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Daniel A. Kamhöfer
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Editor:
Prof. Dr. Hans-Theo Normann
Düsseldorf Institute for Competition Economics (DICE)
Tel +49 (0) 211-81-15125, E-Mail normann@dice.hhu.de

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The Long-term Effects of Student Absence: Evidence from Sweden

Sarah Cattan, Daniel A. Kamhöfer, Martin Karlsson and Therese Nilsson*

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Abstract

Despite the relatively uncontested importance of promoting school attendance in the policy arena, little evidence exists on the causal effect of school absence on long-run socio-economic outcomes. We address this question by combining historical and administrative records for cohorts of Swedish individuals born in the 1930s. We find that primary school absence significantly reduces contemporaneous academic performance, final educational attainment and labor income throughout the life-cycle. The findings are consistent with a dynamic model of human capital formation, whereby absence causes small immediate learning losses, which cumulate to larger human capital losses over time and lead to worse labor market performance.

Keywords: School absence, educational attainment, long-term effects, register data
JEL Classifications: *C23, I14, I21, I26*

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*Cattan: Institute for Fiscal Studies, London and IZA, Bonn; Kamhöfer: Heinrich Heine University Düsseldorf and IZA, Bonn; Karlsson: CINCH, University of Duisburg-Essen and IZA, Bonn; Nilsson: Lund University and IFN, Stockholm. For valuable comments we are grateful to Esteban Aucejo, Sonia Bhalotra, Arnaud Chevalier, Paul Devereux, Martin Dribe, Martin Fischer, Petter Lundborg, Teresa Molina, Erik Plug, Martin Salm, Kjell Salvanes, Hendrik Schmitz, Nina Schwarz, Guido Schwerdt, and Matthias Westphal. We would also like to thank seminar participants at CINCH (Essen), IFN (Stockholm) and Lund University as well as participants of EEA 2015, ESPE 2015, IWAEE 2015, VfS 2015 and the Essen Health Conference 2015. For collecting and digitizing the data used here we are indebted to our colleagues in Essen and Lund as well as a vast team of research assistants. Sarah Cattan gratefully acknowledges financial assistance from the British Academy Postdoctoral Fellowship pf140104 and the ESRC-funded Centre for the Microeconomic Analysis of Public Policy (ES/T014334/1). Therese Nilsson gratefully acknowledge financial support from the Swedish Research Council (dnr 2019-03553), the Gyllenstierna Krapperup Foundation, KEFU and the Crafoord foundation (dnr 20190685). Daniel Kamhöfer is grateful to the Institute for Fiscal Studies, London, and Lund University for hospitality as well as to the German Academic Exchange Service (DAAD) and the University of Duisburg-Essen–ERASMUS+ Mobility Program for financial support. This is an updated version of IZA Discussion Paper 10995 (September 2017) and ISF Working Paper 06/21 (February 2021).

1 Introduction

Student absence from school is pervasive around the world. While raising school attendance has long been the focus of policy in developing countries, the issue has also gained prominence in developed countries over the past decade. State and national governments have started taking concrete measures to reduce absenteeism, ranging from better monitoring and public awareness campaigns to monetary fines.

Despite the relatively uncontested importance of promoting school attendance in the policy arena, there is little causal evidence of the effect of absence on socio-economic outcomes. The few papers that credibly establish such evidence find that absences in elementary and secondary school have a detrimental impact on academic achievement and school graduation (Goodman, 2014, Aucejo and Romano, 2016, Liu et al., 2019). While these papers are important in going beyond the correlational evidence, they all focus on educational outcomes in the US. Much remains to be known as to whether (i) impacts on educational outcomes reflect human capital losses that translate into long-term outcomes; and (ii) whether these impacts generalize to other contexts.

This paper provides evidence of the impact of student absence on educational achievement, labor market outcomes and mortality in the context of Sweden. We construct a unique panel following a representative sample of cohorts born 1930-1935, which links digitized records of absence and educational performance in elementary school with census and tax register data on socio-economic outcome throughout the life-cycle. To our knowledge, this is the first dataset that allows for an analysis of the long-term impact of absences. This is an important innovation in light of evidence showing early-career advantages either fading relatively fast (such as the effect of the business cycle on earnings, cf. Genda et al., 2010; Oreopoulos et al., 2012; Altonji et al., 2016) or becoming more pronounced at higher ages (such as the effects of schooling, cf. Bhuller et al., 2017).

Even with rich data, analyzing the short- and long-term impacts of student absences requires a robust strategy to tease out their causal impacts from the vast array of unobserved confounding factors. Indeed, students who miss school may have poorer health, less engaged parents, and/or less inspiring teachers, which could all lead to spurious correlations between absence, education and labor market outcomes. To deal with the endogeneity of absence, we exploit two features of our data. For the short-term analysis of student absences on school performance, we exploit within-student, between-grade variation in absence and performance at two time points (grade 1 and grade 4). For the long-term analysis, we exploit the presence of siblings in our dataset and use within-family variation in absence and long-term outcomes to purge the correlation between them from all family-level time-invariant factors. We present a series of robustness tests supporting the identification strategy and estimate bounds following Oster (2019), suggesting that biases associated with our estimates are unlikely to be neither statistically nor economically large.

Our analysis yields two main findings. First, we find a negative and significant impact of student absence on academic performance in elementary school equivalent to 4.5 per cent of a standard deviation for ten days of absence. This impact is similar to estimates presented for more recent cohorts of elementary school children growing up in the US (Goodman, 2014,Aucejo and Romano, 2016). Linking school performance to later-life earnings, we also express the effect of student absence in terms of the long-term earnings potential.

Second, estimates of the long-term effects of absence are most pronounced for our income measures: income measured at ages 35–40 and pension income, which is a proxy for lifetime earnings. Ten annual days of absence in elementary school are estimated to decrease these income measures by 1-2 per cent. The estimates of the impacts on other long-term outcomes are similar in size, but not as precisely estimated, with the exception of a significant negative impact on men's likelihood to complete secondary education. The results are consistent with a model of skill accumulation, where early losses in human capital grow into later skill deficiencies, which affect educational achievement, employment and income, but are only large enough to be picked up precisely on later outcomes in the life-cycle.

Our paper relates to a broad literature examining the impact of instructional time on educational achievement and adult outcomes. Although school absence is an important determinant of the total amount of time spent in school, most studies exploit exogenous variation in the length of the school year as source of exogenous variation in instructional time (see e.g., Leuven et al., 2010, Pischke, 2007, Agüero and Beleche, 2013, Fischer et al., 2019 using laws and law changes; Carlsson et al., 2015, Fitzpatrick et al., 2011 using variation in test dates; Marcotte and Hemelt, 2008, Marcotte and Hansen, 2010, Hansen, 2011 using unscheduled school closures.)

Regarding individual absence from school, three studies, Goodman (2014), Aucejo and Romano (2016), and Liu et al. (2019), analyze the effects of school absence on educational achievement in the US. Using Massachusetts data (2003–2010) for students attending grade 3 onwards and North Carolina data (2006–2010) for grade 3 to 5 students respectively, Goodman (2014) and Aucejo and Romano (2016) show that school results are negatively affected by absence. Both studies control for institutional heterogeneity using school, teacher and individual fixed effects, as we do. Liu et al. (2019) estimate the impact of absences during secondary school on educational outcomes using data from a Californian school district (2002–2013). They use between-subject, within-individual variation in absence to identify the impact of absence on contemporaneous achievement and estimate bounds around the impact of absence on high school graduation and college enrolment using the method of Oster (2019).

We contribute to the above literature by providing new evidence on the effect of student absence as one determinant of instructional time. Our paper is the first to present estimates of the impact of days of absence outside the US and on adult and later life outcomes. The literature examining the effect of region- or school-level changes in instructional time suggests that institutions and the educational system are relevant factors for observed effects (see, e.g., Pischke, 2007; Galama et al., 2018; Gathmann et al., 2015). Sweden makes a particularly interesting case in comparison to the US, given that its labor market was characterized by active labor market policies and compressed wages (Erixon, 2008) and embedded in a Social Democratic welfare state providing comprehensive social insurance against health and social risks that workers face (Bergh, 2014).

Our results show that these innovations to the literature matter for our understanding of the impact of school absences. Considering effects throughout the life-cycle sheds new light on previous findings regarding the role of school absence and indicate that the short-term human capital losses manifested as impacts on test scores translate into long-term penalties in the labor market.

2 Background and data

2.1 The Swedish education system

In the 1930s and 1940s, all children in Sweden were required to attend public school, *Folkskola*, starting at the age of seven. Education was free and and co-educational. The first four grades of *Folkskola*, which we refer to as elementary school, were mandatory. Admissions to secondary school was very selective and depended on academic performance, and the minority of student who progressed to secondary school generally matriculated after grade 4. Students not pursuing post-compulsory schooling had to remain in *Folkskola* for another two years.[1]

The responsibility of providing elementary education was decentralized to the 2,400 school districts, but the Ministry of Ecclesiastical Affairs provided nationwide standards, for example for curriculum design and grading. Three academic subjects were taught in elementary school: math, reading and speaking, and writing. The government established grading principles, dictating that teachers should reward the quality of knowledge and regularly take notes to ensure that grades reflected performance through the year (SOU, 1942). Appendix A provides details on the school system.

As their main organizational tool, teachers kept daily records of students' performance, absence and reasons for absence in an exam catalog (Appendix Figure A1). For every school year, teachers noted the total number days of absence by type and final grades by subject. To conduct our analysis, we digitized end-of-year information from these catalogs and linked this information to administrative data on long-term outcomes.

2.2 Data sources

The base data is a census of children born 1930–1935 in a representative sample of 133 out of about 2,400 Swedish parishes. We construct our analysis datasets by combining this information with a number of historical and administrative sources described below. We provide information about the representativeness of the sample in Appendix B.1. Appendix B.2 defines all the variables included in the analysis.

Church records. The church records contain the child's name, gender, the date, parish and location (hospital or home) of birth, whether the birth was a singleton, and the mother's marital status at child's birth. These records also include information on the parents' names and occupations at the time of the child's birth. We create an indicator for maternal employment status and indicators for whether the father was an agricultural, a production or a service worker. We use all these variables as controls in our analysis (see summary statistics in Appendix Table B2).

[1]During elementary school our cohorts were exposed to two educational reforms, rolled out across municipalities between 1936 and 1948. One reform expanded instructional time per school year from 34.5 to 36.4 and 39 weeks. We control for the number of days of the school year in all specifications. The second reform introduced a mandatory seventh grade of *Folkskola*. While about half of the individuals in our sample were affected by the reform, only 6% of the sibling pairs were affected differently. Controlling for the reform does not change our point estimates, see column 6 of Table 4.

School records. Schooling information comes from exam catalogs kept in historical archives, with yearly student-level absence by type (sickness and non-sickness) and grades by subject (math, reading and speaking, writing) in grades 1 and 4 (years 1937–1947). Grade 1 and 4 are pivotal as grade 1 is the first occasion academic performance can be observed and grade 4 represents the last before some students proceed to secondary schooling. Grade points range from 1 (lowest) to 15 (highest). To facilitate interpretation we standardize the grade points to have mean 0 and standard deviation (SD) 1.

School records were matched to church records using information on parish, birth date, and full name. We mitigate data lost on account of migration out of birth parish by tracking migrants and collecting school records from destination parishes (see Appendix B.3 for details regarding the tracking of migrants and the degree of selection bias possibly arising from attrition).

Final education. Information on highest educational level completed comes from the 1970 population census (SCB, 1972), when individuals are aged 35–40. We create a binary indicator for whether an individual attains more than *Folkskola*.

Labor market outcomes. Information about employment at ages 25–30 and 35–40 comes from the 1960 and 1970 population censuses (SCB, 1962, 1972). For each age group, we construct employment variables that equal 1 if the individual works at least part-time, and 0 otherwise. From the 1970 census we also have labor market income when individuals were in prime working age. The income measure has imputed zeros for individuals who were not employed.

Pension income. From tax registers we measure average annual pensions 2003–2008, when our youngest cohort is aged 68–79. Because for our cohorts full pensions require thirty years of contributions and is based on the fifteen highest income years (Sundén, 2006), this measure is a proxy for lifetime earnings. That pensions is less sensitive to year-to-year fluctuations in labor supply than annual earnings is a desirable feature, especially for women.[2]

Death records. Church records and the Swedish Death Index (Federation of Swedish Genealogical Societies, 2014) provides the exact date of death for all individuals that passed away. We generate indicators for whether the individual died before 1960, 1970 and 2003.

For adult outcomes, we match individuals in the matched schooling data to the different registers using either full name, sex, birth date and parish of birth, or a unique social security number

[2] Appendix D provides details about the pension system and related rules.

(see Appendix B.4 for details about the matching procedure). Appendix Table B5 reports the number of individuals, siblings and families included in our analyses.

2.3 Descriptive statistics

Table 1 presents descriptive statistics for our long-term outcomes, and Appendix C.1 presents descriptive statistics for absence by type and school achievements. In grade 1, the average number of missed days is 11.1 days (median 7 days) versus 11.6 (median 8 days) in grade 4. Despite a very different context, the distribution of total days of absence is comparable with that reported in recent US studies of absence in elementary school (Goodman, 2014; Aucejo and Romano, 2016). We observe a slightly higher density of very high number of absence days (Appendix Figure C1), but unlike Goodman (2014) who excludes observations with more than 60 days of absence, we do not cap absence days.[3] Most absences are sickness absences. The average number of missed days for other reasons than sickness is 1.7 in grade 1 and 3.3 in grade 4.

Average grade point in math, reading and writing, and speaking is higher in grade 4 than in grade 1. In line with national guidelines (cf. SOU, 1942), only few students receive a very low or a very high grade point, and the variance of the grade points is higher in grade 4 than in grade 1 (Appendix Figures C3–C4). The selective nature of the education system is reflected by the fact that only 12 per cent of our sample has more than *Folkskola* education.

The cohorts studied were born between 1930 and 1935 and entered the labor market around 1950.[4] Appendix C.2 provides insights into the most prevalent occupations of our cohorts and shows that there was a high degree of gender segmentation in the labor market. Employment and earnings measured at ages 25–30 and 35–40 reflect such segmentation. As expected pensions are more equal between genders than earnings are.

Our main outcomes are all negatively correlated with total days of absence (see figures in Appendix C.3). While these correlations point to a potentially negative effect of school absences

[3] Instead we winsorize our data at the top 98^{th} percentile of the distribution of days of sickness and non-sickness absence.

[4] The cohorts under review were born during the Great Depression and were in elementary school during World War II. Although the depression was not as severe in Sweden as it was in many other countries, we re-estimate our model for long-term outcomes, controlling for local economic conditions (poverty rate, taxable income per capita, and the annual change of the latter) in the year of birth, see column 7 of Table 4. These estimates are extremely similar to our main estimates and, if anything, more precise. WWII is unlikely to have disrupted either the education or the archiving of school records. Sweden was neutral in WWII and there was an oversupply of teachers (Paulsson, 1946). We have not found historical sources suggesting that WWII caused disruptions in education and the probability that we found exam catalogs in archives was the same for years during and not during the War.

on short- and long-term outcomes, they are obviously not evidence of a causal link. To start exploring the extent of selection, we look at how total days of absence vary across groups defined by different observable characteristics (see Appendix Figure C9). Overall, these statistics show limited signs of selection based on these observables. Whether pupils positively or negatively select into absence is also unclear. As we turn to describe, our identification strategies exploit within individual or within family variation in absence and outcomes. We show there is relevant such variation in Appendix Table C1 to consider performing such analyses.

Table 1: Descriptive statistics on long-term outcomes

		Mean				
	Age range	All	Female	Male	# obs	% female
Education						
More than *Folkskola* (in per cent)		11.8	12.1	11.4	5,976	49.6
Employed (in per cent)						
in 1960	25–30	65.7	36.6	94.1	7,434	49.4
in 1970	35–40	74.3	55.9	92.6	5,976	49.6
Earnings (in Swedish krona, current values)						
Labor market income 1970	35–40	19,275	10,039	28,475	5,886	49.9
Pensions 2003–2008, if>0	68–79	160,237	138,568	183,468	4,772	51.7

Notes: Own calculations. The numbers refer to the individuals in the siblings sample (second row of Appendix Table B5) we are able to match to the long-term outcomes. The age range gives the individual's age at which the variable is measured. Education is taken from the census 1970 but is likely completed schooling for most individuals. The education indicator takes the value 1 if the individual has more than compulsory *Folkskola* education, and 0 otherwise. Employment in 1960 and 1970 is taken from the census information in these years and take the value 1 if the individual is employed, and 0 otherwise. Labor market income 1970 is based on census 1970, unemployment enters the labor market income as zero. Pensions information is taken from tax registers and averaged over 2003–2008. Zero pension are dropped (very few cases). Labor market income and pensions are measured in Swedish krona.

3 Empirical strategy

3.1 Identification of short-term effects

While the main focus of this paper is on the long-term effects of school absence, a natural starting point is to estimate the contemporaneous effect of absence on school performance. This also allows us to compare effects with those found for the current US context.

Our aim is to identify the effect of the number of days of absence in one year on academic achievement measured at the end of that year. We exploit the fact that we observe performance and absence in two grades (1 and 4) in order to control for individual-specific time-invariant unobservable factors. We estimate the following equation:

$$Y_{istg} = \beta_0 + \tau D_{ig} + Q_{stg}\beta_2 + S_{sg} + T_{tg} + \delta_i + u_{istg}, \tag{1}$$

where Y_{istg} is grade g performance of a student i attending school s taught by teacher t, and D_{ig} is the number of days student i was absent from school in a grade. Q_{stg} is a set of school/grade-specific controls, S_{sg} is a grade g school fixed effect (FE), T_{tg} a grade g teacher FE, and δ_i is an individual FE.[5]

As argued by Bond and Lang (2013), which measure of educational achievement is used can significantly alters the conclusion of the analysis. To address this issue and provide economically meaningful interpretations of our estimated effects of absence on achievement, we anchor the grade point scale to pensions, a policy-relevant outcome measured in a meaningful metric (Swedish krona). Appendix E provides details on the anchoring procedure and reports estimates of the anchoring equations.

The main identifying assumption in model (1) is that unobserved grade- and child-specific factors affecting student i's achievement in grade g are uncorrelated with the child's absence, conditional on the observables and fixed effects included in the model. The assumption is the same made in Aucejo and Romano (2016), but stronger than the assumption required in the design of Liu et al. (2019) who exploit within-grade, between-subject variation in absences to control for time-varying individual-level unobserved shocks.[6] Given the main focus of the paper on long-term outcomes and the fact that this strategy has been used in several related papers, we refer the reader to Appendix F.3 for a discussion of threats to its validity and robustness checks.

3.2 Identification of long-term effects

Our main analysis focuses on the effect of school absences in elementary school on adult outcomes. We define our treatment variable as the average number of days of absence in grades 1 and 4. Since the average days of absence in elementary school are fairly stable across all grades,

[5]The specification assumes homogeneous effect of absence in grades 1 and 4 in order to interpret τ as the marginal effect of a day of absence. This assumption cannot be tested in the context of the individual FE model, but it can be tested in the sibling FE model. We report results of such test in the lower panel of Appendix Table F4, which shows that the effects of absence in grade 1 and in grade 4 are statistically indistinguishable.

[6]On the other hand, Liu et al. (2019) require an absence of spillover effects across subjects for identification.

we can think of the treatment variable as closely measuring the average yearly number of days of absence in each year throughout elementary school.[7]

Our research design exploits the fact that we can identify siblings in our data. We propose to identify the impact of school absence on long-term outcomes from within-family variation and estimate the following equation:

$$W_{if} = \beta_0 + \tau D_i + X_i\beta_1 + \delta_f + v_{if}, \tag{2}$$

where W_{if} is an outcome of individual i of family f. X_i is a set of individual-specific time-invariant controls, including demographic characteristics, grade 1 school identifier, grade 1 teacher identifier, as well as information about class size, lowest and highest grade taught to students in the same classroom and length of the school year in weeks, averaged across grade 1 and 4. We also control for a dummy if the individual changes school or teacher between grade 1 and 4. δ_f is a family FE.[8]

The advantage of this strategy is that, in contrast with the bounding method used in Liu et al. (2019), it allows us to point identify the effect of absence on long-term outcomes. However, for the parameter τ to have a causal interpretation, it requires that any child-specific unobservable that does not have the same additive effect on outcomes for both siblings is uncorrelated with the child's absence. We discuss possible threats to identification in Section 4.3, where we present the results of a number of exercises to gauge the research design's validity.

4 Results

4.1 Short-term effects of school absence

Column (1) of Table 2 reports OLS estimates of parameter τ in equation (1) and shows that one additional day of absence is significantly associated with a 0.51 per cent of a SD decrease

[7] We have not collected information on grades 2 and 3 systematically, but some exam catalogs cover students in several grades. Average days of absence in grades 2 (10.7 days) and 3 (11.9) are close to the averages for grades 1 (11.1) and 4 (11.6). We also estimate the model allowing for a different effect of grade 1 and grade 4 absences on long-term outcomes. In the sibling FE model, estimates of these two effects are not statistically different from each other (Appendix Table G2).

[8] Appendix Table C1 show that a large fraction of variation in absence and performance in the data comes from within family variation, which provides confidence that there is sufficient variation to consider using a sibling FE strategy.

in performance. The individual FE estimate in column (2) is slightly smaller (0.0045), but also statistically significant at the 1 per cent level.[9]

Table 2: Estimates of the short-term effect of school absence on academic performance

	(1)	(2)
	OLS	Individual fixed effects
Average grade points in units of SD (mean: 0, SD 1)		
Days of absence	-0.0051^{***}	-0.0045^{***}
	(0.0007)	(0.0013)
Average grade points in units of pension (anchoring by grade, mean pension in sample: 160,237 Swedish krona)		
Days of absence	-54.7344^{***}	-16.1192
	(7.4068)	(20.0252)
Average grade points in units of pension, by gender (anchoring by grade and gender, mean pension in sample: 138,568 Swedish krona for women and 183,468 Swedish krona for men)		
Days of absence	-69.2581^{***}	-37.5660^{**}
	(9.2558)	(18.4195)
# observations	14,066	8,934
# individuals/families		4,467

Notes: Each cell reports the coefficient associated with days of absence in separate regressions where the dependent variable is the variable indicated in the first row of each panel. The dependent variable in panel 1 is average performance over math, reading and speaking, and writing, standardized to have mean 0 and standard deviation 1. In panel 2, it is a measure of average grade points in units of pensions, where the relationship between grade points and pensions is estimated separately for grade 1 and grade 4, see Appendix E. The third panel repeats the anchoring of grade points in units of pensions, but estimates the relationship between grade points and pensions separately by school grade and gender. Both OLS and individual FE models control for grade, range of grades instructed in the same classroom, and length of the school year in weeks. The OLS specification also controls for female, born out of wedlock, twin birth, mother employed at the time of birth, born in hospital, full sets of fixed effects for the year and month of birth, year and month interactions, age, parent's year of birth, and family's socio-economic status based on the first-digit HISCO code of the father. Standard errors clustered at the parish level in parentheses. Significance: $^{*}p \leq 0.1$, $^{**}p \leq 0.05$, $^{***}p \leq 0.01$.

Assuming linearity, the effect of ten days of absence – the average in our sample – corresponds to 4.5 per cent of a SD decrease in performance. Despite analyzing absence in a very different context and literally in another century, our results measured in SD units are comparable to those in the related literature. Using recent US data, Goodman (2014) finds an effect of 0.8 per cent of a SD in math and English, and Aucejo and Romano (2016) find effects of 0.55 per cent of

[9] Appendix Table F1 reports estimates for control variables. Appendix Table F2 shows negative and significant estimates of the effect of absences on performance for each of the three subjects included in the aggregate grade point measure and for alternative aggregate measures.

a SD in math and 0.29 per cent in reading. Appendix F.1 presents evidence strongly suggesting that the relationship between absence and academic performance is linear. Moreover, we show that there are no significant differences in the short-term impact of absences between gender and across individuals from different socio-economic groups (Appendix F.2).

By anchoring student performance to pension incomes, we translate the short-term effect of absence on school performance into its effect on earnings potential. Table 2 (second and third row) reports the results. In the individual FE specification, the impact of ten additional absence days on school performance translates into a non-significant decrease in earnings potential of 16 SEK, equivalent to a 0.1 per cent decrease in average pension income. We obtain similarly-sized effects when anchoring by grade and gender (third row), and when using earnings at age 35–40, as anchor.

Appendix F.3 presents results of tests we conduct to probe the validity of the individual FE strategy to recover the causal effect of absence on performance. These results strongly suggest that our short-term estimates of absence are likely robust to the presence of individual-level time-varying unobserved heterogeneity.

4.2 Long-term effects of school absence

Table 3 reports the effects on long-term outcomes of the average days of absence across grades 1 and 4 in columns 1 (OLS) and 3 (siblings FE). To ease interpretation, we also report the effect of ten days of absence relative to the mean of the outcome considered in columns 2 and 4. We calculate this by multiplying the coefficient by five (the treatment variable measures average number of days of absences across two grades) and dividing it by the outcome mean.

A first observation is that the OLS and sibling FE estimates are similar both in terms of magnitude and statistical significance. This aligns with one of the conclusions from the short-term analysis that selection into absence based on unobservables may not be particularly prominent in our context.

Looking at sibling FE estimates of Table 3, we find that ten days of absence in elementary school lead to a statistically insignificant 2.2 per cent reduction in secondary school completion relative to baseline. Given the gender differences in educational attainment that existed during this period, impacts could vary by gender. Indeed, the effect of absence on secondary school completion is negative and statistically significant for men, while it is close to zero for women

(Appendix Table G1). The negative effect of elementary school absence on secondary schooling for men is consistent with secondary school admissions depending on elementary school performance.

Table 3: Long-term effects of school absence

	(1)	(2)	(3)	(4)
	OLS		Siblings fixed effects	
	coeff.	rel. size	coeff.	rel. size
More than Folkskola *(1=yes)*				
Absence (average, grades 1 and 4)	−0.0007	−0.0297	−0.0005	−0.0215
	(0.0005)		(0.0009)	
Employment 1960 (1=yes)				
Absence (average, grades 1 and 4)	−0.0012*	−0.0088	−0.0007	−0.0053
	(0.0007)		(0.0013)	
Employment 1970 (1=yes)				
Absence (average, grades 1 and 4)	−0.0008	−0.0050	−0.0020	−0.0133
	(0.0007)		(0.0014)	
Labor market income 1970				
Absence (average, grades 1 and 4)	−54.2973**	−0.0141	−80.7042**	−0.0209
	(22.7088)		(37.2151)	
Pensions 2003–2008				
Absence (average, grades 1 and 4)	−330.0940***	−0.0103	−396.0887**	−0.0124
	(71.2631)		(169.3640)	

Notes: Each panel reports the coefficient associated with total days of absence (average over grades 1 and 4) in separate regressions where the dependent variable is indicated in the left column. Controls include female, born out of wedlock, twin birth, born in hospital, mother employed at the time of birth and sets of fixed effects for the year and month of birth, year and month interactions. Specifications also controls for class size, the lowest and highest grade taught to students in the same classroom, and length of the school year in weeks, averaged across grade 1 and 4. Models also control for school and teacher fixed effects, and a dummy if the individual changes school or teacher between grade 1 and 4. The OLS models further control for parent's year of birth, and family's socio-economic status based on the first-digit HISCO code of the father. Number of observations: More than *Folkskola* 5,976, employment 1960 7,434, employment 1970 5,976, income 1970 5,886, pensions 4,772. Standard errors clustered at the parish level in parentheses. Significance: $^{*}p \leq 0.1$, $^{**}p \leq 0.05$, $^{***}p \leq 0.01$.

Next, we turn to the effects of absence on labor market outcomes. Impacts on employment are not statistically significant, but the point estimates suggest that impacts on employment may grow slightly with age, from a 0.5 per cent reduction in employment at ages 25–30 to a 1.3 per cent reduction ten years later. As shown in rows 4 and 5 of Table 3, a one-day increase in the average number of days of absence in elementary school leads to a 80 SEK reduction in earnings in 1970 and a 396 SEK reduction in pensions;[10] both estimates are significant at the 5 per cent level. Relative to the means of these variables, the impacts are fairly similar in magnitude to each other, between 1-2 per cent reduction in income.

[10] Appendix Table D1 shows that pension results are robust when controlling for the receipt of a widow pension.

As with the short-term outcomes, we explore whether the effects of school absence on long-term outcomes are non-linear but find no strong evidence that it is the case (Appendix Figure G1). We also find no strong evidence that the effect of absence depends on whether absence happened in grade 1 or grade 4 (Appendix Table G2).

Overall, our estimates point to a consistently negative effect of absences in elementary school on economic outcomes through the life-cycle, with more pronounced and statistically significant impacts on educational attainment for men and on income both for genders. In terms of magnitude, the effects are larger than what is implied by the short-run effects of absence on school performance.[11] Moreover, the effects become slightly larger and more significant when measured further into the life-cycle. Taken together, these results are consistent with a dynamic model of skill accumulation where skill deficits resulting from absences in elementary school translate into lower skill levels progressively accumulating over the life-cycle. These results also underline the importance of measuring impacts of absence at various points in the life-cycle.

4.3 Robustness checks

The interpretation of the above results as causal effects of absences on long-term outcomes rely on the assumption that school absences are uncorrelated with unobservable determinants of long-term outcomes that vary across siblings. There are several threats to the validity of this assumption. First, long-term outcomes could be affected by child-specific unobserved endowments, which may also affect children's likelihood to miss school. For example, one sibling may be born frailer, have lower grit and work ethics than the other, which would make her more likely to both be absent and have worse adult outcomes than her sibling. Second, long-term outcomes could be affected by idiosyncratic, child-specific shocks that directly affect the incidence of absence and have long-lasting effects on adult outcomes (over and beyond the absence). An obvious example are health shocks during elementary school, which makes one sibling (but not the other) particularly sick one year and have long-lasting effects on his/her adult outcomes. Other types of relevant shocks may include changes to the institutional and/or economic environments over time, which affect siblings differently because they are born in different years.

[11] Specifically, the (sibling FE) effect of school performance, measured as average grade points in grade 1 and 4, on secondary schooling completion is about 0.07 (Appendix Table G3). Multiplying this estimate by the estimated effect of absence on school performance of −0.0045 (Table 2), we would expect an effect of absence on enrollment of −0.0003 in the siblings FE specification. In contrast, our direct siblings FE estimate in Table 3 (−0.005) is considerably larger than the indirect estimate.

Table 4: Robustness of the effect of absence on long-term outcomes

A. Estimates of the effect of absence in different sibling fixed effect specifications							
	Baseline results (Table 3)	i) Grade 4 effect with and without controlling for grade 1 performance		ii) Differentiating for reason of absence	iii) Same sex siblings	iv) Contr. for compulsory schooling	v) Contr. for local conditions at birth
Reported coefficient on:	Avg., gr. 1 and 4	Grade 4 absence	Grade 4 absence	Avg., gr. 1 and 4	Avg., gr. 1 and 4	Avg., gr. 1 and 4	Avg., gr. 1 and 4
More than Folkskola (1=yes)	-0.0005	-0.0010	-0.0011	-0.0006	-0.0011	-0.0005	-0.0004
	(0.0009)	(0.0008)	(0.0008)	(0.0007)	(0.0016)	(0.0009)	(0.0012)
Employment 1960 (1=yes)	-0.0007	0.0016	0.0016	-0.0005	-0.0023	-0.0007	-0.0016
	(0.0013)	(0.0011)	(0.0011)	(0.0010)	(0.0019)	(0.0013)	(0.0014)
Employment 1970 (1=yes)	-0.0020	-0.0016	-0.0016	-0.0015	-0.0048	-0.0019	-0.0039***
	(0.0014)	(0.0010)	(0.0010)	(0.0012)	(0.0030)	(0.0014)	(0.0013)
Labor market income 1970	-80.7**	-61.1	-62.1	-50.0	-117.4	-81.1**	-86.0*
	(37.2)	(45.4)	(45.2)	(32.7)	(85.14)	(37.3)	(45.9)
Pensions 2003–2008	-396.1**	-284.9**	-248.2**	-319.1***	-689.2*	-395.8**	-572.1***
	(196.4)	(119.4)	(105.0)	(121.5)	(405.4)	(169.2)	(199.8)
B. Additional results: Effects of absence (average, grades 1 and 4) on mortality							
Passed away before 1960 (1=yes)	0.00001						
	(0.00030)						
Passed away before 1970 (1=yes)	0.00036						
	(0.00049)						
Passed away before 2003 (1=yes)	0.00056						
	(0.00078)						

Notes: Each panel reports the coefficient associated with total days of absence (average over grades 1 and 4) in separate regressions where the dependent variable is indicated in the first column. Controls as reported in Table 3. Panel A shows estimates in different sibling fixed effects specifications: (i) grade 4 effect with and without controlling for grade 1 performance (see Appendix Table G6 for grade 1 performance coefficients), (ii) differentiating for different reasons of absence (see Appendix Table G7 for non-sickness absence coefficients) (iii) estimates using same sex siblings (iv) controlling for school reforms and (v) controlling for local conditions at birth (poverty rate, taxable income per capita, economic development (the year-to-year change in the taxable income per capita), all assessed at the time of birth). Panel B shows estimates on absence on mortality by 1960, 1970 and 2003. Standard errors clustered at the parish level in parentheses. Significance: $^{*}p \leq 0.1$, $^{**}p \leq 0.05$, $^{***}p \leq 0.01$.

Below we discuss a series of empirical exercises to help us gauge the likely salience of these different threats to the validity of our research design. First, we re-estimate the short-term effect of absence on educational achievement using the sibling FE strategy and compare it to the individual FE estimates of Table 2. As reported in Appendix Table G4, the two estimators yield very similar estimates of the impact of absence on achievement in elementary school. This suggests that child-specific unobservable determinants of educational achievement that vary between siblings are unlikely to be correlated with absences. While this does not guarantee that the same is true of unobservable determinants of long-term outcomes, it does reinforce the message that selection into absence based on unobservables is unlikely to be strong.

We then estimate the effects of grade-4 absence on grade-1 performance. If sibling FEs fully account for unobserved child-specific time-invariant factors, future absence should have no power in predicting grade-1 performance. Appendix Table G5 shows that, while grade-4 absence and grade-1 performance are negatively correlated in the OLS model, the grade-4 absence coefficient drops and becomes insignificant in the siblings FE model.

The next exercise uses the idea that, if the siblings FE specification does not control for important unobservable (fixed or time-varying) determinant of long-term outcomes, the siblings FE estimate of grade-4 absence on long-term outcomes would change through the inclusion of grade-1 performance, to the extent that grade 1 performance captures these unobservables (e.g., idiosyncratic work ethic or health shock occurring before grade 1). The results are reported in the second and third columns of Table 4 and show that the estimated effect of grade-4 absence on long-term outcomes remains virtually unchanged through the inclusion of grade-1 performance.

We then conduct two exercises to assess the possibility that individual-specific health shocks during childhood confound our long-term estimates. First, we test whether absence due to sickness affects long-term outcomes *differently* from absence due to other reasons. If health shocks simultaneously determined absence and long-term outcomes, we would likely find that absence due to sickness is more detrimental than absence due to other reasons. We test this hypothesis by re-estimating our model also controlling for the average number of non-sickness days across grades 1 and 4. We report the coefficients for any absence in the fourth column of Table 4 (see Appendix Table G7 for non-sickness absence coefficients). Across all long-run outcomes, we cannot reject that the coefficient on the average days of non-sickness absence is equal to the coefficient on the average days of (any) absence, which suggests that the long-term effects of absence in Table 3 are more likely to be driven by the loss of instructional time rather than by a health shock.

Our second test for confounding health shocks looks directly at whether school absences have an effect on long-term health, as measured by mortality. To do so, we re-estimate our sibling FE model, this time using indicators for mortality at three time points (1960, 1970 and 2003) as outcomes. We cannot reject that the coefficient on absence in any of these regressions is zero (see Panel B of Table 4). Again, this suggests that the effect of absence we identify through the sibling FE strategy is likely to capture the effect of losing instructional time rather than the effect of a health shock associated with the absence.

Given that the labor market was gender segmented we also test whether sibling sex composition matters. Column 5 of Table 4 reports estimates for the sample of families with same sex siblings, which are very similar to our main results and suggest that low female labour force participation is unlikely to bias the analysis using the entire sample. The last two columns of Table 4 show that the baseline results are also robust to controlling for siblings' differential exposure to the instructional time school reform and to economic environments at birth.

4.4 Bounds

The above robustness checks all suggest that the identifying assumption underlying the sibling FE strategy is unlikely to be violated. However, given that it does remain an untestable assumption, we conclude our analysis by bounding our estimates, using the approach of Oster (2019) which is also implemented in Liu et al. (2019). As we describe in Appendix H, this approach exploits observables in order to gauge how unobservables may affect the estimates and is therefore only helpful to the extent that the selection on observables is informative of the selection on unobservables one might suspect. As our empirical strategy controls for a breadth of factors that are likely to affect absence and long-term outcomes, the bounding approach seems useful here.

Column 1 ("short regression") of Table 5 reports the coefficients associated with absence in a regression without any controls. Column 2 ("intermediate regression") replicates the sibling FE estimates from Table 3. Column 3 (4) reports the bound under the assumption that the unobservables move the point estimate in the same (opposite) direction and by the same magnitude as the introduction of the observables does. Despite the R^2 of the intermediate regression

Table 5: Oster bounds for the long-term effects of absence in school

	(1)	(2)	(3)	(4)
	Coefficient of absence		Selection bias	
Dependent variable	Short regression	Intermediate regression	Same direction	Opposite direction
More than *Folkskola*	-0.0009^{*}	-0.0005	-0.73850	-0.00014
	(0.0005)	(0.0009)		
	[0.00]	[0.41]		
Employment 1960	-0.0013	-0.0007	0.00306	-0.00177
	(0.0008)	(0.0013)		
	[0.00]	[0.60]		
Employment 1970	-0.0008	-0.0020	-0.04959	-0.00155
	(0.0008)	(0.0014)		
	[0.00]	[0.50]		
Labor market income 1970	-65.4768^{***}	-80.7042^{**}	-107.88081	-73.15848
	(22.3882)	(37.2151)		
	[0.00]	[0.64]		
Pensions 2003–2008	-384.5674^{***}	-396.0887^{**}	-90.55889	-423.49521
	(81.8270)	(169.3640)		
	[0.00]	[0.59]		

Notes: Column 1 gives the coefficient associated with absence in the short regression where the outcome variable (stated on the left) is regressed on absence and an intercept (without any controls), denoted by $\dot{\beta}$ in Appendix H. The intermediate regression in column 2 reiterates the siblings FE specification estimates of the coefficient associated with absence (reported in Table 3 and corresponding to $\tilde{\beta}$ in the model 2). Columns 3 and 4 report the bounds (β^{*}) when selection on unobservables is of the same magnitude as selection on observables and goes in the same direction ($\delta = 1$) and the opposite direction ($\delta = -1$) respectively. The R^2 of the short and intermediate models are reported in brackets in columns 1 and 2 respectively. Number of observations as in Table 3. Standard errors clustered at the parish level in parentheses.

being higher than 0.5 for all outcomes, there is remarkably little change between columns 1 and 2. This modest influence of selection on observables is reflected in the estimated bounds: all bounds are negative, with the exception of the bound for the effect of absence on 1960 employment in Column 3. Thus even under the extreme scenario that our siblings FE model fails to account for as much selection on unobservables as it does account for selection on observables, we can't reject there would be an negative effect of absence on long-term outcomes.[12]

5 Conclusion

School absences are an important but often overlooked determinant of instructional time. To date, little is known about the long-run impact of students missing school, and the only studies providing causal evidence of the impact of student absence on academic performance focus on the US. The contribution of this paper is to estimate the impact of student absence in elementary

[12] Bounds for short-term effects in Appendix Table H1 underline this conclusion.

school on short- and long-term outcomes for a non-US context by using a unique combination of historical records and administrative datasets from Sweden.

Our analysis shows that absence in elementary school has a significant and negative impact on student performance: increasing total absence in one grade by ten days leads to a reduction in grade point average of 4.4 per cent of a standard deviation, an effect of comparable magnitude to that found in the US. For men, this immediate impact on school performance spills over onto secondary school admissions, which were based on elementary school performance. This effect is at least as large as one would expect based on the effect of absence on performance – even though we are unable to attribute it to a certain school grade.

For other long-term outcomes, we find consistent evidence that there is a penalty to absence in elementary school: estimates have the expected negative sign for all long-term outcomes, and they are statistically significant for earnings along the entire life-cycle. Together, the short- and long-term effects of absence suggest that a key mechanism underlying these results is the effect of instructional time losses on early levels of skills, which accumulate over the life-cycle and eventually create non-negligible income penalties.

Our research starts filling an important gap in the evidence base on the long-term impacts of school absence and thereby inform policy discussions about high rates of absences around the world. Our findings hone in on the impact of individual absences, as opposed to school closures. In this light, they may only partly be relevant to predict the long-term effects of the school closures during the early phases of the COVID-19 pandemic. But, as school absenteeism becomes increasingly driven by individual students self-isolating, our results can provide useful evidence that associated learning losses may have a long-term impact if not appropriately compensated.

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Appendices

Table of Contents

Appendix A Education in Sweden in the 1930s

A.1 Schooling and the school system

Compulsory schooling in Sweden was introduced in 1842. In the 1930s and 1940s, the period during which our cohorts grew up, all children were required to attend public school, *Folkskola*, for at least six years, starting at the age of seven. In 1936 the government decided to increase compulsory schooling by one year in all districts over a twelve year period, and in 1937 the school year length was increased stepwise from 34.5 to 39 weeks (Fischer et al., 2019).[13] Students attended elementary school full time, six days a week, with instruction ending at noon on Saturdays.[14] In rural areas children in several grades were sometimes taught together. In the regressions we therefore control for the lowest and highest grade taught to students in the same classroom.

The responsibility for providing compulsory education was decentralized to 2,400 school districts, but the Ministry of Ecclesiastical Affairs provided clear nationwide standards that applied to all school districts. The most central decree was the 1919 Educational Plan (*Utbildningsplanen*), which included the full curriculum of the *Folkskola*. Instruction was generally done in classes separated by grade. When the number of students was low, schools were allowed to pool students in different grades into one classroom, so that a teacher instructed, for instance, students of grade 1 and grade 2 in the same room during the same lesson. The content of the education was grade-specific, however, as stated in the Educational Plan.

The educational system of the 1930s exhibited several features of a modern educational system – like absence of tuition fees and joint instruction of boys and girls at all educational levels (Erikson and Jonsson, 1993) – but education was very selective (Fischer et al., 2019). Students who decided to take more than compulsory education followed a tracking system and generally left *Folkskola* after grade 4 or grade 6 to enter lower secondary school (*Realskola*). All other students remained in *Folkskola* until they reached the compulsory years of schooling. From 1939 and onwards the admission to *Realskola* was based on grades received in elementary school. After four or five years of lower secondary schooling, students either entered upper secondary school (*Gymnasium*) or finished their education.

A.2 The grading system

Three theoretical subjects were taught in *Folkskola*: math; reading and speaking; and writing. A 1940 Royal Commission established precise guidelines for teachers to evaluate and grade their students' performance (SOU, 1942). For example, to assess a student's math performance, teachers were to take both the ability to solve "standard problems" and more sophisticated

[13] About half of the individuals in our dataset are affected by the reform extending compulsory schooling by one year, but only 6.5 per cent of sibling pairs have a different reform status. Although all our specifications include a full set of parish fixed effects in order to control for differences across parishes, differences in the timing of the introduction might still be unaccounted for by parish fixed effects. We therefore additionally control for years of compulsory schooling in the long-term outcomes specifications, but this does not affect our results. Regarding the length of the school year, we control for length of the school year in weeks in our specifications.

[14] Following an exception rule, schools in rural areas had the possibility to offer half-time reading (students went to school every second day or only during certain periods of the year) but this option was very limited in the 1930s and only 0.5 per cent of our sample took half-time reading.

ones into account. For reading and speaking, grades were supposed to reflect loud and silent reading and the ability to express a familiar topic in own words. For writing, grades were supposed to assess both the form and content of essays. While all students had to take math, writing, and reading and speaking, writing was not always graded in the first school year.

While exams were not standardized across the country, teachers were provided with clear grading guidance. The grading scale included seven levels, where the highest possible grade was A ("passed with great distinction") and the poorest grade was C ("not passed"). Teachers were also allowed to add a plus or minus sign in order to express the strength or weakness of the grade. While the grading scheme remained unchanged during the period of interest, the grading guidelines changed slightly. From the school year 1940/41 onward, teachers were advised to award the grade BA ("passed with credit") for an average performance.[15] Before the school year 1940/41, teachers were more likely to award a student the grade B for an average performance. The highest grade A was reserved for exceptional students and less than 1 per cent of all students should be expected to have knowledge corresponding to this level and receive this grade. The recommendation was to also have BA as the normal mark in grade 1 and grade 2, but the the recommendation to teachers were to be restrictive in the use of any high or low marks for children in these grades.

We assign to each letter grade a numerical value, ranking from 1 (lowest grade) to 15 (highest grade) also taking into account that teachers could assign a plus and a minus sign in order to express the strength or weakness of a grade. Table A1 gives the mapping of the potentially ordinal grades into these grade points. While all students had to take math, writing, and reading and speaking, writing was not always graded in the first school year. For the 31.3 per cent of students in our sample with missing writing grade points in the first grade, we calculate the average grade points using the grade points in the other two subjects.

Section 2.3 shows that the distribution of grades observed in our sample is remarkably in line with the Royal Commission's grading principles. This gives us confidence that it is a meaningful measure to compare achievement across students. We also test the robustness of our results to replacing the baseline outcome (performance measured on the 15-point grading scale) to the 7-point grading scale and into a binary indicator that takes the change in the Royal Commission's grading principles into account, see Appendix Table F3.

Table A1: Grading scale

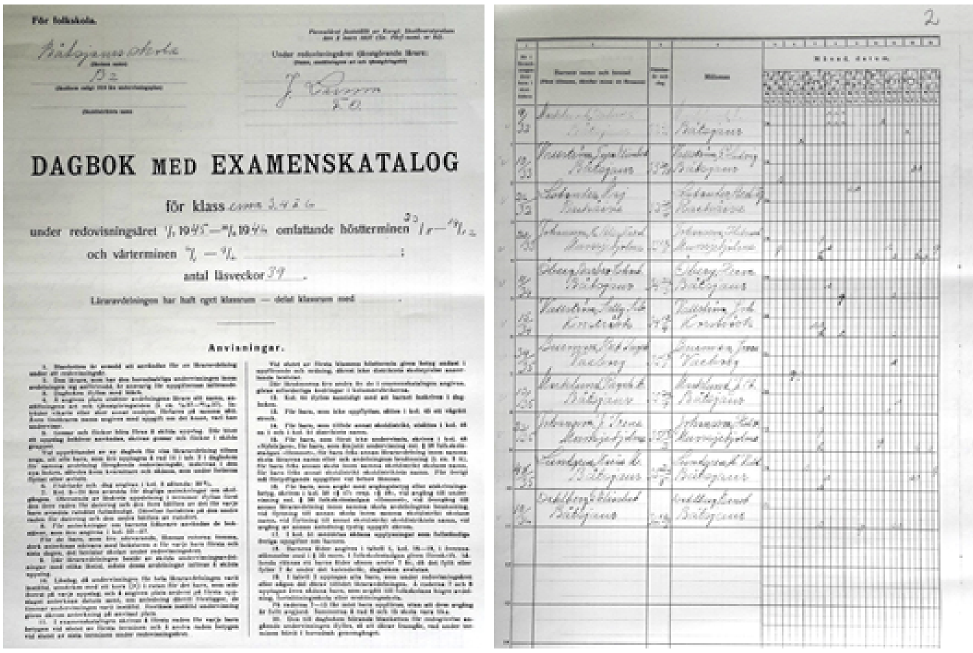
	Grade														
	Passed...														
	with great distinction		with distinction		with great credit			with credit			without credit			Not passed	
Observed symbols	A	A-	a	a-	AB+	AB	AB-	BA+	BA	BA-	B+	B	B-	BC	C
15-points scale	15	14	13	12	11	10	9	8	7	6	5	4	3	2	1
7-points scale	7		6			5			4			3		2	1

Notes: Own illustration based on historical records. The first row states the original grade point as denoted in the exam catalog. Rows 2 and 3 give our translation into numerical values on a 15-point and 7-point scale, respectively. The baseline models use the 15-point scale.

[15] One third of students of a cohort should receive a better grade and one third a poorer grade.

As their main organizational tool teachers kept daily records in an exam catalog called *Dagbok med examenskatalog*, see Figure A1. In these catalogues, the teachers recorded students' performance and absences, and noted whether absences were due to sickness, natural obstacles (e.g. heavy snowfall), inappropriate clothes and shoes, other valid reasons for absence, or no valid excuses (truancy).[16] They also included general information about the school and the school year length. Regarding student performance teachers were encouraged to take notes on the student's performance throughout the entire school year. At the end of the school year, the teachers summarized the days of absence by type and the final grades by subject in a separate column for end-of-school-year information.[17] Unlike tests that take place on a certain date, the frequent recording of student performance throughout the year reduces the likelihood that educational achievement scores are driven by teaching-to-the-test behavior of teachers and/or idiosyncratic factors on the day of the test. Moreover, because teachers kept records of performance throughout the year, the scores assigned to students at the end of the school year are unlikely to suffer from recall bias.

Figure A1: Example of an exam catalog



Notes: Pictures of an exam catalog taken in an archive in Sweden.

WWII falls in the time when we observe our sample in primary school. Sweden was neutral in the war, and there was an oversupply of teachers (Paulsson, 1946). We have not found any

[16]Although the exam catalogs include columns for several reasons for non-health related absence, teachers often only noted other absence without naming the reason. Therefore we focus only on sickness absence and non-sickness absences.

[17]Given how resource-intensive the task of digitizing historical archives is, we focused on digitizing the yearly summaries of absence only, as opposed to individual absences throughout the year. This means that the data at hand do not allow us to identify the length of absence spells and, as opposed to Liu et al. (2019), we are not able to distinguish the effect of absences earlier versus later in the school year.

historical sources suggesting that the war caused major disruptions in primary education,[18] nor do we find that the probability that we found exam catalogs in the local archives is lower during the war years. In addition, statistics on secondary school enrollment suggest there was a gradual increase of students matriculating 1930-1950, but no major jumps (Schånberg, 1993).

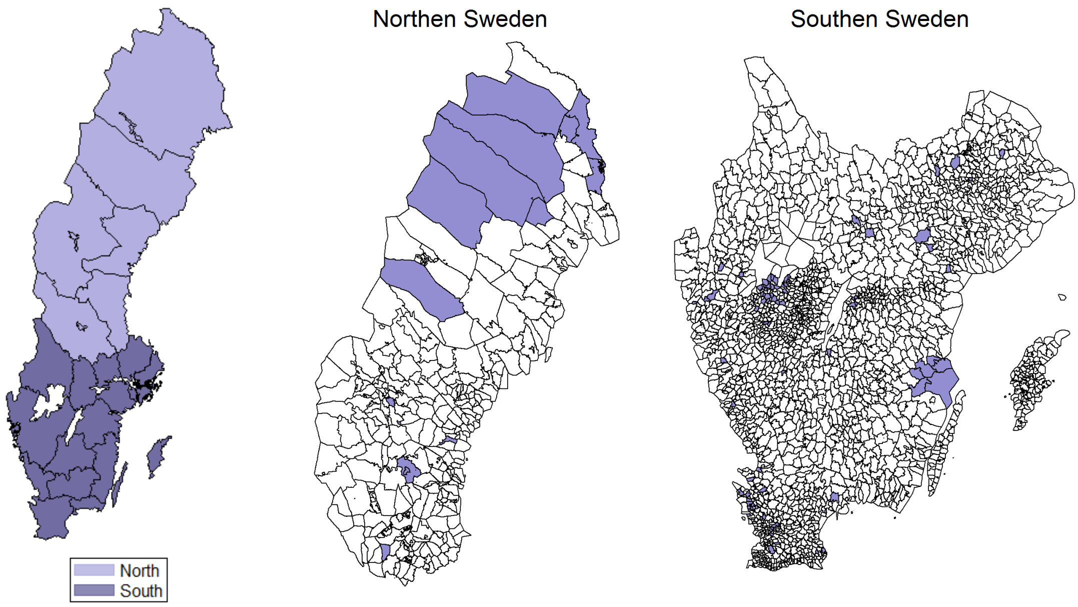
[18]In fact, about 50,000 children from Finland were sent to and educated in Sweden 1941-1944 because Sweden was much less affected by the war compared to Finland. Children spent on average two years in the country (Santavirta, 2012) and were very evenly distributed across Swedish counties (as share of total population)(De Geer, 1986). The Finnish children were taught by the regular teacher in the same class as natives but the government granted money to school districts to provide extra hours of Swedish language class for migrant children in school age (Fredriksson, 1971).

Appendix B Data and matching

B.1 The administrative church records data and its representativeness

The data we use in this paper combine several historical and administrative data sources. The base of our dataset is individual-level data from administrative church records covering all 30,150 children born between 1930 and 1935 in a representative sample of 133 out of about 2,500 Swedish parishes. The base dataset was initially used to evaluate an infant and maternal health program introduced by the Swedish Government (see Bhalotra et al. (2017) and Bhalotra et al. (2020)). For these purposes individual information in treated and matched parishes were digitized. Figure B1 presents the spatial distribution of the sample parishes across Sweden.

Figure B1: Spatial distribution of 133 sample parishes within Sweden



Notes: Own illustration. The plot on the left shows the map of Sweden in its regions (*Län*) and the plots in the center and on the right show Northern and Southern Sweden, respectively, in parishes in the time under review. The left plot indicates which regions belong to the Northern and Southern Sweden in the plots in the center and on the right. Parishes belonging to our sample are depicted darker in the plots in the center and on the right.

The base data includes information on all births as recorded in church records in the included parishes.[19] These records contain individual information on name, gender, date of birth and parish of birth. The records also provide information on whether the child was born in a hospital, whether the birth was a twin birth, and whether the child was born out of wedlock. It also includes information on the parents' occupation at the time of the child's birth, which we use to create an indicator for whether the child's mother was employed at the child's birth and indicators for whether the father is an agricultural, a production or a service worker.[20]

To evaluate the representativeness of the parishes included in our analysis compared to the country as a whole, we provide Table B1 which includes information from Bhalotra et al.

[19]Since the eighteenth century, the Swedish clergy created an information system that included all individuals in their parishes. Membership of the Church of Sweden could actively be cancelled if an individual wanted to enter another denomination, but church records nevertheless covered all citizens.

[20]To construct these indicators, we use the first digit of the Historical International Standard Classification of Occupations (HISCO) code for the fathers' occupation. The HICSO code is historical version of today's International Standard Classification of Occupations (ISCO) code, see van Leeuwen et al. (2002).

(2017) and presents summary statistics for observable characteristics from the 1930 census for all parishes in Sweden (column 1), treated and matched parishes (columns 2 and 5) and all non-treated parishes in the country (column 3). A test for balance (column 6) indicate that the treated and matched parishes are balanced on observable characteristics. Important for our purposes we also show that the standardised difference between the treatment group and the rest of Sweden (column 4) also indicate balance, which suggests that the sample of 133 parishes were representative of Sweden.[21]

Table B1: Characteristics of districts included in analysis compared to Sweden as a whole

	All (1)	Treated (2)	Other (3)	Std. Dif. (2) vs.(3)	Matched (5)	Std. Dif. (2) vs. (5)
Characteristics from the 1930 Census						
Agriculture	0.340	0.324	0.340	-0.040	0.302	0.054
Manufacturing	0.318	0.340	0.318	0.096	0.345	-0.018
Fertile Women	0.121	0.101	0.121	-0.135	0.100	0.060
Income	811	839	810	0.042	847	-0.013
Wealth	2,525	2,703	2,521	0.080	2,655	0.022
Urban	0.334	0.439	0.331	0.158	0.437	0.003
Population	6,271,266	258,418	6,004,052		160,987	

Notes: The table contains local characteristics from the 1930 census. We compare statistics for treated (column 2) and matched (column 5) parishes with national averages in column 1 (the whole of Sweden) and column 3 (averages for all non-treated parishes in Sweden).'*Std Dif.*' presents the standardised difference (cf. Imbens and Wooldridge, 2009). A standardised difference of less than 0.25 is generally viewed as acceptable.

B.2 Variable definitions

This appendix section provides the source of each variable used in the analysis and defines how it is constructed.

Information from Church Parish Records:

Birth year Birth year.

Month of birth Month of birth.

Female Dummy variable taking the value one for female child.

Twin Dummy variable taking the value one for (mono- and dizygotic) twins.

Wedlock Dummy variable taking the value one for children born to married mothers.

Hospital birth Dummy variable taking the value one for child being born in hospital.

SES Classification of head of household profession according to HISCO 9-point scale. HICSO code is historical version of today's International Standard Classification of Occupations (ISCO) code, see van Leeuwen et al. (2002). SES variables corresponds to indicators for whether the child's father was an agricultural, a production or a service worker.

[21] The programme documentation of the infant trial indicates that the National Board of Health selected areas where the program was implemented "randomly" to be representative of the country, and Table B1 confirms that they managed well with this task.

Mother employed Dummy variable taking the value one for mother of child being employed at the time of birth.

Mother birth year Mother birth year.

Father birth year Father birth year

<u>Variables from Exam Catalogues:</u>

Days of absence Days spent absent in grade 1 or 4.

Days of sickness absence Days spent absent due to sickness in grade 1 or 4.

Days of non-sickness absence Days of total absence minus days spent absent due to sickness in grade 1 or 4.

Math Mark for "math" in grade 1 or 4.

Writing Mark for "writing" in grade 1 or 4.

Reading and Speaking Mark for "reading and speaking" in grade 1 or 4.

Average grade points Grade point average of subjects "math", "reading and speaking" and "writing" in grade 1 or 4.

Class size Class size in grade 1 or 4.

Grade range Lowest and highest grade taught to students in the same classroom in grade 1 or 4.

Weeks Length of the school year measured in weeks in grade 1 or 4.

School School in grade 1 or 4.

Teacher Teacher in grade 1 or 4.

Parish Parish of residence in grade 1 or 4.

<u>Variables from 1960 Population and Household Census:</u>

Employed Dummy variable taking the value one for someone working parttime (at least 20 hours per week) or fulltime (at least 35 hours per week).

<u>Variables from 1970 Population and Household Census:</u>

More than *Folkskola* Dummy variable taking the value one for someone achieving more than compulsory Folkskola education.

Employed Dummy variable taking the value one for someone working parttime (at least 20 hours per week) or full time (at least 35 hours per week).

Labor market income Taxable labour earnings in SEK.

<u>Variables from Tax registers:</u>

Pensions Average annual pensions 2003–2008 in SEK.

Variables from the Swedish Death Index:

Passed away before 1960 Dummy variable taking value one for someone who passed away before Census enumeration in 1960.

Passed away before 1970 Dummy variable taking value one for someone who passed away before Census enumeration in 1970.

Passed away before 2003 Dummy variable taking value one for someone who passed away before 2003 (the first year with pension income information).

Variables from historical Municipality Year Books

Poverty The local headcount poverty rate in the child's birth year.

Income per capita The local taxable income per capita in the child's birth year.

Growth The annual change of the local taxable income per capita in the child's birth year.

B.3 Matching of base data and schooling data

Individual schooling information was collected from historical archives in each parish. Specifically, we collected the exam catalogs in which teachers made systematic notes about types of absence and reported grades for each student, for each primary school in the 133 parishes in our base dataset. As shown in Figure 1 each student is listed with their first name, surname, date of birth and parents' name. Using this information, we merge the schooling information onto the base dataset. We focus on information from grade 1 and grade 4 when students are 7 and 10 years old.

We are able to match schooling information for 17,999 children out of the 30,150 children with church records born between 1930 and 1935. The reasons why we are not able to get a perfect match are that (a) exam catalogues were destroyed or cannot be found in the archives[22], (b) the record linkage algorithm fails to identify a match, (c) an individual passed away before reaching school age, or emigrated, or (d) an individual left the sample parish and moved before school age.

The first two reasons are due to the data collection and operationalization and not subject to individual selection. The decision to move and an early death are, however, likely non-random with respect to (sickness) absence and skills. We significantly reduce the matching problem related to migration by tracing migrants and their exam catalogues in a different parish than their birth parish using official registers on movers. For the very few children leaving Sweden before enrolling into *Folkskola* we have no information after they left the country. The assumption we have to make is that the decision to migrate out of Sweden is unrelated to school absence and educational performance given the observable covariates we can control for in the model.

[22] WWII falls in the time under review. Sweden was neutral in the war and there was an oversupply of teachers (Paulsson, 1946). We have not found any historical sources suggesting that the war caused major disruptions in education and the probability that we found exam catalogs in local archives was the same for war- and non-war years.

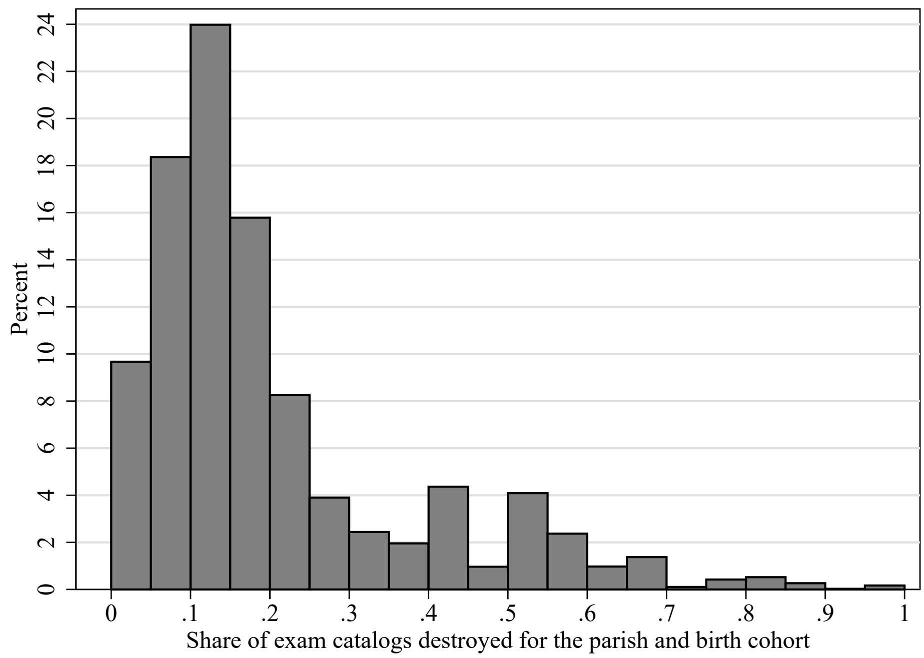
In order to assess the degree of selection bias possibly arising from this attrition, we provide two types of evidence. First, we assess the degree to which attrition appears to be caused by (a) missing archival sources, as opposed to the other reasons (b)-(d) mentioned above. This is an important statistic since (a) is largely unrelated to individual decisions and attributes. Second, we present evidence regarding the representativeness of the linked sample, and concerning the robustness of results in different subsamples.

There is no complete list of schools by which we could assess the quantitative importance of attrition type (a). In principle, catalogues can be missing for entire parish-cohorts or only for some schools within a parish-cohort (the vast majority of parishes had more than one school). However, to the extent that attrition type (a) exists, it would lead to attrition rates being correlated at the parish-cohort level. Conversely, if attrition were only driven by causes (b) and (c) it would be generally uncorrelated between individuals. Therefore, in the absence of attrition of type (a), attrition would be binomial. Therefore, we applied a χ^2 test whether the parish-cohort attrition rates are binomial. We conducted this test for the entire sample and by two individual characteristics (child sex and wedlock status). When looking at the p values for the overall sample; for females and males separately; and by wedlock status, all p values are smaller than 0.00. Hence, we reject the null hypothesis that the attrition is uncorrelated at the parish-cohort level, which suggests that a substantial part of the attrition is due to archival sources being missing.

Next, we made an attempt at quantifying the amount of attrition for this reason. We did this by means of a comparison with record linkage rates for the 1950 census. The school data and the 1950 census are only a few years apart, and the linkage is done on the same individual characteristics (names, birth dates, and place of residence). Hence, the linkage rates achieved for the 1950 census can be thought of as an upper bound for the linkage rates that could be achieved in the school data if there were no attrition of type (a). The match rate with the 1950 census is 88 per cent. The match rate in the school sample is 69 per cent. Hence, we estimate that of the 31 per cent of the sample lost due to attrition, two thirds would be missing due to missing archival sources. In Figure B2 below, we plot the distribution of estimated attrition rates. Apparently, the normal range for this type of attrition is 0-25 per cent.

The socio-economic characteristics in the church books are available for all 30,150 individuals born in the sampled parishes. If exam catalog information are missing at random, the mean value of those characteristics between individuals we are able to trace down in the matched school data should equal the mean of the base data. Table B3 shows the results from an exercise where we test this. The first two columns show the means and standard deviations (SD) over all individuals, while the second two columns give the means and SD of the sub-sample of individuals for that we have exam catalog information. The right-most column indicates whether the difference of the means is statistically significant at any of the conventional levels. Only the share of individuals born in certain years differs occasionally at the 5 per cent level. Individuals we are able to trace down are more likely to be born in 1935. But the absolute difference is quite small and we do not see why this should be correlated with the relationship between absence and performance. A likely reason for the difference is that exam catalogs are

Figure B2: Estimated rates of attrition due to missing sources.



Notes: Own illustration. The plot shows estimates of attrition due to missing archival sources, estimated at the parish-cohort level. Based on a comparison with linkage rates for the 1950 census.

often missing for entire schools and school years so that the data are missing for a larger number of individuals. All in all, Table B3 does not indicate systematic sample selection.

Still, to investigate sample selection further Table B4 gives the short-term effect of absence on academic performance for the full sample that includes all individuals we could match and separately for individuals who did not move parishes between birth and grade 4 (same-parish matches) and individuals who did move (movers) in our sibling-grade panel. If moving is selective with respect to absence and performance in school, the effects should differ between the samples. This does not seem to be the case, the estimated effects are very close to each other (and they are also close to the main short-term effects, despite restricting our sample to observations on siblings in our preferred specification).

B.4 Matching of later life outcomes

Educational attainment, employment and income

We merged individuals in the matched school data set to the 1960 and the 1970 census which cover the entire population of Sweden on 1st November in these two years (SCB, 1972, 1962). To match individuals we use information on first name, date of birth and parish of birth. The censuses contains information on employment status, and the 1970 census also has information on final education and income.[23] We observe 11,570 and 10,246 individuals in the matched school data set in 1960 (at ages 25–30) and 1970 (at ages 35–40), respectively. Upon matching

[23]The educational attainment measure we use is an indicator taking the value 1 if an individual leaves *Folkskola* after grade 4 and attends lower secondary or if the individual leaves *Folkskola* after the compulsory years of

to death registers, we see that 37% of the unmatched individuals died before the 1970 census enumeration.

Pensions

The matched school data set is also merged with official tax registers available on an annual basis for 2003-2008 measured in SEK.[24] The tax register includes records for pension income. Individuals were matched to the matched school data set using their unique social security number. For our cohorts full pensions require thirty years of contributions, and the level of the pension is based on the best fifteen years (Sundén, 2006). This means that pensions mirror lifetime income. Another advantage of using pension income compared to annual income is that it is insensitive to career interruptions, such as those associated with childbearing, which could influence the income observed in 1970. Upon matching official tax registers to death registers, we see that 66% of the unmatched individuals had died before the year 2003. Appendix D provides details on the pension system.

Mortality

The parish records include subsequent mortality for infants covering all deaths for the period up until 1946. A very detailed and strict reporting procedure regarding death causes was applied where local clergymen had to make monthly reports to Statistics Sweden in cases where no doctor had been involved.[25] The church records thus allow us to track mortality during the first 10-15 years of life for our sample.

To identify mortality beyond age 10-15, and to validate information on mortality during childhood, we use the Swedish Death Index (Federation of Swedish Genealogical Societies, 2014) which includes the universe of all deaths occurring between 1901–2013. Individual records were matched based on date of birth, sex, forename, surname, and birth parish. To validate the matching, we use a dataset containing burial records (Swedish Genealogical Society, 2012). As a second source of validation of adult mortality we also use the official tax records as individuals identified as dead before 2003 should not show up in these registers. Most individuals can be uniquely matched based on first name, date of birth, and parish of birth. To get information in surnames, which is especially important for women who generally change their names if getting married, we used the 1950 census. Surnames were used to validate the match, and in the rare cases of duplicates, to identify the correct match.

Missing information on adult outcomes might be due to individuals migrating away from Sweden or passing away. We can directly investigate mortality and see that 50 per cent of the unmatched individuals died before the 1960 census enumeration, 37 per cent died before the 1970 census enumeration, and 66 per cent died before we have tax records 2003.

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schooling and enrols into secondary education afterwards. The indicator takes the value of 0 for anyone living in districts with 8 or 9 years of compulsory schooling, but not completing further education, as well as anyone dropping out of lower secondary education.

[24] While tax registers are available from 2002, data for 2002 is of worse quality compared to the other years wherefore we use annual information from 2003 onwards. Our pension measure refers to average pensions 2003–2008.

[25] For details on the reporting of deaths, cf. Karlsson et al. (2014); Statistics Sweden (1915) and Hultkvist (1940).

B.5 Sample selection

Table B5 provides information on how our matched schooling data corresponds to number of individuals, siblings and families. With matched schooling information in either grade 1 or 4 for 18,056 children we have almost 29,000 student–grade observations. Using information on parents we can identify 8,567 siblings born between 1930–1935 for which we have more than 14,000 student–grade observations. These siblings stem from 3,716 families and for 960 families we even have matched information for more than two siblings. The sibling sample is used to identify the long-term effects of absences using a family fixed effect estimator. To identify short-term effects we employ an individual fixed effects approach and further restrict the sibling sample to individuals for whom we have schooling information both in grade 1 and grade 4. This gives an individual panel with 4,467 individuals with 8,934 student–grade observations.

Table B2: Summary statistics for control variables

Time-invariant variables	Mean
Female (in per cent)	49.5
Number of siblings	1.5
Year of birth (in per cent)	
1930	18.1
1931	16.5
1932	18.0
1933	17.5
1934	16.5
1935	13.4
(we additionally control for the month of birth and interaction terms between the year and the month of birth)	
Family's SES based on father's occupation (mutually exclusive HISCO classification, in per cent)	
Father: farmer, fisherman, hunter	39.8
Father: service and sales worker	50.9
Father: production workers	7.4
Additional indicators for the family's SES and living conditions (in per cent)	
Father works on family's own farm	33.2
Mother is employed	2.5
Born out of wedlock	4.8
Born in hospital	8.3
Twin birth	4.0
Parents' average year of birth	
Mother's year of birth	1902
Father's year of birth	1898
(mother's and father's year is controlled for by using 10-year dummies)	

	Mean grade	
Time-variant variables	1	4
Age (in years)	8.13	11.27
(included through age-in-months fixed effects)		
Class characteristics		
All classmates in same grade (in per cent)	34.3	30.2
Some classmates in lower grade (in per cent)	0.0	63.085
Some classmates in higher grade (in per cent)	65.7	30.6
Class size (number of students)	13.6	15.9
(measured through 5-day splines from 0 to 25)		

Notes: Own calculation based on church records and exam catalog information. Sample restricted to individuals with available sibling information. Observations: 14,066. Mutually exclusive indicators may not add up to 100 per cent because of missing information. For the estimations, missing information are coded as separate category, taking into account that the reason for the missing information might be meaningful in its own right (e.g., the father's occupation is missing because the father is unknown).

Table B3: Balancing check for church and school data samples

	(1)	(2)	(3)	(4)	(5)
	Background variable in				
	full sample		exam catalog sample		Difference
Variable	mean	SD	mean	SD	significant
Female	0.49	(0.50)	0.47	(0.50)	
Year of birth: 1930	0.18	(0.38)	0.17	(0.38)	*
Year of birth: 1931	0.17	(0.38)	0.16	(0.37)	**
Year of birth: 1932	0.17	(0.38)	0.16	(0.37)	**
Year of birth: 1933	0.16	(0.36)	0.16	(0.37)	
Year of birth: 1934	0.16	(0.37)	0.15	(0.36)	**
Year of birth: 1935	0.15	(0.36)	0.19	(0.39)	
Father: farmer, fisherman, hunter	0.32	(0.47)	0.26	(0.44)	
Father: agricultural worker	0.27	(0.44)	0.22	(0.42)	
Father: service and sales worker	0.09	(0.29)	0.12	(0.32)	
Father: production worker	0.57	(0.50)	0.60	(0.49)	
Father: occupation unknown	0.23	(0.42)	0.31	(0.46)	
Mother employed	0.04	(0.19)	0.08	(0.27)	
Born out of wedlock	0.08	(0.28)	0.15	(0.36)	
Born in hospital	0.11	(0.32)	0.13	(0.34)	
Twin birth	0.02	(0.15)	0.04	(0.19)	
Observations	30,150		17,424		

Notes: Own calculations on church records. Columns 1 and 2 gives the mean value and the standard deviation (SD), respectively, for the full sample. Columns 3 and 4 state the corresponding values for the sample restricted to individuals for that we are able to find exam catalog information. Column 5 indicates whether the difference in the means is statistically significant based on the p-value of a t-test of equal means. Significance: $^{*}p \leq 0.1$, $^{**}p \leq 0.05$, $^{***}p \leq 0.01$.

Table B4: Short-term effects of absence on performance for the full sample and the sample restricted to same-parish matches and movers

	OLS
Full sample	
Average grade points in units of SD	
Days of absence	-0.0040^{***}
	(0.0006)
# observations	28,931
Same-parish matches	
Average grade points in units of SD	
Days of absence	-0.0041^{***}
	(0.0006)
# observations	25,673
Movers	
Average grade points in units of SD	
Days of absence	-0.0037^{**}
	(0.0018)
# observations	3,258

Notes: See note to the baseline results table. Parish-clustered standard errors in parentheses. Significance: $^{*}p \leq 0.1$, $^{**}p \leq 0.05$, $^{***}p \leq 0.01$.

Table B5: Sample composition

Sample	Number of...			
	student–grade observations	students (individuals)	families	families w/ >2 children
Exam catalog data+church records				
– grades 1 or 4, cohorts 1930–35	28,931	18,056	13,205	960
– siblings panel	14,066	8,567	3,716	960
– siblings+individual panel	8,934	4,467	1,987	445

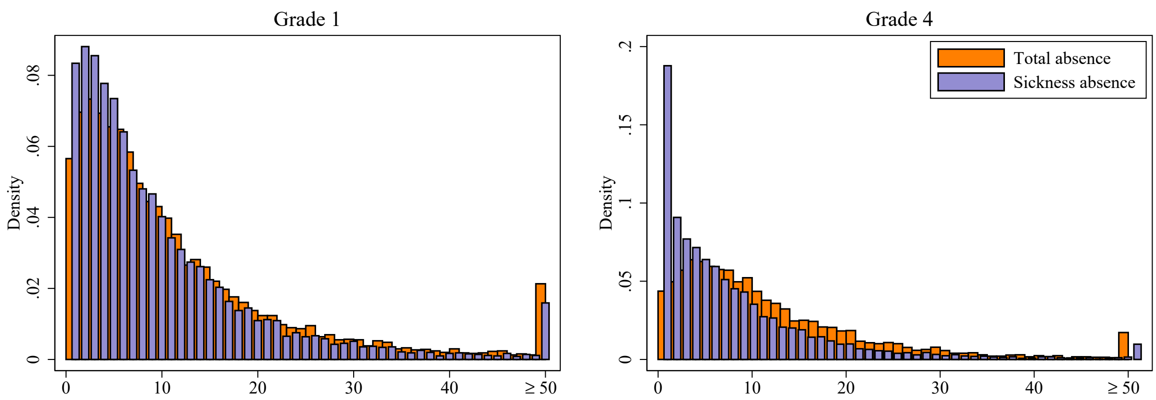
Notes: Own calculations based on exam catalogs and church records. All samples are restricted to observations with non-missing information in the key variables (grade points, absence, and grade). Starting are all 30,150 children born between 1930 and 1935 in our sample of 133 parishes, representative for all approximately 2,500 Swedish parishes at the time. The first row gives the number of individuals, individual–grade, and sibling pair observations we are able to match to exam catalog data based on the name, the date of birth, and the parish of birth. Parental information in the church records allows to identify siblings. The second row restricts the sample to siblings pairs, where we observe each sibling in at least one grade. The final row states the number of pairs of siblings for whom we observe both grades for each sibling.

Appendix C Descriptive statistics

C.1 Short-Term Outcomes

Our main explanatory variable is the number of missed school days in grade 1 and in grade 4, and we can distinguish between absences due to sickness and absences due to other reasons. Figure C1 shows the distribution of individual days of total absence and sickness absence in grade 1 and 4, respectively. In grade 1, 63 per cent of all students miss 10 or less days and about 6 per cent of all students have no absence. The average number of missed days in grade 1 is 11.1 days (median 7 days). In grade 4, students tend to miss slightly more days (mean 11.6 days, median 8 days). 60 per cent of all students miss 10 or less days and 4 per cent never miss school. Despite a very different context and time period, the distribution of total days of absence is comparable with that reported in recent US studies of absence in elementary school (Goodman, 2014; Aucejo and Romano, 2016). We observe a slightly higher density of very high number of absent days than these studies report, but unlike Goodman (2014) who excludes observations with more than 60 days of absence, we do not cap absence days here.[26]

Figure C1: Distribution of (sickness) absence in grade 1 and grade 4



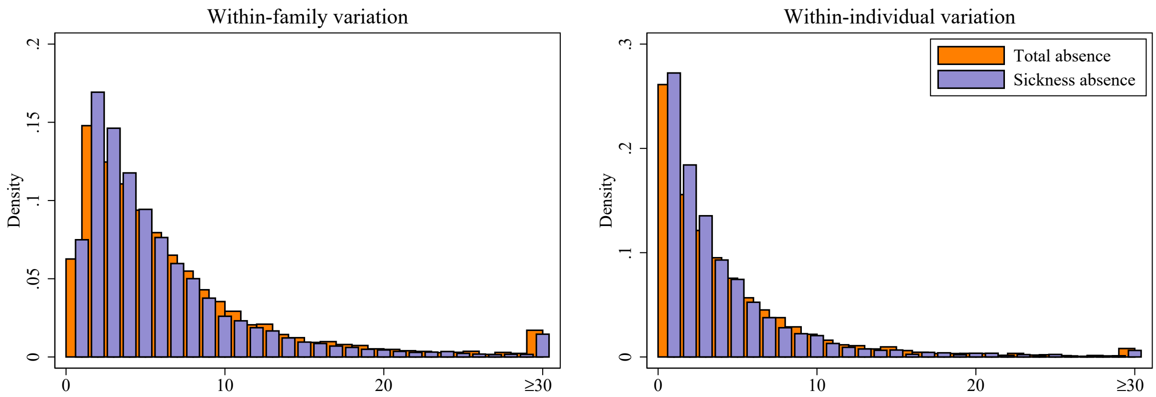
Notes: Own calculations based on exam catalog information. 14,066 observations. Each plot shows the density for total days of absence and days of sickness absence in grade 1 and 4, respectively. To allow for a comparison by type absence (all reasons or sickness) the density is calculated such that the sum is 1 for each type and in each grade.

Figure C1 further illustrates that most absences are sickness absences. The average number of missed days for other reasons than sickness is 1.7 in grade 1 and 3.3 in grade 4. In grade 1 and 4, 61 per cent and 39 per cent of all students, respectively, never miss a day for other reasons than sickness. Figure C2 plots the within-family and within-individual distributions of days of (any) absence and days of sickness absence.

Turning to school achievement, Figure C3 shows the distribution of the raw average grade points over math, writing, and reading and speaking, by grade. In line with the guidelines of the Royal Commission (SOU, 1942), only a few students receive a very low or a very high grade point

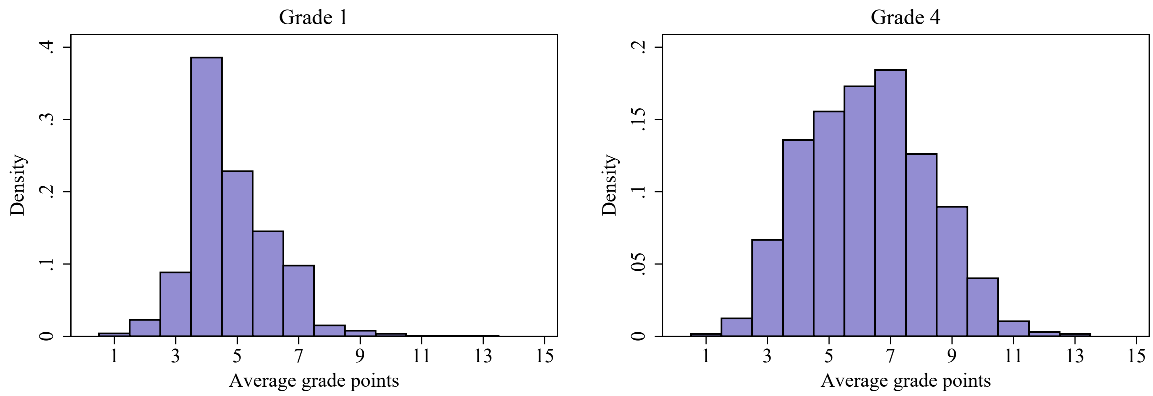
[26] In the empirical analysis we winsorize our data at top two per cent of days of sickness absence and top two per cent of days of non-sickness absence.

Figure C2: Distribution of the within-family and within-individual variation in (sickness) absence



Notes: Own calculations based on exam catalog information. 14,066 observations. Similar to Figure C1 this figure plots the density of total days of absence and sickness absence. Unlike to Figure C1, this figure plots on the within-family and within-individual difference, respectively, in days of absence. Densities are, again, calculated such that the sum of the densities for each type (all reason or sickness) and in each plot is 1.

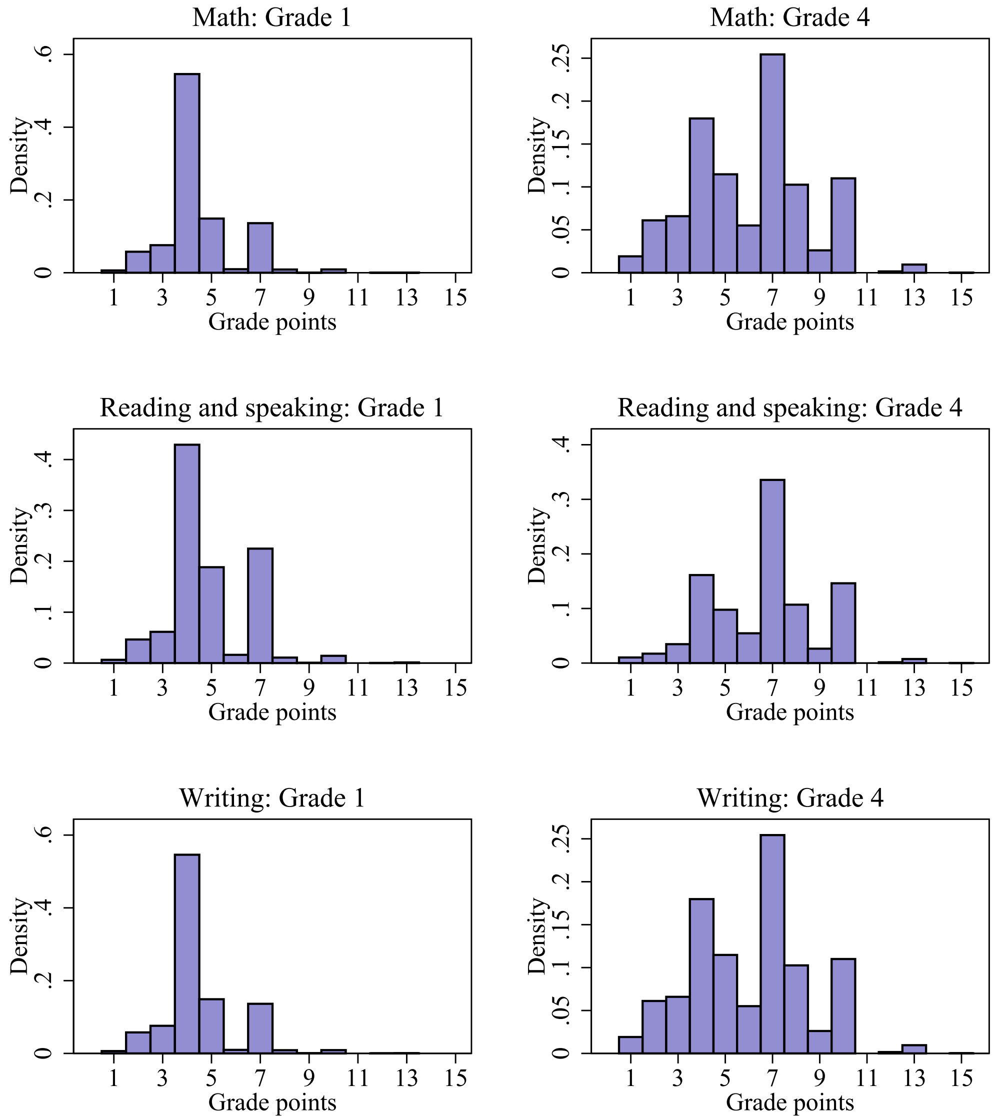
Figure C3: Distribution of average grade points in grade 1 and grade 4



Notes: Own calculations based on exam catalog information. 14,066 observations.

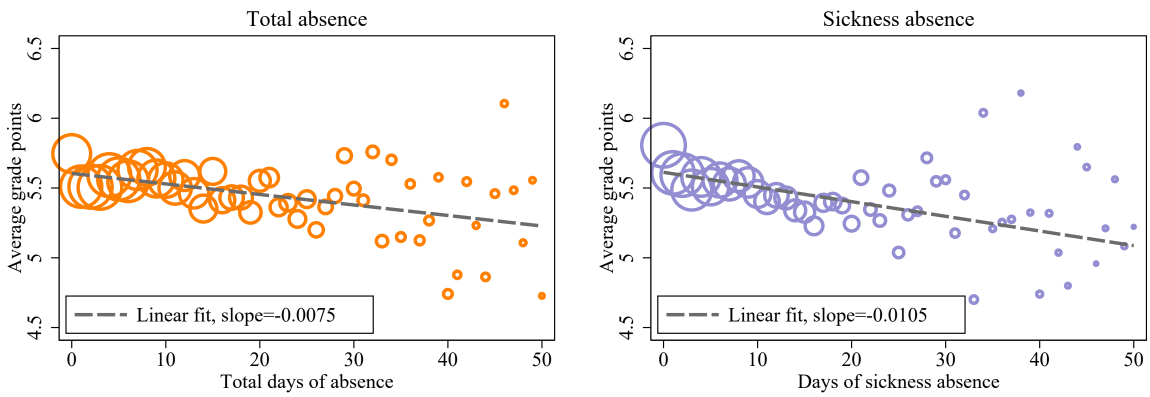
and the variance of the grade points is higher in grade 4 than in grade 1. Figure C4 shows the distributions of grade points by subject and school grade.

Figure C4: Distribution of grades by subject



Notes: Own calculations based on exam catalog information.

Figure C5: Descriptive relationship between (sickness) absence and academic performance



Notes: Own calculations based on exam catalog information for 14,066 observations. Grade points are collapsed on the integer of the days of absence. The size of the marker indicates the relative number of observations in the days-of-absence cell. For legibility we only plot cells up to 50 days of absence. The fitted line is taken from a simple linear regression of performance on total absence and sickness absence, respectively, using all information.

Table C1: Descriptive statistics on absence and performance

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Days of absence				Average performance			
		Standard deviation				Standard deviation		
	Mean	Overall	Between	Within	Mean	Overall	Between	Within
Samples for short-term analysis								
Siblings FE sample on grade level	11.4	12.2	8.5	9.2	5.5	1.9	1.3	1.4
Individual FE sample on grade level	11.3	12.0	9.4	7.5	5.5	1.8	1.4	1.2
Sample for long-term analysis								
Siblings FE sample on individual level	11.5	10.9	8.6	6.8	5.5	1.6	1.3	1.7

Notes: This table shows the mean value, overall standard deviation, between standard deviation, and within standard deviation for average days of absence over grades 1 and 4 in columns 1 to 4 and average performance over math, reading and speaking, and writing in grades 1 and 4 (not standardized but on the original 1 to 15 grade points scale) in columns 5 to 8, respectively. Each row of the table reports statistics for different samples. Row 1 relates to the grade-level dataset used for the OLS estimation of short-term effects (column 1 of Table 2). Row 2 relates to the grade-level dataset restricted to individuals we observe in both grades 1 and 4 and used in the individual FE estimation of short-term effects (column 2 of Table 2). Row 3 statistics relate to the sample used for the sibling fixed effect estimation of long-term outcomes (Table 3).

C.2 Occupations and Gender Segmentation

In order to give an idea about the segmentation of the labour market, we provide an overview of the most prevalent occupations in Table C2, differentiating between individuals who earn in the top quintile for their gender and the remaining 80 per cent. In the first column, we ranked occupations by share of women employed till we covered half of that population of women, which led to 8 occupations. We then maintained this pattern in the other columns. Panel A uses our sample, and panel B provides the same information for the entire population of the same cohorts. Comparing the two panels reveals that the analysis sample is very representative of the entire population.

An important takeaway from the table is that there was a high degree of gender segmentation in the labour market at the time: only one occupation (subject teacher) appears on both the male and the female lists. A second is that the top-ranking female occupations are either exclusively public sector (e.g. medical assistant) or such that can be either private or public (e.g. office clerk) while this is much less so for men.

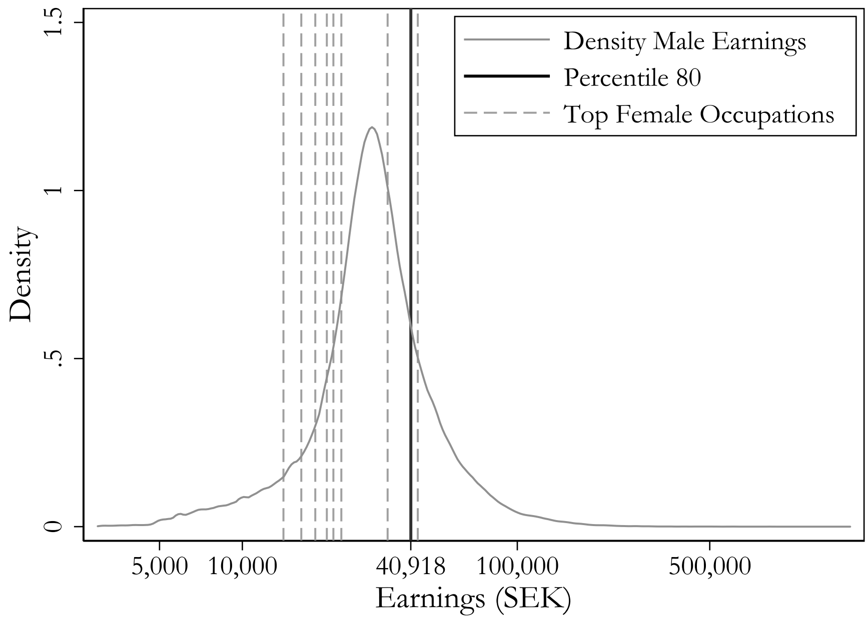
Table C2: Top-8 qccupations by sex and earnings quintile

Females						Males					
Top Income			**Others**			**Top Income**			**Others**		
A. Analysis Sample											
1	Class teacher	0.125	1	Not working	0.505	1	Architecht, engineer in construction	0.069	1	Not working	0.084
2	Medical assistant	0.104	2	Store personnel, other	0.067	2	Engineer, mechanical	0.068	2	Driver	0.064
3	Specialised office worker	0.088	3	Cleaner	0.051	3	Subject teacher	0.057	3	Lumberer	0.041
4	Secretary, stenographer	0.052	4	Medical assistant	0.046	4	Other company managers	0.055	4	Farmer	0.040
5	Nurse	0.051	5	Specialised office worker	0.036	5	Purchasing clerk	0.052	5	Machine repairman	0.036
6	Office clerk	0.045	6	Home carer	0.028	6	Engineer in electricity/telecom	0.038	6	Shop mechanic	0.030
7	Subject teacher	0.044	7	Agricultural worker	0.021	7	Driver	0.031	7	Concrete worker	0.025
8	Store personnel, other	0.032	8	Kitchen-maid	0.016	8	Executive	0.030	8	Engineer, mechanical	0.023
	All other	0.458		All other	0.231		All other	0.601		All other	0.657
B. Entire Population, Cohorts 1930–34											
1	Specialised office worker	0.109	1	Not working	0.467	1	Engineer, mechanical	0.085	1	Not working	0.074
2	Class teacher	0.075	2	Store personnel, other	0.079	2	Purchasing clerk	0.071	2	Farmer	0.060
3	Medical assistant	0.074	3	Cleaner	0.050	3	Architecht, engineer in construction	0.064	3	Driver	0.054
4	Secretary, stenographer	0.071	4	Specialised office worker	0.040	4	Other company managers	0.057	4	Shop mechanic	0.035
5	Office clerk	0.067	5	Medical assistant	0.034	5	Executive	0.054	5	Machine repairman	0.034
6	Store personnel, other	0.043	6	Home carer	0.032	6	Engineer in electricity/telecom	0.042	6	Engineer, mechanical	0.029
7	Nurse	0.039	7	Agricultural worker	0.029	7	Subject teacher	0.042	7	Concrete worker	0.025
8	Not working	0.030	8	Kitchen-maid	0.018	8	Public sector managerial	0.026	8	Woodworker	0.025
	All other	0.494		All other	0.251		All other	0.559		All other	0.664

The table reports the most common occupations within the top income quintile and the bottom four income quintiles in 1970, for our sample (panel A) and the entire population of the same cohorts (panel B), respectively. For example 0.125 for class teachers in row 1, panel A, means that 12.5% of all women who were in the top-quintile of the female earnings distribution were class teachers.

In order to shed further light on the gender segmentation of the labour market, we plot in Figure C6 the male distribution of earnings and the position of the eight predominant female occupations within the top income category in that male distribution. The figure also marks the cut-off for males being in the top income quintile. Clearly, the average earnings within these eight occupations fall short of the male cut-off, with one exception: the one occupation on the list that on average earns in the top quintile for males is 'subject teachers', who also enter the male list of high-earning occupations.

Figure C6: Top female occupations in the male earnings distribution



Kernel density estimates of 1970 male earnings in the entire population for cohorts 1930–34. Dashed vertical lines represent average earnings (for males and females) within the eight occupations listed for females in the top income category in Table C2.

C.3 Bivariate Relationships

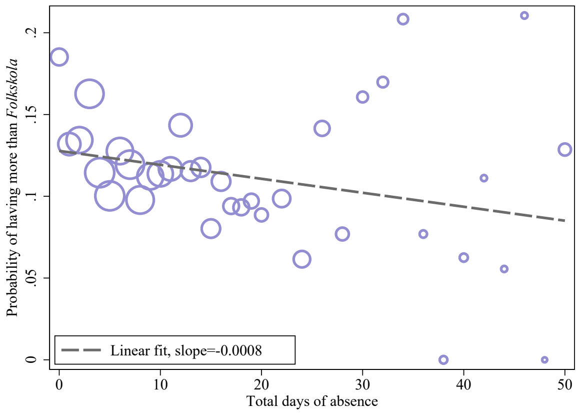
In this section, we document the associations between the number of days of absence (across grade 1 and grade 4) and our main outcomes of interest. As expected, the correlation between total days of absence and academic performance is negative, as shown in Figure C5. The linear fit indicates that the raw correlation is slightly more strongly negative for sickness absence than it is for any absence. Similarly to Figure C5, Figure C7 shows the raw association between the number of days of absence and our long-term outcomes. Plot (a) shows a negative relationship between absence and the probability of having more than *Folkskola*, while plots (b) show a slightly negative relationship between absence and employment both in 1960 and 1970.

In plots (c), we graph the distribution of income in 1970 and of pension income for groups of students with less than 5, between 5 and 20 days, and more than 20 days of absence. Kolmogorov–Smirnov tests of equality between the densities confirm that there are significant differences between them. In all cases, the p-value is either below conventional significance thresholds or (for the 5–20 and more than 20 days comparison for 1970 and the less than 5 and 5–20 days comparison for pensions) only slightly above 0.10 (see figure note). Figure C8 shows the survival rate of individuals who have missed less than 5 days, 5 to 20 days or more than 20 days, respectively. In contrast to income, the differences between the lines are small and statistically insignificant, suggesting no systematic association between absence and mortality.

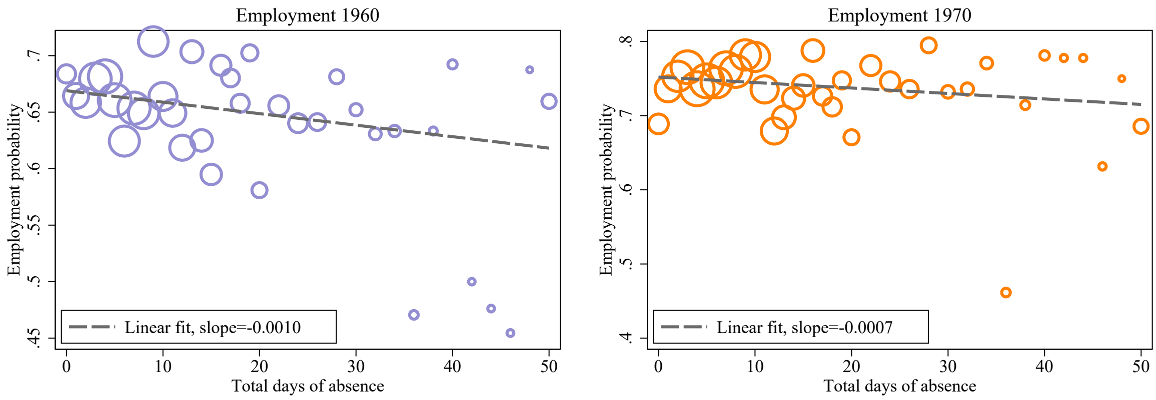
While these correlations point to a potentially negative effect of school absences on short- and long-term outcomes, they are obviously not evidence of a causal link. Indeed, students who are more likely to miss school may also be those of lower ability or those of frailer nature. Figure C9 shows a boxplot of the total days of absence in groups defined by different observable characteristics. The indicator for whether an individual was born in 1933 exhibits the highest inter-quartile range in days of absence, followed by the indicators for being born out of wedlock and for the child's father being employed in the service sector. Overall we note quite limited signs of selection based on these observables, though this obviously does not rule out selection on unobservable characteristics, including individual health. In addition, the statistics in Figure C9 indicate that it is not obvious whether we should expect students to select positively or negatively into absence. Together this emphasizes the need of a sound empirical strategy to deal with potential selection.

Figure C7: Descriptive relationship between total days of absence and long-term outcomes

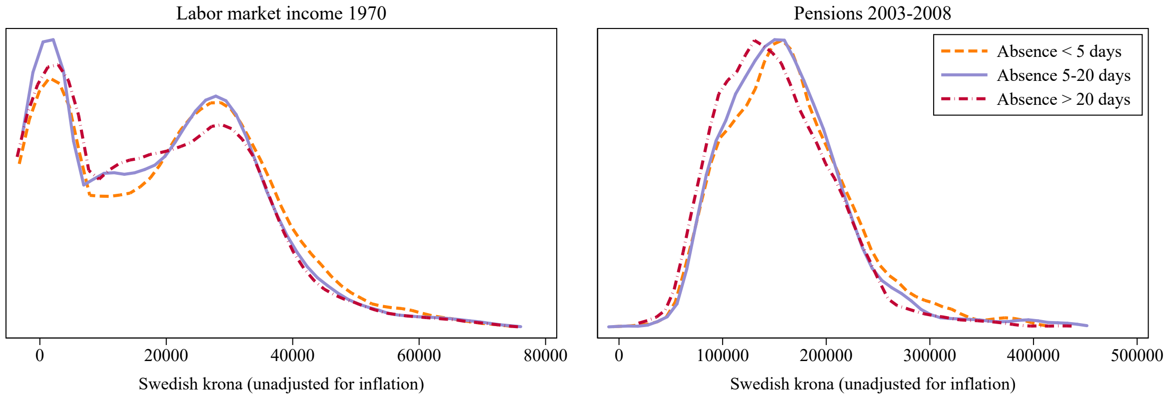
(a) Final education



(b) Employment

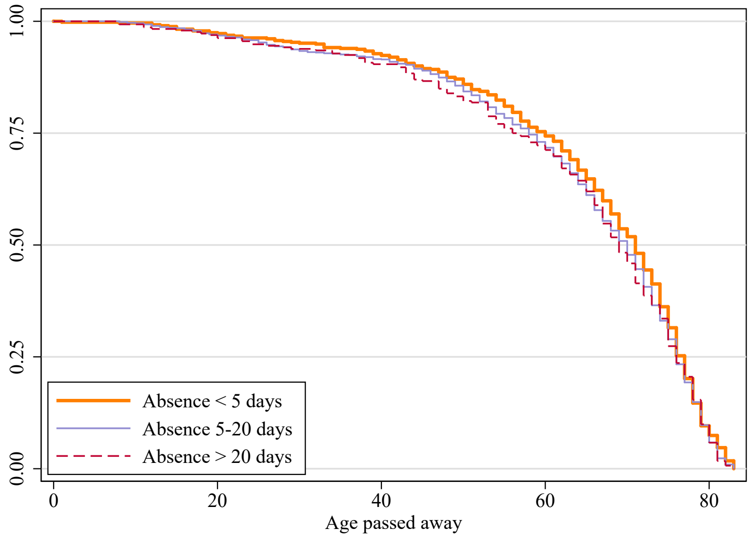


(c) Income



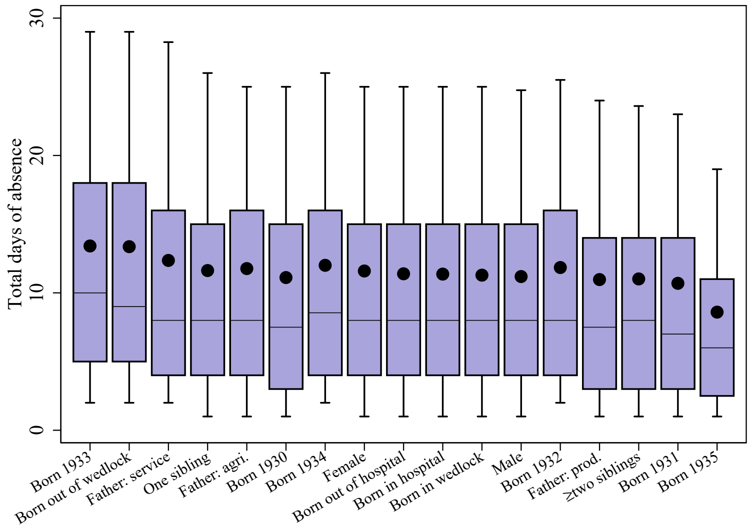
Notes: Own calculation based on exam catalog, census 1960 and 1970 as well as tax register information. Number of observations as in Table 1. Labor market income is zero for unemployed. In panels (a) and (b), the y-axis gives the expectation value of the outcome conditional on the days of absence value on the x-axis (daily bins below 20 days, two-day bin from 20–50 days). Marker size gives relative number of observations in the days-of-absence cell. The line states the linear fit. In panel (c), we use a Kolmogorov–Smirnov test for the equality of the distributions. Income 1970: corrected p-value are 0.019 for <5 days versus 5–20 days, 0.001 for <5 days versus >20 days, and 0.102 for 5–20 days versus >20 days. Pensions. 0.108 for <5 days versus 5–20 days, 0.000 for <5 days versus >20 days, and 0.001 for 5–20 days versus >20 days.

Figure C8: Kaplan–Meier survival function by total days of absence



Notes: Own calculations based on exam catalog and Swedish Death Index information, 8,567 observations. A Kolmogorov–Smirnov test for the equality of the distributions indicates that the conditional distributions to not differ significantly.

Figure C9: Total days of absence by socio-economic characteristics



Notes: Own calculations based on exam catalog information for 14,066 observations. Each characteristic (on the x-axis of the figure) gives a boxplot of the mean values of total days of absence (depicted by the dot), the median (the horizontal line within the box), the 75^{th} and 25^{th} percentile (the upper and lower hinge, respectively), and the 90^{th} and 10^{th} percentile (the upper and lower adjacent line, respectively) o the y-axis. The characteristics are ordered along the inter-quartile range of days of absence.

Appendix D The pension system

Sweden implemented its first public old-age pension system in 1913 whereby all citizens were entitled to a pension (Lundberg and Åmark, 2001). The 1913 version of the system was modified in two larger reforms in 1959 and in 1998, respectively. For cohorts born 1930–1935 the system implemented in 1959, the so-called ATP system, is the only scheme of relevance.

The ATP system, with the first pensions paid in 1963, was a pay-as-you-go defined-benefit scheme with a stated pension age at 65, with the possibility to receive an early pension from age 60 and to postpone retirement to age 70 (Kridahl, 2017). The pension scheme consisted of two main parts: (i) a flat benefit independent of previous income financed by the national budget and (ii) an earnings-related benefit covering all employees. The earnings-related benefit corresponded to 60 per cent (up to a ceiling) of 15 years of the highest earnings during 30 years of active labor force participation (Selén and Ståhlberg, 2007).[27] The pension right included earnings, but also social security transfers, e.g. unemployment insurance and parental benefits. Individuals with low or no earnings-related benefit received a supplementary benefit.[28]

With pensions based on the 15 years of highest earnings, pensions in the ATP system mirrors earnings at advanced stages of the career. With earnings generally levelling off around age 40-45 a majority of workers did not get any substantial increase in pension benefits from continuing an employment after 65 (Laun and Wallenius, 2015). Still, individuals could postpone retirement until age 70 and in 2010 20 per cent of all men in the ages ages 65–69 and ten per cent in the ages 70–74 were still employed (SOU, 2015).[29]

Married women born before 1945 whose husband passed away could receive an additional widow pension. The widow pension represents the most important deviation from the general pension rules. The widow pension corresponded to 40 per cent of the husband's earnings-related benefit and lasted until the widow turned 65 (Olofsson, 1993). After age 65 the widow pension was still 40 per cent of the husband's earnings-related benefit if the widow had never been employed, but was reduced if the widow herself had an earnings-related pension. This reduction also to some extent related to the birth year of the widow, with larger reductions for younger cohorts. Widow pensions were abolished in 1990, but eligibility was not lost for women born before 1945 and who were married before the rule was abolished and at the time of the spouse's death.

Table D1 shows estimates of effects of absence on pensions, in a specification including a proxy of the widow pension. The top panel presents results from our main specification as a benchmark. The middle panel adds a widow(er) dummy to the specification, which is based on the subsequent mortality of the person who is recorded as the spouse in the 1970 census (thus is it not a perfect measure of later marital status, as the index person may be divorced when the former spouse dies). Adding this indicator does not affect estimates, but it does suggest that widowhood is strongly correlated with pension earnings. In the bottom panel we test whether this coefficient

[27] If an individual had worked less than 30 years a deduction in relation to the number of missing years was done.

[28] Individuals who could not perform gainful employment, could claim retirement through the disability insurance scheme (Johansson et al., 2014).

[29] Individuals claiming an early pension from age 60 received a reduced flat pension.

can be attributed to the widow pension. We regress now add the spouses 1970 earnings, and an interaction between them and the widow dummy, to the specification. Indeed, the interaction term picks up the bulk of the correlation between widowhood and pension earnings, suggesting that the observed correlation is largely driven by those pensions. The widow(er) dummy is still large and positive, however. Since spousal earnings are proxied by their earnings in one single year, this remaining correlation might also be due to the widow pensions – but it may also reflect selection into marriage and widowhood. Most importantly, the main results remain remarkably robust to controlling for widow pensions.

Table D1: Long-term effects of school absence on pensions when controlling for widow(er) and spouse's income 1970

	(1)	(2)	(3)	(4)
	OLS		Siblings fixed effects	
	coeff.	rel. size	coeff.	rel. size
Baseline specification				
Absence (average, grades 1 and 4)	-330.0940^{***}	-0.0103	-396.0887^{**}	-0.0124
	(71.2631)		(169.3640)	
Controlling for widow(er)				
Absence (average, grades 1 and 4)	-344.2317^{***}	-0.0107	-418.3779^{***}	-0.0131
	(84.9572)		(156.7399)	
Widow(er)	18634.2773^{***}	0.5813	20264.8477^{***}	0.6322
	(2308.6794)		(5707.0498)	
Interacted with spouse's income 1970				
Absence (average, grades 1 and 4)	-313.3909^{***}	-0.0098	-386.1420^{**}	-0.0120
	(84.3462)		(154.1560)	
Widow(er)	-785.3165	-0.0245	7016.5293	0.2189
	(4768.9072)		(8452.0967)	
Spouse's income 1970 (in 1,000 SEK)	186.4989^{***}	0.0058	18.0618	0.0006
	(68.6552)		(113.7992)	
Widow(er) × spouse's income 1970	801.2049^{***}	0.0250	533.5577^{**}	0.0166
	(161.7437)		(266.1057)	

Notes: Each column states the coefficients of the variables on the left when pensions is regressed on them and the control variables in the main long-term effects. Number of observations: pensions 4,161. If spouse's income is missing we impute it with zero. Parish-clustered standard errors in parentheses. Significance: $^{*}p \leq 0.1$, $^{**}p \leq 0.05$, $^{***}p \leq 0.01$.

Appendix E Anchoring

As argued by Bond and Lang (2013), which representation of educational achievement we use can significantly alter the conclusion of the analysis. To circumvent this problem and provide economically meaningful interpretations of our estimated effects of absence on academic achievement, we anchor the grade point scale to pension income, a policy-relevant outcome measured in a meaningful metric (Swedish krona). To do so, we re-run our main models replacing our measure of educational achievement (grade points) as dependent variable with the fitted value of the following auxiliary anchoring regression:[30]

$$y_{ig}^{\text{anchor}} = \omega_{0g} + \sum_{j=1}^{13} \omega_{1g,j}\text{math}_{ig} + \sum_{j=1}^{13} \omega_{2g,j}\text{reading}_{ig} + \sum_{j=1}^{13} \omega_{3g,j}\text{writing}_{ig} + \xi_{ig},$$

where y_{ig}^{anchor} is individual i's average pension income between 2003–2008, math_{ig}, reading_{ig} and writing_{ig} are her grade points in the particular subject in school grade g, and ξ denotes the estimation error. The anchoring is performed separately for grade points in grade 1 and for grade points in grade 4.[31] In a first exercise we use information for all individuals (columns 1 and 4). In a second exercises we perform the same anchoring exercises but separately by gender as men and women in our cohorts faced quite different labor markets (columns 2-3 and 5-6).

Table E1 provides the estimates for the anchoring regressions.

[30]The grade points in each subject enter the regression through full sets of dummy variables. Grade points of 14 and 15 are omitted as these grades are very rare.

[31]Educational achievement anchored in earnings potential should not be confused with the effect of school absence on pensions. The anchored effect of absence still gives the short-term effect on educational performance, but scaled in units of Swedish krona instead of the somewhat hard-to-interpret standardized numerical grade points.

Table E1: Anchoring results

	(1)	(2)	(3)	(4)	(5)	(6)
	Dependent variable: pensions 2003–2008					
	Grade 1			Grade 4		
	all	women	men	all	women	men
Math points: 3 or less	$-26,452.6^{***}$	$-16,148.6^{***}$	$-31,127.9^{***}$	$-14,287.7^{***}$	$-7,557.8^{**}$	$-11,010.5^{***}$
	(3,842.6)	(4,789.0)	(5,276.9)	(2,827.6)	(3,269.3)	(4,089.9)
Math points: 4	$-19,508.2^{***}$	$-10,915.8^{***}$	$-21,860.5^{***}$	$-16,831.8^{***}$	$-7,457.0^{**}$	$-18,428.8^{***}$
	(2,658.8)	(3,055.1)	(3,907.8)	(2,814.0)	(3,349.6)	(3,935.1)
Math points: 5	$-11,637.7^{***}$	$-4,436.6$	$-17,956.5^{***}$	$-8,314.0^{***}$	$-3,699.7$	$-6,697.2$
	(2,844.8)	(3,283.2)	(4,153.8)	(3,191.0)	(3,727.6)	(4,534.2)
Math points: 6	$-21,180.0^{***}$	$-19,948.0^{**}$	$-28,194.5^{***}$	$-6,795.9^{*}$	-678.5	$-4,546.0$
	(7,369.1)	(8,827.3)	(10,393.8)	(3,714.3)	(4,188.2)	(5,497.5)
Math points: 8	3,113.8	$-6,998.3$	4,558.6	2,679.6	4,609.2	-513.0
	(6,576.2)	(7,694.5)	(9,641.1)	(3,005.4)	(3,376.7)	(4,444.9)
Math points: 9 or more	$-2,801.4$	4,481.0	$-20,063.2$	$18,667.0^{***}$	$12,420.2^{***}$	$15,070.9^{***}$
	(8,052.8)	(8,821.6)	(12,782.8)	(2,874.1)	(3,250.0)	(4,271.3)
Reading points: 3 or less	$-1,551.2$	$-10,952.3^{**}$	$-12,596.2^{**}$	$6,843.7^{*}$	4,664.5	1,103.2
	(3,708.6)	(4,921.4)	(5,018.7)	(4,028.2)	(5,302.1)	(5,251.3)
Reading points: 4	$-2,427.1$	$-5,965.8^{**}$	$-8,144.1^{**}$	$7,352.8^{***}$	5,713.3	1,421.9
	(2,591.0)	(2,940.1)	(3,888.1)	(2,837.2)	(3,633.4)	(3,769.0)
Reading points: 5	-324.7	$-5,389.6^{*}$	-872.7	2,339.9	-771.6	$-3,017.4$
	(2,839.3)	(3,197.6)	(4,290.1)	(3,170.1)	(3,990.3)	(4,246.3)
Reading points: 6	$-11,507.4^{*}$	$-18,378.8^{**}$	$-17,042.3^{*}$	$8,788.0^{**}$	4,549.3	4,893.7
	(6,797.9)	(8,163.2)	(9,595.1)	(3,820.1)	(4,690.5)	(5,190.0)
Reading points: 8	$-13,723.4^{*}$	$-11,605.3$	$-2,624.8$	2,647.9	2,951.5	3,983.3
	(8,121.1)	(8,489.6)	(14,206.4)	(2,921.7)	(3,172.1)	(4,534.9)
Reading points: 9 or more	$21,555.8^{***}$	$18,606.5^{**}$	$25,165.7^{**}$	$7,555.2^{***}$	$12,556.5^{***}$	$14,727.6^{***}$
	(7,864.9)	(8,542.8)	(12,666.8)	(2,838.0)	(3,003.4)	(4,676.1)
Writing points: 3 or less	$12,126.8^{***}$	6,597.6	$-1,852.3$	$7,947.8^{**}$	5,655.8	$-7,498.4^{*}$
	(4,436.2)	(5,567.5)	(6,501.2)	(3,331.3)	(4,208.0)	(4,550.2)
Writing points: 4	$10,753.2^{***}$	294.7	5,241.5	$10,457.5^{***}$	1,346.5	2,702.2
	(3,359.7)	(3,663.7)	(5,452.3)	(2,871.8)	(3,568.9)	(3,949.9)
Writing points: 5	$8,668.6^{**}$	3,882.2	10,673.9	$9,694.4^{***}$	$7,894.6^{**}$	401.7
	(3,898.6)	(4,105.3)	(6,505.8)	(3,020.7)	(3,616.9)	(4,229.3)
Writing points: 6	11,533.2	$-2,710.0$	$30,997.3^{*}$	-269.0	-831.9	$-8,072.1$
	(10,783.5)	(11,462.1)	(17,750.8)	(3,975.3)	(4,597.3)	(5,705.4)
Writing points: 8	563.7	1,839.0	3,619.4	3,466.5	4,500.8	$14,222.4^{***}$
	(11,193.1)	(11,418.6)	(20,261.2)	(3,041.9)	(3,167.4)	(5,066.9)
Writing points: 9 or more	$-37,322.5^{***}$	$-31,119.2^{**}$	$-37,106.7$	2,179.2	$8,623.5^{**}$	8,210.2
	(13,198.7)	(13,542.2)	(25,854.8)	(3,222.5)	(3,374.5)	(5,423.5)

Notes: Dependent variable: pensions taken from tax registers 2003–2008. Explanatory variables: binary indicators of the points in math, reading and speaking and writing (reference category is 7 points). Missing grade points for each subject enter the regression through separate indicators (coefficients suppressed from the table). Standard errors in parentheses. Significance: $^{*}p \leq 0.1$, $^{**}p \leq 0.05$, $^{***}p \leq 0.01$.

Finally we also perform the anchoring exercises using income at ages 35–40 instead of pensions. As pensions mirror the best fifteen years they are less sensitive to fluctuations in labor supply than earnings and should thus be more representative, especially for women. Together with the fact that the pension system at the time had a progressive component (see Appendix D), the effect in terms of earnings potential using incomes in middle age is somewhat larger in magnitude than the estimates anchoring to pensions. Yet, we get very similar results to what was noted in Table 2, with ten additional days of absence on school performance translating into a very small decrease in average earnings.

Appendix F Short-term effects

Table F1: Full estimation output for all fixed effects specifications

	(1) OLS	(2) School FE	(3) Teacher FE	(4) Sibl. FE	(5) Indi. FE
Total days of absence	-0.0051^{***}	-0.0058^{***}	-0.0060^{***}	-0.0055^{***}	-0.0045^{***}
	(0.0007)	(0.0009)	(0.0009)	(0.0009)	(0.0013)
Female	0.2949^{***}	0.3084^{***}	0.3126^{***}	0.3374^{***}	
	(0.0239)	(0.0235)	(0.0253)	(0.0301)	
Birth year: 1931	0.0277	0.0908	0.1238	-0.1809	
	(0.0915)	(0.1014)	(0.1122)	(0.1188)	
Birth year: 1932	0.4758	0.4509^{***}	0.8561^{**}	-0.7520	
	(0.4682)	(0.1627)	(0.4123)	(0.5501)	
Birth year: 1933	0.7233^{*}	0.5821^{***}	1.1033^{**}	-0.1541	
	(0.3924)	(0.2084)	(0.4722)	(0.3457)	
Birth year: 1934	-0.0145	0.0029	0.0407	-0.8640	
	(0.0929)	(0.1100)	(0.1241)	(0.5882)	
Birth year: 1935	0.5722	0.4061^{**}	0.8614^{**}	-0.3603	
	(0.4187)	(0.1914)	(0.4088)	(0.3722)	
Wedlock	0.0154^{***}	0.0185^{***}	0.0182^{***}	0.0135^{***}	
	(0.0044)	(0.0039)	(0.0032)	(0.0016)	
Hospital	0.1122^{***}	0.1148^{***}	0.1132^{**}	0.0547	
	(0.0401)	(0.0425)	(0.0461)	(0.0543)	
Twin	-0.1436^{**}	-0.1653^{**}	-0.1673^{**}	-0.1901^{**}	
	(0.0574)	(0.0703)	(0.0798)	(0.0927)	
Grade 4	1.4707^{***}	1.3496^{***}	1.3366^{***}	0.6706^{***}	0.5571^{***}
	(0.1614)	(0.1340)	(0.1463)	(0.1608)	(0.0378)
Occup. father: agriculture	0.0590	0.0290	0.0277	0.0003	
	(0.0621)	(0.0582)	(0.0629)	(0.0677)	
Occup. father: services	0.1910^{***}	0.2113^{***}	0.2155^{***}	-0.1033	
	(0.0498)	(0.0550)	(0.0546)	(0.0869)	
Occup. father: farmer	0.0051	0.0339	0.0303	-0.0458	
	(0.0576)	(0.0505)	(0.0524)	(0.0676)	
Occup. father: unknown	0.0166	0.1420^{***}	0.1566^{***}	0.0861	
	(0.0638)	(0.0480)	(0.0443)	(0.0564)	
Mother employed	-0.1279^{*}	-0.1181^{*}	-0.1269^{*}	-0.0638	
	(0.0715)	(0.0647)	(0.0694)	(0.0614)	
Classmates in lower grade	0.0668	0.0851	0.1456^{***}	0.1548^{***}	0.1957^{***}
	(0.0494)	(0.0540)	(0.0431)	(0.0380)	(0.0659)
Classmates in higher grade	0.0217	-0.0545	-0.0765	-0.0793	-0.0365
	(0.0321)	(0.0337)	(0.0760)	(0.0672)	(0.0557)
Class size 1–5	-0.0304^{*}	-0.0239	-0.0213	-0.0136	0.0306
	(0.0178)	(0.0241)	(0.0268)	(0.0281)	(0.0397)
Class size 6–10	0.0083	0.0067	0.0038	-0.0030	0.0011

Notes: See note to the baseline short-term results table. Fixed effects are suppressed. Parish-clustered standard errors in parentheses. Significance: $^{*}p \leq 0.1$, $^{**}p \leq 0.05$, $^{***}p \leq 0.01$.

Table F1 – *continued*

	(1) OLS	(2) School FE	(3) Teacher FE	(4) Sibl.	(5) Indi.
	(0.0097)	(0.0118)	(0.0122)	(0.0107)	(0.0238)
Class size 11–15	−0.0096	−0.0049	0.0023	0.0110	0.0194
	(0.0080)	(0.0081)	(0.0084)	(0.0106)	(0.0195)
Class size 16–20	0.0032	0.0149*	0.0000	0.0033	0.0035
	(0.0082)	(0.0079)	(0.0130)	(0.0109)	(0.0186)
Class size 21–25	0.0025	−0.0089	0.0051	−0.0019	0.0138
	(0.0101)	(0.0098)	(0.0148)	(0.0166)	(0.0243)
Class size > 25	−0.0004	0.0109**	0.0122***	0.0131***	0.0131*
	(0.0038)	(0.0043)	(0.0045)	(0.0049)	(0.0070)
# observations	14,066	14,066	14,066	14,066	8,934
# units		955	1,639	3,716	4,467

Notes: See note to the baseline results table. Fixed effects are suppressed. Parish-clustered standard errors in parentheses. Significance: $^{*}p \leq 0.1$, $^{**}p \leq 0.05$, $^{***}p \leq 0.01$.

Table F2: Estimates of the short-term effect of school absence on academic performance by subject

	(1) OLS	(2) Individual fixed effects
Average grade points over all three subjects: the average over the grade points is taken first, and then mean-0, SD-1 standardized (baseline approach)		
Days of absence	-0.0051^{***}	-0.0045^{***}
	(0.0007)	(0.0013)
Average grade points over math and reading and speaking: the average over the grade points is taken first, and then mean-0, SD-1 standardized (baseline approach)		
Days of absence	-0.0107^{***}	-0.0076^{***}
	(0.0014)	(0.0025)
Average grade points over all three subjects: subject grade points are mean-0, SD-1 standardized before taking the average, the final score is not		
Days of absence	-0.0043^{***}	-0.0039^{***}
	(0.0006)	(0.0011)
Average grade points over math and reading and speaking: subject grade points are mean-0, SD-1 standardized before taking the average, the final score is not		
Days of absence	-0.0048^{***}	-0.0036^{***}
	(0.0006)	(0.0012)
Math grade points in units of SD (mean: 0, SD 1)		
Days of absence	-0.0058^{***}	-0.0022^{***}
	(0.0007)	(0.0013)
Reading and speaking grade points in units of SD (mean: 0, SD 1)		
Days of absence	-0.0037^{***}	-0.0049^{***}
	(0.0008)	(0.0018)
Writing grade points in units of SD (mean: 0, SD 1)		
Days of absence	-0.0032^{***}	-0.0035^{*}
	(0.0009)	(0.0018)

Notes: Each cell states the coefficient of days of absence for a separate regression. The dependent variable differs across columns and panels. The first two rows give the average grade point over all three subject and the average grade point over math and reading, respectively, where the average is taken over the original grade points and then standardized to mean 0 and SD 1. This the is standardization used throughout the main analyses and row 1 repeats the baseline results for the short-term effects. Rows 3 and 4 give the same average as the first two rows, but use grade points standardized before but not after the aggregation. The remaining three rows give the effect of absence on the separate subjects (grade points are standardized to mean 0 and SD 1). The specifications in the columns are the same as in the main results (for average grade points). Number of observations for math: 14,040 (OLS) and 8,919 (siblings fixed effects), for reading and speaking: 14,046 and 8,930, for writing: 11,837 and 7,589, for the average over math and reading and speaking: 14,020 and 8,915. Parish-clustered standard errors in parentheses. Significance: $^{*}p \leq 0.1$, $^{**}p \leq 0.05$, $^{***}p \leq 0.01$.

Table F3: Short-term effects measuring performance on a 7-point grading scale

	(1) OLS	(2) Indi. FE
Average grade points (7-point scale) in units of SD (mean: 0, SD 1)		
Days of absence	-0.0050^{***}	-0.0053^{***}
	(0.0008)	(0.0016)
# observations	14,066	8,934

Notes: See note to the baseline short-term results table. Parish-clustered standard errors in parentheses. Significance: $^{*}p \leq 0.1$, $^{**}p \leq 0.05$, $^{***}p \leq 0.01$.

F.1 Non-linearities and heterogeneity

We also explore the extent to which impacts of student absences have non-linear effects on contemporaneous achievement. While a student may be able to easily catch up on a few days of absence, this may be less possible for longer periods out of school. If this were the case, it would result in a non-linear relationship between absence and educational performance. To investigate the presence of non-linearities we employ the individual fixed effects specification (similar to column 2 in Table 2), where we estimate the marginal effect of an additional day of absence on performance. For a threshold or cutoff value c, we estimate the effect of an increase in absence from $c-1$ to c days. In the implementation, we vary c from 1 to 50.[32]

Specifically, we perform our test of non-linearity as follows. Suppose we want to test if the effect of absence $\tau(D)$ is linear in D. For this purpose, estimate with a moving cutoff c: $\mathbb{E}[\tau(D) \mid D \geq c] - \mathbb{E}[\tau(D) \mid D < c], \forall c = 1, \ldots, \infty$. This is estimated using the regression equation[33]

$$Y = \beta_0 + \tau_c 1(D \geq c) + X\beta_1 + Q\beta_2 + S + T + \delta + u. \tag{F1}$$

This is similar to our preferred short-term estimation specification, but allows the coefficient of absence to differ in the days of absence. If we replace this regression function with another one, defined as

$$Y = \beta_0 + \tau_c' 1(D \geq c) \cdot \left(\bar{D}_c - \bar{D}_{-c}\right) + X_i\beta_1 + Q\beta_2 + S + T + \delta_i + u, \tag{F2}$$

where $\bar{D}_c = \mathbb{E}[D \mid D \geq c]$ and $\bar{D}_{-c} = \mathbb{E}[D \mid D < c]$, the estimand becomes

$$\tau_c' = \frac{\mathbb{E}[\tau(D) \mid D \geq c] - \mathbb{E}[\tau(D) \mid D < c]}{\mathbb{E}[D \mid D \geq c] - \mathbb{E}[D \mid D < c]} \tag{F3}$$

Under the assumption of linearity, $\tau(D) = \beta D$, this simplifies to:

$$\tau_c' = \frac{\mathbb{E}[\tau(D) \mid D \geq c] - \mathbb{E}[\tau(D) \mid D < c]}{\mathbb{E}[D \mid D \geq c] - \mathbb{E}[D \mid D < c]} = \frac{\beta(\mathbb{E}[D \mid D \geq c] - \mathbb{E}[D \mid D < c])}{\mathbb{E}[D \mid D \geq c] - \mathbb{E}[D \mid D < c]} = \beta \tag{F4}$$

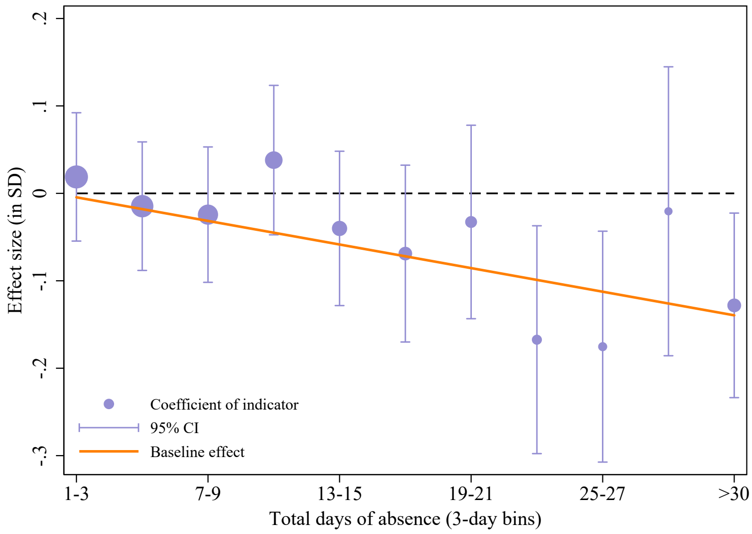
Hence, if the effect of sickness absence is linear in D, we should find that $\tau_c' = \beta \forall c$.

Figure F2 plots the marginal effects (on the y-axis) along the cut-off value (on the x-axis). If the effect of days of absence is linear, we would expect the marginal effect to be equal to the average effect. The average effect is given by the red reference line in Figure F2. A comparison of the orange markers and red line indicates that the per-day effect of absence of the non-linear estimations using the marginal effect does not substantially differ from the linear effect: the confidence intervals of the marginal effect estimates always include the estimated average

[32] Because the number of students that miss a large number of days in one school year is often rather low, it is not meaningful to regress performance on a full set of binary indicators for each number of days in a single regression. Figure F1 gives the results of a regression using indicator variables that bin days of absence. The coefficients of the indicator variables lie around the linear effect reference line.

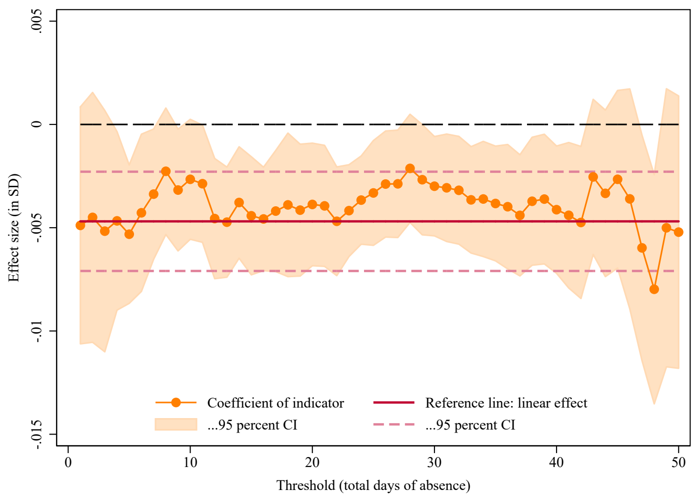
[33] For legibility, we leave subscripts implicit.

Figure F1: Nonlinearities in the short-term effect of grouped sickness absence



Notes: Own calculations based on exam catalog information. 14,066 observations. To detect non-linearities in the effect of sickness absence we regress performance on indicator variables giving the number of days of sickness absence in groups of 3. This graph plots the coefficients of the indicator variables. The size of the markers give the relative number of observations for which the group indicator is 1. The spikes around the markers state the 95 per cent confidence interval. The orange line depicts the linear effect of an additional day of absence in the baseline short-term results.

Figure F2: Non-linearities in the short-term effect of absence for different threshold values



Notes: Own calculations based on exam catalog information for 8,934 observations. This graph assesses linearity in days of absence. The solid red line gives the baseline effect, the dashed red lines the 95 per cent confidence interval. The orange markers give the marginal effect of an additional day of absence, when absence increases from $c-1$ to c days, where c is the cutoff value given on the x-axis, control variables and fixed effects are as in the baseline specification for the short-term effects.

effect. This finding is in line with Aucejo and Romano (2016) who do not find evidence of non-linearities either.

F.2 Heterogeneity

Next, we explore the extent to which the effect of absence are heterogeneous across socio-demographic subgroups. If there are heterogeneous effects across groups, these results could inform policy by identifying who may benefit the most from reforms targeting school absence. In Table F4, we report the results of OLS and individual fixed effects models where we include an interaction between our measure of absence and a dummy for indicating membership to the particular subgroup. Panel A reports the estimates for men and the interaction shows the difference in impacts between men and women. While the interaction is negative (suggesting a possibly stronger impact of absences for women than for men), it is highly insignificant.

Panel B exploits heterogeneity of impacts across children whose fathers are agricultural workers versus other children. As mentioned above, paternal occupation is our main proxy for socio-economic status, but it could also proxy for ability. Aucejo and Romano (2016) find evidence that students in the lowest tercile of the prior attainment distribution are the most adversely affected by an additional absence, which is consistent with the hypothesis that lower ability students or poorer students have a harder time making up missed work. In contrast, in our context, we find no significant difference in impacts between children whose fathers are agricultural workers and those whose fathers are production or service workers. This absence of a social gradient might be due to the comparably low levels of socio-economic inequality observed in Sweden during this period: income and wealth inequalities were at least as large as in other Western countries at the turn of the 20^{th} century; however, the first decades of the 20^{th} century were characterised by rapidly declining earnings inequality (Roine and Waldenström, 2015; Bengtsson and Prado, 2020). In particular the years around the elementary schooling period of these individuals have been characterised as an "equality revolution" (Gärtner and Prado, 2016) due to a sharp reduction in inequality.

Table F4: Heterogeneity in the short-term effects by subgroup

	(1) OLS	(2) Sibl. FE	(3) Indi. FE
Gender			
Absence	-0.0040^{***}	-0.0052^{***}	-0.0039^{***}
	(0.0011)	(0.0014)	(0.0015)
Absence×female	-0.0022	-0.0006	-0.0014
	(0.0016)	(0.0019)	(0.0023)
Father's occupation			
Absence	-0.0037^{***}	-0.0059^{***}	-0.0046^{***}
	(0.0010)	(0.0011)	(0.0018)
Absence×agri. worker	-0.0035^{**}	0.0010	-0.0001
	(0.0012)	(0.0011)	(0.0027)
Grade			
Absence	-0.0071^{***}	-0.0071^{***}	
	(0.0013)	(0.0013)	
Absence×grade 1	0.0037^{***}	0.0030	
	(0.0014)	(0.0018)	
# observations	14,066	14,066	8,934

Notes: Each panel states the coefficient of total days of absence differentiated for different subgroups. Columns 1 and 2 use the siblings panel and employ OLS and siblings fixed effects estimation, respectively. Column 3 gives individual fixed effects results using the individual–grades panel. In all models, days of absence enter the specification linearly and interacted with a subgroup indicator. Other control variables are as in the baseline short-term results. Parish-clustered standard errors in parentheses. Significance: $^{*}p \leq 0.1$, $^{**}p \leq 0.05$, $^{***}p \leq 0.01$.

F.3 Validity of the identification strategy to recover short-term effects of absence

The main threat to the identification of the causal effect of a day of absence in our short-run analysis is the presence of time-varying individual-level unobservables correlated with both absences and achievement. Given that absences are mainly driven by sickness absences, a particularly concerning threat is that idiosyncratic health shocks would confound the effect of the loss of instructional time resulting from a day of absence with that of the health shock. To address this concern, we estimate a version of equation 1 where we also include a variable N_{ig} measuring the number of days of absence due to non-sickness absence in grade g. In this model, the coefficient on D_{ig} measures the marginal effect of a day of sickness absence and the coefficient on N_{ig} measures the difference between the marginal effect of a day of non-sickness absence and that of a day of sickness absence. Our hypothesis is that if there is no difference between those two marginal effects, our strategy is unlikely to yield biased estimates of the effect of a day of absence due to idiosyncratic health shocks.

Table F5 reports the coefficient on D_{ig} and N_{ig} across specifications. In neither specification is the coefficient on N_{ig} statistically significant, and in the individual fixed effect model, it becomes extremely small (0.0004), which indicates that time-varying health shocks are unlikely to confound our estimates of τ. This evidence supports an interpretation of the findings where the reduced academic performance associated with absence is driven by the loss of instructional time, and not by a shock in the student's health.

Table F5: Short-term effects – total absence versus sickness absence

	(1) OLS	(2) Individual fixed effects
Average grade points in units of SD		
Total days of absence	-0.0038^{***}	-0.0033^{***}
	(0.0007)	(0.0013)
Days of non-sick. absence	-0.0035	-0.0004
	(0.0029)	(0.0032)
# observations	14,066	8,934
# individuals/families		4,467

Notes: Each column shows the coefficients of total days of absence and days of non-sickness absence when student performance is jointly regressed on both kinds of absences and the control variables according to the baseline specification. The first row gives the coefficient of total absence, whereas the second row gives the coefficient of non-sickness absence. A one-day increase in non-sickness absence, given the total days of (sickness and non-sickness) absence, gives the relative effect of missing a day of school for reasons other than health compared to missing school for health. Parish-clustered standard errors in parentheses. Significance: $^{*}p \leq 0.1$, $^{**}p \leq 0.05$, $^{***}p \leq 0.01$.

While a health shock is likely to be the most common type of grade-specific unobservable factor possibly not being controlled for by the individual fixed effect strategy, it is of course possible that there are other factors whereby this test would not be particularly informative. To guard against this possibility, we also provide bounds following the approach by Oster (2019).

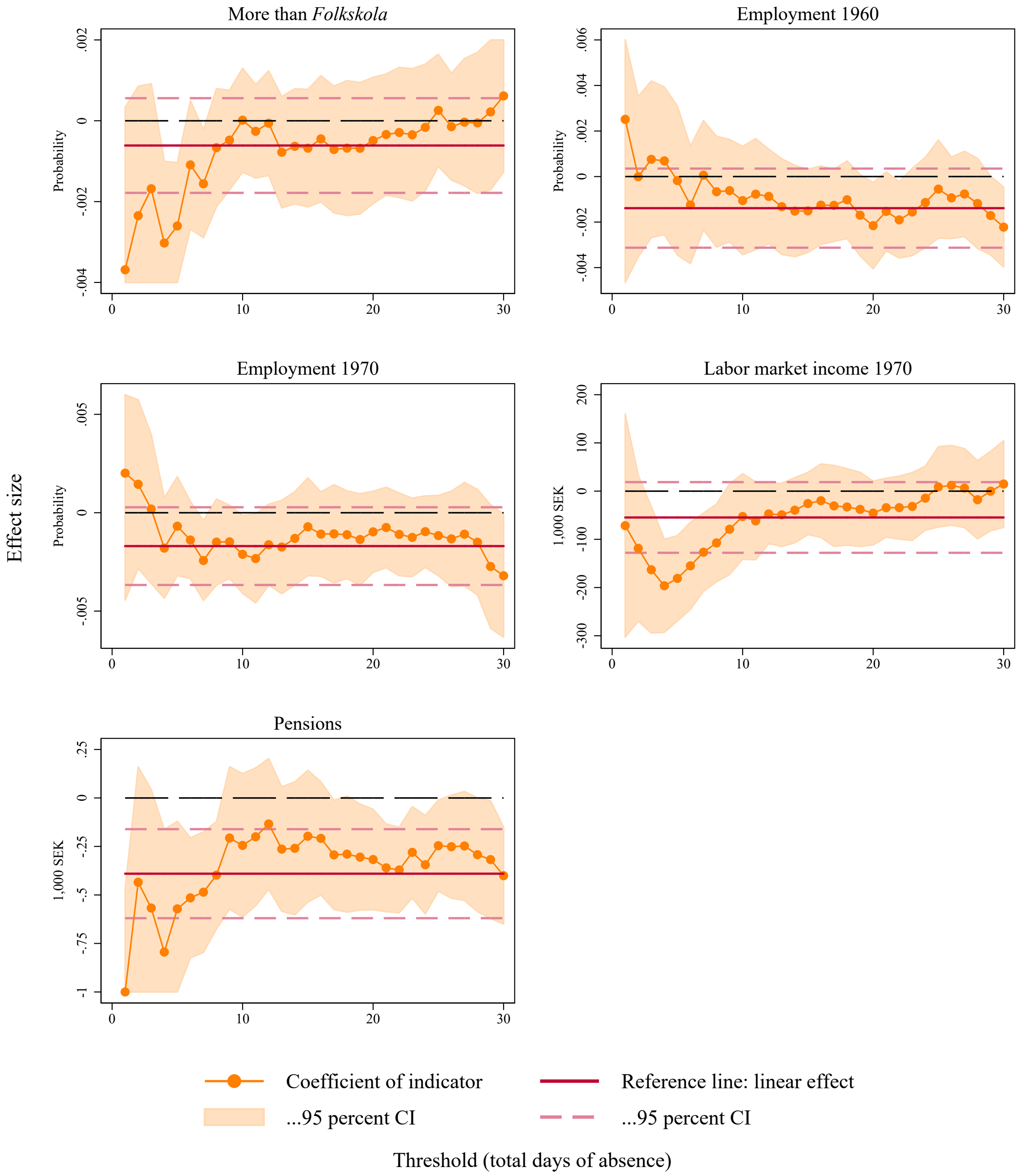
We describe this exercise and report the bounds around the short-term effects of absence in Appendix Section H.

Appendix G Long-term effects

Figure G1 reports the results of an exercise where we examine linearity for our long-term outcomes. Because of the fewer observations for the long-term outcomes, we only run the absence indicator up to the threshold of 30 or more days of absence. Overall, there is no strong evidence that absence has non-linear impacts on final educational achievement, adult employment, income, pensions or mortality.

Table G1 reports the results of our heterogeneity analysis. With the exception of a stronger effect of absence on male secondary education, we do not find significant differences in the effect of absence on long-term outcomes between gender or socio-economic groups.

Figure G1: Non-linearities in the effect of average days of absence in both grades on long-term outcomes using individual fixed effects



Notes: Own calculations, data sources and number of observations as for the baseline ling-term results. This graph plots the coefficient of a siblings FE regression of the long-term outcome on a binary indicator for average days of total absence over grades 1 and 4 and the same control variables as in the baseline long-term specification. The indicator threshold is given on the x-axis. The size of the orange coefficient plot is proportional to the number of observations for that the indicator is 1. The gray area indicates the significance band of the coefficient estimates. The red line depicts the linear effect of an additional day of absence in the baseline specification. Number of observations: more than *Folkskola* 8,567, employment 1960 7,434, employment 1970 8,567, income 1970 4,154, pensions 4,770, alive at age 70 8,567.

Table G1: Heterogeneity in the long-term effects by subgroup

	(1)	(2)	(3)	(4)	(5)
	Dependent variable				
	$>$ *Folk-skola*	Empl. 1960	Empl. 1970	Income 1970	Pensions
Gender (siblings FE)					
Absence	-0.0026^{**}	-0.0005	-0.0022	-102.6306^{*}	-359.9496^{*}
	(0.0012)	(0.0013)	(0.0017)	(56.9857)	(216.3130)
Absence× female	0.0037^{**}	-0.0004	0.0003	38.8400	-64.9566
	(0.0015)	(0.0015)	(0.0019)	(58.8859)	(290.5578)
Father's occupation (siblings FE)					
Absence	-0.0001	-0.0010	-0.0012	-68.4765^{*}	-484.4240^{**}
	(0.0012)	(0.0010)	(0.0016)	(38.7539)	(234.0316)
Absence× agri. worker	-0.0010	0.0008	-0.0019	-29.2021	195.8226
	(0.0017)	(0.0017)	(0.0022)	(47.2187)	(284.0396)
Sibling in sample (teacher FE)					
Absence	0.0017^{***}	0.0001	-0.0008	-13.6743	11.5491
	(0.0005)	(0.0005)	(0.0005)	(16.0547)	(74.3546)
Absence× sibling	-0.0035^{***}	-0.0013^{***}	-0.0003	-43.3646^{**}	-359.7924^{***}
	(0.0005)	(0.0005)	(0.0005)	(19.0670)	(96.3214)

Notes: Each panel states the coefficient of total days of absence (average over grades 1 and 4) as well as of an interaction between total days of absence and the subgroup indicator. The first two panels give results of a siblings FE specification. Number of observations: more than *Folkskola* 5,976, employment 1960 7,434, employment 1970 8,567, income 1970 4,154, pensions 4,770. The third panel uses all individuals with exam catalog and church record data to investigate whether the family size (and our sample selection for the baseline analysis) matters for the results. While cannot employ siblings FE in this specification, we do use teacher FE. The number of observations are: more than *Folkskola* 14,631, employment 1960 16,579, employment 1970 14,631, income 1970 14,462, pensions 12,457. About half of all individual have at least one sibling. Dependent variables defined as in the baseline long-term results. Parish-clustered standard errors in parentheses. Significance: $^{*}p \leq 0.1$, $^{**}p \leq 0.05$, $^{***}p \leq 0.01$.

Table G2: Long-term effects by school grade when absence occurs

	(1)	(2)
	OLS	Siblings fixed effects
More than Folkskola *(1=yes)*		
Grade-1 absence	-0.0002	-0.0010
	(0.0006)	(0.0010)
Grade-4 absence	-0.0009^{**}	-0.0009
	(0.0004)	(0.0007)
Employment 1960 (1=yes)		
Grade-1 absence	-0.0011^{**}	-0.0025^{***}
	(0.0005)	(0.0008)
Grade-4 absence	-0.0006	0.0017
	(0.0005)	(0.0011)
Employment 1970 (1=yes)		
Grade-1 absence	-0.0010	0.0009
	(0.0006)	(0.0013)
Grade-4 absence	-0.0002	-0.0016
	(0.0006)	(0.0010)
Labor market income 1970		
Grade-1 absence	-36.7989^{***}	-5.4947
	(12.7278)	(30.3230)
Grade-4 absence	-38.4402^{***}	-62.9667
	(14.8815)	(45.4935)
Pensions 2003–2008		
Grade-1 absence	-50.6785	-127.1366
	(76.3194)	(168.6815)
Grade-4 absence	-270.6766^{***}	-281.8781^{**}
	(80.9523)	(115.0553)

Notes: This table shows the results of absence in school on long-term outcomes by grade of absence. Each coefficient is taken from a separate regression. The first column gives OLS results, the second column shows the siblings fixed effects specification. Other than the absence measures the specification are the same as for the main results for average absence. Parish-clustered standard errors in parentheses. Significance: $^{*}p \leq 0.1$, $^{**}p \leq 0.05$, $^{***}p \leq 0.01$.

Table G3: Effect of academic performance in school on long-term outcomes (siblings FE specification)

	(1)	(2)	(3)	(4)	(5)	(6)
	Performance in					
	grades 1 and 4 pooled		grade 1		grade 4	
	women	men	women	men	women	men
More than Folkskola *(1=yes)*						
Performance	0.0600***	0.0798***	0.0677***	0.0318	0.0404*	0.0736***
	(0.0189)	(0.0283)	(0.0222)	(0.0244)	(0.0234)	(0.0267)
Observations	2,965	3,011	2,448	2,485	2,444	2,473
Employment 1960 (1=yes)						
Performance	0.0585	−0.0321*	0.0383	0.0041	0.0267	−0.0388***
	(0.0423)	(0.0167)	(0.0405)	(0.0163)	(0.0284)	(0.0143)
Observations	3,674	3,760	3,018	3,095	3,038	3,072
Employment 1970 (1=yes)						
Performance	0.0488	0.0246	−0.0386	0.0167	0.0406	0.0250*
	(0.0351)	(0.0178)	(0.0481)	(0.0225)	(0.0343)	(0.0135)
Observations	2,965	3,011	2,448	2,485	2,444	2,473
Labor market income 1970 (in 1,000 SEK)						
Performance	1.3119	2.7577***	−0.7691	2.7291**	0.3860	1.7826***
	(0.9908)	(0.8290)	(0.7512)	(1.3009)	(1.0503)	(0.6293)
Observations	2,937	2,949	2,427	2,434	2,420	2,419
Pensions 2003–2008 (in 1,000 SEK)						
Performance	6.8966*	14.2957***	6.3621**	15.5818**	2.2277	22.5995**
	(3.9785)	(4.9948)	(2.8612)	(7.3959)	(4.6353)	(9.2656)
Observations	2,469	2,303	2,032	1,890	2,031	1,866

Notes: This table gives the estimated effects of average grade points in math, reading and speaking, and writing (performance) on the long-term outcome variables by school grade and gender. The specification controls for the same factors as the siblings fixed effects specification in the main results for absence on long-term outcomes. Unlike to the main results, labor market income 1970 and pensions are measured in 1,000 Swedish krona (SEK) instead of SEK. Parish-clustered standard errors in parentheses. Significance: $^{*}p \leq 0.1$, $^{**}p \leq 0.05$, $^{***}p \leq 0.01$.

Table G4: Short-term effects using siblings fixed effects

	(1) Individual fixed effects	(2) Sibling fixed effects
Average grade points in units of SD (mean: 0, SD 1)		
Days of absence	-0.0045^{***}	-0.0055^{***}
	(0.0013)	(0.0009)
# observations	8,934	14,066
# individuals/families	4,467	3,716

Notes: Each cell states the coefficient of days of absence for a separate regression. Average performance over math, reading and speaking, and writing is standardized with mean 0 and standard deviation 1. The second row measures average grade points in units of pensions, see the data description in the text for details. Time-variant conditional variables: grade, range of grades instructed in the same classroom, length of the school year in weeks. Time-invariant conditional variables: female, born out of wedlock, twin birth, mother employed at the time of birth, born in hospital. Socio-economics fixed effects include full sets of fixed effects for the year and month of birth, year and month interactions, age, parent's year of birth, and the family's socio-economic status based on the first-digit HISCO code of the father. Parish-clustered standard errors in parentheses. Significance: $^{*}p \leq 0.1$, $^{**}p \leq 0.05$, $^{***}p \leq 0.01$.

Table G5: The effect of future absence on performance

	(1)	(2)
	Dependent variable: grade-1 performance	
	OLS	Siblings fixed effects
Absence in grade 4	-0.0029^{*}	0.0003
	(0.0018)	(0.0025)
# observations	5,499	5,499
# individuals/families		3,019

Notes: This table show the effect of days of absence in grade 4 on performance in grade 1. Control variables and fixed effects similar to the main results. Parish-clustered standard errors in parentheses. Significance: $^{*}p \leq 0.1$, $^{**}p \leq 0.05$, $^{***}p \leq 0.01$.

Table G6: Long-term effects of grade-4 absence, controlling for grade-1 performance

	(1)	(2)	(3)
	Siblings fixed effects		
	Baseline	Controlling for grade-1 performance	
	Grade-4 absence	Grade-4 absence	Grade-1 performance
More than Folkskola *(1=yes)*			
Coefficient	-0.0010	-0.0011	0.0645^{***}
	(0.0008)	(0.0008)	(0.0114)
Employment 1960 (1=yes)			
Coefficient	0.0016	0.0016	-0.0052
	(0.0011)	(0.0011)	(0.0190)
Employment 1970 (1=yes)			
Coefficient	-0.0016	-0.0016	0.0143
	(0.0010)	(0.0010)	(0.0136)
Labor market income 1970			
Coefficient	-61.0702	-62.0899	1461.5752^{***}
	(45.3810)	(45.1671)	(421.6319)
Pensions 2003–2008			
Coefficient	-284.9445^{**}	-248.1596^{**}	10101.5840^{***}
	(119.3931)	(105.0070)	(1602.2490)

Notes: Column 1 of this table shows the effect of absence in grade 4 on the long-term outcomes. This is similar to the (siblings fixed effects) main specification, but considers only absence in grade 4 (instead of pooled absence in grades 1 and 4). In column 2 and 3, we repeat this regression, additionally controlling for performance in grade 1 as proxy for unobserved confounding factors that correlate with absence (in grade 4) and the long-term outcomes. Column 2 gives the grade-4 absence coefficient, column 3 the grade-1 performance coefficient. Number of observations: More than *Folkskola* 3,874, employment 1960 4,789, employment 1970 3,974, income 1970 3,814, pensions 3,047. Parish-clustered standard errors in parentheses. Significance: $^{*}p \leq 0.1$, $^{**}p \leq 0.05$, $^{***}p \leq 0.01$.

Table G7: Differentiating for different reasons of absence for long-term outcomes

	(1)	(2)	(3)	(4)
	OLS		Siblings fixed effects	
	Total absence	Non-sick. absence	Total absence	Non-sick. absence
More than Folkskola *(1=yes)*				
Absence (average, grades 1 and 4)	0.0001	-0.0060^{***}	-0.0006	0.0006
	(0.0006)	(0.0017)	(0.0007)	(0.0022)
Employment 1960 (1=yes)				
Absence (average, grades 1 and 4)	-0.0001	-0.0052^{***}	-0.0005	-0.0036
	(0.0005)	(0.0017)	(0.0010)	(0.0045)
Employment 1970 (1=yes)				
Absence (average, grades 1 and 4)	-0.0005	-0.0005	-0.0015	0.0003
	(0.0006)	(0.0022)	(0.0012)	(0.0038)
Labor market income 1970				
Absence (average, grades 1 and 4)	-37.9619^{**}	-13.7218	-50.0327	7.4814
	(19.0897)	(61.6756)	(32.7365)	(154.4145)
Pensions 2003–2008				
Absence (average, grades 1 and 4)	-217.2837^{***}	-87.9434	-319.0513^{***}	706.7839
	(57.2104)	(268.0873)	(121.5442)	(430.9648)

Notes: The table reports coefficient associated with days of absence (average over grades 1 and 4) in separate regressions where the dependent variable is indicated in the first column. In columns 1 and 3 coefficients refer to total days of absence. In columns 2 and 4 coefficients refer to non-sickness absence. Controls include female, born out of wedlock, twin birth, born in hospital, mother employed at the time of birth and sets of fixed effects for the year and month of birth, year and month interactions. Specifications also controls for class size, the lowest and highest grade taught to students in the same classroom, and length of the school year in weeks, averaged across grade 1 and 4. Models also control for school and teacher fixed effects, and a dummy if the individual changes school or teacher between grade 1 and 4. The OLS models further control for parent's year of birth, and family's socio-economic status based on the first-digit HISCO code of the father. Standard errors clustered at the parish level in parentheses. Significance: $^{*}p \leq 0.1$, $^{**}p \leq 0.05$, $^{***}p \leq 0.01$.

Appendix H Bounds analysis

We employ the bounding approach suggested by Oster (2019), building on the idea of Altonji et al. (2005). Our goal is to bound the effect of absence assuming that the selection on unobservables is as strong as the selection on observables. We consider the case where the selection on unobservables is in the same or the opposite direction as the selection on observables, thus allowing the true effect to be overestimated or underestimated. The exercise is only helpful if the observables are informative with respect to the selection, so we control for a large array of control variables and the full set of siblings fixed effects. This removes factors such as constant family resources, parental preferences, and genetic endowment that are likely negatively correlated with absence and positively correlated with performance. As omitting these factors would likely cause an upward bias that challenges our implications, the bounding approach seems particularly useful in the application at hand.

The starting point is to compare the coefficient of absence in the baseline model ($\tilde{\beta}$), with the coefficient of absence in a simple linear regression of the dependent variable on absence and an intercept ($\dot{\beta}$). Formally, the bound around the coefficient of absence β^* is:[34]

$$\beta^* \approx \tilde{\beta} - \delta(\dot{\beta} - \tilde{\beta})\frac{R_{max} - \tilde{R}}{\tilde{R} - \dot{R}},$$

where the degree of proportionality of selection on observables to selection on unobservables, δ, is either set to 1 (unobservable selection goes in the same direction) or -1 (unobservable selection is in the adverse direction). In a second step, the movement in the coefficient of absence, $\dot{\beta} - \tilde{\beta}$, is re-scaled by the movement in the R^2 relative to the potential change in the R^2 (where $\tilde{R}$ and $\dot{R}$ denote the R^2 of the baseline model and the simple regression, respectively, and R_{max} denotes the highest possible value of the R^2).[35]

Table 5 in the main text shows the bound estimates for the long-term effects of days of absence. The bounds for the short-term effects are in Table H1. Comparing estimates in columns 1 and 2 gives an idea of the extent to which the raw correlation between absence and the outcome is robust to conditioning on a large number of observables (including siblings and other fixed effects). The estimates are remarkably robust and hardly change at all when including the full set of observables. This should to be kept in mind when considering the estimated bounds in columns 3 and 4.

[34] This expression is only an approximation, see Oster (2019) for the exact calculation. To calculate the bounds we use the Stata ado-file `psacalc` provided online by Emily Oster. All errors are our own responsibility.

[35] Following Hener et al. (2016) we consider as $R_{max} = \min(2.2 \times \tilde{R}, 1)$.

Table H1: Oster bounds for the short-term effects of absence in school

	(1)	(2)	(3)	(4)
	Coefficient of absence		Selection bias	
Dependent variable	Short regression	Intermediate regression	Same direction	Opposite direction
Average performance	-0.0047^{***}	-0.0045^{***}	-0.00986	-0.00194
	(0.0011)	(0.0013)		
	[0.00]	[0.65]		

Notes: Column 1 gives the coefficient of absence in the short regression where the outcome variable is regressed on absence and an intercept (without any controls), denoted by $\dot{\beta}$. The intermediate regression in column 2 repeats the individual fixed effects specification from Table 2 ($\tilde{\beta}$ in the model). Columns 3 and 4 report the bounds (β^*) when selection on unobservables goes in the same ($\delta = 1$) and the opposite ($\delta = -1$) direction as selection on observables, respectively, and is of the same magnitude. Number of observations as in Table 2. Parish-clustered standard errors for the regression coefficients in parentheses. The R^2 of the model is given in brackets.

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Heinrich-Heine-Universität Düsseldorf

Düsseldorfer Institut für Wettbewerbsökonomie (DICE)

Universitätsstraße 1, 40225 Düsseldorf

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