Health, Education and Cognition: Evidence from a Social Experiment[*]

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Abstract

The correlation between human capital and educational achievement is one of the most robust findings in the social sciences. Most studies analyzing the effects of exogenous changes in education restrict the outcome under study to wages or earnings from labor. In this paper we examine the effect of an education policy intervention - the introduction of a comprehensive school in Sweden that increased the number of compulsory years of schooling from 7 to 9 - on individual long-term health, cognitive and non-cognitive skills. We show that extra education results in significant gains in skills among children, but the estimated effects on long-term health are small. We also demonstrate the schooling reform had heterogeneous effects across family socio-economic backgrounds and initial skill endowments. Compulsory schooling helps close the skills gap and reduces health inequalities between individuals from high and low socio-economic backgrounds.

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1. Introduction

Human capital affects earnings and employment over the lifecycle and as such is the basis of the determination of standards of living. The set of skills that constitute it are persistent across generations undermining upwards intergenerational mobility and entrenching poverty. Thus the question of whether and how policy can be used to improve human capital development is of central importance.

We do know from early childhood experiments that early interventions can have substantial effects on both cognition and on earnings. However can later interventions, such as extending schooling, help? What effects do they have on important outcomes that determine human capital such as cognitive and noncognitive skills and health? We have a rare opportunity to examine these key questions by utilizing excellent cohort and administrative data in Sweden, collected for individuals some of whom were exposed to a major schooling reform. We are not the first to consider these issues, but we offer new evidence based on a quasi-experimental framework and high quality data that allow us to analyze many important outcomes within the same framework. Recently Cunha, Heckman and Schennach (2010) provide a systematic analysis of the optimal timing and effectiveness of interventions, based on a nonlinear factor model; Neal and Johnson (1995) also show that schooling can improve cognitive skills based on the quarter of birth instrument.[4] As far as health is concerned Lleras-Muney (2005) shows large effects of increasing schooling on mortality for the US, while more recently Clark and Royer (2012) report a zero impact for the UK. We bring to bear new evidence on all these outcomes and more, thus estimating the effect of education reform on a comprehensive set of characteristics that underlie human capital.

In this paper we analyze the effect of an education policy intervention on individual long-term health, cognitive and non-cognitive skills. Between 1949 and 1962 Swedish municipalities implemented a comprehensive school reform in a context of a social experiment. The reform implied an extension of the number of years of compulsory schooling from 7 or 8 years depending on municipality to a new national compulsory level of 9 years. The reform had a

[4] see Angrist and Krueger (1986)

sizeable impact on educational attainment[5] and led to an increase in labor earnings later in life in particular for children born in homes with low educated fathers (Meghir and Palme, 2005).

Our aim in this paper is to investigate some of the mechanisms that contributed to the effects of the education reform. In particular, we study its effects on measures of human capital such as the accumulation of cognitive and social skills, intermediate and longer-term health outcomes.

For this purpose, we construct a unique data set linking administratively held information together with survey data. We use register data including all Swedish men born between 1946 and 1957, which enables us to link assignment to the reform and health outcomes through different national data registers as well as results from military enlistment tests. For a random subset of the population born in 1948 and 1953 these data include cognitive skills test scores taken in 6th school grade and at the time of the military draft exams. The data also allow assessment of non-cognitive skills such as leadership ability from psychologists' evaluation that was conducted at the time of military enlistment.

The Swedish compulsory schooling reform had a significant positive effect on the cognitive skills of men of military enlistment age (18-19 years old). The average individual experienced an increase of seven to ten percent of a standard deviation, the strongest gains accruing to boys coming from low SES families and with low initial cognitive skills measured pre-reform in the 6th school grade. To test the robustness of the linear difference-in-difference estimates we also present results from a non-parametric difference-in-differences estimation following Altonji and Blank (1999) and Athey and Imbens (2006). The non-parametric estimates confirm the results from the main analysis. Gains in non-cognitive skills, as measured by the Army draft exams, accrued mostly to boys from high SES backgrounds.

To measure the effects on health we use mortality and hospitalization by cause as well as sick-leave pay receipt. The national Cause of Death Register enables us to distinguish between different causes of death - and groups of causes, such as "treatable" and "preventable" - to account for the fact that education can affect different aspects of health investment, and thereby cause-specific morbidity and mortality, differently. Data from the national Patient Register,

[5] Meghir and Palme (2005); Holmlund (2007); Spasojevic (2010); Meghir, Palme and Simeonova (2012)

including information on all hospital stays in Sweden enable us to do similar analysis on a set of intermediate health measures.

The evidence we present is an important step towards our better understanding of the way policy affects human capital even more so because many of the outcomes we consider are interconnected. For example it has been hypothesized that cognitive skills are important in improving health (Grossman, 2006; Cutler and Lleras-Muney, 2008). However, there is an additional important contribution of our research that relates to the impact of interventions relatively late in a child's life and on the more general question of the appropriate timing of interventions in human capital. There exists evidence that investments in human capital at early ages (such as during the pre-school years) can have positive and lasting effects on skills and labor market success (see review by Cunha et al, 2006). Previous research has also demonstrated that extra education obtained as late as the teenage years could have positive effects on cognitive skills as measured by the Armed Forces Qualification Test (AFQT). The AFQT is an appealing comparative test measure as it is administered around the same time as the Swedish Army draft tests and purports to measure similar skills. Hansen, Heckman and Mullen (2004) find that on average, one additional year of schooling in high school results in an increase of 0.16 to 0.19 standard deviations in the AFQT score. They also report that the effects are larger for years of schooling attained before the 9^{th} grade (as in this research) than in grades 10-12. Cascio and Lewis (2005) use National Longitudinal Survey of Youth (NLSY) '79 data and differences in schooling attainment arising from school-entry cutoff dates to show that an extra year of schooling increases minorities' achievement on the AFQT by one third of a standard deviation. Higher AFQT scores are strongly correlated with labor market success in the US. Neal and Johnson (1996) report that one standard deviation difference in AFQT scores increases log male earnings by around 0.17 log points in a sample of NLSY youth in the US. Carneiro et al (2006) add extra years of observation and show that the effect becomes stronger over time. The estimated effects of AFQT on wages in the US are about 50% larger than the comparable estimates for Sweden. This is not surprising given the compressed wage distribution in Sweden compared to the US (see discussion below and results in Table A4). Our estimates are almost identical with those of Lindqvist and Vestman (2011), who also use Swedish data.

A large portion of the literature attempting to evaluate the causal effect of extra schooling on health has made use of compulsory schooling reforms for identification. Lleras-Muney (2005) considers the case of the US in the first half of the 20th century, when many states increased the number of years children had to attend school. Her results imply that an extra year of schooling reduces the 10-year mortality rate by over 6 percentage points given a mean mortality rate of ten percent. She considers cohorts born in the US in the first quarter of the 20th century, which may be less relevant to the more recent population where health infrastructure and education have both improved substantially. [6]

All previous large-sample studies examining the causal relationship between education and health (Royer and Clark, 2012; Lleras-Muney, 2005; Oreopoulos, 2006) conduct the analysis on the cohort level. In general this will provide consistent estimates, but does not allow an analysis of heterogeneous effects depending on socioeconomic background. This poses the question as to whether the effects of compulsory schooling reforms and generally of interventions that are most likely to affect lower socio-economic status individuals are missed when averaged with the effects on higher SES groups that are unaffected. In this study we are able to estimate effects both by parental background and by early ability, for both the cognitive and the health outcomes.

Beyond this, because of the detailed nature of our data we are also able to look at a number of different health indicators. While mortality is obviously important, we can also look at other outcomes such as hospitalizations and sick leave pay: these measures may capture intermediate, but quite relevant, improvements in health, even if mortality is unaffected. Moreover we can also consider mortality by cause distinguishing between more preventable conditions that depend on behavior, such as lung cancer and liver cirrhosis, vis a vis causes that are not considered preventable and obviously linked to suboptimal health behaviors (see e.g. McGinnis and Foege, 1993; Mokdad et al, 2004).

Taken together our results show that the compulsory schooling reform improved the cognitive skills of those from lower SES backgrounds and with low initial levels of ability, and had small positive effects on the health outcomes of this group. The most convincing

[6] Mazumder (2008) revisits these results by including state-specific time trends that significantly diminish the estimated coefficients. Also see the corrigendum to Lleras-Muney's original 2005 paper on her website http://www.econ.ucla.edu/alleras/research/papers/8523_Lleras_Correction.pdf

manifestation for the latter comes from the decline in sick-pay incidence, but can also be seen in very small declines in early mortality. These results match up with the increases in earnings for this group, established in an earlier paper[7]. For the higher SES people there are no health benefits and no earnings gains. However, they score much higher on the non-cognitive skills test administered at the time of enlistment in the army; perhaps the fact that this group now attends nonselective school with a broader mix of people improves their social skills; this however is not reflected as far as we can see in their labor market performance.

2. The Comprehensive School Reform

3.1. The Swedish school system before and after the reform

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

In 1948 a parliamentary committee proposed a school reform that implemented a new nine-year compulsory comprehensive school.[8] The comprehensive school reform had three main elements:

1. An extension of the number of years of compulsory schooling to 9 years in the entire country.

2. Abolition of early selection. Although pupils in the comprehensive schools were able to choose between three tracks after the sixth grade - one track including vocational

[7] Meghir and Palme (2005)

[8] The school reform and its development are described in Meghir and Palme (2003), Meghir and Palme (2005), and Holmlund (2007). For more detailed reference on the reform, see Marklund (1981).

training, a general track, and an academic level preparing for later upper secondary school - they were kept in common schools and classes until the ninth grade.

3. Introduction of a national curriculum. The pre-reform compulsory schools were administrated by municipalities and the pre-reform curriculum varied between municipalities.

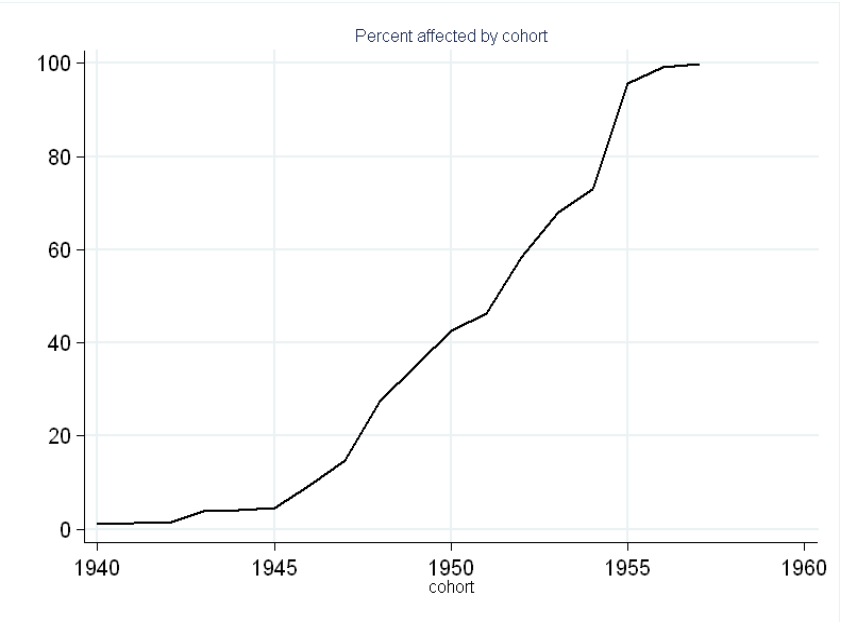
3.2. The social experiment

The social experiment with the new comprehensive nine-year compulsory school started during an assessment period between 1949 and 1962, when the final curriculum was decided.[9] The proposed new school system, as described above, was introduced in municipalities or parts of city communities, which in 1952 numbered 1,055 (including 18 city communities). Figure 1 shows the take up rate of the experiment by cohort. It is evident from Figure 1 that the cohorts included in our empirical analysis, born between 1945 and 1957, cover the entire period of implementation of the comprehensive school. In 1962 it was decided that the new comprehensive school would become the standard education in Sweden. The last class that graduated from the old schooling system did so in 1970.[10]

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

[10] Table A1 in the appendix shows the number of observations in each birth cohort and the proportion assigned to the reform.

Figure 1: Percentage share of birth cohort assigned to the post reform (comprehensive) school system.



The selection of municipalities was not based on random assignment. However, the decision to select the areas was based on an attempt to choose locations that were representative for the entire country, both in terms of demographics as well as geographically. At first the National Board of Education contacted the municipalities, or sometimes they themselves applied to participate. From this pool of applicants a "representative" sample of municipalities was chosen. Municipalities could elect to implement the comprehensive school starting with first or fifth grade cohorts. Once the grade of implementation was fixed, all individuals from the cohort immediately affected and all subsequent cohorts went to comprehensive school. The older cohorts continued in the per-reform school.

Meghir and Palme (2005) and Holmlund (2007) study the effect of the comprehensive school reform on educational attainments.[11] Meghir and Palme's (2005) estimates for their entire sample are 0.252 additional years for males and 0.339 years for females; for low SES persons the estimates are 0.3 extra years for males and 0.512 for females. Holmlund has estimates in the range 0.21-0.61 additional years of schooling for men and 0.13-0.44 for women.

3. Data and Measurement

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

Our dataset combines survey data from 1948 and 1953 together with various sources of administrative data to link information on income, schooling, military tests and health.

The data for the *1948 and 1953 cohorts* was originally obtained by the Individual Statistics (IS) project of the Institute for Education at the University of Göteborg (see Härnqvist, 1968a, 1968b, and 2000, for an overview of the project). These data include all children born the 5th, 15th or 25th in each month – accounting for about 10 percent of each cohort – and were collected when the respondents were in 6th grade, in the spring of 1961 and 1966 for the 1948 and the 1953 cohorts, respectively. The data include information on the educational level of children's parents as reported by the children, administrative information on grades in Swedish, English, and Mathematics, as well as results from three cognitive (IQ) skills tests – number series (math), opposites (verbal), and a visual/spatial IQ test called "folding". We use these cognitive tests to construct a measure of cognitive ability for individuals in 6th grade based on the cumulative score obtained. The score on each test ranges from 1 (lowest) to 40 (highest). The highest cumulative score obtainable is 120. A more detailed description of the cognitive skills tests can be found in the Appendix. The measure we use is based on the original score, but standardized to have mean zero and unit standard deviation. We construct separate standardized score measures for the two cohorts, so that each child is compared only to children from the same birth cohort.

The second source of data is *The Swedish Army draft tests*. All Swedish males are required to take the Army Enlistment tests in their 18th or 19th year. The enlistment exams include a detailed physical exam, a battery of cognitive ability tests, and an interview with a psychologist. There are four types of cognitive tests – a verbal test, a test of spatial and visual intelligence called "metal folding" in Swedish, technical comprehension and logic. An assessment of non-cognitive skills is given after an interview with trained psychologist. The interview is based on a manual that specifies topics to be discussed and conducted as a guided conversation between the psychologist and the conscript. The purpose of the interview is to assess young men's ability to cope with the stress of military service and their expected performance in case of war[12]. Military

[12] These include: willingness to assume responsibility, independence, outgoing character, persistence, emotional stability, initiative (see Linqvist and Vestman, 2011, citing Lothigius, 2004). Difficulty accepting authority and violent aggressive behavior are among the negative characteristics psychologists look to identify. Obsession with the army and military topics is considered a negative trait (Lothigius, 2004)

experts realized early on that social skills are important in generating group cohesion, and therefore men who were thought incapable of functioning well in a social group were considered unfit for military service. The nature of the assessment emphasizes social skills, thus we use non-cognitive skills and social skills interchangeably in the text.

In the early years when the non-cognitive skills component was first assessed, such as in the 1948 cohort we study here, only men who scored above average in the cognitive skills tests were also interviewed by psychologists. This rule was abolished over time and in the 1953 cohort all men had to sit for both types of assessment. Importantly, a draftee could not avoid being called for service by flunking the cognitive test. The cognitive skills test could only affect the type of unit one was called to serve in. On the other hand, obtaining a very bad score on the non-cognitive test could potentially lead to being judged unfit for military service. A very bad non-cognitive skills test score would indicate that the interviewer noticed serious anti-social and potentially dangerous traits in the draftee that could make him unfit to bear arms. Lindqvist and Vestman (2011) offer a detailed explanation of background leading to the introduction of these tests, as well as some evidence that bad non-cognitive skills were deemed somewhat important in the draft decision.

The cognitive skills tests consisted of 40 questions each. A draftee could obtain a maximum score of 160. A detailed description of the cognitive tests components can be found in the Appendix. Similarly to the 6^{th} grade cognitive measure, we construct a cognitive abilities measure based on the Army draft IQ scores. We take the simple sum of all four Army tests and standardize the resulting score to have a mean of zero and unit standard deviation. We construct separate standardized scores for the two cohorts, so that each young man is only compared to young men born in the same year.

Table 1.1: Descriptive statistics of the 6th grade and Army draft data

	Schooling and skills			
	1948 cohort		1953 cohort	
Variable	No reform	Reform	No reform	Reform
Verbal IQ 6th grade test	21.97	22.13	22.51	23.76
	[6.85]	[6.91]	[6.79]	[6.62]
Spatial IQ 6th grade test	21.39	21.91	21.30	22.67
	[7.44]	[7.66]	[7.60]	[7.58]
Maths IQ 6th grade test	19.43	19.66	19.10	20.59
	[8.02]	[8.14]	[8.16]	[8.21]
Army test verbal score	27.30	28.01	24.24	25.23
	[5.58]	[5.50]	[5.49]	[5.32]
Army test visual score	22.28	23.06	21.85	23.50
	[6.24]	[5.93]	[6.21]	[6.07]
Army test technical score	12.13	12.55	13.59	14.04
	[4.25]	[4.18]	[3.82]	[3.77]
Army test logic score	32.75	33.59	31.93	32.69
	[7.89]	[7.90]	[7.85]	[7.95]
Composite army cognitive score	62.80	63.72	62.92	67.02
	[17.95]	[18.53]	[18.33]	[18.19]
Non cognitive skill assessment+	5.16	5.40	4.83	5.26
Army (scale 1-9)	[1.78]	[1.74]	[1.98]	[1.94]
Father's education > compulsory	11.5%	15.5%	12.4%	19.2%
Completed years of schooling in '00	11.1	11.8	10.7	11.5
Old compulsory level	24%	4%	23%	2%
New compulsory level	9%	22%	19%	35%
Vocational school[13]	27%	30%	27%	27%
Higher education[14]	40%	44%	31%	36%
Sample size	3201	1431	829	3195
		Labor market outcomes panel		
Mean labor earnings in '000 SEK	190.6	205.5	168.5	185.8
(period: 1985-2005)	[106.7]	[116.5]	[90.1]	[104]
Sample size	45760	23232	13232	55776

+Note: non-cognitive skill assessment for the 1948 cohort is conditional on a cognitive skills score being above the mean

Basic descriptive statistics for the 6th grade and Army draft data are presented in Table 1.1. The data are split by birth cohort and reform status.[15] The key take-away from this table is

[13] Old compulsory school is 7 or 8 years of education, depending on municipality; new compulsory school is 9 years nation-wide; vocational school is schooling beyond 9 years of education (but less than college) that includes vocational training. Note that only academic high school completion enables students to apply for college.

[14] Includes: completed high school, some college, college, and post-graduate education

that the differences in average 6th grade cognitive skill tests scores between the reform and the non-reform groups (which were taken at a stage of schooling not affected by the reform) are minimal for both the 1948 and 1953 cohort. While we will be basing our estimation strategy on a difference in differences approach, which allows the distribution of baseline characteristics to differ between reform and non-reform municipalities, such similarity does reinforce the validity of the empirical strategy. Nevertheless, the need to control for differences across reform and non-reform municipalities becomes apparent when we notice that the individuals assigned to the reform during the experiment tend to have a higher level of SES as reflected in the average education of their fathers.

Data on educational attainments is obtained from the *1990 Swedish education register*, containing detailed information on the highest education obtained by each individual in 1990. Data on educational attainment of the parent generation is obtained from the 1970 census. Since this census only contains information on all individuals of age 60 and younger, we are missing information on all parents who were older, deceased or had permanently emigrated by 1970. We code family socio-economic status based on the father's highest level of education. If the father had any education beyond the basic compulsory level, we code the child as coming from a high socio-economic status family. If the father had only the minimum required level of education (7 years), the child is coded as low SES.

The fourth source of data is *the Swedish population censuses*, which we use to estimate the effects of extra schooling on long-term health. The sample comprises all individuals born in Sweden between 1946 and 1957. These cohorts consist of 1,461,785 individuals, of whom 746,201 are males and are 715,584 females. Information on reform status is available for 745,330 men and 714,694 women. The reform assignment variable is obtained in two steps. First, we use the name of the church parish of birth in order to obtain the municipality code according to the 1953 Swedish municipality division. Second, based on the year and municipality of birth, we use an algorithm based on historical evidence on reform implementation in each municipality provided by Helena Holmlund and described in Holmlund (2007) to assign reform status to each individual in the sample. We also use

[15] For variables that can be affected by the reform no causal interpretation should be given at this point because we need to control for municipality effects through a diff-in-diffs strategy as the reform was not randomly allocated.

information on the parents of these individuals. Identities of the parents were obtained from the *multi-generation register* provided by Statistics Sweden.[16]

Data on date of death is obtained from the *national Cause of death register* (Socialstyrelsen, 2009a). This register contains date and three digit ICD 9 codes for the main underlying cause of death for all Swedish citizens. Mortality data for this sample are available between 1985 and 2005. Thus we can only estimate effects on mortality between the ages of 28 and 60. While this puts age restrictions on the conclusions we draw using this sample, the period does cover the most productive periods of a person's life. Our mortality findings are economically meaningful even though we do not observe mortality among older adults and the elderly.

Data on all hospital admissions between 1987 and 2005 was collected from the *Swedish National Patient Register* (Socialstyrelsen, 2009b) This register also contains administrative information such as date of admission, number of days in hospital care as well as discharge diagnoses classified according to the 9th and 10th versions of International Classification of Diseases (ICD). The National Patient Register records a hospital admission only if it included an overnight hospital stay. Emergency room visits and shorter-term (less than 24 hours) inpatient stays are not recorded.

The data on sick-pay insurance income for each year between 1991 and 2006 stems from the *National Social Insurance Board registers* including all transactions between the social insurance administration and the insured citizens. These registers were matched to our original sample. Sweden has a compulsory sick-pay insurance replacing foregone earnings from temporarily lost work ability due to illnesses or other health deficiencies. The replacement level is currently 80 percent of forgone earnings up to a social security ceiling, but has varied somewhat during the period covered in the data (for a detailed description of the sick-pay insurance system in Sweden, see Johansson and Palme, 1995 or 2005). A certificate from a physician, certifying inability of the insured individual to do his or her regular work, is required on the eighth day in the sickness spell. Payments for the first week in a sickness spell is provided by the employer and consequently not included in our data.

[16] Statistics Sweden (2003) Flergenerationregistret 2002. En beskrivning av innehåll och kvalitet. Statistics Sweden. Avdelning för Befolknings och Välfärdsstatistik.

Table 1.2 below reports the basic characteristics of the individuals born between 1946 and 1957 and educated in the two systems. The summary table represents national data available through different registries. Swedes who went through the old schooling system are older as the reform was rolled out nationally over time. Those who went through the post-reform school system obtained on average half a year more of education. The difference in education levels is, as expected, most striking at the lowest level. We see the old compulsory schooling level attained by only 2.7% of the treated vs. 19.9% of the untreated individuals. The second largest difference between the two groups is in attaining education level 2, the new compulsory level. Swedes affected by the reform are 8.8 percentage points more likely to attain that education level compared to those who went through the old schooling system.

Table 1.2: Summary statistics of main outcomes and controls [standard deviations in square brackets]. Population of men born 1946-1957

Variable	Pre-Reform	Post-Reform
Died by 2006	0.056	0.04
Died from cancer	0.011	0.007
Died from circulatory disease	0.0125	0.007
Died from preventable causes	0.0127	0.01
Died from treatable causes	0.0056	0.0034
Hospitalized from any cause 1986-2005[17]	0.549	0.51
Hospitalized with cancer	0.036	0.022
Hospitalized with circulatory disease	0.132	0.092
Hospitalized with preventable disease	0.007	0.004
Hospitalized with treatable disease	0.185	0.157
Sick leave pay received	0.437	0.397
Average age in 2006	57	53
	[2.43]	[3.02]
Number of Observations	324945	420087
	Education panel	
Years of education	10.92	11.49
	[3.00]	[2.741]
Old compulsory level of education	19.9%	2.7%
New compulsory level	9.4%	18.2%
Vocational education[#]	44%	48%
Higher education	27%	31%
Number of Observations	295677	395538

#Note: Vocational education is generally two years of occupational specific education after the compulsory level; Upper secondary educations is in most cases three years of general education required for entrance to higher education; Higher education is at least three years of post-upper secondary education.

We define the different cause of death categories as mutually exclusive. This means that an ICD death code features in one category only. For example, death from lung

[17] Hospitalizations excluding hospitalizations for delivery and related obstetrics procedures

cancer is clearly due to cancer, but we have grouped lung cancer and liver cirrhosis under the preventable causes of death category. That is why an individual who died from lung cancer would be classified as having died from a preventable cause of death. At the same time, someone who died from pancreatic cancer is classified as having died from a cancer-related cause. In Appendix Table A2 we list the ICD causes of death that feature in each separate cause of death category as defined in Table 1.2. The same grouping of diagnoses is used to define hospitalization by cause.

4. The Association between Education and Human Capital

As a descriptive device we document the relationship between our various outcome measures and schooling. A summary of the results for cognitive test scores, mortality, hospitalizations and sick pay are presented in Table 2 below. A year of education is associated with 17% of a standard deviation improvement in test scores. It is also associated with better health: a year of education is associated with a decline in the mortality hazard rate by about 10.5 percentage points (pp); a decline in hospitalizations by 1.4pp and a decline of sick pay receipts by 1.9pp.

The overall mortality rate up to 2006 in the population of men born 1946-1957 is 4.7%. An additional year of schooling reduces this rate by 10.06% to 4.23%. In 1986 when our mortality data start, the average man from these cohorts could expect to live for 228.7 months in the next 20 years ((1-0.047)*12 months*20 years). An additional year of education is associated with an increase in life expectancy of about a month (expected months of life for someone with an extra year of education compared to the mean level of education (1-0.1006*4.7)*12 months*20 years).

The average hospitalization rate in the sample is 52 per cent. According to the estimates in column (3) of Table 2 below, an additional year of education is associated with a decline in the risk of even having been hospitalized in 20 years by 1.4 percentage points, equivalent to 2.7 percent of the mean.

Table 2: Years of schooling, cognition and health (men only)

	(1)	(2)	(3)	(4)
	Cognitive test scores	Mortality	Ever hospitalized 1986-2005	P(sick pay) in the year
Sample	’48 and ’53 cohorts	Population	Population	Population panel
Years of education	0.1705***	0.894***	-0.014***	-0.019***
95% confidence interval	(0.16, 0.18)	(0.88, 0.91)	(-0.015, -0.013)	(-.0191, 0.0189)

N	7894	691756	691756	12223260
Model	OLS	Cox	OLS	OLS
All regressions include cohort dummies and municipality fixed effects				

Notes: reform status is assigned based on the municipality of birth; all municipalities included except Stockholm.* indicates significance at the 10% level, *** indicates significance at the 1%; all standard errors clustered on the municipality of birth level

An additional year of education decreases the probability of having taken any sick leave in a year by approximately 2 percentage points. This is a non-trivial 4.8 percent decrease from the mean rate of 42 percent.

These are all large effects and the extent to which they represent causal impacts, at least for those affected by the reform at the lower end of the educational distribution, is what we set out to investigate.

The estimated effect of an additional year of schooling on cognitive skills is also large and important. As Table 3 demonstrates, one standard deviation increase in cognitive skills is associated with an increase in lifetime earnings of about 11%. Increasing social skills by one standard deviation is also associated with an increase in earnings, but the effect is about 25 percent lower than the comparable estimate for cognitive skills. Among individuals with identical measures of social skills, one standard deviation increase in cognitive skills is associated with 9.6 percent increase in lifetime earnings.

The lower panel of Table 3 shows the correlations of cognitive and social skills with health outcomes. One standard deviation positive change in cognitive skills is associated with over twenty percent reduction in the mortality hazard, and over 4 percent decrease in the probability of ever having been hospitalized. Interestingly, we obtain estimates of similar magnitude for social and cognitive skills in the mortality regressions, while the estimates for early retirement and hospitalization clearly show a larger impact of cognitive skills.

Table 3: The association between cognitive and non-cognitive skills, labor market success and health. Survey data from the ’48 and ’53 cohorts.

	(1)	(2)	(3)
	Log earnings 1985-1996		
	OLS regression		
Army cognitive score	0.117***		0.096***
(standardized)	(0.005)		(0.006)
Army non-cognitive score		0.082***	0.060***
(standardized)		(0.007)	(0.007)
Observations	86,883	69,640	69,640
R-squared	0.278	0.290	0.307
		Mortality	

	Cox proportional hazard regression		
Army cognitive skills (standardized)	0.788*** (0.706 - 0.880)		0.855 (0.708 - 1.032)
Army non-cognitive (social) skills (standardized)		0.834** (0.722 - 0.963)	0.868* (0.745 - 1.012)
	Hospitalization		
	Linear probability OLS regression		
Army cognitive skills (standardized)	-0.04340*** (-0.0568 - -0.030)		-0.04334*** (-0.06282 - -0.02387)
Army non-cognitive skills (standardized)		-0.02780*** (-0.04164 - -0.01396)	-0.01702** (-0.03225 - -0.00179)
Observations	8356	6555	6555
R-squared	0.12645	0.14605	0.15035

Robust 95% confidence intervals in parentheses**,** clustered on the municipality of birth level**; ***significant at 10%; ** significant at 5%; ***** significant at 1%

5. Empirical Strategy

Our empirical strategy is based on comparing the evolution of outcomes across cohorts in municipalities that introduced the reform for the younger cohort but not the older one to those who either introduced it for none or to those who introduced it for both. Underlying all our specifications is a set of assumptions that justify difference–in-differences. Formally, using the notation in Athey and Imbens (2006) denote the outcome in the untreated state by $Y^0 = h(m_i, C)$ and define m_i as an unobservable characteristic. Its distribution may differ across municipalities, but we require the distribution of this unobservable to be the same across cohorts *C*. Moreover the function h has to be strictly monotonic in the unobservable *m*.[18] These assumptions allow different sorting of individuals in treatment and control municipalities, but this sorting must be permanent and not change across cohorts being compared. Then the cohort effect *C* reflects a trend between cohorts, which is common across municipalities. The fact we use the municipality of birth to assign reform status rather than the actual municipality of treatment strengthens the validity of this assumption because it controls for the composition effects that can arise as people sort themselves into different locations as a result of the reform. On the one hand, this means that the estimates are of the "intention-to-treat" type. On the other hand we avoid selection issues coming from differential endogenous mobility.[19]

[18] Discrete outcomes, which violate strict monotonicity, require further identifying assumptions.
[19] However, the evidence for selective mobility is weak if there at all, as shown in Meghir and Palme (2005).

In addition to these basic assumptions we also impose certain functional form restrictions. In particular for mortality and mortality by cause we use Cox proportional hazards models and competing risk models where it is straightforward to control for the 1000 or so municipality fixed effects and the cohort effects. The Cox proportional hazard function in this case takes the form

$$I_{1,i,m,t}\left(r\middle|R_{i,m,t},T_i,M_i\right)=I_0\left(r\right)\exp\left\{\alpha+\beta_1 R_{i,m,t}+\gamma_1{}'T_i+\gamma_2{}'M_i\right\},$$

For cognitive and social skills we use the standardized test score across all sub-tests and use linear difference–in-differences as well as the nonparametric approach of Athey and Imbens (discussed below). Finally for binary outcomes[20] (incidence of hospitalizations, receipt of sick pay) we use the linear probability model. Our choice has been motivated by the fact we need to control for 1000 or so municipality fixed effects and 12 cohort effects. For relatively small treatment effects, when both approaches have been used in a similar context, the results are almost identical.[21]

Parental inputs into child human capital production and the response to government interventions may differ depending on parental socio-economic (SES) background. For example, while we may expect wealthier parents to substitute resources away from children as the government increases inputs, the behavior of poorer parents may be quite different: if a small amount of resources is not in itself capable of generating returns poor parents may refrain from investing in their children. However, when more resources become available it may now become worthwhile for parents to invest. In this case poorer parents may crowd in resources. Finally, the interventions themselves, such as increasing compulsory schooling, may only be relevant for lower socio-economic groups. For all these reasons we present results by parental socio-economic status (SES) and when using only the data from the 1948/53 cohorts, by ability. Moreover, by controlling for background characteristics we relax the underlying assumptions of the diff-in-diffs specification. Finally we can also allow for differential municipality trends since we observe outcomes for a number of years before and after the reform when we rely on administrative data.

[20] The army cognitive skills tests consist of four separate categories – a verbal (reading comprehension) test, ”metal folding” (visual/spatial test), technical comprehension and logic. The cognitive skills tests consist of 40 questions each. A draftee could obtain a maximum score of 160.

[21] See for example Meghir, Palme and Schnabel (2011).

As a further robustness check we also implement the nonlinear diff-in-diff estimator of Athey and Imbens (2006) for the cognitive test scores. This non-parametric procedure can be particularly useful in the context of test scores because the scale there is completely arbitrary and there is no guarantee that the scale used is the appropriate one for implementing the usual linear difference in differences procedure; indeed even the sign of the treatment effect can switch is we use linear difference in differences with an ad-hoc scale (see Meghir and Rivkin, 2011).

Missing data for non-cognitive skills

For the 1948 cohort the Swedish army only required non-cognitive skills interviews and scoring for those individuals who had achieved a score of 5 or more on a cumulative cognitive skills scale. The scale ranges from 1 to 10 and is a simpler representation of the cumulative distribution of the cognitive skills test scores. Only men with scores 5 and above, as prescribed by the army manual, obtained non-cognitive skills evaluation. In the 1953 cohort all men obtained a score by a psychologist, regardless of their performance on the cognitive tests.

To solve the missing data problem we proceed as follows. Define by y^c the cognitive score and y^{nc} the non-cognitive one and suppose they are determined by the equation

$$y_i^c = a + b'x_i + u_i$$

$$y_i^{nc} = d + c'x_i + v_i$$

where x includes all the regressors on the right hand side of the diff-in-diffs regression. Now we make the control function assumption $E(v_i|u_i, x_i) = E(v_i|u_i)$ and take the conditional expectation of the second equation given the cognitive score. We thus get that

$$E(y_i^{nc}|y_i^c, x_i) = E\left(y_i^{nc}\middle|u_i\ , x_i\right) = d + c^{'}\ x_i + E(v_i|u_i)$$

$$= d + c^{'}\ x_i + f(u_i)$$

where the function f can be estimated non-parametrically.[22] Under joint normality this function is linear. However, the key point is that by conditioning on the residual from the cognitive regression score we control for the endogenous selection. Moreover, in this case

[22] We use a polynomial to approximate the function *f*.

where we observe the entire distribution of the cognitive scores no exclusion restriction is required; the only assumption is the control function assumption.[23]

6. Results

6.1 Effects of the reform on educational attainment

Earlier papers have established the impact of the reform on educational attainment. [24] We confirm that these results hold for our extended administrative sample in Table 3 with similar estimates, although we now include many more cohorts and there is no reason for the effects to be the same. We also exclude individuals who were born in Stockholm. It appears that in Stockholm boys in the same birth cohort could attend schools of both types, and thus it is not immediately clear how they should be classified in terms of reform status.

The average effect of the reform was an increase in attained years of education of 0.22, i.e. little over two and a half months. Both high and low SES background children are affected by the reform, but the magnitudes of the effects and the statistical significance are very different. High SES children's educational attainment increases by 5% of a school year, while low SES children's education increases by 28%, or almost three and a half calendar months.

Table 3: The effect of the educational reform on years of completed schooling for men. Population of men born 1946-1957

Men	*(1)* All	*(2)* Low Father's education	*(3)* High father's education
Reform	0.226***	0.284****	0.058
	(0.045)	(0.022)	(0.043)
Sample size	627537	359251	177614

Notes: reform status is assigned based on the municipality of birth. All birth years (1946-1957) and all municipalities included except Stockholm. Municipality and cohort fixed effects included in all specifications; *** indicates significance at the 1% level; standard errors clustered on the municipality of birth level. The sample sizes of low and high SES subsamples do not add up to the total number of observations because of missing information on father's education.

Thus we expect the bulk of the reform effect on human capital to be demonstrated among men with low SES background. However, since the new educational system abolished tracking for

[23] Using residuals in this way goes back to Telser (1961) and underlies the extensive literature on using control functions.

[24] Due to extensive previous research on the schooling reform our discussion here is limited. See discussions of the nature and the validity of the reform as a social experiment is in Holmlund (2007) and Meghir and Palme (2005).

children over 12, it also affected the education received by the higher SES children who were the main group attending the academic track. It is thus possible that they are also affected by the reform in ways that are not easy to predict.

6.1.1 Cognitive skills

Table 4 shows the results for the effects of the school reform on cognitive skills. These measures are obtained by the army during the army draft tests and are unrelated to the school curriculum. Their nature is that of an IQ test. The main data source here is the 1948 and 1953 cohorts for whom we have this enhanced information set. We present simple diff-in-diff results. We then experiment in turn with adding ability controls or adding differential trends and finally both. All results show clear and strong evidence that the reform improved cognitive ability by 7% to 15% of a standard deviation. This demonstrates that increasing compulsory schooling can improve cognitive outcomes even at this relatively advanced age. Moreover it indicates that those who would have otherwise opted out of school can benefit by being kept in school. Both these results are important because they may justify interventions beyond early childhood at least from a benefit perspective. Of course to complete the picture we need both an evaluation of the cost as well as a market return of this benefit. Nevertheless the point remains that cognitive skills can be improved even at mid adolescence with extra schooling.

In the 2^{nd}-5^{th} columns of the table we then break down this result by parental socioeconomic status and by high/low child ability. Columns 2 and 3 we split the sample by low and high father's education. Because of the relatively small sample size for higher educated fathers we cannot identify a difference in impacts for the two groups; indeed we cannot reject the hypothesis that the impacts are the same. We then take the low father's education group and we split it into those with below median ability at 12 and above median ability.[25] Here the point estimates are much larger for the lower ability group. The differences are significant at the 10% level.

The result is perhaps not surprising since most of the education effect for the low SES group comes from increased schooling of these lower ability students, who otherwise drop out, while the higher ability pupils dropped out much less. However it does make an important point that extra schooling is benefitting the lowest achievement group, a group that is often

[25] This is based on the distribution of a principal components analysis of all test scores at that age.

thought unreachable. While research has shown that interventions at early ages have higher returns for the most disadvantaged, the pattern reverses by the adolescent years when returns to human capital investment are lower for lower ability and disadvantaged children (see the review by Cunha et al, 2006, for a number of references)

Table 4: The effect of educational reform on cognitive skill accumulation; Survey and Army Draft data for ’48 and ’53 cohorts random sample

	(1)	(2)	(3)	(4)	(5)
Fathers' Education	*All*	*Low*	*High*	*Low*	*Low*
Ability	*All*	*All*	*All*	*Low*	*High*
	No ability controls, no trends, municipality of birth				
Reform	0.106**	0.086	0.223*	0.088	0.045
	(0.048)	(0.055)	(0.115)	(0.061)	(0.082)
	Ability controls, no trends, municipality of birth				
Reform	0.069**	0.091**	0.049	0.095*	0.029
	(0.033)	(0.035)	(0.081)	(0.051)	(0.068)
	No ability controls, trends, municipality of birth				
Reform	0.144***	0.117*	0.192	0.201***	0.002
	(0.054)	(0.065)	(0.148)	(0.065)	(0.104)
	Ability controls, trends, municipality of birth				
Reform	0.074**	0.115***	-0.022	0.164***	-0.020
	(0.038)	(0.044)	(0.078)	(0.060)	(0.088)

Notes: reform status is assigned based on the municipality of birth. All municipalities included except Stockholm. *** indicates significance at the 1% level; standard errors clustered on the municipality of birth level

The results vary somewhat depending on whether we include ability controls and/or municipality specific trends; however qualitatively they do not change and these alternative specifications do not affect whether the impact is significant or not. Moreover, the results are not significantly different from each other.

As shown in Athey and Imbens (2006), the key assumption underlying the difference-in-differences estimator is that in the absence of treatment, test scores are a monotonic transformation of an unobservable; the transformation may change over time, but the distribution of the unobservable, while different between treatment and comparison groups, must remain the same over time. Imposing a specific test score scale and the additive separability embodied in the linear DiD estimator may distort the results.

To examine the robustness of our linear DiD results we use an estimator proposed by Altonji and Blank (1999) and Athey and Imbens (2006). One practical complication is that we have many treatment and comparison groups and we need to control for parental background and for ability. To achieve this we use all possible pairs of treatment and control municipalities and we stratified by high ability and low ability as well as by father's education. We then took the weighted average of all the results, the precision of each of the estimates serving as weights. The standard errors were computed using the bootstrap. We also computed the linear DiD estimator in the same way for comparability.

Table 5: Comparing linear and non-parametric difference-in-differences estimates of the effect of schooling reform on cognitive skills. '48 and '53 Cohort Survey.

	Linear Difference in Differences	Nonparametric Difference in Differences
Reform	0.11	0.16
	(0.059)	(0.065)

Bootstrap standard errors clustered at the municipality level in parentheses

The results suggest that relaxing the functional form assumption leads to higher estimates than when using the linear procedure. While this result makes sense if the transformation from ability to scores is concave, the difference form the linear DiD estimate is not significant. Thus overall, our results are robust to relaxing the functional form assumption and if anything they seem to be underestimates of the impact of the reform on cognitive scores.

The point estimates for the impact of the reform are quite high: indeed if we take the group with low fathers' education and we suppose that the entire effect of the reform operates through schooling our estimates imply that an extra year of schooling at about 14 years of age causes an increase in cognitive scores in the range of 32%-40% of a standard deviation. First, it is important to stress that we do not believe we can apply IV in such a way because we believe that the reform may have affected achievement for this group in more ways than only through an increase in the quantity of schooling. In particular, the reform also changed schooling quality by imposing a national curriculum and by abolishing tracking, so that high and low ability children now coexisted in the same classrooms thus changing the peer group structure for all.

Beyond these qualifications, the magnitude of the point estimates is similar to what has been found in the US-based literature studying the effects of education on AFQT scores. Cascio

and Lewis (2005) find that an extra year of education improves minority students' cognitive scores by 33% of a standard deviation (SD). Neal and Johnson (1996) use US compulsory school age laws as in Angrist and Krueger (1991) to identify similar effects – gains of 0.25SD for women, and of 0.22SD for men; these gains are estimated for an older treated group, as the compulsory schooling laws bind around age 16 in the US. [26] Hansen, Heckman and Mullen (2004) based on NLSY data and using a factor model to control for the endogeneity of education find very similar gains. [27]

Our result bring to bear new evidence, with a causal interpretation that more and better schooling can have substantial impacts on cognitive outcomes and hence on adult standard of living and social mobility.

6.1.2 Non-cognitive skills

The role of non-cognitive skills is an important and separate dimension of human capital has been emphasized by Heckman and co-authors in a number of papers.[28] Indeed analysis of the Perry-preschool experimental results, for example, seems to suggest that that particular intervention had no long run effect on IQ but did affect social skills; this in turn is the mediating factor to which the subsequent relative labor market success of the treatment group was attributed to. Table 6 shows the results for our sample. The polynomial in the first stage residual is included to control for the missing data in the 1948 cohort as explained before. The residual is highly significant as we expect. We only present the results from the most general specification including differential trends and controls for ability at 12.

The results are interesting and show a different pattern to those on cognition: the overall effect is an improvement of 17% of a standard deviation attributable to the reform. The surprising effect is that the impact is larger for those from a higher SES. Comparing columns 2 and 5 the difference is significant at 5% with a t-statistic of 2.2. Breaking down the effect for the low SES pupils by prior ability we obtain no significant differences although the point estimate is

[26] See also Whinship and Korenman, 1997 for a detailed review of the existing literature to date

[27] An early study of this issue was carried out by Harnqvist (1968a, 1968b) who uses data from the 1948 Swedish cohort. He found gains in the range of 44% of a SD those who completed only the general comprehensive school and 54% SD for men who completed the theoretical comprehensive school as a terminal degree. The estimates are even larger than ours but he does not control for the endogeneity of education.

[28] Cunha et al, 2006; Heckman, Stixrud and Urzua, 2006; Cunha, Heckman and Schennach, 2010; Carneiro, Heckman and Masterov, 2005

larger for the lower ability children. The result is a bit surprising because schooling itself did not increase for those from a higher SES background, from which we need to conclude that the other aspects of the reform, such as abolishing tracking and mixing children from various backgrounds in the classroom, also had important effects on outcomes at least in this dimension.

Table 6: The effect of education reform on non-cognitive skills accumulation; '48 and '53 random sample included

	2SLS results non-cognitive skills army				
	(1)	(2)	(3)	(4)	(5)
Father's education (SES)	*All*	*Low*	*Low*	*Low*	*High*
Ability	*All*	*All*	*Low*	*High*	*All*
Reform	0.171**	0.065	0.089	0.193	0.530***
	(0.077)	(0.076)	(0.127)	(0.123)	(0.198)
First stage residual	0.272***	0.335***	0.298***	0.418***	0.044
	(0.041)	(0.047)	(0.074)	(0.081)	(0.162)
First stage residual ^2	-0.053	0.110*	0.138	0.016	-0.258**
	(0.033)	(0.059)	(0.086)	(0.116)	(0.119)
First stage residual^3	0.037	-0.034	-0.018	-0.041	0.254*
	(0.034)	(0.044)	(0.060)	(0.080)	(0.153)
First stage residual ^4	0.007	-0.067***	-0.076**	-0.034	0.047*
	(0.005)	(0.021)	(0.037)	(0.053)	(0.026)
Linear trends	Yes	Yes	Yes	Yes	Yes
N	5,937	4,916	2,721	2,195	1,021

Notes: Reform status assigned on the municipality of birth level; Cluster-robust standard errors in parentheses; standard errors clustered at the municipality of birth level; *** indicates significance at the 1% level; all regressions include municipality of birth and cohort dummies and 6th grade cognitive ability controls; all municipalities of birth included except Stockholm

7.2.1 Health

The next dimension of human capital we look at is longer term-health outcomes. We will report on three types measures of health: mortality and mortality by cause; hospitalizations; sick leave pay. While mortality by the age of 60 is a relatively clean indicator of important health problems, hospitalizations confound the impact of illness with the action of attending hospitals as a health investment. Finally sick leave pay will confound health outcomes with behaviors relating to the incentives induced by the system (see Johansson and Palme, 1995 and 2005). Nevertheless these incentives are common across treatment and comparison groups; the main concern there is whether the increase in wage due to the education reform induces a reduction in sick leave pay: this will still be a causal effect of the reform but could

possibly be attributed to other factors than improvements in health. Despite the potential issues with hospitalizations and sick leave pay we include them because they may pick up smaller changes in health that have no substantial impact on mortality but that have the potential of affecting productivity and wellbeing more generally.

Mortality

The results on mortality are reported in Tables 7A and 7B. Table 7A shows the results on aggregate mortality from the 1948/53 random sample cohort data as well as results from the entire administrative data. We compare results estimated in the same way, i.e. including differential municipality trends, but with no ability controls as these are not available in the population-level administrative data. The reported coefficients represent the proportional effect on the exit (mortality) hazard implied by the reform. A coefficient of 1 implies no change in mortality hazard, while a coefficient of less than one implies a decrease. The reform effect for the high SES background groups cannot be estimated because there are too few deaths in this group in the '48-'53 cohort random sample. All regressions include municipality and cohort dummies as well as municipality specific linear trends.

From these results there is no indication that the school reform reduced mortality. In the much larger administrative data the overall effect is almost exactly zero. In the smaller cohort data set the point estimates point to a substantial reduction in the mortality hazard of about 50% but the effect is far from being significant.[29]

The only indication that there may be an improvement in mortality for some disadvantaged groups is a significant mortality decline (albeit imprecisely estimated) for low ability individuals from the 48/53 cohort studies. This is consistent with the educational attainment results - the decline in mortality is strongest in the population group for which the reform increased education most. The average mortality rate among men with low cognitive skills in 6th grade is 5.5 percent. The point estimate reported in Table 7A implies a decline of mortality of 3.5 percentage points among those who attended the reformed school system, or 64 percent lower mortality than the base mortality of 5.5% in the group. On average, a man with low initial cognitive skills could expect to live for 227 months in 20 years. Such a man who went to reform school will live for 235.2 months, a non-trivial increase in longevity of over 8 months in twenty years.

[29] Such a large point estimate is not out of order because the overall mortality hazard is quite low for most people who are quite young.

Table 7A. The effects of education reform on total mortality by 2005. Cox proportional hazard regressions

	(1)	(2)	(3)	(4)	(5)
SES	*All*	*Low*	*All*	*Low*	*Low*
Ability	*All*	*All*	*Low*	*Low*	*High*
Sample	'48-'53 cohorts, linear trends, no ability controls, municipality of birth FEs				
	0.466	0.541	0.360**	0.421*	0.609
	(0.183 - 1.185)	(0.216 - 1.351)	(0.134 - 0.968)	(0.155 - 1.139)	(0.093 - 3.997)
Sample	Population born1946-1957, linear trends, no ability controls, municipality of birth FEs				
	0.994	0.994	-	-	-
	(0.950 - 1.039)	(0.926 - 1.067)			
N	676,267	304,932			

Notes: * significant at 10%; ** significant at 5%; cluster-robust confidence intervals in parentheses; standard errors clustered on the municipality of birth level; all municipalities of birth included, except Stockholm

We next investigate the effect of the reform on mortality at different stages in life. Table 7B reports the estimates on mortality from any cause for age groups 40-49 and 50-60, respectively, as well as for the entire population. The data also allow us to divide the sample on the basis of father's education. The results show a marginally significant decrease in mortality as a result of the reform for males in the 40-50 age group. However there is no significant effect on mortality in the older age group, where the point estimates imply a slight and insignificant increase. The decline in mortality is fully attributable to men from a lower SES background. Thus educational reform seems to reduce mortality at a younger age for individuals from a poorer background. This translates only to a very small effect on life expectancy, which is not surprising given mortality for that age group is very low. The crude mortality rate in the male population from low SES backgrounds aged 40-50 is 2.22 percent. The reform reduces this by 9 percentage points to 2.02 percent. The average low SES background male at age 40 could expect to live for 117.3 months in the next 10 years. The reform increased that to 117.6 months, an increase in life expectancy of around 10 days over the next 10 years.

Table 7B. The effects of education reform on mortality in different age groups. Cox proportional hazard regressions; population born 1946-1957

Age	*40-50 age group*	*50-60 age group*	*aggregate*
Father's education		*All SES Groups*	
Reform	0.943+	1.076	0.994
	(0.881 - 1.009)	(0.970 - 1.193)	(0.950 - 1.039)
N	670,372	501,338	676,267
Deaths	16,351	12,231	35,142
		Low Father's Education	
Reform	0.911+	1.122	0.994
	(0.823 - 1.009)	(0.964 - 1.307)	(0.926 - 1.067)
N	302,942	220,119	304,932
Deaths	6,702	4,867	13,701
		High Father's Education	
Reform	1.024	0.992	0.992
	(0.880 - 1.192)	(0.805 - 1.222)	(0.900 - 1.093)
Deaths	3,416	2,303	*6,946*
N	163022	111291	164058

Notes: * significant at 10%; ** significant at 5%; cluster-robust confidence intervals in parentheses; standard errors are clustered on the municipality of birth level; all municipalities of birth are included except Stockholm; linear trends by year of reform implementation included in all estimations. The samples of low and high SES background men do not add up to the aggregate sample size because of missing information on father's education in the registry data

Mortality can be affected by behavior; for example smoking can cause lung cancer and excess drinking can cause liver disease. While education may induce healthier behaviors as the life-time returns increase, they may also lead to increases in consumption of unhealthy goods; on the other hand other health outcomes may not be directly affected by such consumption patterns but could be sensitive to health investments such as exercise and obtaining treatment. Improved education may act in opposite directions in each case.

Table 8: The effect of education reform on total mortality by cause of death: Cancer, Circulatory, Treatable, and Preventable causes of death. Cox proportional hazard regressions. Population of men born 1946-1957

	(1) *Circulatory*	*(2)* *Cancer*	*(3)* *Preventable*	*(4)* *Treatable*
Father's education		*All SES Groups*		
Reform	0.967	1.010	1.143*	1.074
	(0.875 - 1.068)	(0.906 - 1.125)	(0.980 - 1.334)	(0.935 - 1.235)
Deaths	7,107	6,475	8,557	3,278
N	676267	676267	676267	676267
		Low Father's Education		
Reform	0.929	1.044	1.034	1.037
	(0.797 - 1.083)	(0.884 - 1.233)	(0.791 - 1.351)	(0.827 - 1.299)
Deaths	2,841	2,655	3,149	1,310
N	304932	304932	304932	304932

		High Father's Education		
Reform	1.041	0.946	1.063	1.186
	(0.819 - 1.322)	(0.740 - 1.208)	(0.730 - 1.549)	(0.814 - 1.729)
Deaths	1,265	1,458	1,538	661
N	164058	164058	164058	164058

Notes: * is significant at the 10% level; Cluster-robust confidence intervals in parentheses, clustered at the municipality of birth level; all municipalities included except Stockholm; all estimations stratified at the municipality of birth level; linear trends by year of reform implementation included in all estimations.
Mortality by cause categories are mutually exclusive, meaning that ICD codes of death are assigned to only one mortality cause. Individual mortality from different causes is defined as follows. An individual who died from any kind of cancer except lung cancer (which is a preventable cause of death) is assigned 1 in the cancer death category. Individuals who did not die from cancer, even if they died from another cause or from lung cancer (preventable) is assigned a 0 in the cancer death category. Appendix table A2 lists individual ICD codes of death that are assigned to each mortality category.

Thus, in Table 8 we present a competing risk analysis based on different causes of death. We look separately at "Treatable" and "Preventable" causes of death since these relate directly to health behaviors. We also look explicitly at circulatory diseases and cancer because in the descriptive analysis we found that mortality from circulatory diseases had a stronger education gradient than mortality due to cancer. Again, we present results separately for the low and high SES groups.

While the point estimates suggest a decline in deaths due to circulatory diseases the effects are not significant, while being very precisely estimated. There is however some evidence (significant at the 10%) that mortality from preventable diseases increased slightly, possibly reflecting income effects on the consumption of alcohol and cigarettes. Recall that preventable causes of death include only pulmonary cancer (related to smoking) and liver cirrhosis (related to excessive alcohol consumption).

Hospitalization

We now turn to hospitalizations, which may pick up subtler changes in health that do not necessarily reduce mortality but may improve productivity and well-being. Of course hospitalizations may increase with education as access to and willingness to use health-care may increase. This may counteract, or maybe exceed, the health effect of the education reform. However, the results on the relation between observed education and hospitalization, reported in Section 5, suggest a significant inverse relationship for all hospitalizations and for most of the diagnoses.

Table 9 shows linear probability model estimates for the effect of the reform on the probability of being hospitalized for any cause between 1987 and 2006 along with estimates

restricted to the same causes as considered for the causes of death in Section 7.1. The results are also shown separately for groups with high and low educated fathers.

Although the estimates are obtained with quite high rate of precision, we are not able to reject a zero effect for any of the sub-groups or diagnoses under study. For overall hospitalization we can reject effects larger than 0.6 percent in any direction and for circulatory diseases the corresponding number is 0.5 percent. The background to these small effects could either be that the effects on the underlying health are very small or that the effects are counteracted by increased healthcare consumption given health status. Note however, that this hypothesized behavioral response to increased health-care consumption that could be counteracting a possible decrease in hospitalizations due to improved health would not be related to costs, since care is free in Sweden.

Taken together the results imply either zero or very small effects of the education reform on health. For the set of outcomes where we do find health effects, these point to improvements for individuals from lower socio-economic background, which is consistent with the nature and impact of the reform, and for a decline in mortality at younger ages (40-50). However, even there the effects are either very small or only significant at the 10% level.

Table 9: The effect of the reform on hospitalizations for preventable and treatable causes. Linear probability regressions. Population of all men born 1946-1957

Ever hospitalized	*(1)*	*(2)*	*(3)*	*(4)*	*(5)*
1986-2005	All causes	Circulatory	Cancer	Preventable	Treatable
Father's education			*All SES backgrounds*		
Reform	-0.002	0.0015	-0.00035	0.00029	-0.00227
	(-.006 -.002)	(-0.002, 0.005)	(-0.002, .001)	(-0.0004, 0.001)	(-0.006, 0.002)
Hospitalizations	392,187	74,739	19,344	3,719	115,146
N	676,267	676,267	676,267	676,267	676,267
Hospitalization rate	1	0.191	0.049	0.009	0.294
			Ever hospitalized 1986-2005		
			Low Father's Education		
Reform	-0.003	0.00103	0.00052	0.00039	-0.00414
	(-0.01, -.004)	(-0.003, 0.006)	(-0.002, 0.003)	(-0.00, 0.001)	(-0.01, 0.001)
Hospitalizations	173,962	33,338	8,340	1,559	52,574
N	304,932	304,932	304,932	304,932	304,932
Hospitalization rate	1	0.192	0.048	0.009	0.302
			Ever hospitalized 1986-2005		
			High Father's Education		
Reform	0.003	0.00541*	0.00079	0.00008	-0.00014
	(-.006 -.011)	(-0.001, 0.012)	(-0.003, .004)	(-0.001, 0.002)	(-0.008, 0.008)
Hospitalizations	96,413	15,282	4,443	681	25,426
N	164,058	164,058	164,058	164,058	164,058

Hospitalization rate	1	0.159	0.046	0.007	0.264

Notes: Cluster-robust standard errors in parentheses, clustered at the municipality of birth level; * is significant at the 10% level; all estimations include municipality of birth fixed effects; all municipalities of birth except Stockholm are included; linear trends by year of reform implementation included in all estimations. The number of observations for low and high SES background do not add up to the total number of observations because of missing data on father's education.
Hospitalization by cause categories are mutually exclusive, meaning that primary cause ICD codes are assigned to only one group. Individuals are counted only once and categorized into different hospitalization categories based on the first hospitalization cause. Hospitalization from different causes is defined as follows. An individual who was hospitalized with any kind of cancer except lung cancer (which is a preventable cause) is assigned 1 in the cancer hospitalization category. Individuals who were hospitalized with lung cancer are assigned a 0 in the cancer hospitalization category and a 1 in the preventable cause category. Appendix table A2 lists individual ICD codes that are assigned to each hospitalization category.

7.3.2. Sick pay

In Table 10 we present results from linear probability regressions using the full panel of individual level data across years (1991-2002 and 2004-2006) for which we have information on sick leave payments. Although previous research (see Johansson and Palme, 1995 and 2005) has shown that economic incentives affect the propensity to claim sick-pay insurance benefits, and the reform indeed increased labor earnings, the estimated elasticities from this research suggest that the effect is negligible in this context. In addition, the data we use on sick-pay insurance benefits stems from spells longer than seven days where a certificate from a physician is needed for the right to receive benefits. This limits the influence of other factors than health for this outcome measure.

The results in Table 10 show that men who went to reform schools have a 0.4 percent lower probability of taking sick leave in any year, which is a decrease of 1.4 percent estimated at the mean. This estimated impact for men coming from low SES backgrounds is 1.7 percent evaluated at the mean. The effect of education reform on the sick-leave taking of high SES men is quite precisely estimated to be zero.

Table 10: The probability of taking paid sick leave in the year; linear probability models, individual panel data comprise all years for which data on sick pay are available. Population of all men born 1946-1957

	(1)	(2)	(3)
		Parental education background	
Prob(took sick pay)	*All*	*Low Father's Education*	*High Father's Education*
Reform	-0.004***	-0.005***	.000337
	(0.001)	(0.002)	(.0020651)
Mean dep var	0.275	0.306	0.243
Observations	12,645,792	5,608,134	3,002,220
R-squared	0.022	0.003	0.003

Note: * significant at 10%; ** significant at 5%; *** significant at 1%; cluster-robust standard errors in parentheses, clustered at the municipality of birth level; all birth municipalities included except Stockholm; all estimations include municipality of birth fixed effects; linear trends by year of reform implementation included in all estimations

The impact is small, and particularly so if we note that the overall claim rate is actually quite high. The fact we only obtain an effect for the lower SES group is also consistent with the rest of the results and the way the reform impacted; the fact hospitalizations did not decline implies that this small improvement either does not really reflect health, but changed incentives to claim due to higher opportunity cost of time, or that the underlying health improvements are small. However we interpret it though, this is a further indication that the reform improved outcomes in a number of dimensions.

7. Conclusions

A large literature has documented the positive causal effects of compulsory schooling reforms on later-life labor market outcomes. The effect of extra education on health reported in the previous literature less unambiguous. For example, Lleras-Muney (2005; also corrigendum on the author's website) finds a strong negative effect of education on the10-year mortality among the affected individuals in the US, while Royer and Clark (2012) find no significant mortality effects in a cohort study from the UK. One possible explanation for these diverging findings is that Lleras-Muney's instrumental variables methodology identifies the local average treatment effect on those children who directly benefited from the reform, while Royer and Clark identify an average treatment effect across entire cohorts. Cohort-level analysis could potentially miss significant heterogeneities in effects across children from different SES groups.

This study uses a comprehensive schooling reform from Sweden together with a number of registry and survey-based data to study the effect of compulsory schooling reform on human capital accumulation in children coming from different SES groups. We first identify the reform's effects on cognitive and non-cognitive skills and find that extra education at a relatively late stage of childhood can significantly improve cognitive and social skills. We then show that the low SES background boys, who benefitted most in terms of skills, also incurred small gains in intermediate health and life expectancy.

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APPENDIX TABLES

[FOR ONLINE PUBLICATION ONLY]

Table A1: Number of individuals in a cohort by reform status of the municipality of birth

	No reform		Reform	
	Alive in 1985	Died by 2005	Alive in 1985	Died by 2005
1946	120,808	7,097	16,034	962
1947	113,341	6,114	20,999	1,124
1948	92,009	4,505	40,632	2,009
1949	79,238	3,479	48,316	2,182
1950	64,571	2,540	57,410	2,387
1951	58,112	2,217	58,490	2,249
1952	44,314	1,615	72,447	2,473
1953	34,340	1,088	82,395	2,647
1954	27,765	833	84,900	2,429
1955	4,433	106	109,936	2,969
1956	781	16	114,436	2,886
1957	61	3	113,691	2,545

Table A2: ICD 9 and ICD10 codes groups used to establish different causes of death and hospitalization

Cause	ICD9	ICD10
Treatable causes of death		
Tuberculosis	010-018, 137	A15-A19. B90
Malignant neoplasm of cervix uteri	180	C53
Chronic rheumatic heart disease	393-398	I05-I09
All respiratory diseases	460-519	J00-J99
Asthma	493	J45, J46
Appendicitis	540-543	K35-K38
Abdominal hernia	550-553	K40-K46
Hypertensive and cerebrovascular disease	401-405,430-438	I10-I15, I60-I69

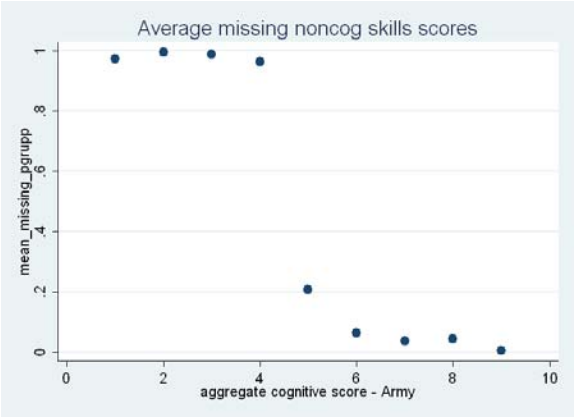
Chollelthiasis and cholecystitis	574, 575.0, 575.1	K80-K81
Cancer deaths		
Cancers, excl lung cancer	140-239	C00-C99; D00-D48
Circulatory deaths		
Diseases of the circulatory system, excl chronic rheumatic heart disease, hypertensive and cardiovascular disease	390-459	I00-I99
Preventable causes of death		
Lung cancer	162	C33-C34
Cirrhosis of liver	571.0-571.3, 571.5-571.6	K70, K74.3-K74.6

Table A3: Mortality by different cause as a fraction of total mortality by reform status of the municipality of birth

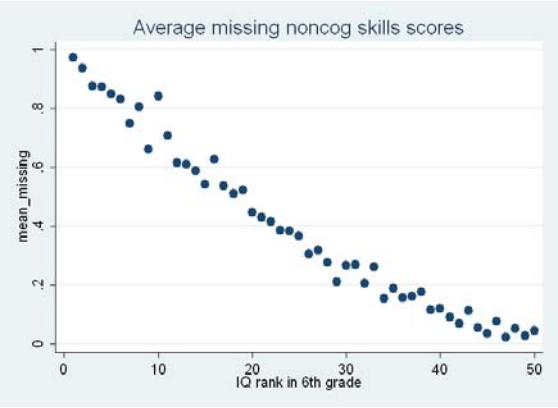
	Reform		No reform	
N dead by 2005 (total)	26,862	% total deaths	29,613	% total deaths
Preventable	1,962	7,304%	2,587	8,736%
Treatable	2,834	10,550%	3,336	11,265%
Circulatory (excluding hypertension; rheumatic heart disease)	3,921	14,597%	5,169	17,455%
Cancer (excluding lung cancer and cervical cancer)	6,964	25,925%	8,472	28,609%
Breast Cancer	1,418	5,279%	1,579	5,332%

Figure 1A: Probability of missing the Army non-cognitive score by cognitive score group and by IQ rank in 6th grade in the 1948 cohort

By cognitive group in the Army



By IQ rank in 6th grade



Description of the cognitive skills tests in 6th grade and in the Army draft

The description of the 6th grade cognitive skills tests is taken from Härnqvist (1968), who designed the 6th grade assessment and carried it. In the same publication he also describes the Army draft cognitive tests, and we reproduce his description below.

Test administered in 6th grade

The test was specifically designed for a study by researchers in the University of Goteborg headed by Kjiel Härnqvist. It consisted of three sub-tests, designed to capture the verbal, reasoning, and spatial components of intelligence. We describe the different components below, following Härnqvist (1968).

Opposites: To find the opposite of a given word among four choices. 40 items, 10 minutes

Number series: To complete a number series, of which six numbers are given, with two more numbers. 40 items, 18 minutes

Metal folding: To find the three0dimensional object among four choices that can be made from a flat piece of metal with bending lines marked on the drawing. 40 items, 15 minutes.

Answers were written directly on an exam booklet that also contained questions about school interest and attitudes. The test was administered in the classroom by the teachers according to instructions provided by the researchers.

Army draft test (also following Härnqvist, 1968)

Subtest A, Instructions: To follow verbal instruction to mark, cancel, or write things. For a few items simple information of a very general nature is needed. A few items imply changes of directions on dials or simple diagrams. The main character of the test, however, is decidedly verbal. 40 items, 12 minutes

Subtest B, Concepts: To select among five verbally expressed concepts the one that does not belong to the same class as the remainder. 40 items, 7 munites.

Subtest C, Form-board. To find among four choices the set of pieces that can be fitted together to form a given flat board, with lines between pieces marked on it. 25 items, 4 minutes

Subtest D, Mechanical comprehension. A variation of the Bennett test. 52 items, 15 minutes

All answers are marked on separate answer sheets.

Education and Human Capital Outcomes

The health results have been derived by Table A4 shows the hazard rates estimated from Cox proportional hazard regressions broken down into mortality by cause. Mortality causes are

grouped into five exclusive categories: cancer, circulatory, preventable, treatable, and others. Appendix table A2 lists the ICD9 diagnoses that enter in each group. In Appendix table A3 we show the distribution of deaths by difference cause in the treated and control populations.

Hazard rates that are lower than unity indicate a protective effect; those above unity imply an increase in the hazard associated with a unit change in the variable. The confidence interval is shown in brackets. An extra year of education is associated with a lower overall mortality risk up to age sixty by 10.5% among men. This is within the range implied by US estimates reported by Lleras-Muney and Cutler (2006), who find a 1.8 percentage point reduction associated with an extra year of education in the 5-year mortality rate among adults aged 25 and over.[30] These are large significant gains in life expectancy associated with more education.

Table A4: The relationship between years of education and mortality from different causes. Cox stratified proportional hazard models. 95 percent confidence intervals in parentheses.

Men	*(1)*	*(2)*	*(3)*	*(4)*	*(5)*	*(6)*
	Any cause	*Cancer*	*Circulatory*	*Preventable*[31]	*Treatable*	*Other*
Years of Schooling	0.894* (0.881 - 0.906)	0.939* (0.916 - 0.962)	0.884* (0.867 - 0.902)	0.813* (0.782 - 0.845)	0.899* (0.868 - 0.931)	0.885* (0.868 - 0.902)
Sample size	691,756	691,756	691,756	691,756	691,756	691,756

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

Interesting patterns emerge when we break down mortality by the main causes – cancers, circulatory disease, and potentially treatable and avoidable mortality. Consistent with the view that the risk of getting cancer diseases are less affected by lifestyles and health behaviors, increases in education are associated with the smallest gains in cancer-related mortality. It is important to stress here that cancer mortality depends both on the incidence and the treatment of cancer, thus even if we expect completely random incidence, disparities would emerge if more educated individuals receive better or more timely care.

[30] In a robustness check we defined the 5-year mortality rate by municipality and birth cohort as defined by Cutler and Lleras-Muney (2006) and estimated OLS coefficients of the effects of an extra year of education on the 5-year mortality rate controlling for municipality of birth and cohort fixed effects. We found estimates very similar to the ones reported for the US – around 4 percent decrease in the 5-year rate for men and 3.7 percent decrease for women.

[31] Includes lung cancers and liver cirrhosis only

The strongest gains due to education are in preventable mortality for men. This cause of death category groups lung cancer and liver cirrhosis, which are strongly associated with smoking and drinking, respectively. These types of mortality are most strongly affected by health behaviors and lifestyle, suggesting that health behaviors are a major channel through which education affects health.

In table A5 we show the relationship between education and the probability of being hospitalized. The outcome is a binary variable equal to one if the person was hospitalized overnight for any cause at any time during the period 1987-2005. We run linear probability models. Even if we were to take education as exogenous, the probability of hospitalization depends on two potentially conflicting channels. First, worsening underlying health would increase the incidence of hospitalizations. This implies that more educated individuals would be hospitalized less often. However, hospitalization is also a means of health investment. Higher educated individuals may have higher returns from such investments. This implies that more educated people would be more likely to be hospitalized for planned treatments. In addition, higher educated individuals may have lower discount rates and thus invest more anyway, further increasing hospitalizations. As it turns out (Table A5) hospitalizations are negatively associated to increases in education. An extra year of schooling decreases the probability of hospitalization by 1.4 percentage points among men. We emphasize that cancer hospitalizations do not appear to be significantly affected by education; however cancer mortality is lower among the better educated. This suggests that one of the channels through which education lowers mortality is by improving survival chances post-diagnosis.

Table A5: The effects of education on hospitalizations. OLS estimates controlling for municipality and birth cohort fixed effects. 95 percent confidence intervals in parentheses.

Men	*(1)*	*(2)*	*(3)*	*(4)*	*(5)*
	Any cause	*Cancer*	*Circulatory*	*Preventable*	*Treatable*
Years of schooling	-0.014*** (-0.015 - 0.013)	-0.00015* (-0.00032 - 0.00001)	-0.00501*** (-0.00539 - 0.00463)	-0.00031*** (-0.00037 - 0.00026)	-0.00610*** (-0.00656 - 0.00564)
Sample size	691756	691756	691756	691756	691756

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

Table A6 shows the estimated hazard ratios from a Cox proportional hazard model for retirement using years of schooling and dummy variables for cohort of birth as independent variables. The results reveal a very strong association between education and the timing of

retirement: every year of additional education is associated with an about 9 percent decrease in the probability of exiting from the labor force for both males and females. As a sensitivity analysis, we have repeated these estimates using different thresholds for labor earnings and the results are robust to these checks.

Table A6: Hazard ratios from Cox proportional hazard models of the association between years of schooling and retirement. 95 percent confidence intervals in parentheses.

Variable	Males
Years of schooling	0.9103*
	(0.8983 - 0.9224)
Sample size	755,634

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