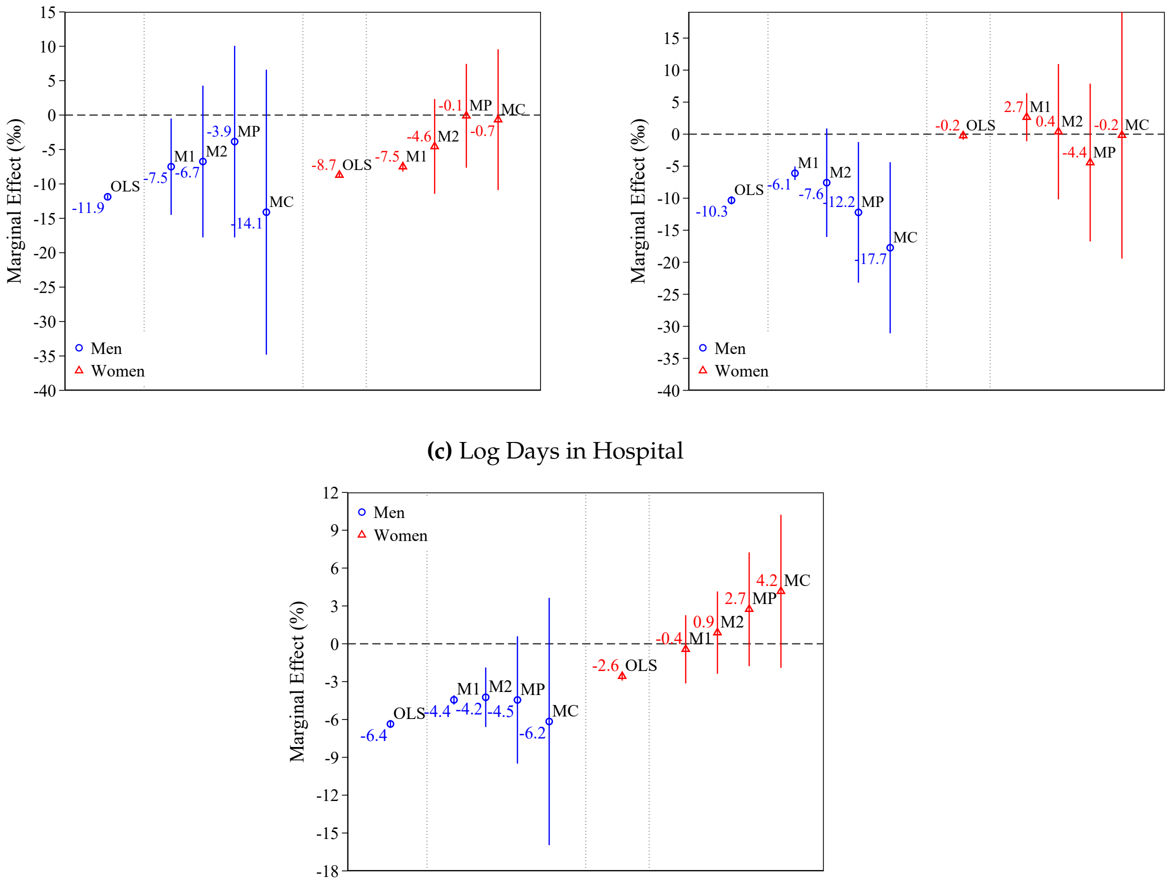
Stratifying our sample by gender and separately estimating the effects of education between men and women indeed reveals a substantial gender divide in the effects of education on health also in our setting, and that men are driving most of the estimated causal relationship. Figure 1 show that there are substantial health gains from education for all our main outcomes for men. The meta-analytic estimate M1 suggests a decrease of 7.5 deaths per 1000 for each year of education (95% CI: 0.5;14.5). Also across the other meta-estimates we see that education reduces mortality for men, with compulsory education reforms (MC) having the largest effect size. Estimates for hospitalization show 6.1 fewer men ever hospitalized when combining all estimates in M1 and even 17.7 fewer hospitalizations from on year of additional compulsory education (MC). Focusing instead on the length of hospital stay we find a reduction corresponding to 3.6 per cent decline when combining all estimates in M1. This effect size doubles when specifically considering the impact of compulsory education reforms. Regardless of the chosen outcome measures, the estimates for men consistently indicate a substantial causal effect of education, corresponding to at least 50% of the OLS association.

Results for women give a very different picture. As illustrated in Figure 1 M1 suggests a substantial decrease in all-cause premature mortality of 7.5 fewer deaths per 1,000 (95% CI: 6.8;8.2), but the effect attenuates substantially if siblings are excluded (M2) and estimates based on policy estimates (MP, MC) suggests no reduction in mortality from education and CI:s not including the OLS correlation. A similar picture emerges for the other two outcomes. The length of duration in hospital visit behaves similar to mortality and for hospitalizations even the OLS association is absent. Based on these measures, women's health is not improving by additional education.

To further understand the baseline results and the significant gender divide, we explore the relationship between education and causes of death and hospitalization, respectively. Differentiating by the three major causes of death suggest that more education decreases premature deaths for men especially through circulatory diseases. This result is consistent with the notion that health-related behavior, life style and working conditions would change as a result of more education. Interestingly, we also see reductions in mortality circulatory diseases and in deaths following external causes and for women. Given the lower baseline risk that we have for women, effects on the relative risk for these outcomes are de facto of the same magnitude as for men. At the same time our results suggest that women experience a large and statistically significant increase in the risk of dying prematurely from cancer by having more education. The increase in cancer deaths in turn lead to an overall small effect from education on premature mortality for women. The gendered pattern is even more pronounced when examining hospitalization by cause. The results in Appendix Table A.4 shows that a reduction in hospitalizations caused by circulatory diseases for men, but substantial increases in all three major causes of hospitalization for women.

Figure 1: Observational Association and Causal Estimates

(a) Premature Mortality **(b)** Ever Hospitalized



NOTES: The figure shows the marginal effects from one additional year of education on the main outcomes. OLS gives observational association. Causal estimates: M1 refers to a random effects meta estimate including all school policy based instrumental variable, sibling and twin fixed effect estimates. M2 excludes sibling FE. MP is based only on all school policies excluding all within family estimates. MC is based only on compulsory schooling reforms. Point estimate are given with 95% confidence intervals. Sibling and twins are identified via their biological mother. *Source:* SIP. Own calculations.

Table 5: Regression Estimates - Cause of Death

Outcome	(1) OLS	(2)	(3) Pooled Meta Estimates	(4)	(5)
		M1	M2	MP	MC
Panel A: Men					
Cancer (35)	-2.4	-1.6	-0.9	1.6	-3.8
	[-2.5,-2.2]	[-2.0,-1.1]	[-5.0,3.3]	[-3.1,6.3]	[-15.7,8.0]
Circulatory diseases (47)	-4.8	-3.4	-4.3	-3.0	-6.8
	[-5.0,-4.7]	[-4.8,-2.0]	[-8.7,0.1]	[-10.2,4.3]	[-19.4,5.7]
External Causes (13)	-3.5	-2.8	-0.7	1.2	-0.6
	[-3.6,-3.4]	[-3.1,-2.5]	[-4.5,3.0]	[-3.8,6.2]	[-6.5,5.4]
Panel B: Women					
Cancer (37)	-3.4	-3.1	0.1	2.8	4.5
	[-3.6,-3.3]	[-3.6,-2.5]	[-5.6,5.7]	[-3.3,8.8]	[-3.2,12.2]
Circulatory diseases (24)	-3.2	-2.6	-3.2	-2.4	-2.0
	[-3.3,-3.0]	[-3.0,-2.2]	[-5.5,-0.9]	[-6.4,1.6]	[-7.5,3.4]
External Causes (6)	-1.2	-1.3	-2.0	-2.3	-4.5
	[-1.3,-1.1]	[-3.6,0.9]	[-5.9,1.9]	[-7.8,3.2]	[-12.6,3.6]
Sibling FE		✓			
Twin FE		✓	✓		
Compulsory Education Policy		✓	✓	✓	✓
Increased Opportunity		✓	✓	✓	

NOTES: This table shows the observational association and pooled causal meta estimates for main cause of death. Otherwise see note in Table 4.
Source: SIP. Own calculations.

Income as a Mediator

From the linked administrative data we also have information on income by which we can examine the role of earnings as a mediator. Table 6 shows the observational association and the various pooled causal meta estimates using log earnings measured at ages 38–60 as an outcome. Following the clear gender divide in the education effects on health and mortality we also estimate the results separately for men and women.

The results from this exercises show that the labour market returns from education when using meta-estimates ranges between 2.5 and 5 percent. These effects are largely driven by females, who experience larger increases in lifetime earnings as compared to men. Notably, these gains for men are attenuated when focusing only on policy instruments (MP and MC). This result is in line with the recent economics literature suggesting that the returns to additional years of compulsory schooling are small (see, e.g., Pischke and Von Wachter [2008], Devereux and Hart [2010] and Fischer et al. [2019], but perhaps a bit surprising given that our policy reforms include increased opportunities for secondary and tertiary education. Generally, obtaining higher degrees and credentials is viewed as a key mechanism for changes of socioeconomic trajectories since it allows access to more valued jobs. But this aspect may also point to a limitation concerning the generalizability of our results: the cohorts under study experienced a labor market marked by full employment and a highly compressed wage structure [Bhalotra et al., 2022]. In a different context, the reforms we analyze might have yielded varying and more substantial impacts on labor market outcomes. Taken together, however, we argue that the labour market is unlikely to be an important mediator of the identified effects on health and mortality.

Table 6: Regression Results – Earnings

OUTCOME	(1) OLS	(2)	(3) POOLED META ESTIMATES	(4)	(5)
PANEL A: MEN & WOMEN					
Earnings (%) (545)	8.8 [8.8,8.9]	5.0 [2.8,7.1]	3.8 [1.3,6.3]	2.5 [0.5,4.4]	3.0 [-0.3,6.2]
p-value: No effect	0.000	0.000	0.003	0.012	0.076
p-value: Homogeneity		0.000	0.000	0.149	0.099
PANEL B: MEN					
Earnings (%) (673)	7.9 [7.9,8.0]	4.2 [1.6,6.7]	3.6 [0.6,6.6]	2.7 [-0.6,6.0]	2.1 [-2.6,6.7]
p-value: No effect	0.000	0.001	0.018	0.111	0.381
p-value: Homogeneity		0.000	0.000	0.014	0.032
PANEL C: WOMEN					
Earnings (%) (365)	10.1 [10.0,10.1]	5.2 [1.7,8.6]	4.2 [0.5,7.9]	2.9 [-0.8,6.5]	5.1 [-1.4,11.6]
p-value: No effect	0.000	0.003	0.026	0.122	0.124
p-value: Homogeneity		0.000	0.001	0.085	0.080
Sibling FE		✓			
Twin FE		✓	✓		
Compulsory Education Policy		✓	✓	✓	✓
Increased Opportunity		✓	✓	✓	

NOTES: This table shows the observational association and the various pooled causal meta estimates by gender. The mean of each outcome is given in parenthesis. Estimates show the marginal effects from one additional year of education on the log earnings at age 38–60. Otherwise see note in Table 4.
Source: SIP. Own calculations.

5 Conclusion

Socioeconomic disparities in health and mortality are large within and between populations. Education stands as a central indicator of socioeconomic status, acquired early in life and typically fixed beyond a certain point, intricately linked to subsequent income, employment, social connections, and behaviors. In recent decades educational achievement has emerged as an increasingly influential factor in forecasting adult health and longevity in the US and across various European countries [Shkolnikov et al., 2012, Montez and Zajacova, 2013, Hederos et al., 2018, Mackenbach, 2019]. Despite a substantial literature affirming that increased educational attainment correlates with improved health outcomes and prolonged lifespan, the causal relationship remains elusive.

Drawing upon a unique historical context marked by substantial educational expansion in Sweden, we combine data on seven distinct educational policies that all led to higher achievements in education to assess the causal influence of education on health outcomes and mortality. Taking advantage of having information on multiple quasi-natural experiments we can estimate general causality applying meta-analytic approaches. Our pooled meta estimates based on estimation strategies handling confounding suggest that education lead to better health and reduced mortality. We find evidence supportive of that approximately half of the noted association between education and health is causal. The causal effect is thus substantially smaller than the observational association, but also far from negligible: one additional year of education lead to 7.19 per cent fewer premature deaths (6.4 fewer deaths per 1000 before age 65; baseline 89; excluding siblings fixed effect: 5.39 per cent or 4.8/89), reduced risk of ever being hospitalized decreases by 0.4 percent (2.7/762) and length of stay in hospitals by 2.6 per cent.

Causal health effects are found for men with especially strong effects from compulsory schooling policies. Women in contrast experience higher cancer risks which counterbalances overall reduction in premature mortality. This finding is in line with growing evidence demonstrating weaker evidence on health and mortality from education for women [Galama et al., 2018, Grytten et al., 2020, Cowan and Tefft, 2020] and with previous research showing a positive link between cancer and education for women [Palme and Simeonova, 2015]. The causal education gradient by gender is mirrored and even clearer for hospitalizations than for mortality.

While we may conclude that education often and on average have non-negligible benefits for health, policy makers ultimately want to know which education measures work under which circumstances and for which groups in society. This is a substantially more difficult question to answer. Our empirical design theoretically allow us to estimate causal effects of education on health using plausible exogenous variation from various types of educational policies that differed by increased opportunities or compulsory uptake. Exploring heterogeneity in effects from such different types of intervention is of high policy relevance in order to find out *what* works in education. As such, each of our different education interventions identifies a separate policy relevant parameter.

In line with the mixed findings in the literature providing casual evidence on the relationship between education and health, our estimates for single interventions vary substantially across methods and health outcomes (see Appendix Figure A.1). Viewed individually as separate IV studies, although all the reforms analyzed represent valid natural experiments, our estimates across these reforms present a somewhat rather mixed picture. Part of this variability is clearly noise. Funnel plots for our different educational policy instruments as illustrated in Appendix Figures A.2 -A.3 are compatible with the notion that it is imprecision in the estimates and not systematic biases that is the basis for the large differences across estimates. Our results thus confirm that arguably unbiased quasi-experimental approaches and twin designs come at the expense of high variation in estimates. Despite the fact that we use individual level data covering the whole Swedish population as well as high-quality reform exposures, we have low precision for many of our individual estimates, especially relative to the observational association between education and health. This outcome is somewhat disheartening, particularly considering that theory propose that there are different policy tools that can influence individual's skill development [Galama et al., 2018] and that insights from separate policy tools affecting different sets of compliers and/or at different margins, could be informative to further understand the mixed findings in the literature.

Nevertheless, our meta-analytic statistics can identify some systematic variability. Compulsory reforms forcing individuals to take on more education, seem surprisingly effective in improving health. This result is interesting but also a bit intriguing given the prevailing arguments suggesting that policy interventions aimed at reducing the cost of attending more education might yield more favorable results compared to mandatory schooling policies. As highlighted by Lleras-Muney [2022], the discrepancy observed between quasi-experimental and observational research concerning health effects have been thought of as possibly originating from the predominant experimental variation stemming from compulsory attendance (potentially resulting in reduced impacts on health), while voluntary educational choices contribute significantly to educational variance withing the broader population. We additionally find substantial gender heterogeneity with positive health effects being concentrated almost exclusively in men. The fact that all the specific causes of death and hospitalization are partially linked with lifestyle suggests that the benefits of education on health partly come through altered health-related behaviors.

In summary, we provide evidence that education often and on average affects health, that compulsory education reforms are effective, that especially men seem to benefit from education, and that changes in health-related behavior mediate these effects. Our study also provides arguments for the inclusion and reporting of non-significant results and lower powered studies. If we believe that our causal estimates are on average unbiased, the exclusion of insignificant findings and selective reporting of significant results would likely overstate the positive education effects on health and mortality. Appendix Figure A.4 demonstrates the consequences of such practice by comparing our baseline results to a meta-estimate based on only our significant quasi-experimental estimates. Clearly selective reporting of this kind overstate the education effect and even suggest a causal estimate that is larger than the observational association. The *include all* meta-analytic approach taken in this paper overcomes this shortcoming. This benefit, and the noted imprecision of individual estimates from quasi-experimental approaches, suggest that future work should combine efforts over many labs and studies and take inspirations from endeavors in fields that face the same challenges, such as genetics, and more recently, psychology.

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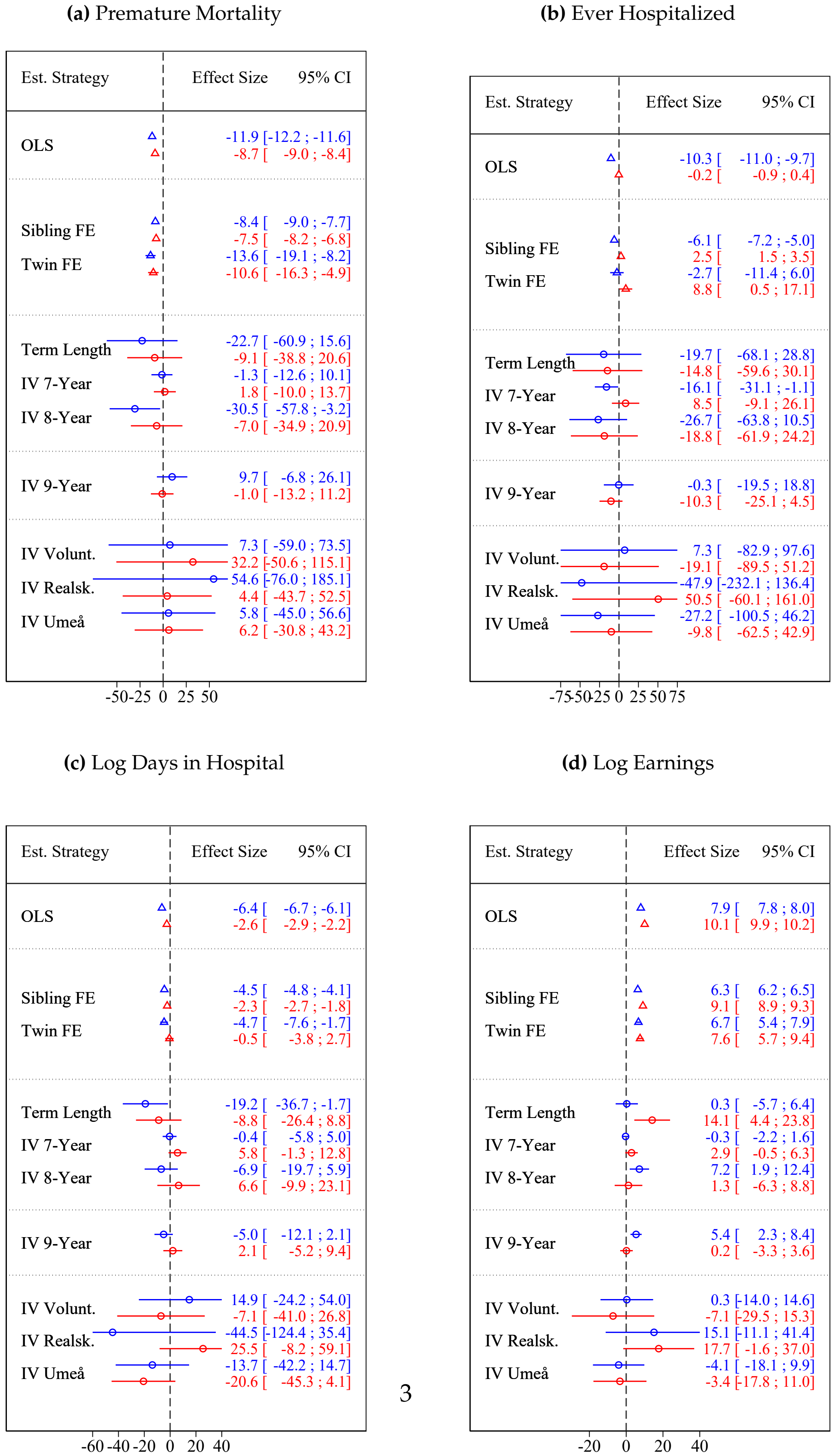
Supplementary Information - For Online Publication

Appendix A Figures and Tables

Figures

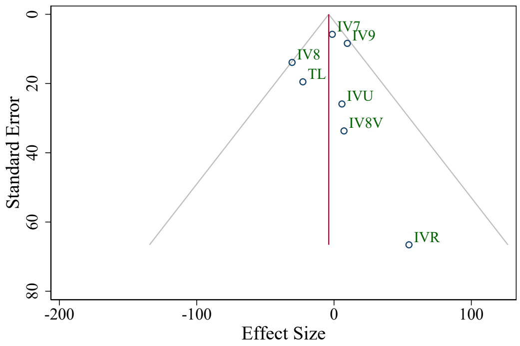
Figure A.1: Forest Plots Individual Studies (by gender)



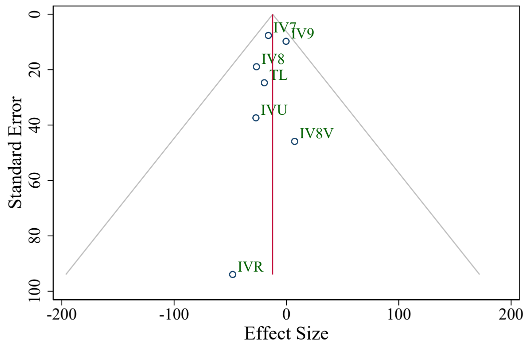
NOTES: Estimates for men given in blue, for women in red.
Source: SIP. Own calculations.

Figure A.2: Funnel Plot (Men)

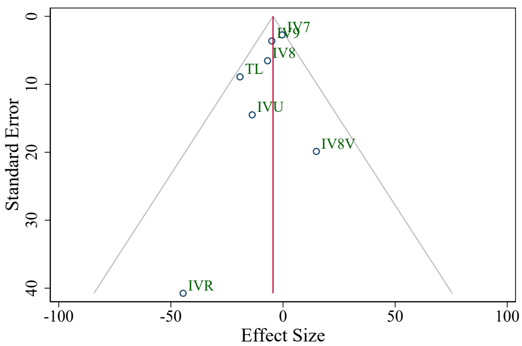
(a) Premature Mortality



(b) Ever Hospitalized



(c) Log Days in Hospital



(d) Log Earnings

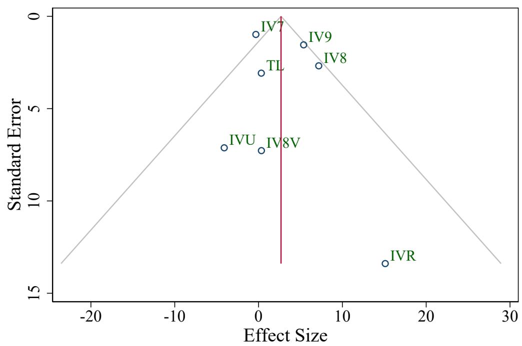
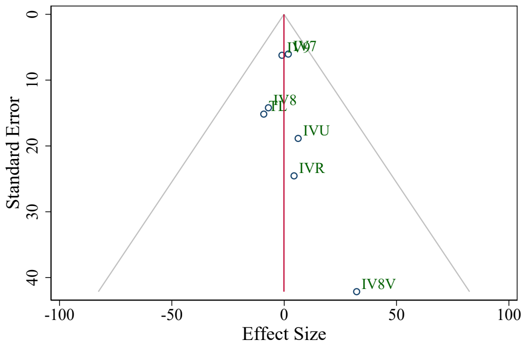
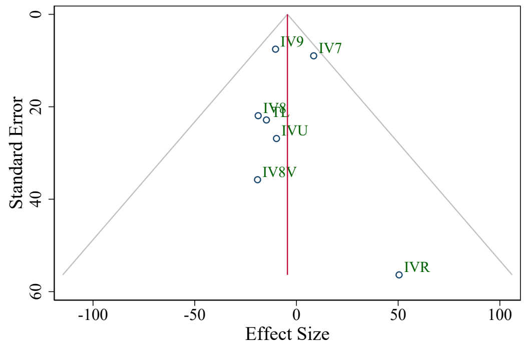


Figure A.3: Funnel Plot (Women)

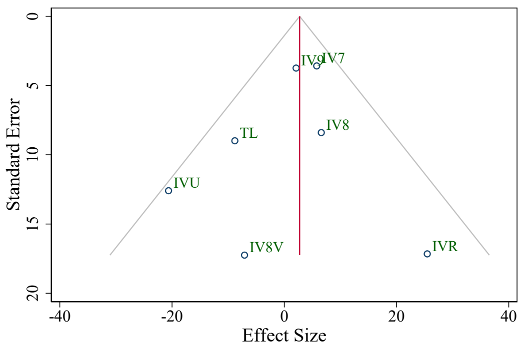
(a) Premature Mortality



(b) Ever Hospitalized



(c) Log Days in Hospital



(d) Log Earnings

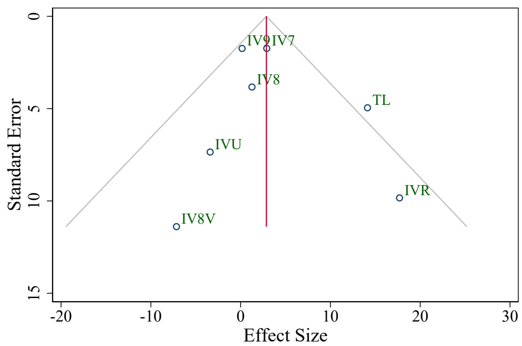
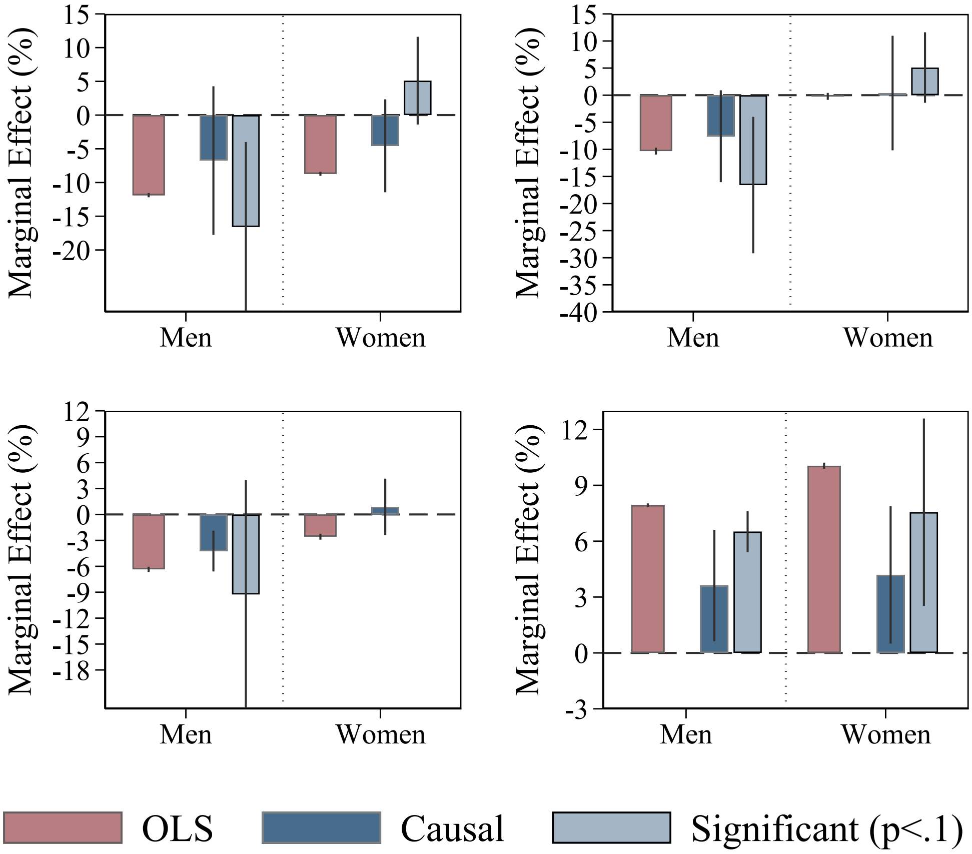


Figure A.4: Selective Reporting



NOTES: The figure shows the observational correlation, the causal meta M2 estimate excluding siblings and a meta estimate based on only significant findings ($p < .1$) for premature mortality, ever hospitlized, log days in hospital and log earnings. *Source:* SIP. Own calculations.

Tables

Table A.1: Descriptive Statistics (by gender)

	Men					Women				
	Mean	SD	Min	Max	Obs.	Mean	SD	Min	Max	Obs.
Panel A: Demographics										
Year of Birth	1941.31	5.84	1930	1950	805,005	1941.42	5.77	1930	1950	769,984
Years of Education	9.80	2.93	6	21	805,005	9.90	2.60	6	21	769,984
Childhood in Urban Region	0.33	0.47	0	1	803,004	0.33	0.47	0	1	768,026
Panel B: Health Outcome										
Death $<$ Age 65	0.13	0.33	0	1	805,005	0.08	0.27	0	1	769,984
Ever in Hospital $<$ Age 65	0.73	0.44	0	1	805,005	0.83	0.38	0	1	769,984
Days in Hospital $<$ Age 65	35.81	204.82	0	15808	805,005	37.85	168.68	0	17592	769,984
Panel C: Treatment										
Term Length (Weeks)	38.20	1.01	34.5	39.0	160,347	38.22	1.00	34.5	39.0	148,221
7^{th} Year Compulsory	0.63	0.48	0	1	134,722	0.65	0.48	0	1	121,721
8^{th} Year Compulsory	0.56	0.50	0	1	186,860	0.56	0.50	0	1	181,939
Comprehensive School	0.30	0.46	0	1	249,618	0.29	0.46	0	1	241,587
8^{th} Year Voluntary	0.47	0.50	0	1	54,687	0.47	0.50	0	1	52,720
Secondary School	0.44	0.50	0	1	805,005	0.45	0.50	0	1	769,984
Distance to University (km)	202.85	180.48	0	1007	805,005	202.06	180.13	0	1007	769,984

Source: SIP. Own calculations.

Table A.2: First Stage: Effect on years of education

	First Stage			Variance
	(1)	(2)	(3)	(4)
Compulsory 7th grade	0.754***	0.764***	0.734***	-0.281***
	(0.026)	(0.020)	(0.021)	(0.033)
F-stat	812.52	1,394.27	1,232.14	74.28
Compulsory 8th grade	0.198***	0.193***	0.204***	-0.126***
	(0.022)	(0.018)	(0.022)	(0.034)
F-stat	79.04	115.20	83.68	13.44
Voluntary 8th grade	0.158***	0.107***	0.114***	-0.025
	(0.036)	(0.017)	(0.021)	(0.036)
F-stat	19.40	37.74	29.98	0.48
Grundskola Reform	0.343***	0.484***	0.419***	-0.259***
	(0.036)	(0.033)	(0.037)	(0.047)
F-stat	90.33	219.37	129.98	30.69
Secondary School Opening	0.144	0.177***	0.178***	-0.073
	(0.098)	(0.040)	(0.041)	(0.120)
F-stat	2.14	19.10	19.03	0.37
Distance to University	-0.047***	-0.031***	-0.034***	0.036*
	(0.010)	(0.011)	(0.011)	(0.020)
F-stat	20.87	7.58	9.23	3.16
Cohort FE × Rural/Urban FE	✓	✓	✓	✓
District FE	✓	✓	✓	✓
5 Year Pre-Post Window	✓			✓
County Cohort Trends		✓		
Reform Year Trends			✓	

NOTES: This table shows the effect of reform status on years of education (*first stage*) and educational equality. We measure effects on educational equality by reform effects on the (conditional) variance of education. All regressions control for school district fixed effects and birth cohort fixed effects interacted with a rural/urban indicator. Results refer to cohorts 1930–50. Column (1) gives the baseline specification. The specification restricts the sample to a 5-Year pre-post window around the pivotal reform cohort. Column (2) is based on the full sample but adds county specific linear cohort trends. Column (3) adds reform year linear specific cohort trends. Column (4) gives effects of the reforms on the conditional variance for our preferred main specification. Robust standard errors are clustered at the school district level and shown in parentheses. Significance levels: $*$ 0.10 $**$ 0.05 $***$ 0.01.
Source: SIP. Own calculations.

Table A.3: Regression Results (by gender)

Outcome	(1) OLS	(2) M1	(3) M2	(4) MP	(5) MC
		Pooled Meta Estimates			
Panel A: Men					
Died (102)	-11.9	-7.5	-6.7	-3.9	-14.1
	[-12.1,-11.6]	[-14.5,-0.5]	[-17.8,4.3]	[-17.8,10.1]	[-34.8,6.6]
p-value: No effect	0.000	0.036	0.230	0.587	0.182
p-value: Homogeneity		0.077	0.058	0.226	0.106
Ever hospitalized (743)	-10.3	-6.1	-7.6	-12.2	-17.7
	[-10.7,-10.0]	[-7.2,-5.0]	[-16.1,0.9]	[-23.2,-1.2]	[-31.1,-4.4]
p-value: No effect	0.000	0.000	0.080	0.029	0.009
p-value: Homogeneity		0.786	0.694	0.822	0.871
Days in Hospital (%) (41)	-6.4	-4.4	-4.2	-4.5	-6.2
	[-6.5,-6.2]	[-4.8,-4.1]	[-6.6,-1.9]	[-9.5,0.6]	[-16.0,3.6]
p-value: No effect	0.000	0.000	0.000	0.084	0.218
p-value: Homogeneity		0.492	0.389	0.304	0.104
Panel B: Females					
Died (71)	-8.7	-7.5	-4.6	-0.1	-0.7
	[-9.0,-8.5]	[-8.2,-6.8]	[-11.4,2.3]	[-7.7,7.4]	[-10.9,9.6]
p-value: No effect	0.000	0.000	0.194	0.978	0.898
p-value: Homogeneity		0.614	0.516	0.963	0.712
Ever hospitalized (787)	-0.2	2.7	0.4	-4.4	-0.2
	[-0.6,0.1]	[-1.0,6.3]	[-10.2,11.0]	[-16.8,7.9]	[-19.4,19.1]
p-value: No effect	0.000	0.155	0.942	0.481	0.987
p-value: Homogeneity		0.403	0.315	0.615	0.376
Days in Hospital (%) (43)	-2.6	-0.4	0.9	2.7	4.2
	[-2.7,-2.4]	[-3.1,2.3]	[-2.4,4.1]	[-1.8,7.2]	[-1.9,10.2]
p-value: No effect	0.000	0.751	0.596	0.234	0.179
p-value: Homogeneity		0.086	0.220	0.229	0.306
Sibling FE		✓			
Twin FE		✓	✓		
Compulsory Education Policy		✓	✓	✓	✓
Increased Opportunity		✓	✓	✓	

Notes: Otherwise see note in Table 4.
Source: SIP. Own calculations.

Table A.4: Pooled Regression Estimates (Hospitalization)

OUTCOME	(1) OLS	(2) POOLED META ESTIMATES M1	(3) M2	(4) MP	(5) MC
PANEL A: MEN					
Cancer (97)	-1.2	-0.8	-1.7	1.6	1.3
	[-1.4,-0.9]	[-1.5,-0.0]	[-6.1,2.6]	[-5.5,8.8]	[-7.5,10.0]
Circulatory diseases (262)	-7.6	-4.5	-6.0	-10.3	-5.7
	[-7.9,-7.2]	[-5.4,-3.5]	[-12.1,0.1]	[-25.1,4.5]	[-27.6,16.3]
External Causes (243)	-10.7	-7.2	-4.1	-1.4	3.0
	[-11.1,-10.4]	[-8.2,-6.2]	[-10.5,2.4]	[-12.2,9.5]	[-8.9,15.0]
PANEL B: WOMEN					
Cancer (223)	-1.2	4.3	8.1	14.0	14.0
	[-1.6,-0.9]	[-2.6,11.2]	[-0.5,16.7]	[1.9,26.0]	[-1.3,29.2]
Circulatory diseases (183)	-9.2	0.3	3.4	8.0	4.2
	[-9.5,-8.8]	[-6.6,7.2]	[-5.0,11.7]	[-1.6,17.6]	[-8.9,17.4]
External Causes (181)	-3.0	1.9	5.2	10.1	13.4
	[-3.4,-2.7]	[-5.0,8.8]	[-2.0,12.5]	[-1.3,21.5]	[-0.1,27.0]
Sibling FE		✓			
Twin FE		✓	✓		
Compulsory Education Policy		✓	✓	✓	✓
Increased Opportunity		✓	✓	✓	

NOTES: This table shows the observational association and pooled causal meta estimates for main cause of death. Otherwise see note in Table A.3.
Source: SIP. Own calculations.

Table A.5: ICD codes used to define causes of death and hospitalization

DIAGNOSIS	ICD 10 CODE	ICD 9 CODE	ICD 8 CODE
Cancer	C00-D48	140-239	140-239
Circulatory disease	I	390-459	390-458

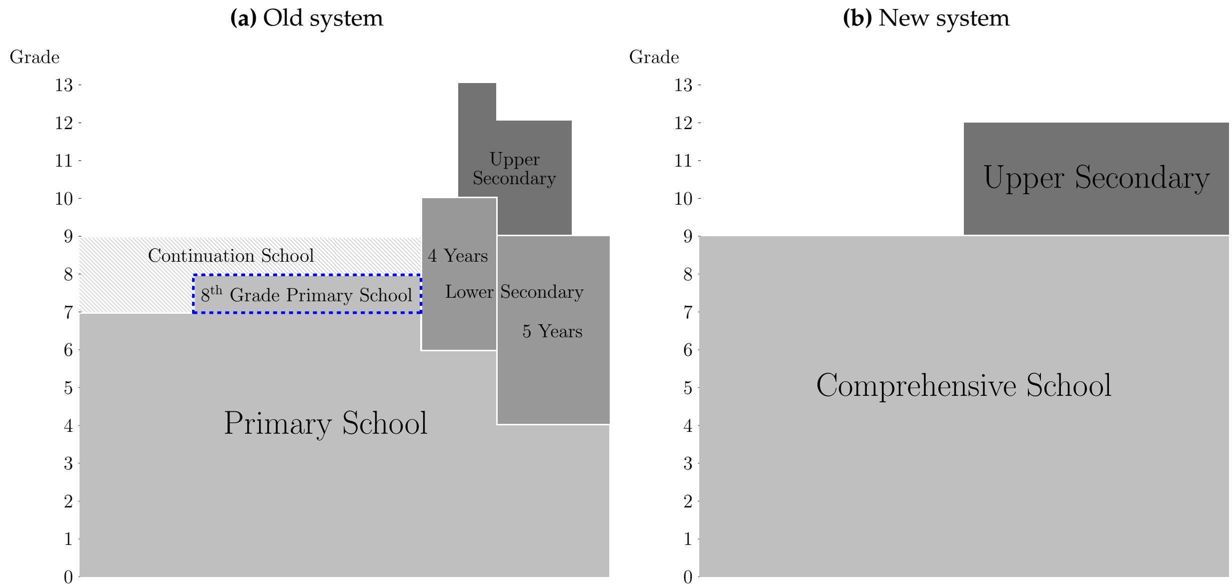
Notes: Lung cancer is included in cancer.

Appendix B Background to the Swedish School System prior to Reform Period (1936–1968)

Universal compulsory schooling was implemented in Sweden in the late 19th century. Primary schooling, *Folkskola*, started in the year an individual turned seven, was compulsory for six years and free of charge [Edgren, 2011]. There were about 2,500 school districts (generally coinciding with a parish) and each district had to provide primary education (including reading, writing, math, religion, history, geography, science and sports), and all teachers should have a teacher degree [Ljungberg and Nilsson, 2009]. A child went to primary school in the district s/he resided in. With the establishment of a central education plan in 1919, all school districts came to follow central guidelines in turn harmonizing and consolidating primary education across the country[Waldow, 2013].

Figure B.1a gives a stylized presentation of the Swedish school system before the change to a comprehensive school system during the 1950's and 60s. In the 1930's the school system was a tracking system whereby student progressing to lower secondary schools, named *Realskola* or *kommunal mellanskola*, left primary education after grade 4 or after the final grade of Folkskola. The school system was highly selective and on average barely 20 per cent of all children attended secondary school [Fredriksson, 1950]. After completion of lower secondary education, students were allowed to matriculate to upper secondary education, *Gymnasium*, and later university. Students not enrolling in secondary education completed compulsory education in *Folkskola*. They also had to take on low intensity courses six week per year in local continuation schools for two years after finishing compulsory schooling [Sweden, 1974].

Figure B.1: Parallel Swedish School Systems during Transformation Period (1936–1968).



Source: **?**

Appendix C Educational Reforms (1936–1968)

This section provides brief background information for each of the education reforms used in the empirical analysis. When existing, we also give a short overview about the most relevant literature evaluating the specific education reform with respect to its effects on health or potential mediators to health, such as income.

C.1 Term length extension in Primary School (Folkskola)

Reform Background

The aim of the 1919 central education plan was to provide central guidelines so that primary education became equivalent across all school districts. This harmonization process also concerned instructional time.

A school year was divided into autumn and spring semester, with the academic year starting in the fall. In a series of reforms the Parliament decides that the school year in primary education should be extend. In 1937 a school year could be 34.5, 36.5 or 39 weeks long; in 1939 the minimum school year duration is set to 36.5 weeks – a change that had to be implemented by the school year 1941/42 – and by the school year 1952/53 all school districts had to offer 39 weeks of schooling [Paulsson, 1946].

School districts could choose the timing of the implementation independently within the time windows, generating large variation in instructional time across cohorts and school districts.The implementation of the reforms was smooth and facilitated by that funding of school buildings, teaching materials and teacher's salaries was the responsibility of the central government and not the school districts [Larsson, 2011].

None of the described term length extensions implied any curricula changes, just more time to complete the stated learning outcomes. In total the term length extension corresponded to an increase in instructional time increase 15-31 weeks distributed over the complete course of primary education.

On top of the harmonization motive, the term length extensions was seen as important as teacher organizations regularly alerted that the school year was too short for many students to complete the stated course work [Paulsson, 1946, Fredriksson, 1971]. These reforms were also justified by that Sweden was lagging behind in terms of instructional time compared to other countries. No other Western European country had similarly short school years. In Denmark children attended primary education for 41 weeks, while 40 weeks was the minimum in Preussen and France [Stråfelt, 1930].

The Reform as Quasi-Experiment

One study have examined the effects of the term length extension in Folkskola [Fischer et al., 2019]. Evaluating the role of more instructional time, using information on the implementation of longer terms and the parallel compulsory 7$^{\text{th}}$ grade reform (see details below), results suggest that that school term extensions is an efficient mean for improved labour market outcomes. Specifically, the returns to the term length extension were sizeable, especially for women. With no significant heterogeneity across individuals' socioeconomic background, the reform seem to have benefited broad ranges of the population.

Table C.1: Diagnostics: Balancing Test for Differences in Fathers's Predetermined Characteristics by Reform Status (Term length extension)

Predetermined Characteristics	(1)	(2)	(3)	(4)
Years of Schooling (7.500)	0.530***	-0.054	-0.136**	0.024
	(0.100)	(0.091)	(0.057)	(0.060)
N	591,289	56,152	588,860	588,860
No Occupation (0.060)	0.001	-0.007	-0.011*	-0.005
	(0.008)	(0.011)	(0.006)	(0.007)
Agricultural Worker (0.215)	-0.563***	0.010	0.008	0.009
	(0.078)	(0.018)	(0.011)	(0.012)
Blue Collar Worker (0.500)	0.334***	-0.001	0.033***	-0.020
	(0.039)	(0.019)	(0.012)	(0.013)
White Collar Worker (0.226)	0.229***	-0.002	-0.029***	0.015
	(0.045)	(0.013)	(0.008)	(0.009)
N	1,302,805	273,663	1,299,143	1,299,143
High Socioeconomic Status (0.313)	0.244***	-0.001	-0.022**	0.006
	(0.049)	(0.016)	(0.009)	(0.011)
N	1,303,398	273,789	1,299,734	1,299,734
Patronymic (0.492)	-0.316***	0.010	-0.004	-0.014
	(0.032)	(0.014)	(0.007)	(0.010)
N	1,574,513	388,759	1,570,554	1,570,554
Cohort FE × Rural/Urban FE	✓	✓	✓	✓
District FE		✓	✓	✓
5 Year Pre-Post Window		✓		
County Cohort Trends			✓	
Reform Year Trends				✓

NOTES: This table shows impact of reform status on various predetermined characteristics. The mean of the outcome variable is given in parenthesis. Column (1) gives observational associations controlling for a nonparametric trend in year of birth interacted with an indicator for urban districts. Column (2) gives the baseline specification. The specification controls for linear cohort trends at the county level and school district fixed effects (SD FE) and restricts the sample to a 5-Year pre-post window around the pivotal reform cohort. Column (2) is based on the full sample and adds SD FE and county specific linear cohort trends. Column (3) controls for SD FE reform year linear specific cohort trends.Results refer to cohorts 1930–1950. Robust standard errors clustered at the school district level are shown in parentheses. Significance levels: $*$ 0.10 $**$ 0.05 $***$ 0.01. *Source:* SIP. Own calculations.

C.2 Compulsory 7th grade in Folkskola

Reform Background

Since the implementation of universal compulsory schooling in Sweden, primary schooling was compulsory for six years. In 1920 a clause was introduced in the primary school code on that a seventh year could be made compulsory in a school district [Fredriksson, 1950], but school districts were generally reluctant to extend compulsory schooling, the exception being districts in some urban areas [Sweden, 1974].

In the national political debate at the time it was argued that Sweden was lagging behind in terms of instructional time compared to other countries. In 1934 the Ministry of Ecclesiastical Affairs appoint a commission to inquiry the question of extending instruction time in primary education and their report clearly recommend compulsory education to be extended by a seventh year [Ecklesiastikdepartementet, 1935]. The main motive for implementing an extra year was that six years of schooling is too short for achieving and fulfilling the stated learning outcomes – a point that also often had been raised by teacher organizations – but also arguments on that more education is important to maintain a democratic society and economic arguments, such as high youth unemployment and the need to reduce the existing economic and societal urban-rural duality, were brought forward [Paulsson, 1946, Waldow, 2013].

In 1936 the Swedish parliament decides that a 7th grade in Folkskola should be compulsory. In line with the main motive of the reform, the policy change did not come with fundamental changes in learning outcomes or curricula [Ecklesiastikdepartementet, 1935]. The decision to extend compulsory schooling with a year was taken by the school district. The reform was not implemented at the same time across all school districts, but it was stipulated that the reform had to be completed by 1949 [Fredriksson, 1950]. The compulsory seventh year was thus implemented during a twelve-year period.

Due to the soft transition rules the reform was not reported to have caused any major difficulties in the school districts, and the implementation followed the initial time plan [Paulsson, 1946], but with spatial and temporal variation [Fischer et al., 2019].

The Reform as Quasi-Experiment

The implementation of the 7th grade have been evaluated in two studies. The first study examines the effect of more compulsory education on labor market outcomes [Fischer et al., 2019]. The results suggest that a modest effect on adult earnings and pensions, and the effect is partially mediated by a positive effect on post-compulsory education for treated individuals. If at all, the 7th year extension seems to mainly have benefited individuals from a low-socioeconomic background.

The second study investigates the relationship between the 7th school year on all-cause mortality but only using aggregated county-level data in relation to proportions of reformed school districts [Fischer et al., 2013].

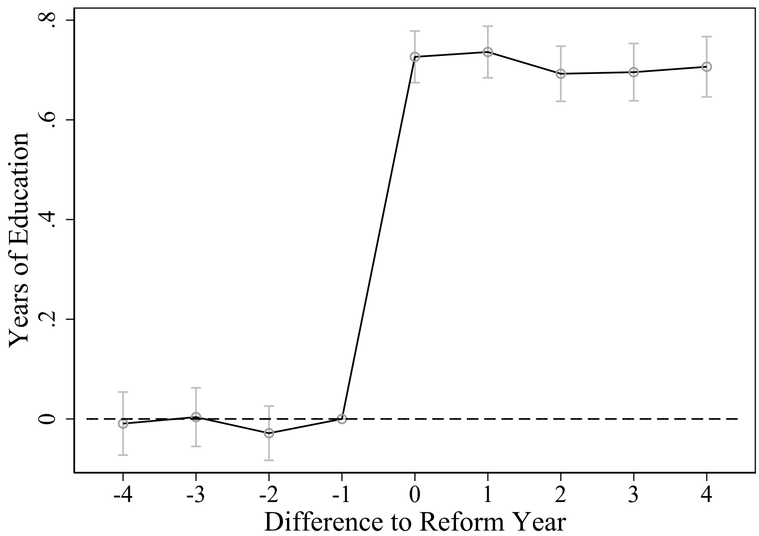


Figure C.1: Event Study Graphs: 7-Year Reform.

Notes: The figure shows the coefficients and 95% confidence intervals from an event-study regression for years of education on the difference from the reform year. Regression controls for SD FE and linear reform year trends. Column (4) controls for SD FE and linear cohort SD trends. Results refer to cohorts 1930–1940. *Source:* SIP / Census 1950. Own calculations.

Table C.2: Diagnostics: Balancing Test for Differences in Fathers's Predetermined Characteristics by Reform Status (Compulsory 7$^{\text{th}}$ grade)

Predetermined Characteristics	(1)	(2)	(3)	(4)
Years of Schooling (7.500)	0.102***	0.000	-0.031	0.017
	(0.022)	(0.028)	(0.019)	(0.019)
N	591,289	16,170	588,860	588,860
No Occupation (0.060)	-0.013***	-0.004	-0.006**	-0.003
	(0.004)	(0.004)	(0.003)	(0.003)
Agricultural Worker (0.215)	-0.206***	0.001	-0.001	-0.000
	(0.017)	(0.005)	(0.004)	(0.004)
Blue Collar Worker (0.500)	0.136***	0.004	0.013***	0.005
	(0.011)	(0.006)	(0.004)	(0.004)
White Collar Worker (0.226)	0.083***	-0.001	-0.006*	-0.002
	(0.009)	(0.004)	(0.003)	(0.003)
N	1,302,805	162,455	1,299,143	1,299,143
High Socioeconomic Status (0.313)	0.088***	-0.005	-0.005	-0.005
	(0.010)	(0.005)	(0.003)	(0.003)
N	1,303,398	162,515	1,299,734	1,299,734
Patronymic (0.492)	-0.078***	-0.003	-0.006**	-0.001
	(0.011)	(0.004)	(0.003)	(0.003)
N	1,574,513	256,375	1,570,554	1,570,554
Cohort FE × Rural/Urban FE	✓	✓	✓	✓
District FE		✓	✓	✓
County Cohort Trends		✓		
Reform Year Trends			✓	
District Cohort Trends				✓

NOTES: see Table C.1 *Source:* SIP. Own calculations.

C.3 Compulsory 8th grade in Folkskola

Reform Background

Despite two on-going reforms, both increasing students' instructional time in *Folskola*, there is a continued debate in the 1940's on extending compulsory education and on replacing the tracking system with a comprehensive school system. Similar to the discussion related to the introduction of a 7th mandatory year, the arguments brought forward in the debate are the role of education in democratic fostering and improved student performance with respect to elementary skills, but also that such reforms would reduce rural-urban differences and that Sweden was lagging behind compared to other European countries [Waldow, 2013].

Spurred by the debate some municipalities (which coincided with the school district) started to extend *Folkskola* from 7 to 8 years [folkskolläarförbund, 1943]. The number of municipalities implementing an 8th compulsory year gradually increased from 33 in 1946/47 to 207 in 1958/59. In the beginning these municipalities were more urban and most of the larger cities of Sweden were early birds in this development. Smaller municipalities followed and in the end more than half of the about 1,000 municipalities had introduced a mandatory 8th grade [Fischer et al., 2021].

Normative and binding curricula regarding the 8th year were initially missing, but a standard was introduced in 1946. The mandatory subjects were the same as in the seventh grade, but local preferences e.g. regarding a foreign language, could to some extent be met [Fredriksson, 1971].

The Reform as Quasi-Experiment

The implementation of the 8th grade in Folkskola has only been evaluated once. The question examined is whether education matters for health outcomes. Specifically the study study the implementation of the 8th grade and the parallel roll-out of the comprehensive school system, which implied an additional year of compulsory schooling but also a removal of tracking (see below) [Fischer et al., 2021]. By differencing the effects of the parallel reforms the effect of de-tracking and peers can be separated from that of more schooling. The results suggest that the compulsory 8th year reduced mortality and improved current health.

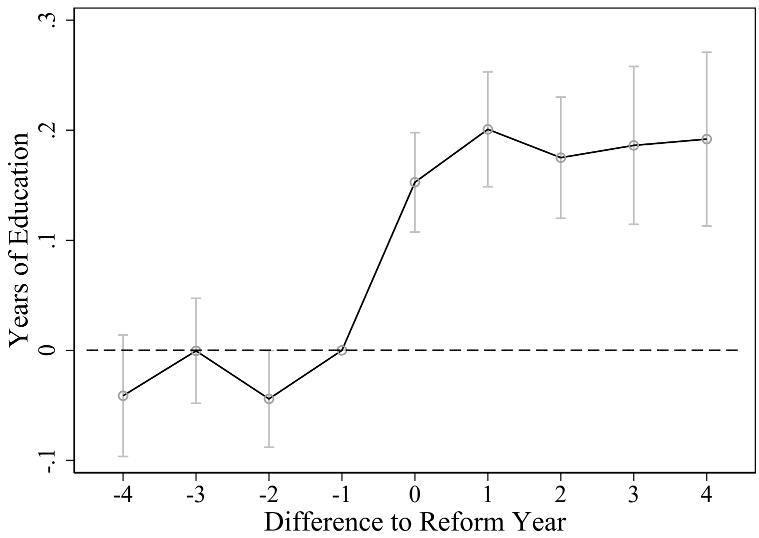


Figure C.2: Event Study Graphs: 8-Year Reform.

Notes: The figure shows the coefficients and 95% confidence intervals from an event-study regression for years of education on the difference from the reform year. Regression controls for SD FE and linear reform year trends. Column (4) controls for SD FE and linear cohort SD trends. Results refer to cohorts 1930–1950. *Source:* SIP / Census 1950. Own calculations.

Table C.3: Diagnostics: Balancing Test for Differences in Fathers's Predetermined Characteristics by Reform Status (Compulsory 7th grade)

Predetermined Characteristics	(1)	(2)	(3)	(4)
Years of Schooling (7.500)	0.395***	-0.014	0.012	-0.007
	(0.065)	(0.012)	(0.009)	(0.009)
N	591,289	151,319	588,860	588,860
No Occupation (0.060)	-0.006***	-0.000	0.000	-0.000
	(0.001)	(0.001)	(0.001)	(0.001)
Agricultural Worker (0.215)	-0.178***	0.001	-0.001	0.002
	(0.017)	(0.002)	(0.002)	(0.002)
Blue Collar Worker (0.500)	0.055***	0.004	-0.004	0.000
	(0.010)	(0.004)	(0.003)	(0.003)
White Collar Worker (0.226)	0.128***	-0.005*	0.004	-0.001
	(0.014)	(0.003)	(0.003)	(0.002)
N	1,302,805	313,387	1,299,143	1,299,143
High Socioeconomic Status (0.313)	0.129***	-0.004	0.005*	0.001
	(0.014)	(0.003)	(0.003)	(0.003)
N	1,303,398	313,538	1,299,734	1,299,734
Patronymic (0.492)	-0.042***	0.001	-0.002	0.000
	(0.015)	(0.003)	(0.002)	(0.002)
N	1,574,513	367,220	1,570,554	1,570,554
Cohort FE × Rural/Urban FE	✓	✓	✓	✓
District FE		✓	✓	✓
County Cohort Trends		✓		
Reform Year Trends			✓	
District Cohort Trends				✓

NOTES: see Table C.1 *Source:* SIP. Own calculations.

C.4 Comprehensive School Reform

Background

The parallel two-tier tracking school system had continuously been debated from the mid 1920's on-wards [Larsson, 2011]. From 1948 to 1969 municipalities (which coincided with school districts) gradually replaced the existing two-tier school system with a new comprehensive school system, *Grundskola*, where student were kept in the same school type for nine years. The main motive, on top of those relating to more education and instructional time, was to reduce inequality of opportunity[Holmlund, 2008].

The first period from 1949 to 1962 represented a trial period [Marklund, 1982]. Based on specific characteristics, the *National School Board* chose municipalities from a group of applicants to form a representative set of trial districts [Holmlund, 2008]. After an evaluation, the Swedish parliament decided in 1962 to fully replace the two-tier school system with the new comprehensive school system across Sweden. The implementation should be implemented latest in 1969. The replacement included several changes in the lower secondary school system, most importantly:

1. Setting compulsory length of schooling to 9 years
2. Abolishing of academic tracking after grade 4 or 6
3. English compulsory from 5^{th} grade

The Reform as Quasi-Experiment

The comprehensive school reform has been extensively studied with respect to its long-term effects on various socio-demographic outcomes. for example studies have noted positive reform effects on incomes [Meghir and Palme, 2005] and improved financial litteracy [Black et al., 2018], decreasing likelihood of conviction and incarceration[Hjalmarsson et al., 2015, Meghir et al., 2012] and positive intergenerational effects on e.g. human capital [Lundborg et al., 2014, Holmlund et al., 2011, Lundborg and Majlesi, 2018]. Several studies also examine health effects of the comprehensive school reform [Meghir et al., 2018, Palme and Simeonova, 2015, Lager and Torssander, 2012, Lager et al., 2016, Spasojevic, 2011], all overall reporting small health effects. None of these studies are able to separate out the effects of more years of education and the removal of tracking (in turn affecting the peer composition). A recent study suggests that the de-tracking and the subsequent peer effects, following the reform resulted in worse health [Fischer et al., 2021].

We use the comprehensive school reform as an instrumental variable for the years of education. Table A.2 and the event study graph in figure C.3 show that the reform led to a sizable increase of 0.5 years of education. Table C.4 gives the balancing regression for pre-treatment characteristics. Column (1) underlines that the reform first implemented in more urban districts with a higher share of students from high socio-economic background. Column (2) in Table C.4 shows that controlling for district fixed effects and county trends is insufficient to fully control for the selection. Column (3) and (4) demonstrate that trends on the year of implementation and district level are sufficient for a conditional quasi-random exposure. In addition to conditional randomness, it has been debated whether the reform fulfills the necessary *exclusion* restriction for an instrumental variable approach as the de-tracking lead to changes in peers and potentially also quality of education. See [Lundborg et al., 2014, Hjalmarsson et al., 2015] and [Meghir et al., 2018, **?**] for different views on the severity of this issue. By comparing effects from the comprehensive school reform to estimates from the parallel 8^{th} grade compulsory schooling extension in Folkskola, [Fischer et al., 2021] argue that differences reflect negative health effects from de-tracking and thus a violation of the exclusion restriction.

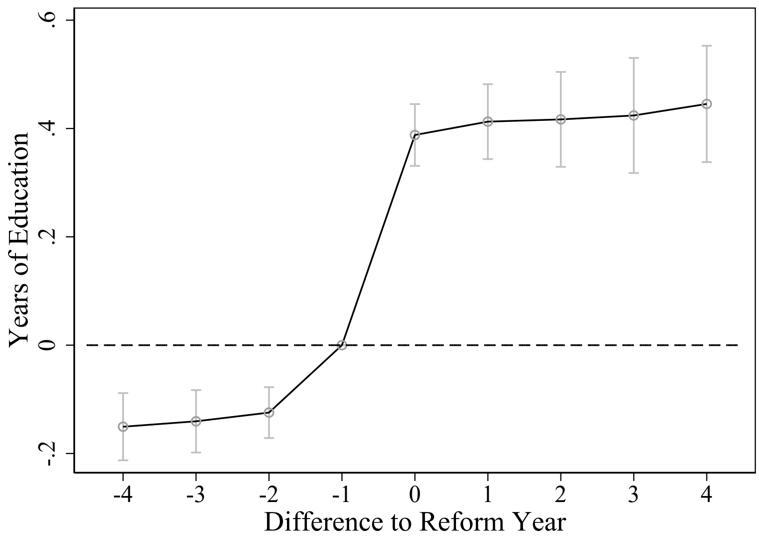


Figure C.3: Event Study Graphs: Comprehensive School Reform.

Notes: The figure shows the coefficients and 95% confidence intervals from an event-study regression for years of education on the difference from the reform year. Regression controls for SD FE and linear reform year trends. Column (4) controls for SD FE and linear cohort SD trends. Results refer to cohorts 1930–1950. *Source:* SIP / Census 1950. Own calculations.

Balancing Regression

Table C.4: Diagnostics: Balancing Test for Differences in Fathers's Predetermined Characteristics by Reform Status (Comprehensive School Reform)

Predetermined Characteristics	(1)	(2)	(3)	(4)
Years of Schooling (7.500)	0.221***	-0.008	-0.004	-0.015
	(0.057)	(0.012)	(0.014)	(0.012)
N	591,289	296,748	588,860	588,860
No Occupation (0.060)	0.002	0.001	0.001	0.001
	(0.002)	(0.001)	(0.001)	(0.001)
Agricultural Worker (0.215)	-0.086***	-0.000	-0.002	-0.000
	(0.015)	(0.002)	(0.002)	(0.002)
Blue Collar Worker (0.500)	0.020**	-0.003	-0.005*	-0.003
	(0.009)	(0.003)	(0.003)	(0.002)
White Collar Worker (0.226)	0.064***	0.002	0.007**	0.003
	(0.015)	(0.003)	(0.003)	(0.002)
N	1,302,805	449,217	1,299,143	1,299,143
High Socioeconomic Status (0.313)	0.059***	0.001	0.006**	0.002
	(0.015)	(0.003)	(0.003)	(0.002)
N	1,303,398	449,429	1,299,734	1,299,734
Patronymic (0.492)	-0.108***	-0.002	-0.002	-0.002
	(0.011)	(0.002)	(0.002)	(0.002)
N	1,574,513	488,641	1,570,554	1,570,554
Cohort FE × Rural/Urban FE	✓	✓	✓	✓
District FE		✓	✓	✓
County Cohort Trends		✓		
Reform Year Trends			✓	
District Cohort Trends				✓

NOTES: see Table C.1 *Source:* SIP. Own calculations.

C.5 Voluntary 8th grade in Folkskola

Reform Background

Certain municipalities decided to offer more than the compulsory grade in Folkskola, but on a voluntary basis. Students could consequently take take on extra years of schooling within the primary education system in some municipalities. The right to offer voluntary grades was regulated by the main Folkskola statue that also stated the subjects that could be taught. This included that mandatory subjects in compulsory primary education, but also foreign languages like English and German [Fredriksson, 1971].

During the first half of the 20th century this type of voluntary education within the primary school system was named *folkskolans högre avdelning* but re-named as *frivillig årskurs* from 1958 onward. In this analysis we focus on voluntary 8th and 9th grades in folkskola. By 1941 21 school districts offered this type of schooling, including only about 3,200 students, but the more and more districts initiated frivillig årskurser. By 1959 147 school districts offered voluntary 8th and sometimes even a 9th grades, including about 9,500 students [Sweden, 1974].

The voluntary 8th grade in Folkskola have never been evaluated before, and the reform data was purposely collected from historical sources for this study.

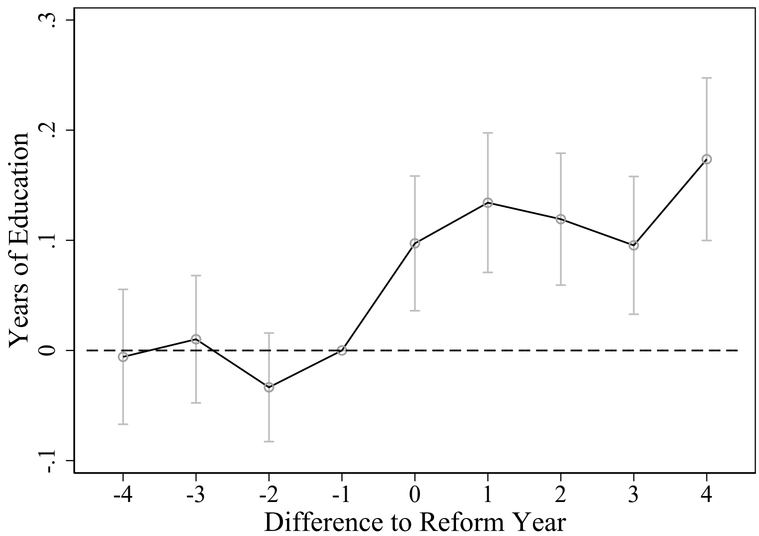


Figure C.4: Event Study Graphs: Voluntary 8th grade in Folkskola.

Notes: The figure shows the coefficients and 95% confidence intervals from an event-study regression for years of education on the difference from the reform year. Regression controls for SD FE and linear reform year trends. Column (4) controls for SD FE and linear cohort SD trends. Results refer to cohorts 1930–1950. *Source:* SIP / Census 1950. Own calculations.

Table C.5: Diagnostics: Balancing Test for Differences in Fathers's Predetermined Characteristics by Reform Status (Voluntary 8th grade in Folkskola)

Predetermined Characteristics	(1)	(2)	(3)	(4)
Years of Schooling (7.500)	0.002	-0.012	0.006	0.000
	(0.089)	(0.017)	(0.009)	(0.010)
N	591,289	54,292	588,860	588,860
No Occupation (0.060)	-0.002	-0.000	0.003*	0.001
	(0.001)	(0.002)	(0.001)	(0.002)
Agricultural Worker (0.215)	-0.090***	-0.000	-0.007**	-0.001
	(0.026)	(0.005)	(0.003)	(0.002)
Blue Collar Worker (0.500)	0.052***	0.000	0.002	0.002
	(0.009)	(0.006)	(0.003)	(0.003)
White Collar Worker (0.226)	0.040*	-0.000	0.003	-0.003
	(0.024)	(0.005)	(0.003)	(0.003)
N	1,302,805	97,623	1,299,143	1,299,143
High Socioeconomic Status (0.313)	0.043*	-0.004	0.005	0.000
	(0.024)	(0.005)	(0.003)	(0.003)
N	1,303,398	97,685	1,299,734	1,299,734
Patronymic (0.492)	-0.031**	-0.004	-0.005**	-0.001
	(0.015)	(0.006)	(0.003)	(0.003)
N	1,574,513	106,801	1,570,554	1,570,554
Cohort FE × Rural/Urban FE	✓	✓	✓	✓
District FE		✓	✓	✓
County Cohort Trends		✓		
Reform Year Trends			✓	
District Cohort Trends				✓

NOTES: see Table C.1 *Source:* SIP. Own calculations.

C.6 Secondary School Opening

Reform Background

Until 1968 the Swedish school system was a two-tier tracking system. Students who matriculated to secondary school either left Folkskola after grade 4 (at age 11) and then took on a four-year track in secondary, or after finalizing Folkskola and then took on a five-year track [Larsson, 2011]. Secondary education was in general free of charge.

In the early 20th century the school system was highly selective, with only a limited share of students taking on more than primary education [Fredriksson, 1950]. Secondary education was generally only offered in larger urban areas, but series of reforms 1909-1945, driven by demand and political will to reduce educational inequalities between urban and rural areas, lead to a large number of secondary school openings which increased access and reduced the geographical distance to secondary schools. Children born in 1905 had on average sixteen kilometres to the closest secondary school when being old enough to matriculate, while the corresponding distance for children born in 1945 was 6 kilometres [Lindgren et al., 2017].

Secondary education was offered in so-called *Realskola* and *Kommunal Mellanskola*. Before 1927 girls could only take on secondary education in private schools, *Flickskola*, but a reform in 1927 gave girls equal access to secondary education [Sweden, 1977]. The share of boys and girls attending secondary schools was similar already in 1930 [Schånberg, 2004].

The Reform as Quasi-Experiment

The reforms leading to openings of secondary schools in Sweden have never been evaluated in terms of economic or health effects, but one study uses the openings and distance to secondary education to examine the relationship between education and political participation. The opening of a new secondary school in a district increased the probability of individuals running for political office and the probability of holding office [Lindgren et al., 2019].

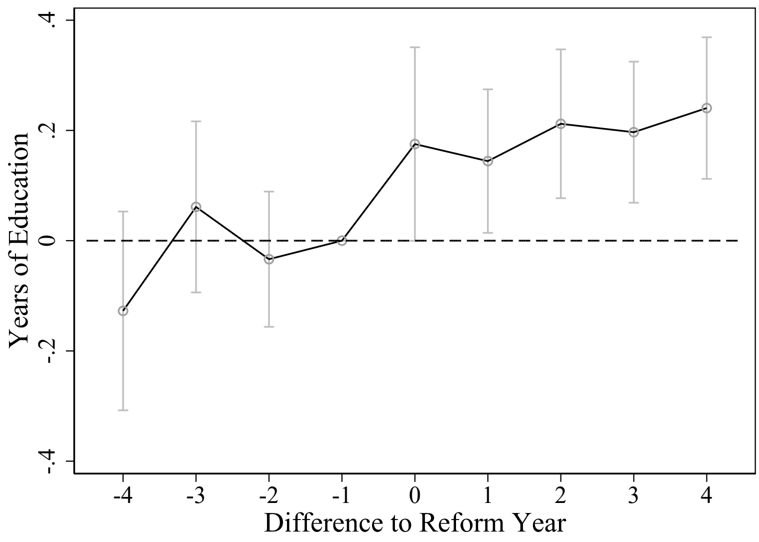


Figure C.5: Event Study Graphs: Lower Secondary School.

Notes: The figure shows the coefficients and 95% confidence intervals from an event-study regression for years of education on the difference from the reform year. Regression controls for SD FE and linear reform year trends. Column (4) controls for SD FE and linear cohort SD trends. Results refer to cohorts 1930–1950. *Source:* SIP / Census 1950. Own calculations.

Table C.6: Diagnostics: Balancing Test for Differences in Fathers's Predetermined Characteristics by Reform Status (Secondary School Opening)

Predetermined Characteristics	(1)	(2)	(3)	(4)
Years of Schooling (7.500)	0.440***	-0.044	0.016	-0.019
	(0.064)	(0.047)	(0.045)	(0.037)
N	591,289	4,584	588,860	588,860
No Occupation (0.060)	-0.001	-0.015	0.001	-0.009
	(0.002)	(0.013)	(0.009)	(0.007)
Agricultural Worker (0.215)	-0.244***	0.012	-0.002	0.006
	(0.014)	(0.015)	(0.015)	(0.011)
Blue Collar Worker (0.500)	0.081***	0.009	-0.005	0.005
	(0.009)	(0.014)	(0.013)	(0.010)
White Collar Worker (0.226)	0.164***	-0.006	0.006	-0.002
	(0.012)	(0.008)	(0.009)	(0.009)
N	1,302,805	19,538	1,299,143	1,299,143
High Socioeconomic Status (0.313)	0.167***	-0.003	0.008	0.003
	(0.012)	(0.011)	(0.010)	(0.009)
N	1,303,398	19,547	1,299,734	1,299,734
Patronymic (0.492)	-0.109***	0.007	0.003	0.010
	(0.013)	(0.013)	(0.008)	(0.008)
N	1,574,513	26,870	1,570,554	1,570,554
Cohort FE × Rural/Urban FE	✓	✓	✓	✓
District FE		✓	✓	✓
County Cohort Trends		✓		
Reform Year Trends			✓	
District Cohort Trends				✓

NOTES: see Table C.1 *Source:* SIP. Own calculations.

C.7 Distance to university - Umeå University opening

Reform Background

In the early 20th century, higher education was a privilege of the elite provided by eight universities located in the surroundings of the three largest cities Stockholm, Gothenburg and Malmö [Hallonsten and Holmberg, 2013]. Just like in other European countries, an increasing number of individuals in Sweden took on university studies in the post-war period. The development implied a five-fold increase of the total number of enrolled students between 1960-1990 [Bauer et al., 1999]. This process included a founding of two new universities in the 1960's – Umeå and Linköping – and an additional eleven university colleges in the 1970's [Ljungberg et al., 2009].[5]

The study makes use of the opening of Umeå university. Already in the mid 1940's there had been advocates in favour of localizing a university in the north of Sweden. Umeå – which is the largest urban area in the most northern land of the country – was the natural candidate, but the national parliament rejected the motion [Holmberg, 1946]. In 1956, however, an odontological department was established (formally a branch of Uppsala university) and in 1957 Umeå was also given the right to offer medical training [Bylund and Universitet, 1990]. These establishments paved the way for a parliament decision in 1963 to found Umeå University.

The university was officially inaugurated in 1965. During the first years about 2,500 students took on higher education in the three (medical, ontological and philosophical) faculties of the university [Bylund and Universitet, 1990]. Given its geographical location, the opening of Umeå University substantially reduced the distance to higher education for a large share of the population. Yet, the opening has never been studied from an economic nor a health perspective.

[5] The Swedish higher education system is legally uniform. The system differentiates between university and university college (*högskola*) which differs by that universities have the right to grant doctorates in any subject while a university college has to apply for the right in a specific subject, but both types of institutions are organized as governmental agencies.

Table C.7: Diagnostics: Balancing Test for Differences in Fathers's Predetermined Characteristics by Reform Status (Umeå University opening)

Predetermined Characteristics	(1)	(2)	(3)	(4)
Years of Schooling (7.500)	-0.108***	0.008**	0.002	0.002
	(0.023)	(0.003)	(0.004)	(0.004)
N	591,289	477,972	588,860	588,860
No Occupation (0.060)	0.008***	0.003***	-0.003***	0.000
	(0.001)	(0.001)	(0.001)	(0.001)
Agricultural Worker (0.215)	0.025***	0.002**	0.002	0.002
	(0.006)	(0.001)	(0.001)	(0.001)
Blue Collar Worker (0.500)	-0.008***	-0.004***	0.002*	-0.000
	(0.003)	(0.001)	(0.001)	(0.002)
White Collar Worker (0.226)	-0.024***	-0.001	-0.001	-0.002
	(0.004)	(0.001)	(0.001)	(0.001)
N	1,302,805	836,420	1,299,143	1,299,143
High Socioeconomic Status (0.313)	-0.024***	-0.001	0.000	-0.001
	(0.004)	(0.002)	(0.002)	(0.002)
N	1,303,398	836,823	1,299,734	1,299,734
Patronymic (0.492)	-0.022***	-0.001	-0.000	0.000
	(0.005)	(0.001)	(0.001)	(0.001)
N	1,574,513	916,404	1,570,554	1,570,554
Cohort FE × Rural/Urban FE	✓	✓	✓	✓
District FE		✓	✓	✓
County Cohort Trends		✓		
Reform Year Trends			✓	
District Cohort Trends				✓

NOTES: see Table C.1 *Source:* SIP. Own calculations.

Appendix D Identification of Causal Effects

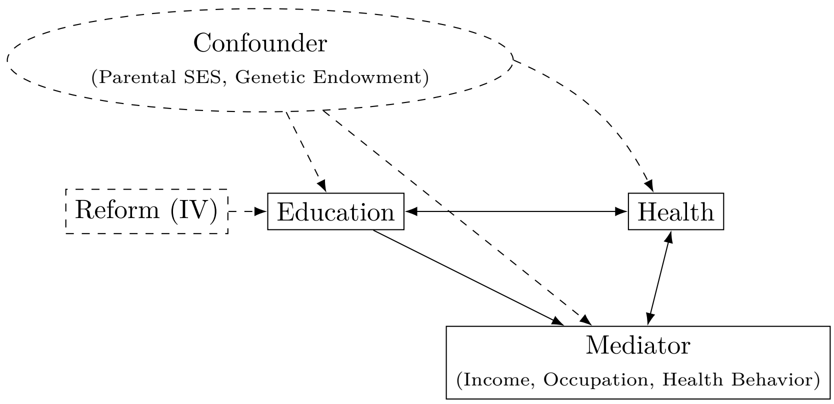


Figure D.1: Causal Path Diagram

Appendix References

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