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Compulsory Schooling Reforms in Sweden

UNIVERSITÄT DUISBURG ESSEN

#733

Imprint

Ruhr Economic Papers

Published by

RWI – Leibniz-Institut für Wirtschaftsforschung
Hohenzollernstr. 1-3, 45128 Essen, Germany

Ruhr-Universität Bochum (RUB), Department of Economics
Universitätsstr. 150, 44801 Bochum, Germany

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Editors

Editorial Office

Ruhr Economic Papers #733

Responsible Editor: Volker Clausen

ISSN 1864-4872 (online) – ISBN 978-3-86788-853-0

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Bibliografische Informationen der Deutschen Nationalbibliothek

The Deutsche Nationalbibliothek lists this publication in the Deutsche Nationalbibliografie; detailed bibliographic data are available on the Internet at http://dnb.dnb.de

RWI is funded by the Federal Government and the federal state of North Rhine-Westphalia.

http://dx.doi.org/10.4419/86788853
ISSN 1864-4872 (online)
ISBN 978-3-86788-853-0

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Abstract

We evaluate the impact on earnings, pensions, and further labor market outcomes of two parallel educational reforms increasing instructional time in Swedish primary school. The reforms extended the annual term length and compulsory schooling by comparable amounts. We find striking differences in the effects of the two reforms: at 5%, the returns to the term length extension were at least half as high as OLS returns to education and benefited broad ranges of the population. The compulsory schooling extension had small (2%) albeit significant effects, which were possibly driven by an increase in post-compulsory schooling. Both reforms led to increased sorting into occupations with heavy reliance on basic skills.

JEL Classification: J24, J31, I28

Keywords: Educational reforms; compulsory schooling; term length; returns to education

December 2017

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1 Introduction

The role of time as an educational input is getting increasing attention in the policy debate and policy makers across the globe are considering giving schoolchildren more classroom time. During his time in office, US president Obama called for a longer time spent in school, and in recent years several states have increased the number of days of teaching to a minimum of 190 days (Times, 2012). Across Europe the number of school days in primary education varied between 162 and 200 days in 2016/2017, and many education reformers agree that more instructional time is a key step to improve educational outcomes (EACEA, 2017).

Despite this ongoing discussion, the evidence on the effects of term length on later-life outcomes is extremely limited. Pischke (2007) analyzes the effects of short school years in Germany in 1966 and 1967. While the reduction in instructional time by 26 weeks over a time period of two years increased grade repetition and decreased higher secondary schooling attendance, the short years did not significantly impact the affected cohorts' income or employment. A recent study using survey data from Indonesia (Parinduri, 2014) comes to similar conclusions. Both these studies have in common that the considered extensions were not planned as such; the term length change was the accidental consequence of another policy change. Our paper aims to contribute to this literature by evaluating the long-run effects of a structural policy on term length extension affecting the entire population of Sweden. Our analysis is based on a clear quasi-experimental research design with a well-defined control group.[1]

The limited number of studies on the long-term effects of term extensions is in sharp contrast to the large literature exploiting changes in compulsory schooling laws, regulating the minimum years of schooling that an individual has to acquire, to estimate the causal effect of human capital. Early studies evaluating this alternative policy measure, like Oreopoulos (2006) or Harmon and Walker (1995) following Angrist and Krueger (1991), generally reported large *causal* effects of schooling on earnings based on instrumental variable estimates.[2] However, more recent studies suggest smaller and

[1]In the economics literature term length has sometimes been considered a proxy variable for school quality (Betts, 1995; Card and Krueger, 1992).This is a valid interpretation in some contexts, but *ceteris paribus* changes in term length are de facto primarily changes in instructional time.

[2]Belzil, Hansen and Liu (2017) argue that IV estimates on compulsory schooling reforms are quite uninformative of the average effects of education because of overestimation. Average effects of compliers tend to be significantly lower than IV estimates.

often negligible impacts of additional schooling on economic outcomes or health.[3] In particular, recent studies on European countries suggest that changes in the number of compulsory years of schooling have rather small effects on pecuniary returns (Pischke and Von Wachter, 2008; Devereux and Hart, 2010; Chib and Jacobi, 2015). Also the large causal estimates for education and labor market returns in the US have recently been challenged as potentially spurious and possibly driven by differences in school quality between states (Stephens and Yang, 2014).

At a first glance the recent findings of limited effects of compulsory schooling extensions seem to suggest that expanding compulsory instruction by term extensions would be a quite inefficient means to improve human capital and long-term labor market outcomes. Still there are important conceptual differences between term extensions and extensions affecting the minimum years of schooling – differences arising from the *timing* of the interventions. First, a term length extension will affect children at younger ages compared to a compulsory schooling extension. This may be central as a large body of research suggests that investments in cognitive and non-cognitive skills are more effective when done earlier in life (Tominey, 2010; Cunha and Heckman, 2007), and to a certain extent supported by the findings in the literature analyzing the short-run effects of term length extensions.[4] Studies examining term extension and student performance generally note positive effects on test scores, although there seems to be important heterogeneity with regard to effect sizes and regarding the students and subjects affected (Lavy, 2015; Sims, 2008; Agüero and Beleche, 2013; Huebener, Kuger and Marcus, 2016; Bellei, 2009; Fitzpatrick, Grissmer and Hastedt, 2011).[5] In fact, one potential explanation of why compulsory schooling extensions are ineffective in improving labor market outcomes is that basic skills may be fixed already when an

[3]Recent contributions on the effects of education on health include Gathmann, Jürges and Reinhold (2015); Brunello, Fabbri and Fort (2013); Clark and Royer (2013); Meghir, Palme and Simeonova (2012); Lager and Torssander (2012); Mazumder (2012). Other outcomes considered in the literature are cognitive abilities (Schneeweis, Skirbekk and Winter-Ebmer, 2014; Crespo, López-Noval and Mira, 2014), fertility (Cygan-Rehm and Maeder, 2013; Fort, Schneeweis and Winter-Ebmer, 2016), intergenerational transmission (Piopiunik, 2014; Lundborg, Nilsson and Rooth, 2014; Chevalier et al., 2013; Güneş, 2015), and crime (Hjalmarsson, Holmlund and Lindquist, 2014).

[4]Several studies analyze the effect of instruction time on student performance using other identification strategies, cf. Leuven et al. (2010); Lavy (2015); Battistin and Meroni (2013); Cortes, Goodman and Nomi (2015).

[5]Some recent studies suggest that the effects of an extension may depend on the school system and that instructional time is complementary to other inputs (Rivkin and Schiman, 2015; Cattaneo, Oggenfuss and Wolter, 2016)

extension is introduced.[6] A second important difference between term extensions and compulsory schooling extensions is that the former do not affect school leaving age. In contrast, compulsory schooling extensions often affect both the amount of schooling and potential labor market experience. Finally, term extensions and compulsory schooling extensions may have different effects on the opportunity costs of secondary education: in a system with tracking, extensions applying to the lowest track will have implications also for the decision of which track to take. For these reasons, it seems highly desirable to improve our knowledge about the long-term effects of term extensions and to understand how these effects compare with those induced by alternative policy instruments.

This paper examines two policies increasing compulsory instructional time of the Swedish primary school (*Folkskola*) in the 1930s and 1940s. Implemented in the first half of the twentieth century when a large majority of all Swedes only completed primary education, the reforms raised human capital on a large scale. Following national parliamentary decisions in 1936 and 1937, respectively, more than 2,500 Swedish school districts were obliged to extend the annual term length in *Folkskola* from 34.5/36.5 to 39 weeks, and to increase the mandatory amount of schooling from 6 to 7 years within 12 years. School districts could choose the timing of the implementation independently within the given time window, generating large variation in educational attainment between cohorts and small local school districts. While the term length extension corresponded to an instructional time increase of 15-31 weeks distributed over the complete course of primary school, the compulsory schooling reform added one additional year (corresponding to 34.5-39 weeks) at the *end* of primary school. The two educational interventions consequently increased overall instructional time

[6] Most studies analyzing compulsory schooling reforms evaluate the effect of education based on changes taking place in adolescence. For example the UK reform in Devereux and Hart (2010) increased school leaving age from 14 to 15 years, Pischke and Von Wachter (2008) evaluate a secondary schooling extension, and the reform in France studied by Grenet (2013) extends school leaving age to 16 years. In contrast recent findings on other human capital investments suggest that the rates of returns are at highest earlier in life, not the least for disadvantaged populations (c.f. Chetty et al. (2011) and Heckman and Masterov (2007)). This naturally raises the question whether increases in instructional time in early years where students acquire most fundamental skills have effects on later life outcomes. The results and conclusion in Gathmann, Jürges and Reinhold (2015), who use aggregate data from 18 European compulsory school reforms implemented during the twentieth century to examine the dispersion in mortality gain across time and contexts, can also be seen in this light. The authors conclude that early twenty century reforms are more effective in improving health than later reforms. This could be an indication on that it is the impact of schooling in early years that matters as the early twenty century reforms considered affected younger children than later reforms.

by a comparable magnitude, but at different margins. Notably the term length extension constitutes a rare quasi-experiment for instructional time changes within a school year.

This is one of the first papers that examine the long-term labor market effects of a term length extension, and we can exploit appealing quasi-experimental variation as the reforms were executed over several years in a large number of small school districts. A major contribution is that we can make a direct comparison between the term extension and a compulsory schooling reform. The parallel implementation over several years across school districts allows for an estimate of the impact of the reforms on individuals from the same cohorts who were exposed to either the new or the old school regimes. Both extensions captured students at a relatively young age where basic skills are more likely to still be malleable. The term length extension was introduced already in first grade at age seven for some individuals, while the compulsory schooling extension affected students at age 13.

Another contribution is that we can follow our population (born 1930–1940) over an exceptionally long time period – ranging from birth until age 73. The long time frame enables us to distinguish early-career effects from lifetime earnings, which is desirable given that education may affect the steepness of the age-earnings profile (Bhuller, Mogstad and Salvanes, 2014). We thus add to the small literature on the returns to education in the very long term (Van Kippersluis, ODonnell and Van Doorslaer, 2011; Schneeweis, Skirbekk and Winter-Ebmer, 2014; Brunello, Weber and Weiss, 2016; Crespo, López-Noval and Mira, 2014) using a large sample of high-quality administrative register data.

A final contribution of the paper is the detailed reform data with variation across time and local geographical units which allow us to rule out several confounding factors. Measures used in the term extension literature have so far been relatively blunt and without sharp differences in birth years. The extensions we consider are particularly useful since they left all other components of the school system, including secondary and higher education, unaffected. The interpretation of compulsory school reforms as a pure increase of the amount of schooling is challenging whenever evidence is based on reform packages where changes in the instruction time only is one of many components. For example the Scandinavian comprehensive school reforms in the 1950s and 1960s combined extensions of compulsory schooling with the abolishment of tracking and changes in curriculum and teaching practices (cf. Meghir and Palme, 2005; Lundborg, Nilsson and Rooth, 2014; Pekkarinen, Uusitalo and Kerr, 2009; Black, Devereux and Salvanes, 2005), and many historical reforms generated educational degree effects in addition to the prolongation of schooling (Kırdar, Dayıoğlu and Koc, 2015; Grenet, 2013). By comparing similar reforms in the UK and France, Grenet (2013) concludes that sheepskin effects could explain diverging patterns in different European countries. These caveats do not apply in our case since the two extensions considered left all other components of the school system, including secondary and higher education, unaffected. Keeping the overall school system constant, the interventions thus allow us to isolate effects from extensions in instructional time from other school inputs such as teacher wages or class size. We consequently argue that the two reforms were *pure* schooling reforms in the sense that they only aimed at raising the *amount* of education, allowing a straightforward result interpretation.[7]

Using purposively collected school district reform data, excerpted from exam catalogs in historical archives, and high-quality register data, we show that the term extension was very effective in increasing labor market earnings: at around 9 per cent for females and 2.5 per cent for males when scaled to a school year, the effect for females is comparable to conventional OLS-estimated returns to schooling. The effects are persistent over the life cycle, with results suggesting significant improvements in pension earnings for females. Looking into effect heterogeneity at different parts of the earnings distribution and for individuals from different socioeconomic backgrounds, the results suggest that the positive effects of the term extension benefited broad parts of the labor force: we do not find evidence supporting an SES gradient in treatment effects.

Compared to the term extension the compulsory school extension seems to have been a relatively ineffective policy measure. In line with recent findings on returns to education in Europe and the US, at 2% the earning effects of the compulsory schooling extension are relatively modest. If at all, the compulsory schooling extension seems to have benefited mainly individuals from low-SES background.

[7]Still, the reforms may have had an impact on the situation of affected families, to the extent that children were working. Even though labor laws prohibited work during compulsory schooling ages, informal employment may still have been an important source of family income. We provide an analysis of effect heterogeneity by parental SES adressing this concern.

Using administrative data we examine a number of potential mechanisms mediating the term extension effect on earnings. Treated individuals are generally more likely to work in relatively well-paid occupations in administration and sales – and less likely to work in agriculture. In particular, we see an inflow of individuals into white collar occupations heavily reliant on basic reading, writing and math skills, such as book keepers and secretaries. This is consistent with the term extension having improved students' basic skills, in turn affecting occupational choice. We also note a reduction in the probability of dropping out of secondary school, and an increase in the probability of having acquired only primary education suggesting improved sorting into secondary schooling enrollment. We can furthermore rule out that the earning effects from the term extension would be biased by selective mortality or marriage market effects. In addition, the absence of a clear SES gradient in the effect seems to suggest that there are no relevant effects operating via a direct impact on family income while the affected children were still going to school.

In conclusion, our study provides additional empirical support for an important tool in the toolbox of school reformers throughout the World. Just like the recent studies suggesting class size (Fredriksson, Öckert and Oosterbeek, 2012; Dustmann, Rajah and Soest, 2003; Chetty et al., 2011), school starting age (Fredriksson and Öckert, 2014; Black, Devereux and Salvanes, 2011), extra days of schooling instruction (Carlsson et al., 2015; Crawford, Dearden and Greaves, 2014) or more generally additional school resources (Jackson, Johnson and Persico, 2016; Hyman, 2017) may be levers to consider to improve student achievement and adult outcomes, term extensions seem to represent quite an effective instrument to improve human capital. While the long-term effects of the compulsory schooling extension are disappointing, the term extension lead to persistent improvements in labor market outcomes and seems to have benefited the majority of children. By investing in children from the very first year in school the term extension clearly seems to have fulfilled the intended aims of the reform.

The next section gives the institutional background of the Swedish educational system and the reforms. Section 3 describes the data and sample selection, while Section 4 presents the empirical strategy. Section 5 shows the empirical results for labor market outcomes and analyzes potential mediators. Section 6 presents robustness checks and Section 7 concludes.

2 Background

2.1 The School System and the Reforms

Since 1897 children in Sweden start school in the year they turn seven. The school year begins in the fall and ends in the spring. In the 1930s and 1940s children entered a primary school called *Folkskola*.[8] Primary education was free of charge and attendance was compulsory until a student had completed the highest grade of *Folkskola* offered in the school district where he/she was registered as a resident.[9] The mandatory amount of schooling before the compulsory schooling reform in 1936 was 6 years, with some exceptions in the larger cities and in Scania, the southernmost county of the country.

Similar to many other countries in the beginning of the twentieth century, the Swedish school system consisted of different tracks. In the first years of primary school students were kept together. After 4 or 6 years, students could switch to an academic educational track. The lower secondary school (*Realskola*) generally required entrance exams, suggesting that students remaining in primary school were less able. Other factors, such as the availability of a secondary school nearby and costs related to schooling, constituted additional barriers to higher education.

After *Realskola*, students could continue to higher secondary education (*Gymnasium*) and university.[10] Figure 1 gives a stylized presentation of the various school types following basic primary education.

[8] Private schools never played a substantial role in Sweden. A more detailed description of the primary school system is given in Appendix C and in Fischer et al. (2016)

[9] The parents of a child were responsible for the fulfilment of the compulsory school attendance. Parents had to report that they had a child in school starting age to the school district board. According to §51 of the "Royal Decree of the Folkskola" of 1930 parents that did not send their children to school could get penalty payments and in special cases lose custody. The yearbook of the Supreme Administrative Court report precedents from 1935 related to this paragraph. See also Fredriksson (1971). Teachers kept daily records of student absence in the exam catalogue. The county governments were the main legal instance responsible for the enforcement of the regulations related to compulsory school attendance.

[10] Girls could attend *Realskola*, but there were also secondary schools only for girls termed *Högre flickskola*. These were very similar to *Realskola* and tracking took place after 4 years in *Folkskola*. Also some non-degree secondary education existed (e.g. the *Högre Folkskola*).

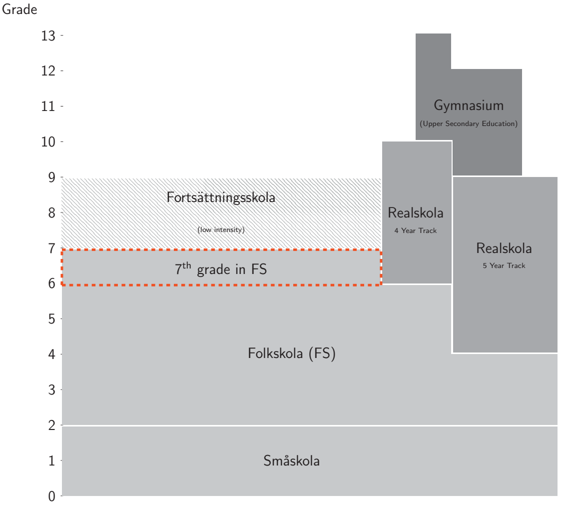
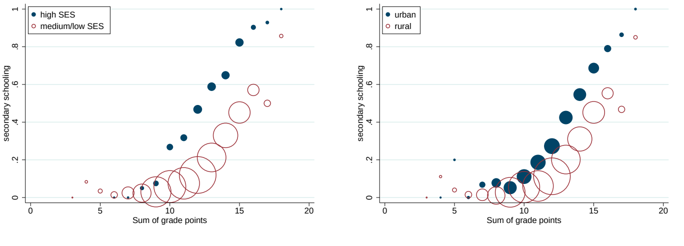


Figure 1. Swedish School System

Figure 2 illustrates the selective nature of admission to lower secondary school by plotting the proportion of students taking secondary schooling by primary school performance and background factors for a sample of individuals born 1930–34 (see Bhalotra et al., 2016, for details) for details. The gap in secondary schooling enrollment between children in families with high and medium/low SES and children residing in urban and rural areas, respectively, is to some extent attributable to high-SES and urban children performing better in primary school. Still there is a gap also after conditioning on primary school performance, particularly pronounced at 29 percentage points when stratifying by the father's SES (Figure 2(a)).



(a) By Father's SES

(b) By Place of Residence

Figure 2. Proportion Admitted to Realskola by School Grades and Background

The graphs show proportion admitted to *realskola* in a sample of 25,000 individuals born between 1930–34. Primary school grades range between 3 and 18 and are calculated as the sum of grades in math, reading and writing at the end of the fourth school year. Source: Bhalotra et al. (2016).

Compared with the US and other European countries, Sweden had a relatively low level of compulsory education in the early 1930s. In an attempt to catch up with international standards, the national Parliament decided in 1937 that the school year should be either 34.5, 36.5 or 39 weeks long. At the time the country had approximately 2,500 school districts and the reform triggered term length extensions in 600 districts already in the following two school years.[11] The 34.5 weeks option was to be discontinued by the school year 1941/42 and by 1952/53 39 weeks became the uniform standard.

The national Parliament also passed a law in 1936 extending compulsory schooling by introducing a mandatory 7th year. It was stipulated that a seventh year had to be implemented across the country by the school year 1948/49. As a result an additional year of compulsory schooling was implemented across school districts over the range of 12 years. The admission to secondary schooling did not change in the course of the reform. Students still switched to the academic track after 4 or 6 years.

Figure 3 shows the share of individuals with only primary education as highest school degree and those with having only 6 years of primary education between 1915 and 1945.[12] At the beginning around 80% of all students only attended primary school and just a small fraction of students completed secondary schooling and higher education. The share of students only attending primary school decreased to about 60% in the following decades. With the introduction of the 7 year reform, also the share of students attending only 6 years of primary school decreased substantially over time.

[11] The school districts generally coincided with the church parishes. Exceptions to this rule are mostly larger cities which were generally defined as one school district but consisted of several church parishes.

[12] Overall the Swedish school system exhibited striking similarities with the contemporary German school system – see e.g. Pischke and Von Wachter (2008).

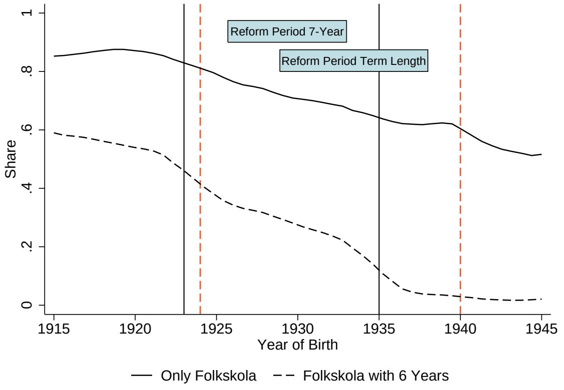


Figure 3. Share of Students with only Primary Education

Notes: Figure shows the share of individuals with only primary education as highest school degree and those with having only 6 years in primary.
The term length extension affected mainly cohorts born between 1924-1940. The 7-year compulsory schooling extension mainly affected individuals born between 1923-1935. Some cities and the southern part of Sweden had already introduced a 7^{th} grade earlier explaining the difference between the curves for cohorts born before 1923.
Source: LNU Survey, 1968,1974,1981. Own calculations.

The two extensions were independent and did not have to be implemented at the same time. Due to the soft transition rules, the extensions did not cause any major difficulties in the school districts. Their implementation was also facilitated by the fact that the funding of school buildings, teaching materials and teachers' salaries was the responsibility of the central government and not the school districts (Larsson, 2011). In addition, the transition period coincided with an oversupply of teachers in the 1930's and the 1940's (Fredriksson, 1971).[13] Also no sources suggest that schools were closed or that schooling was disrupted in other ways due to the Second World war in the neutral country Sweden (see Bhalotra et al., 2016; Fredriksson, 1971, and appendix C).

The two education reforms increased the total instructional time by similar amounts, but there are three important differences between them. First, the term extension affected children at much younger ages and not only at the end of compulsory schooling. Second, the term extension had virtually perfect compliance,[14] while the compulsory

[13]Following the oversupply of teachers in the 1930's the authorities actually cut the intake to the teacher colleges in the early 1940's. It was not until the beginning of the 1960's and the implementation of the comprehensive school system that there was a real teacher shortage (see Fredriksson (1971) for an overview and numbers on unqualified teachers in service).

[14]All pupils were affected in school years 1–4, and the vast majority of pupils (those not proceeding

schooling extension did not affect the minority of pupils who proceeded to lower secondary education. Third, the compulsory schooling extension changed the opportunity cost of secondary schooling: for someone considering enrollment in secondary schooling, the alternative after the reform was to spend one additional year in the lower track. For the term extension, there were no such changes in opportunity costs. Related to this, a change in secondary schooling enrollment accompanying the reforms is likely driven mainly by self-selection in the case of the compulsory schooling extension – whereas such change could also relate to improved learning outcomes in primary school in the case of the term extension.

2.2 Labor Market Entry

For young adults entering the labor market typically constitutes the most relevant alternative to continuing education. Therefore, extending compulsory schooling could potentially lead to losses in labor market experience. If this is the case, estimates for the compulsory schooling extension would constitute a net effect from an increase in formal schooling and a decrease of a year on the labor market. We outline in the following that labor market entry however was unlikely to be affected by the two considered reforms.

Swedish child labor laws have generally been coordinated with the compulsory schooling attendance laws. In 1931 the minimum age for manufacturing and construction work was 14 years, whereas the limit for *light work* was 13 years.[15] The labor laws stated that children could not take on a full-time employment until he/she had fulfilled compulsory schooling. In addition to the labor protection legislation, the main decree of the Folkskola (*Folkskolestadgan*) included specific rules on how much students were allowed to work while taking on compulsory schooling (Sjöberg, 2009).

According to the child labor law, a child was allowed to start working in the year she would reach the above age limits (Sjöberg, 2009). The term length extensions in the first 6 years of primary school left labor market experience unaffected as children were below the age limit. The compulsory schooling extension however could have

to lower secondary schooling) were affected in years 5–6.

[15]Light work referred to work outside factories or construction sites. Child labor laws only applied to employed work and not to work at e.g. the family farm. Importantly the labor laws did not only state the minimum working age, but also that children could not take on a full-time employment until he/she had fulfilled compulsory schooling (Sjöberg, 2009)

reduced labor market experience for students. After the implementation of the compulsory school extension, pupils left school in the middle of the year they turned 14, whereas before they would leave school the year they turned 13. Consequently, the reform reduced the time a child could spend in *light work* by one year, whereas the corresponding reduction for *hard* (industrial) work was 5-6 months.

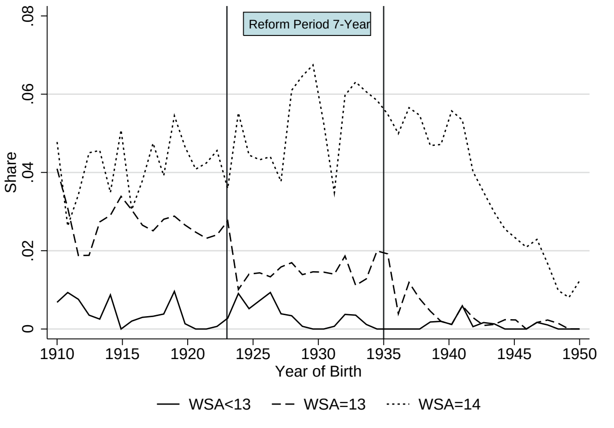


Figure 4. Working Start Age

Notes: Figure shows the share of individuals starting their first *real* job at a given age.
Questionaire: *Vilket år började ni förvärvsarbeta på riktigt, alltså inte feriearbete, praktik eller tillfälligt beredskapsarbete?*
Reform mainly affected individuals born between 1923-1935.
Source: LNU Survey, 1981. Own calculations.

Based on the LNU Survey (Swedish Level of Living Survey) we present descriptive evidence that supports our view that opportunities for adolescents between the ages 12 to 14 to enter the formal labor market were probably low in the 1930s and 1940s and labor market entry rules were enforced. Figure 4 shows that even before the reform only a negligible share of students entered regular employment before the age of 14. It is unlikely that the reform induced a substantial loss in labor market experience. The graph suggests that a very small fraction of students of 1-2% did in fact increase their working start age from 13 to 14 over the reform period. This fraction is tiny given a compliance rate of more than 60%.[16]

[16] Also see additional evidence in Appendix A.2

2.3 The Swedish Labor Market

There is general agreement that the Swedish labor market had the following main characteristics in 1970: a highly compressed wage structure, influential unions, high employment protection, and generous unemployment benefits (Adermon and Gustavsson, 2015). Some of the stylized facts associated with the Swedish labor market apply less to people born in the 1930s than to later cohorts; for example, family background appears to have mattered more to these cohorts, and their returns to schooling were slightly higher (Björklund, Jäntti and Lindquist, 2009). Table 1 provides some descriptives on how the individuals in our analysis sorted into classes of occupations. The classification is based on the 1970 census, and strongly relate to the ISCO 68 classification.

Table 1. Tasks for Occupational Groups

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Mean	**Share**		**Occupational Tasks**					**Grades**
	Earnings	*Occ. Group*	*Sec. Educ.*	*Nonr. Manual*	*Routine Manual*	*Nonr. Cog. Interactive*	*Routine Cog.*	*Nonr. Cog. Analytic*	*GPA*
A. Males and Females									
All	205,374	0.76	0.26	1.527	3.985	1.727	4.897	3.637	-0.009
				(1.388)	(1.079)	(2.660)	(3.793)	(1.990)	(0.769)
Managers & Professionals	276,077	0.21	0.58	1.323	4.285	3.110	3.830	5.547	0.304
Accounting, Admin.	167,571	0.08	0.39	0.123	4.827	0.624	7.797	3.234	0.318
Sales	195,567	0.08	0.22	0.495	3.306	2.578	1.004	4.398	0.091
Agricultural	128,998	0.05	0.07	2.392	2.917	4.660	2.169	3.226	-0.166
Transport, Comm.	199,499	0.05	0.11	3.131	3.179	0.948	2.047	2.097	-0.154
Crafts	200,151	0.22	0.03	1.926	4.386	0.255	8.537	2.781	-0.321
Service	123,529	0.07	0.13	1.517	2.977	0.966	1.273	1.826	-0.066
No Occupation	-	0.24	0.22	-	-	-	-	-	0.022
B. Males and Females / No Secondary Education									
All	180,314	0.76	0.00	1.678	3.934	1.309	5.161	3.207	-0.150
				(1.388)	(1.079)	(2.660)	(3.793)	(1.990)	(0.769)
Managers & Professionals	216,766	0.12	0.00	1.386	4.437	2.270	4.233	5.251	0.031
Accounting, Admin.	158,494	0.06	0.00	0.132	4.876	0.620	7.963	3.274	0.168
Sales	170,412	0.08	0.00	0.521	3.346	2.346	0.935	4.299	0.025
Agricultural	126,525	0.07	0.00	2.395	2.918	4.577	2.188	3.179	-0.202
Transport, Comm.	200,114	0.06	0.00	3.339	3.047	0.841	1.698	1.984	-0.225
Crafts	199,802	0.28	0.00	1.933	4.381	0.255	8.538	2.770	-0.339
Service	111,797	0.08	0.00	1.477	2.982	0.925	1.283	1.789	-0.117
No Occupation	-	0.24	0.00	-	-	-	-	-	-0.076

Notes: Descriptive Statistics for Tasks. **Columns:** (1) Mean Labor Earnings in 1970 for Occupational Group (2) Share (3) Share with Secondary Education Within Occupational Group (4)-(8) Average Tasks for Occupational Group (9) GPA in Primary School (based on representative sample of individuals born during the period 1930-1934). *Source:* Linked 1970 Census. Own calculations.

The top panel includes all individuals in our analysis sample and reports average earnings, educational attainment, task profiles (adapted from Autor, Levy and Murnane, 2003),[17] and primary school performance (normalized to have mean zero and standard deviation one, cf. Bhalotra et al., 2016). We see that individuals with secondary schooling are overrepresented in the classes "Managers & Professionals" and "Accounting, Administrative" and underrepresented in "Crafts", "Agriculture", "Transport/Communication" and "Service". The former group of classes tends to have above-average requirements in cognitive tasks, but also in routine manual tasks. The latter group, on the other hand, tends to have above-average requirements in non-routine manual tasks. These tasks profiles appear to be matched by skills, since the occupations with higher demands in cognitive and routine manual tasks also performed better in primary school on average.

Table 1 also shows that secondary schooling enrollment was not the only individual decision determining labor market performance. In the lower panel we report the corresponding statistics for individuals with only compulsory schooling. Also in this subsample, the first two groups are characterized by high demands in routine cognitive and manual tasks. The occupations in this group also belonged to the better-paid – but this characteristic is muddled by a compositional effect, since it was completely dominated by females.[18] Likewise, we find evidence of positive selection – based on primary school GPA – into the three highest groups, in particular "Accounting, Administrative". In Appendix A we show that a similar skill profile of occupational groups is visible in adult literacy data.

[17]Even though such task profiles, either the Dictionary of Occupational Titles or its successor O*NET (Mariani, 1999), are typically based on the U.S. labor market, it is customary to assume they are approximately correct also for the labor market in other Western countries, including Sweden (Goos, Manning and Salomons, 2009; Adermon and Gustavsson, 2015). Based on crosswalks provided by Bihagen (2007) it is possible to assign task scores at the three-digit level for the vast majority (80%) of occupations. The remaining occupations were either checked manually or assigned a task profile based on their two-digit group membership.

[18]Table 17 in Appendix A presents a breakdown by sex

3 Data and Sample Selection

3.1 Reform data

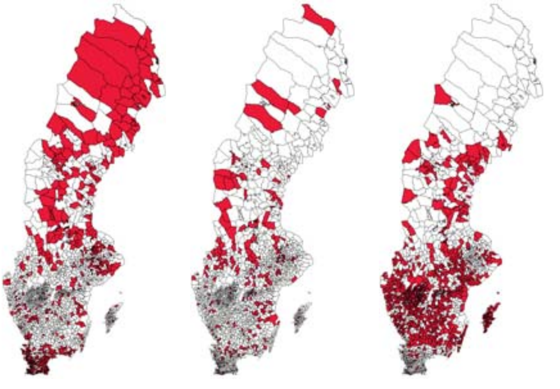
The empirical analysis uses a purpose-built reform data set on compulsory extensions of the Swedish primary school system, extracted and digitized from historical archives. The data set includes information on the year of the introduction of a mandatory seventh grade and the term length which varied between 34.5, 36.5 and 39 weeks across time and space. The data was collected at the school district level.

The primary data source for identifying the institutional features and changes of the primary school system comes from standardized exam catalogues that every school had to file. The exam catalogue is an annual documentation that provides individual information on each student, e.g. their attendance and their grades in various subjects, but also information on the name of the school, the school type, term length and information on the number of years of education provided by the school. The historical exam catalogues are publicly available in local archives across the country. By systematically reviewing the exam catalogues for a school district and each school year we exactly identify the timing of changes in term length and compulsory years of education. We thus obtained two independent reform years, for the term length and the compulsory schooling reform, within each school district. The systematic evaluation of exam catalogues also validate that changes were implemented simultaneously in all schools within the same school district. As the exam catalogues are student based it is also possible to directly infer that the compulsory schooling extension had *bite*, i.e. that students followed the newly implemented rules and did not defy. Appendix D provides an example of an exam catalogue.

The reform data set contains information on the year of reform implementation for more than 98% of all existing school districts at the time of interest. Fischer et al. (2016) give detailed background on the reforms and their connection to later school reforms, and detailed information on sources and data collection procedures. Fischer et al. (2016) also provides information on various sensitivity tests and check-ups performed to assess the quality and validity of the reform data.[19]

[19] Our way of assigning reform status to a district has been validated in various ways: e.g. by comparing our reform data with official statistics on the share of school districts that had implemented the seven year reform in each county of Sweden for the years 1938-1945 and by manual check-ups

Figures 5(a) – 5(c) and 6(a) – 6(c) graphically present the spatial and temporal variation of the term length extension and the compulsory schooling extension, respectively. While there is considerable spatial and temporal variation, the implementation was not random.



(a) 1936–38 (b) 1939–42 (c) 1942–55

Figure 5. Timing of the Term Length Extension

Rural and small districts in the middle west of the country extended the term length to 39 weeks later than other school districts. The geographical unit in the maps is parishes, which generally correspond to the school districts.

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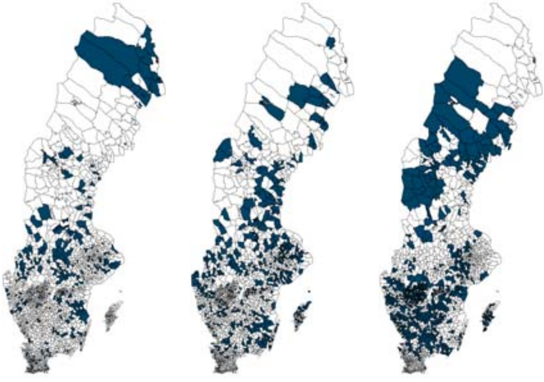
for all schools in certain districts to ensure that all schools implemented the reform at the same time within a district. We are confident that the accuracy of the reform data is very high.

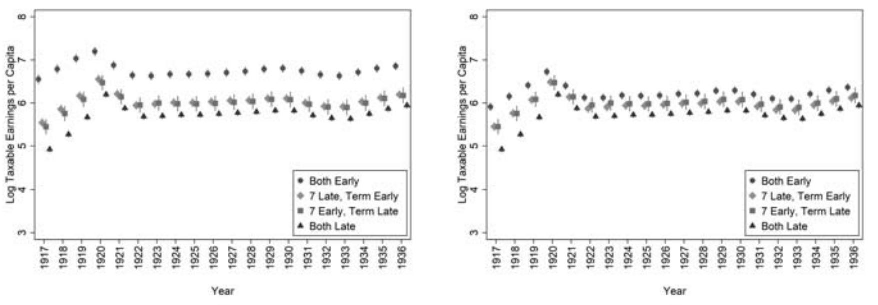


(a) 1936–41 (b) 1942–47 (c) 1948–53

Figure 6. Timing of the Introduction of the 7$^{\text{th}}$ Grade

Rural and small school districts in the middle west and sparsely populated districts the north (with the exception of the most northern county Norrbotten) implemented the compulsory schooling extension at the latest possible date or in some rare exceptions even delayed the introduction. The southern region of Skåne has almost no reform implementation after 1936 as most districts already had a mandatory seventh grade. The geographical unit in the maps is a parishes, which generally correspond to the school districts.

Figure 7 plots average taxable earnings for four groups of school districts: districts that were early adopters of both reforms (with early adoption defined as having implemented the reforms before 1945), late adopters of both reforms, and two groups including districts that were early adopters of one reform and not the other. The figures confirm that reform implementation is strongly related to local characteristics and there is a clear ranking between the four groups considering average taxable earnings. Early adopters are generally richer than the other groups – and this is to a great extent due to these areas being disproportionately urban. The two intermediate groups which implemented one of the reforms early and the other one late represent an intermediate case and are indistinguishable from each other. The districts that adopted both reforms late tend to be the poorest. In Appendix Figures 16(a) and 16(b) we also show that both reforms were implemented later in school districts with high shares of employment in agriculture. We conclude that even though all districts appear to follow a common time trend, it seems desirable to take systematic differences between urban and rural areas into account in the empirical analysis.



(a) All areas

(b) Rural areas

Figure 7. Trends in taxable earnings.

Source: Municipal yearbooks (*Årsbok för Sveriges Kommuner*; 1918–38).

3.2 Assigning Reform Status

We assign reform exposure based on the 1946 place of residence of all individuals. Since the cohorts of interest were born between 1930–40, this assignment captures the *place of residence* during schooling age. This is arguably a near-optimal approximation of the place of schooling, and an assignment that minimizes the measurement error, with respect to e.g. migration. Information on family structure and place of residence in 1946 comes from the 1950 Census. This information is not available for the City of Stockholm due to a different registration system and therefore the city is not part of our final sample.

An alternative way to assign reform status would be to use information on individuals' *place of birth* as a proxy for the place of residence during schooling age. Under normal circumstances, such assignment would allow estimating an "intent-to-treat" effect. In the current context, however, the place of birth variable contains severe measurement error which introduces bias of unknown sign and magnitude. The source of this measurement error is institutional deliveries. For cohorts born in a hospital before 1947 the location of the hospital is recorded as the place of birth instead of the place of registration/residence of the parents.[20] With a growing number of institutional births, the place of birth becomes increasingly uninformative as a measure of place of residence at birth and consequently also as a proxy for place of residence

[20]This potential problem is also mentioned by Holmlund (2008).

during childhood. We evaluate the impact of the hospital birth coding in more detail in Appendix B. Given the above caveats our main analysis uses the place of residence during schooling age to assign reform status.

3.3 Individual-Level Data

The base study population for our main analysis is drawn from the Swedish Census in 1970 and consists of individuals born between 1930 and 1940. The data covers information on individuals' labor market status, occupation, income, education, marital status and place of residence in 1970. Information on living conditions and individual characteristics are based on self-enumeration and refer to the first week of October 1970 when the Census took place. With respect to labor force participation, persons are classified as economically active if they reported themselves as gainfully employed.[21] To explore labor market effects we create a dummy variable indicating whether an individual was gainfully employed in 1970.

Educational attainment is reported by the highest completed schooling degree as well as by the highest completed post-schooling degree (e.g. vocational training or university) attained. In case individuals were currently in training the last completed highest degree is recorded but given that all our individuals are older than 30 the clear majority had finished their education in 1970. As the 1970 Census does not contain direct information on the years of education we add a constructed measure for the years of education (YoE) based on the schooling and post-schooling variables in the Census. We assign the usual years of education associated with different types of schooling and post-schooling degrees[22] and take the sum as an approximation for the total years of education:

$$\mathbf{YoE} = \mathbf{Years\ of\ Schooling} + \mathbf{Years\ of\ Post\ Schooling}.$$

The years of education measure allows us to estimate returns to years of education.

[21] Workers within the family (paid and unpaid) and persons who were temporarily on leave (including parental leave) were also regarded as economically active in case their absence lasted less than four months.

[22] The years associated with a post-schooling degree in the 1970 Census are directly drawn from the Educational Registers. We used the most frequent SUN2000 classification for each degree in the Census and assigned the length from the Educational Registers to the 1970 Census degrees.

Unfortunately, the 1970 Census does not differentiate between the number of years spent in *Folkskola* if instruction time was less than 7 years; i.e. we can not distinguish between 6 or 7 years of primary education here.[23]

Income statistics stem from official tax returns and are considered as highly accurate.[24] We use the combined income from employment (*inkomst av tjänst*), self-employment (*inkomst av rörelse*) and agriculture (*inkomst av jordbruk*) as a measure of annual labor earnings.[25] Furthermore, we add data on pensions from the year when individuals turned 73.[26] Both measures are CPI adjusted to SEK in 2014. For the cohorts born 1930-1940 full pensions require thirty years of contributions and the level of the pension is based on the fifteen highest income years (Sundén, 2006). Pensions can be expected to be less sensitive to fluctuations in labor supply than earnings. This is a desirable feature, especially when analyzing women's returns to human capital as career interruptions due to e.g. childbearing affect the pensions to a smaller degree than annual earnings. Table 2 presents summary statistics for our main outcome variables, the schooling reforms and socio-demographics for males and females.

In order to explore possible mechanisms, we make use of an additional individual level data set on mortality based on the Swedish Death Index (cf. Bhalotra, Karlsson and Nilsson, 2017). The data set stems from official church books and population registers and covers the near-complete number of deaths in the population occurring between 1901-2013, including information on the date and place of death. For our heterogeneity analysis and balancing tests we also add information on parental socio-economic status based on the SEI classification of occupations in the 1950 population census.

[23]This caveat is universal for Swedish Register Data on education including the Swedish Educational Registers.

[24]In general all individuals aged 16 or older are liable of submitting a tax declaration. If individual annual income or aggregated annual income in the case of married falls below 2,350 SEK, individuals were exempted from mandatory tax declaration leading to left censoring of the income distribution. With an annual income of $\sim$ 2,080 US$ (CPI adjusted for 2015) the threshold is however extremely low.

[25]Our choice of the income variable follows Edin and Fredriksson (2000). The income measure in 1970 is not fully consistent with the current standard labor earnings measure (*arbetsinkomst*) used by Statistics Sweden. We do not have information on sick pay benefits which only became taxable in 1974 and which should be included in income from employment. We also lack information on pensions which should be subtracted. Given that pensions are unlikely a major income in 1970 for cohorts born after 1930 and sickness benefits are only a minor part of the income, we conclude that the income measure is a very reasonable approximation of annual labor earnings.

[26]73 is the first age we observe all relevant cohorts receiving a pension.

Table 2. Descriptive Statistics

	MALES Mean	Std.Dev.	Obs	FEMALES Mean	Std.Dev.	Obs
MAIN OUTCOMES						
Labor Earnings (1970, CPI adjusted to 2014)	241,324	131,274	301,296	75,054	90,307	280,607
Pensions (Age 73, CPI adjusted to 2014)	232,739	139,500	214,075	159,086	74,473	225,276
Only Primary School	0.66	0.47	301,296	0.61	0.49	280,607
Secondary School	0.23	0.42	301,296	0.27	0.44	280,607
Dropout Secondary School	0.11	0.31	301,296	0.12	0.32	280,607
TREATMENTS						
Average Term Length	38.45	1.02	301,295	38.49	0.99	280,604
Compulsory 7th Year	0.87	0.34	301,151	0.88	0.32	280,434
SOCIO-DEMOGRAPHICS						
Year of Birth	1935.62	3.04	301,296	1935.76	2.98	280,607
Parents High SES	0.20	0.40	301,296	0.21	0.41	280,607
Parents Academics	0.04	0.20	301,296	0.04	0.19	280,607

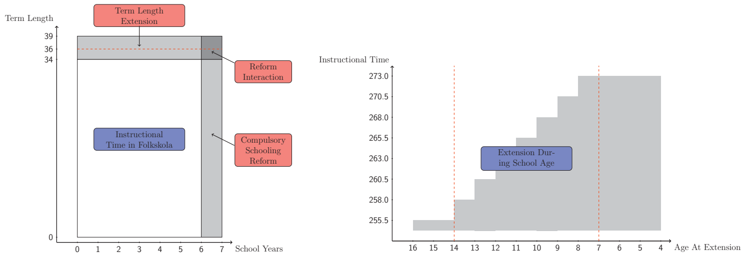
Notes: Descriptive statistics refer to cohorts 1930 to 1940. *Source:* Linked 1970 Census. Own calculations.

4 Empirical Strategy

Our empirical analysis exploits exogenous variation in the amount of instructional time in primary school generated by the two above-mentioned educational reforms. Depending on the compulsory years in *Folkskola* and pre-reform status, the term length extension led to a maximum cumulative increase in instructional time ranging between 15 and 31.5 weeks[27] over the time spent in primary school. The compulsory schooling reform added one complete year with 34.5, 36.5 or 39 weeks of additional instructional time. Figure 8 gives a stylized representation of the variation in instructional time generated by the two educational policies.

Since both reforms were decided and implemented at the local level they may be correlated. We thus use specifications including both reforms at the same time. Figure 8(a) however demonstrates that even strong correlation in the timing of the two independent reforms is unlikely to cause any problems even without such adjustment. The reason is a direct consequence of the step-wise increase of the extension of term length as shown in Figure 8(b). In contrast the compulsory schooling extension generates a sharp discontinuity between pre- and post-reform cohorts.

[27] Equivalent to 1/2 to 1 full school year.



(a) Total Instructional Time

(b) Implementation of Term Length Extension ($36.5 \rightarrow 39$)

Figure 8. Total Instructional Time by Reform Status.

Figure 8(a) compares the total instructional time in primary school. The instructional time varies in two dimensions – term length and length of Folkskola in years. Figure 8(b) shows the variation from term length depending on the age at which individuals were affected by the policy change. School districts changed the term length simultaneously for all grades. Individuals in lower grades at the time of extension received more education in total.

We estimate the causal effect of the interventions using Difference-in-Differences (DID). Denoting by y_{ijk} the outcome variables of individual i of cohort j going to school in school district k, our main regression equation is given by

$$y_{ijk} = \beta_1 T_{jk} + \beta_2 Z_{jk} + \nu_k + \mu_{R_k j} + C_k \cdot j + \epsilon_{ijk} \quad (1)$$

where Z_{jk} is an indicator of whether an individual belonging to cohort j and residing in parish k has been exposed to the extension of compulsory schooling from 6 to 7 years. The continuous variable T_{jk} is the average term length measured in annual weeks of instruction in the school district for cohort j. To enhance a comparison to estimates for the returns to years of education we re-scale the average term length to a full year. By dividing the average term length by the factor 39, the interpretation of the coefficient is equivalent to an additional year of compulsory schooling instruction with 39 weeks. Both treatment variables vary on school district level. We include school district fixed effects (ν_k) and a non-parametric cohort trend $\mu_{R_k j}$ which we allow to differ between urban ($R_k = 0$) and rural ($R_k = 1$) districts. In addition, we allow for regional divergence by including the 24 county-specific time trends $C_k \cdot j$.

The identifying assumption is that in the absence of the intervention, earnings of individuals from treated and untreated districts would have followed a common cohort trend inside each cell defined by county and urban/rural status. Since Section 3 showed that the reform implementation was related to several parish characteristics, it is important to carefully assess the plausibility of this assumption. Below we present balancing tests for household characteristics (Table 3) and for school district characteristics (Table 4). Table 3 shows that reform implementation correlates with several household characteristics: the affected children are more likely to have a high SES background and educated parents. But when we condition on the various fixed effects and trends specified in equation (1), the magnitudes for these characteristics are reduced a lot and all but one turn insignificant. Table 4 shows that the reform correlates with population size (proxied by the number of voters), political preferences and distance to the nearest secondary school. However, in our preferred specification, all of them are diminished and most lose statistical significance. Overall, out of 26 tests carried out in the balancing tables, only one turns out statistically significant at the 5 per cent level. We interpret this as evidence supporting our main specification.

Table 3. Balancing Test; Household Level

	Mean	**Treatment**	
Term Length Extension			
Household Head Male	0.861	-0.034***	-0.005
		(0.002)	(0.002)
High SES	0.207	0.232***	0.003
		(0.007)	(0.002)
Household Size	3.815	-0.300***	0.024
		(0.013)	(0.007)
HHhead Academic	0.040	0.053***	-0.004
		(0.002)	(0.001)
Age at Birth HHead	32.882	-2.845***	-0.236
		(0.068)	(0.048)
7-Year Extension			
Household Head Male	0.861	0.001	0.002
		(0.002)	(0.002)
High SES	0.207	0.039***	0.000
		(0.007)	(0.002)
Household Size	3.815	-0.091***	-0.007
		(0.013)	(0.007)
HHhead Academic	0.040	0.008***	-0.002**
		(0.002)	(0.001)
Age at Birth HHead	32.882	-0.442***	-0.017
		(0.068)	(0.048)
Cohort FE		✓	✓
District FE			✓
Rural Urban Trend			✓
County Trends			✓

Table 4. Balancing Test; School District Level

	Mean	Treatment	
Term Length Extension			
Number of voters	1,654.811	1755.510***	-106.715
		(529.562)	(58.621)
Election turnout	0.716	-0.000	-0.004
		(0.004)	(0.007)
Conservative vote	0.151	-0.016***	0.001
		(0.004)	(0.002)
Agrarian vote	0.281	-0.095***	0.000
		(0.008)	(0.002)
Liberal vote	0.123	-0.002	-0.002
		(0.004)	(0.001)
Labour vote	0.406	0.092***	0.002
		(0.007)	(0.002)
Communist vote	0.034	0.019***	-0.000
		(0.003)	(0.002)
Distance to Realskola	21.035	-0.662	0.070
		(0.840)	(0.398)
7-Year Extension			
Number of voters	1,654.811	1553.435***	-99.296*
		(529.562)	(58.621)
Election turnout	0.716	0.028***	0.009
		(0.004)	(0.007)
Conservative vote	0.151	-0.009*	-0.002
		(0.004)	(0.002)
Agrarian vote	0.281	-0.058***	-0.000
		(0.008)	(0.002)
Liberal vote	0.123	-0.015***	0.002*
		(0.004)	(0.001)
Labour vote	0.406	0.073***	0.001
		(0.007)	(0.002)
Communist vote	0.034	0.007**	-0.001
		(0.003)	(0.002)
Distance to Realskola	21.035	-8.532***	-0.453
		(0.840)	(0.398)
Cohort FE		✓	✓
District FE			✓
Rural Urban Trend			✓
County Trends			✓

We cluster standard errors at the school district level for the 2,390 districts included in the analysis. This is our main basis for statistical inference. Given that our analysis sample represents almost the entire population, the rationale for statistical significance testing may be questioned.We therefore also implement design-based inference and randomly reassign reform years (without replacement) between school districts. Inference is based on the t statistics of the individual regression coefficients – which have been shown to be superior to inference based on the estimated coefficients themselves when the clusters are unequally distributed or small in number (MacKinnon and Webb, 2016).

The parameters β_1 and β_2 capture intention-to-treat effects, i.e. the effect of the policy on the population. With both interventions affecting large shares of the population, effects should be relatively close to the average treatment effect in the overall population. The extension of term length in primary school affected all students in the first 4 years in primary school and after that the children not attending the academic track. Apart from rare exceptions, the compulsory schooling reform was binding and consequently never-takers can be ruled out as a substantial phenomenon. Therefore, the reform only affected students who would otherwise have finished school after 6 years, i.e. 70% of all children.

5 Results

5.1 Compliance

The main dataset does not contain explicit information on compliance with the reforms, but we are able to estimate the "first stage" – i.e. how the time spent in school responds to the two treatments – using other data sources. Table 5 presents results on how the total days spent in school respond to the term length extension. Attendance rates were 0.94 on average, and according to our estimates, each additional day added to the school year translated into 0.86-0.99 additional days spent in school. Using log attendance as the main outcome variable, we also get elasticities very close to the observed share of the school year actually spent in school. The results are clearly indicative of perfect compliance with the term extension reform.

Table 5. Attendance

	Males and Females		Males		Females	
	Grade 1	Grade 4	Grade 1	Grade 4	Grade 1	Grade 4
Attendance	0.9373***	0.8692***	0.8812***	0.8624***	0.9903***	0.8732***
	(0.051)	(0.055)	(0.070)	(0.081)	(0.050)	(0.050)
	[0.837;1.037]	[0.760;0.978]	[0.743;1.019]	[0.703;1.022]	[0.891;1.089]	[0.774;0.972]
Log Attendance	1.0178***	0.9142***	0.9563***	0.9152***	1.0779***	0.9101***
	(0.060)	(0.056)	(0.089)	(0.085)	(0.059)	(0.058)
	[0.900;1.135]	[0.804;1.025]	[0.780;1.133]	[0.748;1.082]	[0.961;1.195]	[0.795;1.025]
Mean Attendance	194.33	197.54	193.87	197.33	194.76	197.75
Rate	0.94	0.94	0.93	0.94	0.94	0.94
N	7,479	8,574	3,654	4,282	3,825	4,292
School District FE	✓	✓	✓	✓	✓	✓
QOB×YOB FE	✓	✓	✓	✓	✓	✓
SES Effects	✓	✓	✓	✓	✓	✓

The estimation sample consists of 16,000 individuals born between 1930–34. Source: Exam catalogs (cf. Appendix D) from 130 school districts (Bhalotra et al., 2016).

An empirical analysis of the compulsory schooling extension based solely on register data cannot identify educational effects of the reform as Swedish register data only states whether the highest schooling degree was primary.[28] To assess the impact

[28]As mentioned in the data section the Census 1970 only identifies students with 7 or less years in *Folkskola*. The Educational Registers are even less informative in this respect as they do not differentiate between any length in *Folkskola* and do not capture the highest attended school for those with post-schooling degrees.

of the compulsory schooling reform from 6 to 7 years, we use the Swedish *Survey on Living Conditions* (ULF)[29] which includes self-reported information on years of schooling. Table 6 presents regression results which suggest that the reform substantially increased educational attainment.

The ULF survey does not contain information on the place of residence during childhood which implies that we have to rely on the place of birth as an approximation when using this data.[30] Given the assignment by parish of birth, we expect estimates for the first stage based on a misclassified instrument to be downward biased. For a first stage

$$S_i = \alpha_0 + \alpha_1 Z_i^* + u_i,$$

with the reform indicator $Z_i^* \in \{0, 1\}$ based on the correct place of residence during schooling age, α_1 gives the first stage for the schooling reform. Regressing the years of schooling S_i on the misclassified reform indicator Z_i (place of birth) leads to

$$\operatorname{plim} \widehat{\alpha}_1 = \mathbb{E}[S_i|Z_i = 1] - \mathbb{E}[S_i|Z_i = 0] = \alpha_1 \underbrace{\{\mathbb{P}(Z_i^*|Z_i = 1) - \mathbb{P}(Z_i^*|Z_i = 0)\}}_{=\delta}.$$

The first stage is attenuated by the factor δ which is strictly lower than 1. In the *Linked 1970 Census* we observe the parish of residence during schooling age Z^* as well as the parish of birth. In order to correct for the attenuation bias in the survey, we estimate δ based on the *Linked 1970 Census* and achieve an unbiased estimate for α_1 by applying indirect least squares:

$$\operatorname{plim} \widehat{\alpha}_1^{ILS} = \frac{\mathbb{E}[S_i|Z_i = 1] - \mathbb{E}[S_i|Z_i = 0]}{\mathbb{E}[Z_i^*|Z_i = 1] - \mathbb{E}[Z_i^*|Z_i = 0]} = \alpha_1.$$

[29] The Swedish *Survey on Living Conditions* (ULF) is a survey conducted on a continuous basis in Sweden since 1975 and comprising a representative sample of the Swedish population aged 16-84 years. Each respondent was randomly selected and participated in a face-to-face interview and was asked to answer questions regarding living conditions. The ULF survey covers a wide range of variables on economic and educational outcomes. Most importantly the survey incorporates detailed questions on the schooling history of individuals including how many years they actually spent in the highest attended school track. This allows us to differentiate between different lengths of compulsory schooling for each individual on a small subpopulation to illustrate the impact of the reform. Based on the ULF survey we construct an indicator of whether a person had more than the old compulsory level of 6 years of schooling. Since the survey does not contain information on term length this data can only be explored for the 7 year extension.

[30] All individuals in the survey were assigned a place of birth regarding their birth parish based on information in the *Befolkningsregistret* (the population register) by Statistics Sweden (SCB).

This is a split-sample IV with measurement error in the instrumental variable. We derive standard errors by applying the delta method.[31]

In the absence of substantial increases in educational attainment beyond primary school we would expected a first stage estimate for α_1 roughly equal to the share of individuals only visiting primary school. Our descriptives suggest that 60 – 70 % of the population only visited primary school. This corresponds well with our ILS estimates for α_1 in Table 6.

Table 6. First Stage: Effects on Schooling

	Males and Females	Males	Females
Outcome: Years of Schooling (ILS)			
$\widehat{\alpha}_1^{ILS}$	0.607	0.503	0.804
	(0.143)	(0.240)	(0.237)
Outcome: Years of Schooling			
$\widehat{\alpha}_1$	0.440	0.365	0.582
	(0.103)	(0.174)	(0.171)
F-Statistic	18	4	12
Observations	6,292	3,108	3,184
Districts	1,333	912	951
Mean Outcome	7.799	7.881	7.719
Outcome: Reform Indicator Z^*			
$\widehat{\delta}$	0.725	0.726	0.724
	(0.014)	(0.015)	(0.014)
Observations	574,856	297,549	277,307
Districts	2,398	2,383	2,383

Notes: Table shows first stage effects for the compulsory schooling extension. Robust standard errors clustered at school district level are reported in parenthesis.
Dependent Variables: *Years of Schooling* refers the self-reported years of schooling in the survey. *Reform Indicator* Z^* refers to the actual place of residence during schooling age in the Linked 1970 Census.
Independent Variable: *Reform Indicator* Z based on place of birth assignment.
Results refer to cohorts 1920 to 1940 for the ULF survey and 1930 to 1940 in the Linked 1970 Census. All regressions control for school district FE (assigned by parish of birth), birth cohort FE and survey year effects.
Source: ULF Survey, Linked 1970 Census. Own calculations.
Source: Linked 1970 Census. Own calculations.

5.2 Earnings

5.2.1 1970 Earnings

Table 7 reports coefficients from regressions of log annual labor earnings in 1970 on the two policy instruments term extension and compulsory schooling extension. In order to compare the magnitude to general returns to education, we add conventional estimates for the years of education on earnings as a benchmark. The conventional results show

[31] In fact, as δ is estimated on the full population $\mathsf{var}\left(\widehat{\alpha}_1^{ILS}\right) \approx \frac{1}{\widehat{\delta}^2}\,\mathsf{var}\left(\widehat{\alpha}_1\right)$ where $\widehat{\alpha}_1$ is the survey estimate based on place of birth. This also implies (almost) no changes in $t-$statistics and $F-$statistics.

substantial returns to years of education on earnings. For males and females combined, each year of education associates with an increase in earnings by 9.9 per cent; when we estimate the correlation separately by sex, the coefficient is almost twice as large for females as for males. The 14-per cent coefficient for females is unsurprising given that annual earnings capture effects from both labor supply and wage increases. Only 66% of females were working in 1970. For males the labor market participation rate was close to 100% with most males working full-time, implying that the returns on annual earnings should be close to the returns on hourly wages. Labor market effects are explored further in Section 5.3.2. Our results are in line with previous studies on the returns to education for Sweden before the 1970s suggesting an average return of about 8% to an additional year of education on hourly wages based on survey data (Edin and Topel, 1997).

Table 7. Main Results: 1970 Earnings

	(1)	(2)	(3)	(4)	(5)	
			Mean	**OLS YoE**	**Term Length Extension**	**7-Year Extension**
	Districts	*N*	*Earnings*			
A. MALES AND FEMALES						
Log Earnings 1970	2,389	476,792	161,138	0.099***	0.048***	0.020***
				(0.001)	(0.019)	(0.006)
B. MALES						
Log Earnings 1970	2,388	290,023	241,389	0.077***	0.025*	0.015***
				(0.000)	(0.014)	(0.004)
C. FEMALES						
Log Earnings 1970	2,381	186,769	75,081	0.139***	0.090**	0.028**
				(0.001)	(0.043)	(0.013)

Notes: Robust standard errors clustered at school district level are reported in parenthesis. Significance levels: $*$ 0.10 $**$ 0.05 $***$ 0.01. Results refer to cohorts 1930 to 1940. All regressions include rural/urban birth cohort FE, linear county trends and a gender dummy if pooled regression. **Dependent Variable:** *Earnings* from 1970 Tax Records. **Legend:** (1) Number of School Districts (2) Number of Observations (3) Mean Labor Earnings 1970 (4) OLS regression coefficient for years of education. (5) Reduced Form Baseline Specification.

The reduced form estimates in Table 7 for the term extension (which are scaled to correspond to an average school year) of 4.8 per cent are at around half the level as the estimated OLS returns to schooling. There are important differences by sex: a year of additional term length increases female earnings by 9 per cent when the school year is extended; male earnings increase by 2.5 per cent only.

In contrast, estimates for the effects from an additional compulsory year of schooling are small. For males and females combined, the estimate is 2.0 per cent, and this effect is also more pronounced for females. Even a simple rescaling of this estimate by the complier rate of 70% delivers much smaller estimates than the measured OLS returns to education. Our result is thus well in line with the recent economics literature suggesting that the returns to additional years of compulsory schooling are small (see e.g. Pischke and Von Wachter, 2008; Devereux and Hart, 2010; Chib and Jacobi, 2015).

5.2.2 Pensions at Age 73

Table 8 presents results using pensions at age 73 as an outcome. As pensions are calculated on the basis of the 15 years with the highest income and less prone to labor supply fluctuations than annual earnings, they offer an alternative measure for earnings. The OLS returns to education corroborate this point. At 7 %, the returns to years of education for females are basically identical to those of males and of similar magnitude as males' labor market returns to education with data on earnings. Interestingly, the term extension appears to have affected peak earnings less than early career earnings: the overall effect is reduced to 2.4 per cent, mainly driven by an effect on female earnings by 4.1 per cent. A comparison of results regarding 1970 earnings and pensions suggests that the term extension increased earnings throughout working life, but also led to a flattening of the age profile of earnings. For the compulsory schooling extension, we find small but significant effects of around 1% on pensions.

Table 8. Main Results: Pensions at Age 73

	(1)	(2)	(3)	(4)	(5)	
			Mean	**OLS YoE**	**Term Length**	**7-Year**
	Districts	*N*	*Earnings*		**Extension**	**Extension**
A. Males and Females						
Log Pensions Age 73	2,390	440,363	195,013	0.069***	0.024**	0.011***
				(0.000)	(0.010)	(0.003)
B. Males						
Log Pensions Age 73	2,388	214,419	232,804	0.068***	0.010	0.018***
				(0.000)	(0.016)	(0.004)
C. Females						
Log Pensions Age 73	2,384	225,944	159,150	0.070***	0.041***	0.006*
				(0.000)	(0.013)	(0.004)

Notes: Robust standard errors clustered at school district level are reported in parenthesis. Significance levels: $*$ 0.10 $**$ 0.05 $***$ 0.01. Results refer to cohorts 1930 to 1940. All regressions include rural/urban birth cohort FE, linear county trends and a gender dummy if pooled regression. **Dependent Variable:** *Pensions* from Tax Records at age 73. **Legend:** (1) Number of School Districts (2) Number of Observations (3) Mean Labor Earnings 1970 (4) OLS regression coefficient for years of education. (5) Reduced Form Baseline Specification.

5.3 Mechanisms

We have seen that the term extension led to a substantial increase in earnings, whereas the compulsory schooling extension had only moderate effects. It seems likely that this difference relates to the age at which the additional instructional time is administered: pupils are affected by the term extension from age 7 onward, while the compulsory schooling extension only becomes effective at age 13. the reforms may also have affected intermediate educational outcomes differently. Below we analyse various mediators in order to shed further light on this issue.

5.3.1 Educational Outcomes

As shown in section 5.1, both reforms had large immediate effects on time spent in school: the term extension applied to all children within a school district, and the compulsory schooling extension to seven years had around 70 per cent compliance rates. In order to understand the mechanisms responsible for the reported increases in earnings, it is important to examine whether the reforms had spillover effects affecting other educational choices. Table 9 provides a summary of results.

Column three presents the effects of the term extension on schooling outcomes.

The reform associates with a significant reduction in secondary school dropout rates for both males and females. Interestingly, this reduction in dropouts does not appear to associate with increased secondary schooling completion, but rather with an increased propensity to stay in the lowest track (although estimates are less precise). Taken together, these effects are possibly the result of improved sorting into secondary schooling enrollment thanks to the extended school year in primary school. The results also imply that the spillover effects of the term extension on other school forms are close to zero, and thus we may rule out spillovers as a mechanism behind the estimated gain in earnings.

Table 9. Secondary Education

	(1)	(2)	(3)	(4)
	Mean		**Term Length**	**7-Year**
	Earnings	*Outcome*	**Extension**	**Extension**
A. Males and Females				
Only Primary	178,546	0.638	0.025*	-0.005
			(0.014)	(0.004)
Dropout Secondary Schooling	190,183	0.115	-0.033***	-0.003
			(0.012)	(0.004)
Secondary Schooling	278,118	0.247	0.008	0.008***
			(0.009)	(0.003)
B. Males				
Only Primary	214,989	0.661	0.030*	-0.007
			(0.016)	(0.005)
Dropout Secondary Schooling	237,037	0.111	-0.040***	-0.004
			(0.014)	(0.004)
Secondary Schooling	352,208	0.228	0.009	0.011***
			(0.012)	(0.004)
C. Females				
Only Primary	100,515	0.613	0.017	-0.004
			(0.018)	(0.006)
Dropout Secondary Schooling	107,315	0.119	-0.025**	-0.002
			(0.013)	(0.004)
Secondary Schooling	175,808	0.268	0.008	0.005
			(0.014)	(0.004)

Notes: Robust standard errors clustered at school district level are reported in parenthesis. Significance levels: $*$ 0.10 $**$ 0.05 $***$ 0.01. Results refer to cohorts 1930 to 1940. All regressions include rural/urban birth cohort FE, linear county trends and a gender dummy if pooled regression. **Dependent Variables:** *Only Primary* refers to individuals with Folkskola as highest attended school. *Dropout Secondary Schooling* refers to individuals that attended but did not finish lower secondary school (Realskola). *Secondary Schooling* refers to having finished at least lower secondary school (Realskola). **Legend:** (1) Mean Labor Earnings in 1970 for Outcome Group (2) Mean Outcome Variable (3) Reduced Form Baseline Specification.

Column four presents the corresponding results for the compulsory schooling extension. Here there seem to be some net spillover effects at work: secondary schooling completion rates increase by around 1 per cent, and the effect appears to be particularly large for males.

In summary, there seems to be spillover effects on further schooling for both reforms. For the term length extension, the spillover effects are so small that it is unlikely that they are responsible for the comparably large earnings effect. But for the compulsory schooling extension we cannot rule out that the increase in secondary schooling enrollment is responsible for a considerable share of the reform's moderate effects on earnings. As indicated in Table 9, mean earnings were around 60 per cent higher among secondary school graduates. If those who were induced to take secondary schooling by the introduction of the seventh year increased their earnings by the same amount, the overall effect on mean earnings would be slightly below 1 per cent, which is around half of the estimated effect in Table 7.

5.3.2 Employment and Occupation

Despite substantial effects of the term extension on earnings, the effects we observe for other educational outcomes are very modest. This suggests that the labor market experience of the individuals affected by the term extension was different, even conditional on completed education. We now turn to an analysis of labor market outcomes for which we construct a number of dummy variables to capture labor market performance in 1970, when the individuals in our sample were between 30 and 40 years old. Table 10 presents results for employment and occupational group. The left part of the table shows results for the term extension, the right part for compulsory schooling extension.

Starting with employment, we find a striking difference between the two reforms: the term extension associates with an 1.8 p.p. increase in the propensity to work, whereas the estimated effect is close to zero for the compulsory schooling extension. Zooming in on particular occupational groups, it becomes clear that the term extension associates with a large reduction in the probability of working in agriculture – which halves compared to the baseline – and a corresponding increase in the probability to work in accounting/administrative, sales, and transport/communication positions, which experience comparable increases in absolute (1.2-1.4 p.p.) and relative (16-22 %) terms.

Splitting the sample by sex, it is clear that the reduction in employment in agriculture is mainly driven by males. It is also only males who enter an occupation in sales due to the extension. Considering female employment, there is one notable effect on occupational choice: females who were exposed to the term extension increased their probability of being employed in accounting and administrative work by almost 25 per cent compared to the baseline of 12% employment in that group. Among females, the class "Accounting, Administrative" is dominated by three main subgroups: specialized office clerks (38% of the total); correspondence clerks, stenographers and typists (25%); and bookkeepers and cashiers (17%). These are occupations that rely heavily on basic reading, writing and math skills that are taught in primary school. Besides, this occupational group was also the second most well-paid for females (c.f. Schånberg (1993)) – so that the inflow into this group could explain the very large reform effects on 1970 female earnings.

For the compulsory schooling extension the estimated effects are in general much smaller, but the overall pattern is fairly similar: the reform associates with an outflow from agriculture and an inflow into the class "Accounting, Administrative". The outflow from agriculture is again primarily driven by males and the inflow into accounting and administrative work by females. In addition, there is a small but significant increase in the probability of working in a "managerial and professional" occupation – and the effect on this category is similar in size to the previously reported effect on secondary schooling completion.

Table 10. Occupation 1970

	(1)	(2)	(3)	(4)	
	Mean		**OLS YoE**	**Term Length Extension**	**7-Year Extension**
	Earnings	*Outcome*			
A. Males and Females					
Working	214,120	0.817	0.014***	0.018**	0.002
			(0.000)	(0.008)	(0.002)
Managers, Profess.	276,085	0.211	0.083***	0.009	0.006**
			(0.000)	(0.009)	(0.003)
Accounting, Admin.	167,585	0.079	0.002***	0.014**	0.005***
			(0.000)	(0.006)	(0.002)
Sales	195,566	0.075	-0.006***	0.012**	0.001
			(0.000)	(0.005)	(0.002)
Agricultural	129,024	0.055	-0.006***	-0.027***	-0.010***
			(0.000)	(0.007)	(0.002)
Transport, Comm.	199,485	0.055	-0.008***	0.012**	-0.003*
			(0.000)	(0.005)	(0.002)
Crafts	200,154	0.219	-0.043***	-0.004	0.001
			(0.000)	(0.009)	(0.003)
Service	123,525	0.068	-0.006***	-0.001	-0.001
			(0.000)	(0.006)	(0.002)
B. Males					
Working	254,628	0.960	0.003***	-0.001	-0.001
			(0.000)	(0.006)	(0.002)
Managers, Profess.	339,554	0.247	0.086***	0.010	0.008**
			(0.000)	(0.013)	(0.004)
Accounting, Admin.	255,430	0.040	0.001***	0.002	0.003*
			(0.000)	(0.005)	(0.002)
Sales	274,347	0.082	-0.002***	0.014*	0.001
			(0.000)	(0.008)	(0.002)
Agricultural	162,577	0.081	-0.007***	-0.041***	-0.013***
			(0.000)	(0.010)	(0.003)
Transport, Comm.	218,494	0.086	-0.013***	0.014*	-0.004
			(0.000)	(0.009)	(0.003)
Crafts	210,641	0.380	-0.065***	-0.003	0.003
			(0.001)	(0.015)	(0.005)
Service	235,917	0.041	0.003***	-0.001	0.001
			(0.000)	(0.006)	(0.002)
C. Females					
Working	134,476	0.663	0.032***	0.041***	0.007
			(0.001)	(0.015)	(0.004)
Managers, Profess.	178,315	0.172	0.078***	0.003	0.004
			(0.000)	(0.012)	(0.004)
Accounting, Admin.	136,911	0.122	0.004***	0.030***	0.007**
			(0.001)	(0.010)	(0.003)
Sales	95,425	0.069	-0.013***	0.009	0.001
			(0.000)	(0.008)	(0.002)
Agricultural	15,906	0.026	-0.003***	-0.007	-0.006***
			(0.000)	(0.006)	(0.002)
Transport, Comm.	118,070	0.021	-0.001***	0.010**	-0.001
			(0.000)	(0.005)	(0.002)
Crafts	107,198	0.046	-0.011***	-0.005	-0.001
			(0.000)	(0.008)	(0.002)
Service	73,315	0.098	-0.018***	-0.005	-0.003
			(0.000)	(0.010)	(0.003)

Notes: Robust standard errors clustered at school district level are reported in parenthesis. Significance levels: $*$ 0.10 $**$ 0.05 $***$ 0.01. Results refer to cohorts 1930 to 1940. All regressions include rural/urban birth cohort FE, linear county trends and a gender dummy if pooled regression. **Dependent Variables:** Occupational Groups according to Census 1970. **Legend:** (1) Mean Labor Earnings in 1970 for Outcome Group (2) Mean Outcome Variable (3) OLS regression coefficient for years of education (4) Reduced Form Baseline Specification.

Our findings regarding labor market outcomes thus provide further evidence of why the two reforms had such a different impact on earnings: the term extension associated with a large inflow of females into the workforce, and the largest net increase in well-paid occupations. The term extension also associated with a large outflow from agriculture and a corresponding inflow into more well-paid occupations for males, so that the net employment rate remained stable for this group. The results for the compulsory schooling extension suggest a similar development, but the effects are generally smaller in magnitude. Finally, the finding that there was a large increase in female employment in occupations relying heavily on reading, writing and math skills, seems to suggest that the extension of instructional time led to an improvement of basic skills, as previously suggested in the literature (cf. Lavy, 2015; Hansen, 2011; Fitzpatrick, Grissmer and Hastedt, 2011; Agüero and Beleche, 2013).

5.3.3 Migration

The outflow from agriculture and the corresponding inflow into more well-paid occupations for males is also mirrored in migration patterns. We construct indicators for across parish migration, across county migration and migration to an urban location by comparing the place of residence during schooling age to the place of residence in 1970. Overall, the reforms enhanced across parish migration by 2.9 p.p. (term extension) and 1.6 p.p. (7-year extension), and migration to urban locations by roughly equal effect sizes. Especially affected males seem to have moved to urban areas which corresponds well to the shift in occupations noted in the previous section. The effects for females are generally not significant, although roughly of the same size as the ones for males with respect to the term extension.

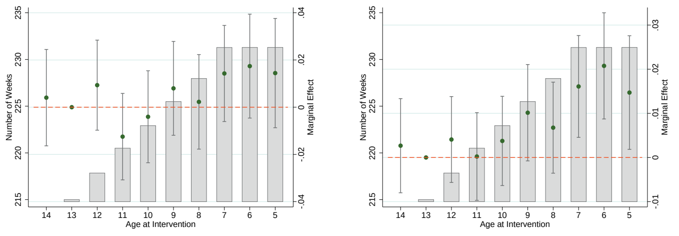
Table 11. Migration

	(1)	(2)	(3)	(4)	
	Mean		**OLS YoE**	**Term Length Extension**	**7-Year Extension**
	Earnings	*Outcome*			
A. Males and Females					
Urban Place of Resid. (1970)	215,781	0.427	0.014***	0.027**	0.012***
			(0.002)	(0.012)	(0.003)
Migration (1970, County)	237,007	0.339	0.056***	0.008	0.007**
			(0.001)	(0.011)	(0.003)
Migration (1970, Parish)	214,960	0.689	0.040***	0.029***	0.016***
			(0.001)	(0.010)	(0.003)
B. Males					
Urban Place of Resid. (1970)	264,701	0.416	0.013***	0.026*	0.019***
			(0.002)	(0.015)	(0.004)
Migration (1970, County)	293,435	0.320	0.057***	0.007	0.009**
			(0.001)	(0.014)	(0.004)
Migration (1970, Parish)	266,307	0.649	0.044***	0.033**	0.023***
			(0.001)	(0.014)	(0.004)
C. Females					
Urban Place of Resid. (1970)	137,118	0.438	0.015***	0.026	0.005
			(0.002)	(0.017)	(0.005)
Migration (1970, County)	144,275	0.359	0.055***	0.008	0.003
			(0.001)	(0.016)	(0.005)
Migration (1970, Parish)	129,248	0.732	0.034***	0.024	0.008*
			(0.001)	(0.015)	(0.004)

Notes: Robust standard errors clustered at school district level are reported in parenthesis. Significance levels: $*$ 0.10 $**$ 0.05 $***$ 0.01. Results refer to cohorts 1930–40. All regressions include rural/urban birth cohort FE, linear county trends and a gender dummy if pooled regression.
Dependent Variables: *Urban Place of Resid. (1970)* is an indicator equal to 1 if an individual lived in a city in 1970. *Migration (1970, County)* is an indicator equal to 1 if an individual moved across county boarders since primary school. *Migration (1970, Parish)* is an indicator equal to 1 if an individual moved across parish boarders since primary school.
Specifications: (1) Mean Labor Earnings in 1970 for Outcome Group (2) Mean Outcome Variable (3) OLS regression coefficient for years of education (4) Reduced Form Baseline Specification.

5.4 Event Study Analysis

Additionally to the main regression results, we show visually that the outcome variables follow the expected trajectory around the reform year. Figure 9 presents event study graphs for the term extension showing how our earnings measures evolve. The estimates suggest that earnings (represented by dots with confidence bands) increase along the increase in instructional time (represented by bars), until they level out at a higher level at the end of the reform window. The proportional increase is more pronounced for pensions where we see an increase of about 2 percentage points at the end of the window.



(a) Log 1970 Earnings

(b) Log Pensions

Figure 9. Event Study Graphs: Term Extension

The figure shows the coefficients and 95% confidence intervals from an event-study regression. The regression includes indicators for the age at the intervention from the introduction of a 39 weeks term. The bar chart shows the associated total length of instruction during the first 6 years in primary school. Being 13 years old at the intervention is the reference category. Results refer to cohorts 1930 to 1940. Robust standard errors are clustered at school district level.

5.5 Heterogeneity

It has been argued that different compositions of compliers are a potential explanation of the low returns to education in Europe compared to the US and Canada (Oreopoulos, 2006). Northern American studies have estimated returns to compulsory education for a small group of drop-outs that may be very prone to be positively affected by extensions of mandatory education. In the current setup, virtually all students were affected by the term extension and 70% of the population were affected by the compulsory schooling extension. Therefore the study design is much closer to compulsory schooling reforms that took place across Europe, such as in Germany (Pischke and Von Wachter, 2008) or the UK (Oreopoulos, 2006; Devereux and Hart, 2010) affecting larger shares of the population than reforms in the US and Canada.[32] Having such broad compliance facilitates the analysis of effect heterogeneity.

We explore heterogeneity with respect to family background. Table 12 reports the effects of the term extension and the compulsory schooling extension on our main educational and labor market outcomes. The leftmost column shows our main estimate, while following columns report how the effect depends on the SES of the household head. For the term extension, there is no evidence supporting a SES gradient in the 1970 earnings effect. The SES gradient in the pension effect appears to be different for

[32] Initially Oreopoulos (2006) found large effects for the UK reform in 1947 which later have been revised by Devereux and Hart (2010) using better data.

males and females, but there is not enough power to draw a definite conclusion. For secondary schooling dropouts, effects are concentrated in lower socioeconomic groups. Conversely, and as expected, the earnings gains from the compulsory schooling extension appear to be concentrated in lower socioeconomic groups. Still, there is no SES gradient in the response to this reform in terms of secondary schooling (even though the increase is much larger in *relative* terms for the low-SES group).

Table 12. Heterogeneity by SES Background

	Males and Females			**Males**			**Females**		
	All (1)	*Low SES* (2)	*High SES* (3)	*All* (1)	*Low SES* (2)	*High SES* (3)	*All* (1)	*Low SES* (2)	*High SES* (3)
DEPENDENT VARIABLE: LOG 1970 EARNINGS									
Term Length	0.048***	0.042**	0.055	0.025*	0.024	-0.002	0.090**	0.083*	0.156
	(0.019)	(0.020)	(0.049)	(0.014)	(0.015)	(0.037)	(0.043)	(0.048)	(0.102)
Compulsory 7-Year	0.020***	0.024***	-0.003	0.015***	0.015***	0.008	0.028**	0.038***	-0.009
	(0.006)	(0.006)	(0.014)	(0.004)	(0.004)	(0.011)	(0.013)	(0.014)	(0.029)
Observations	476,792	375,937	100,855	290,023	230,754	59,269	186,769	145,183	41,586
Districts	2,389	2,386	2,275	2,388	2,386	2,184	2,381	2,380	2,113
Mean Dep. Var.	9.877	9.832	10.046	10.309	10.261	10.493	9.207	9.148	9.409
DEPENDENT VARIABLE: LOG PENSIONS									
Term Length	0.024**	0.025**	0.005	0.010	0.025	-0.054	0.041***	0.029**	0.059*
	(0.010)	(0.011)	(0.029)	(0.016)	(0.017)	(0.050)	(0.013)	(0.014)	(0.035)
Compulsory 7-Year	0.011***	0.016***	-0.008	0.018***	0.018***	0.011	0.006*	0.013***	-0.028***
	(0.003)	(0.003)	(0.008)	(0.004)	(0.005)	(0.012)	(0.004)	(0.004)	(0.010)
Observations	440,291	347,655	92,636	214,378	169,707	44,671	225,913	177,948	47,965
Districts	2,390	2,387	2,250	2,388	2,385	2,112	2,384	2,383	2,123
Mean Dep. Var.	12.040	12.002	12.183	12.223	12.177	12.398	11.867	11.836	11.983
DEPENDENT VARIABLE: SECONDARY SCHOOLING									
Term Length	0.008	0.009	-0.010	0.009	0.017	-0.018	0.008	0.001	0.002
	(0.009)	(0.009)	(0.027)	(0.012)	(0.011)	(0.037)	(0.014)	(0.014)	(0.041)
Compulsory 7-Year	0.008***	0.009***	0.005	0.011***	0.012***	0.009	0.005	0.006	-0.001
	(0.003)	(0.003)	(0.008)	(0.004)	(0.003)	(0.011)	(0.004)	(0.004)	(0.012)
Observations	583,716	462,790	120,926	302,045	240,396	61,649	281,671	222,394	59,277
Districts	2,390	2,387	2,298	2,388	2,386	2,195	2,386	2,385	2,194
Mean Dep. Var.	0.247	0.175	0.523	0.228	0.158	0.497	0.268	0.193	0.551
DEPENDENT VARIABLE: DROPOUTS									
Term Length	-0.033***	-0.038***	-0.006	-0.040***	-0.046***	-0.008	-0.025**	-0.031**	-0.001
	(0.012)	(0.012)	(0.018)	(0.014)	(0.014)	(0.022)	(0.013)	(0.013)	(0.024)
Compulsory 7-Year	-0.003	-0.005	0.005	-0.004	-0.005	0.001	-0.002	-0.005	0.008
	(0.004)	(0.004)	(0.005)	(0.004)	(0.004)	(0.007)	(0.004)	(0.005)	(0.007)
Observations	583,716	462,790	120,926	302,045	240,396	61,649	281,671	222,394	59,277
Districts	2,390	2,387	2,298	2,388	2,386	2,195	2,386	2,385	2,194
Mean Dep. Var.	0.115	0.112	0.125	0.111	0.106	0.130	0.119	0.118	0.120

Notes: Table shows the reduced form effects stratified by household head characteristics. This refers to the father or in absence of a father to the mother. Robust standard errors clustered at school district level are reported in parenthesis. Significance levels: $*$ 0.10 $**$ 0.05 $***$ 0.01. Results refer to cohorts 1930–40. All regressions include sibling FE, a gender dummy, birth cohort FE and birth order effects. **Dependent Variables:** *Log-Earnings* from Tax-Records in 1970. *Log-Pensions* from Tax-Records at age 73. *Secondary Schooling* from Census 1970. *Dropout Secondary Schooling* from Census 1970. **Stratification Variable:** *High and Low Household Indicators* are based on SEI classification of occupation in the 1950 Census.

We conclude that the substantial effects of the term extension on earnings appear to be visible over a wide range of socioeconomic backgrounds, whereas the smaller earning effects associated with the compulsory schooling extension appear to be absent in high-SES groups. Conversely, the smaller effects of the term extension on educational outcomes appear to be concentrated in low-SES groups, whereas we do not detect an SES gradient in the corresponding effects of the compulsory schooling extension. Taken together, these findings are consistent with the interpretation that an increased uptake of post-compulsory schooling is unlikely to be responsible for the term extension effects, whereas they may well be an important part of the effects associated with the compulsory schooling extension.

6 Robustness checks

6.1 Marriage and Survival

Our main focus is labor market returns to schooling, but since marriage market effects and selective mortality may represent confounding factors Table 13 presents results for these outcomes. The term extension seems to associate with a slight increase in marriage rates, driven by females, but the effect is not significant at conventional levels. The compulsory schooling extension associates with an increase in male marriage rates and a reduction in female marriage rates.

Previous work using aggregate data suggests that the compulsory schooling extension was associated with a reduction in mortality (cf. Fischer, Karlsson and Nilsson, 2013). We find some evidence consistent with previous findings, but in general our estimates do not suggest that selective mortality is a major issue.

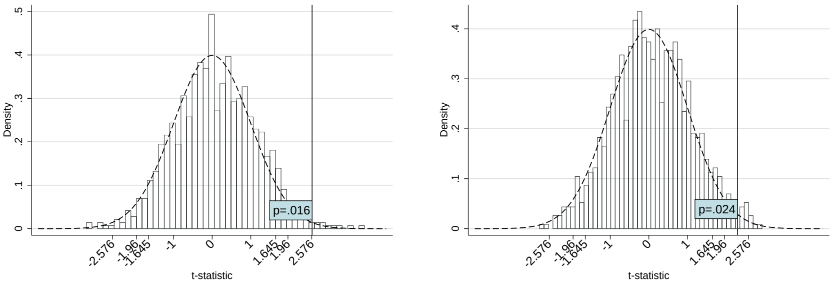
Table 13. Further Outcomes

	(1) Mean Earnings	(2) Mean Outcome	(3) OLS YoE	(4) Term Length Extension	(4) 7-Year Extension
A. MALES AND FEMALES					
Death Prior Census 1970	-	0.016	-	-0.000	-0.000
				(0.003)	(0.001)
Death prior Age 73	-	0.223	-	0.013	-0.001
				(0.009)	(0.003)
Married	209,150	0.861	0.004***	0.005	0.001
			(0.000)	(0.008)	(0.002)
B. MALES					
Death Prior Census 1970	-	0.021	-	-0.001	-0.001
				(0.004)	(0.001)
Death prior Age 73	-	0.273	-	0.016	-0.005
				(0.013)	(0.004)
Married	260,328	0.826	0.013***	-0.003	0.008**
			(0.000)	(0.012)	(0.003)
C. FEMALES					
Death Prior Census 1970	-	0.011	-	-0.000	0.000
				(0.003)	(0.001)
Death prior Age 73	-	0.169	-	0.009	0.003
				(0.011)	(0.004)
Married	115,447	0.899	-0.009***	0.011	-0.007**
			(0.000)	(0.010)	(0.003)

Notes: Robust standard errors clustered at school district level are reported in parenthesis. Significance levels: $*$ 0.10 $**$ 0.05 $***$ 0.01. Results refer to cohorts 1930–40. All regressions include rural/urban birth cohort FE, linear county trends and a gender dummy if pooled regression.
Dependent Variables: Marital Status in Census 1970. *Death Prior Census 1970* is an indicator equal to 1 if an individual died before the Census month in 1970. *Death prior Age 73* is an indicator equal to 1 if an individual died before the age where we measure pensions.
Specifications: (1) Mean Labor Earnings in 1970 for Outcome Group (2) Mean Outcome Variable (3) OLS regression coefficient for years of education (4) Reduced Form Baseline Specification.

6.2 Randomization Inference

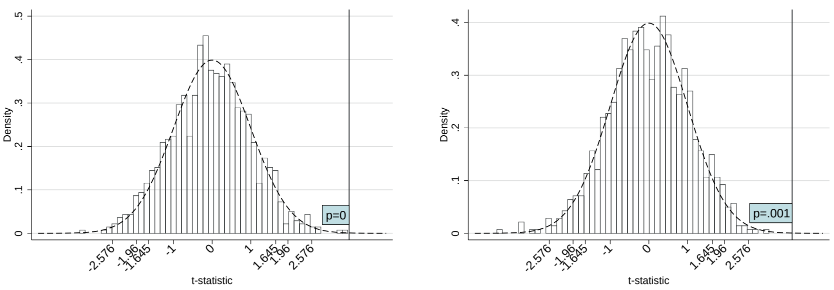
As an alternative basis for inference, we conduct a permutation test where, for each school district, the reform years of the two interventions were randomly drawn from the empirical distribution in 1,000 permutations. Figure 10 shows how our main estimated t statistic, represented by the red line, compares to the distribution of t values arising from the permutation test. For the sake of comparison, we also plot the t distribution which forms the basis of the statistical inference. Interestingly, the two distributions coincide almost perfectly – which implies that the statistical inference we conduct has correct size and power also according to this alternative design. Consequently we also get our main results confirmed: the large effects of the term extension on earnings and pensions are significant ($p < 0.025$) and the effects of the compulsory schooling extension attain even greater statistical significance ($p < 0.001$) effects on earnings and pensions (Figure 11).



(a) Log Earnings (b) Log Pensions

Figure 10. Randomization Inference, Term Length Extension.

Notes: These figures show the distribution of t statistics from 1,000 permutations of reform years. We block randomized on school district level the reform years for extensions of term length and compulsory years of primary schooling. The p value is derived from the permutation test. Results refer to cohorts 1930 to 1940. All regressions include sibling FE, birth cohort FE, a gender dummy, term length in primary school and birth order effects. Robust standard errors are clustered at school district level.
Source: Linked 1970 Census. Own calculations.



(a) Log Earnings (b) Log Pensions

Figure 11. Randomization Inference, Compulsory Schooling Extension

Notes: These figures show the distribution of t statistics from 1,000 permutations of reform years. We block randomized on school district level the reform years for extensions of term length and compulsory years of primary schooling. The p value is derived from the permutation test. Results refer to cohorts 1930 to 1940. All regressions include sibling FE, birth cohort FE, a gender dummy, the treatment indicator for a 7^{th} grade in primary school and birth order effects. Robust standard errors are clustered at school district level.
Source: Linked 1970 Census. Own calculations.

6.3 Alternative specifications

We also test for robustness of our results by controlling for different trends and socio-economic control variables in our baseline specification (which already includes rural/urban birth cohort fixed effects, linear county trends and school district fixed effects). Table 14 presents results for earnings 1970 and Table 15 presents results for pensions at age 73. Overall, our estimates are robust to additional controls and different trends. We also include sibling fixed effects which control for a range of observable and unobservable confounders related to family background. The effects are slightly larger than what we get with the main specification. Since sibling FE specifications may introduce biases via parental investment and birth spacing (which becomes collinear with our term extension variable), we prefer to interpret our main specification as measuring the causal effect of the reforms. Our sibling FE estimates are also less precisely estimated, which is unsurprising given that we lose many observations when performing the sibling analysis.

Table 14. Main Results: Log 1970 Earnings

	(1) Base	(2)	(3)	(4)	(5)	(6) Sibling FE
A. Males and Females						
Term Length	0.048***	0.048***	0.043**	0.045**	0.050***	0.102***
	(0.019)	(0.019)	(0.019)	(0.019)	(0.019)	(0.033)
Compulsory 7-Year	0.020***	0.020***	0.020***	0.019***	0.021***	0.012
	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)	(0.009)
A. Males						
Term Length	0.025*	0.025*	0.026*	0.025*	0.028**	0.029
	(0.014)	(0.014)	(0.014)	(0.014)	(0.014)	(0.027)
Compulsory 7-Year	0.015***	0.015***	0.014***	0.016***	0.015***	0.010
	(0.004)	(0.004)	(0.005)	(0.005)	(0.004)	(0.008)
B. Females						
Term Length	0.090**	0.090**	0.076*	0.084*	0.094**	0.102
	(0.043)	(0.043)	(0.044)	(0.045)	(0.043)	(0.108)
Compulsory 7-Year	0.028**	0.028**	0.030**	0.024*	0.030**	0.016
	(0.013)	(0.013)	(0.013)	(0.014)	(0.013)	(0.032)
Quadratic County Trends		✓				
Np. County Trends			✓			
Fully Interacted Trends				✓		
Household Control Variables					✓	
Sibling FE						✓

Notes: Table shows reduced form effects on labor earnings. Robust standard errors clustered at school district level are reported in parenthesis. Significance levels: $*$ 0.10 $**$ 0.05 $***$ 0.01. Results refer to cohorts 1930 to 1940. All regressions include rural/urban birth cohort FE, linear county trends and a gender dummy if pooled regression.
Dependent Variables: *Log-Earnings* from Tax-Records in 1970.
Specifications: (1) Baseline; (2) quadratic county trends; (3) non-parametric county trends; (4) fully interacted rural/urban/county birth cohort trend; (5) Adding socio-economic control variables of household; (6) Sibling FE.
Source: Linked 1970 Census. Own calculations.

Table 15. Main Results: Log Pensions (Age 73)

	(1) Base	(2)	(3)	(4)	(5)	(6) Sibling FE
A. Males and Females						
Term Length	0.024**	0.024**	0.021**	0.020*	0.024**	0.064***
	(0.010)	(0.010)	(0.011)	(0.011)	(0.010)	(0.018)
Compulsory 7-Year	0.011***	0.011***	0.012***	0.012***	0.012***	-0.001
	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.005)
A. Males						
Term Length	0.010	0.010	0.012	0.010	0.013	0.039
	(0.016)	(0.016)	(0.017)	(0.017)	(0.016)	(0.034)
Compulsory 7-Year	0.018***	0.018***	0.017***	0.019***	0.019***	0.003
	(0.004)	(0.004)	(0.005)	(0.005)	(0.004)	(0.009)
B. Females						
Term Length	0.041***	0.041***	0.032**	0.033**	0.040***	0.034
	(0.013)	(0.013)	(0.014)	(0.014)	(0.013)	(0.030)
Compulsory 7-Year	0.006*	0.007*	0.008**	0.007*	0.007*	0.009
	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)	(0.008)
Quadratic County Trends		✓				
Np. County Trends			✓			
Fully Interacted Trends				✓		
Household Control Variables					✓	
Sibling FE						✓

Notes: Table shows reduced form effects on labor earnings. Robust standard errors clustered at school district level are reported in parenthesis. Significance levels: $*$ 0.10 $**$ 0.05 $***$ 0.01. Results refer to cohorts 1930 to 1940. All regressions include rural/urban birth cohort FE, linear county trends and a gender dummy if pooled regression.
Dependent Variables: *Log-Pensions* from Tax-Records at age 73.
Specifications: (1) Baseline; (2) quadratic county trends; (3) non-parametric county trends; (4) fully interacted rural/urban/county birth cohort trend; (5) Adding socio-economic control variables of household; (6) Sibling FE.
Source: Linked 1970 Census. Own calculations.

7 Conclusion

Policy makers and education reformers seem to agree that more instructional time is a key input to improve human capital. Still, we have very limited knowledge on the long-term effects of term length and how the effects of longer terms compare to alternative extensions. This is the first article that documents significantly positive effects of longer terms in primary school on adult earnings and pensions using a credible identification strategy. We can also examine how term extension effects compare with the effects of a parallel compulsory schooling reform, which increased education by the same amount but at different margins. We use rich labor market information from administrative registers on the universe of individuals born 1930-1940 and affected by the independent reforms in working and pension age.

Our analysis delivers two important results: (i) The compulsory schooling reform which increased primary education from 6 to 7 years was largely ineffective regarding later life returns, while extending the average term length from 34.5/36.5 to 39 weeks had sizeable effects on earnings – at least half as high as the OLS returns – and female employment. (ii) The term extension affected broad parts of the population, and even though it had some spillover effects on post-compulsory education most of the estimated effect seems to reflect labor market returns to basic skills taught in primary school.

Overall, our results are in line with the recent literature arguing that there are only minor or zero returns to an extra year of education at the end of compulsory schooling. This finding and the fact that classroom time in Sweden in the 1940s was similar to what students experience today indicate that the Swedish system was not exceptional and that our findings on term-length are of relevance for today's educational systems. Our results also indicate that it is fruitful to address children's cognitive development through more instructional time at younger ages. While the compulsory schooling policy affected students at the age of 13, the term length extension was introduced already in first grade. In contrast to previous studies we show that the age of exposure to additional education plays a significant role for skill formation and returns in later life. Pischke and Von Wachter (2008) estimate zero returns to education in Germany and interpret this as a consequence of that basic skills are already fixed in grade 8 which was the year of schooling when the German reform was introduced. The extension of one more year of academic training left students basic skills unaffected. Our results suggest that skill formation is crucial at an earlier point in time and that term extensions seem to represent a quite effective instrument for today's policy makers to improve human capital.

While several studies already explore the health effects of compulsory schooling reforms (Clark and Royer, 2013; Gathmann, Jürges and Reinhold, 2015; Kemptner, Jürges and Reinhold, 2011; Albouy and Lequien, 2009) future research could focus on the health effects of term-extensions. Evidence on this topic is scarce; one exception being Parinduri (2017) who does not find any evidence of improved health. Since findings of compulsory schooling reforms are ambiguous, research on term-length extensions as another source of exogenous variation in education could shed further light on the health education nexus.

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For Online Publication

A The Swedish Labor Market

A.1 Measuring Returns to Education

To estimate the returns to education of the term extension and the compulsory schooling extension, respectively, we use annual labor earnings in 1970 and annual pensions as dependent variables. In order to assess the reliability of these income measures we compare our estimates to standard Mincer wage regressions based on information on hourly wages from the LNU 1968. Survey data clearly reduce the sample size, but wages capture productivity differentials better than annual earnings as they are less prone to differences in labor supply. Table 16 compares OLS estimates for the returns to education with hourly wage regressions as the benchmark.

Table 16. Mincer Wage Regression

	ALL	MALES	FEMALES
Log-Wages (LNU)	0.065***	0.064***	0.066***
	(0.004)	(0.004)	(0.010)
Observations	615	396	219
Log-Earnings (1970)	0.099***	0.074***	0.141***
	(0.001)	(0.000)	(0.002)
Observations	384,139	234,228	149,911
Log-Pensions (Age 73)	0.068***	0.068***	0.069***
	(0.000)	(0.000)	(0.000)
Observations	356,199	173,625	182,574

Notes: Table shows the OLS returns to years of education on log-wages, labor earnings and pensions at age 73. Robust standard errors are reported in parenthesis. Significance levels: $*$ 0.10 $**$ 0.05 $***$ 0.01.
Results refer to cohorts 1930 to 1940. All regressions include birth cohort FE. The pooled estimates also includes a gender dummy.
DEPENDENT VARIABLES: *Log-Wages* are (self-reported) hourly wages from the LNU survey. *Log-Earnings* from Tax-Records in 1970. *Log-Pensions* from Tax-Records at age 73.
Source: LNU 1968, Linked 1970 Census. Own calculations.

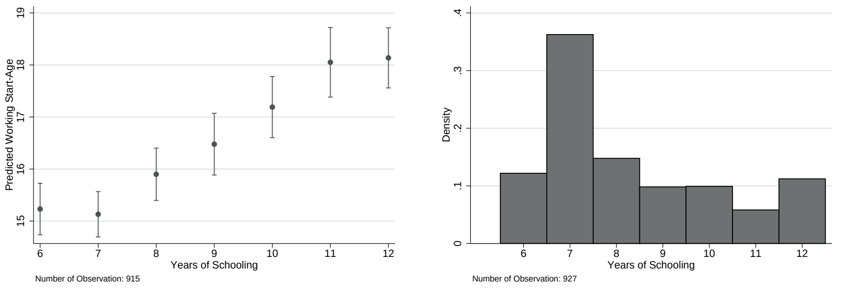
Table 16 suggests our earnings measure leads to very reasonable estimates regarding the returns to education in a Mincerian framework for males. This is not very surprising as it has been documented that labor earnings when men are in their 30s constitute an excellent proxy for their life-cycle earnings. For women estimates for the earnings in 1970 are overestimated due to effects on labor supply. As shown by Böhlmark and Lindquist (2006) the life-cycle bias in earnings estimates is largest for women around their late 30s. For men full-time employment in 1970 was close to 100%, while only 66 % of all women worked full-time. Assuringly the pension estimates for both men and women are extremely close to the estimates based on the LNU survey. Given that pensions were based on the *best* 15 income years it is reasonable that labor supply frictions are minimized.

A.2 Labor Market Entrance

In addition to Figure 4 we also informally test whether an additional school year from 6 to 7 shifts labor market entrance by running a regression of working starting age, wsa_i, on years of schooling:

$$wsa_i = \beta_0 + \sum_{s=6}^{12} \gamma_s I(Schooling = s) + \beta_{\boldsymbol{X}} \boldsymbol{X} + u_i. \quad (2)$$

In the regression we also control for any education beyond any schooling. Figure 12(a) plots the predicted values from this regression. There is no significant difference at labor market entrance between having 6 or 7 years of schooling. This suggests that the opportunities for adolescents between the ages 12 to 14 to enter the formal labor market were low in the 1930s and 1940s.



(a) (b)

Figure 12. Regression Years in School on Working Start Age: (**a**) Predicted WSA; (**b**) Distribution Years of Schooling.

Notes: Figure 12(a) shows predicted values from a regression of Working Start Age (WSA) on years of schooling, birth cohort and years of post-schooling. Post-schooling (e.g. vocational training, university...) is set to zero for predicted values. Figure 12(b) shows the distribution of years of schooling.
Results refer to cohorts 1930-1940.
Source: LNU Survey, 1981. Own calculations.

A.3 Skills, Tasks and Occupation

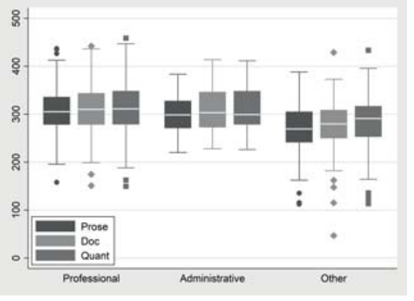
Table 17 provides the information of Table 1 in section 2.3 but for males and females separately.

Table 17. Tasks for Occupational Groups

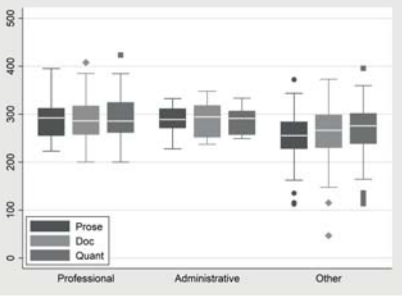
	(1) Mean	(2) Share	(3)	(4)	(5)	(6) Occupational Tasks	(7)	(8)	(9) Grades
	Earnings	*Occ. Group*	*Sec. Educ.*	*Nonr. Manual*	*Routine Manual*	*Nonr. Cog. Interactive*	*Routine Cog.*	*Nonr. Cog. Analytic*	*GPA*
B. MALES									
All	248,787	0.96	0.23	1.750	3.966	2.034	5.466	3.940	-0.140
				(1.388)	(1.079)	(2.660)	(3.793)	(1.990)	(0.769)
Managers, Profess.	339,549	0.25	0.59	1.022	4.429	3.749	4.753	6.568	0.256
Accounting, Admin.	255,421	0.04	0.39	0.148	4.542	0.657	7.797	3.394	0.154
Sales	274,346	0.08	0.30	0.437	3.161	3.823	1.168	4.756	0.008
Agricultural	162,567	0.08	0.07	2.452	2.769	5.763	1.526	3.684	-0.256
Transport, Comm.	218,505	0.09	0.08	3.723	2.897	1.003	1.479	2.079	-0.271
Crafts	210,639	0.38	0.03	1.970	4.353	0.263	8.531	2.843	-0.354
Service	235,917	0.04	0.26	2.263	3.016	1.430	1.761	2.097	-0.152
No Occupation	-	0.04	0.22	-	-	-	-	-	-0.298
C. FEMALES									
All	124,504	0.55	0.31	1.120	4.019	1.168	3.858	3.084	0.199
				(1.388)	(1.079)	(2.660)	(3.793)	(1.990)	(0.769)
Managers, Profess.	178,301	0.17	0.57	1.782	4.065	2.136	2.424	3.990	0.367
Accounting, Admin.	136,910	0.12	0.39	0.114	4.926	0.612	7.797	3.179	0.370
Sales	95,421	0.07	0.12	0.561	3.470	1.169	0.818	3.993	0.161
Agricultural	15,910	0.03	0.09	2.190	3.416	0.944	4.336	1.683	0.208
Transport, Comm.	118,086	0.02	0.25	0.611	4.380	0.712	4.464	2.171	0.232
Crafts	107,187	0.05	0.04	1.532	4.683	0.188	8.593	2.228	-0.093
Service	73,306	0.10	0.07	1.246	2.963	0.798	1.095	1.728	-0.041
No Occupation	-	0.45	0.26	-	-	-	-	-	0.075

Notes: Descriptive Statistics for Tasks. **Columns:** (1) Mean Labor Earnings in 1970 for Occupational Group (2) Share (3) Share with Secondary Education Within Occupational Group (4)-(8) Average Tasks for Occupational Group (9) GPA in Primary School. *Source:* Linked 1970 Census. Own calculations.

The skill profile of occupational groups in terms of primary school GPA which is apparent in Table 1 and Table 17 is also found for adult literacy scores. Figure 13 plots the distribution of scores in the three dimensions *prose literacy*, *document literacy* and *quantitative literacy* from the IALS survey. Due to the low number of observations, we group respondents into three main groups: the two occupations with the highest GPAs in Table 17 ("Professional and Managerial" and "Accounting, Administration" respectively) and a third group consisting of all other occupations. The first two groups have consistently higher literacy scores in all three dimensions, and this difference remains after excluding individuals with secondary education.



(a) All Working Individuals



(b) Working Individuals, No Secondary Education

Figure 13. Adult Literacy Scores by Occupation and Education.

The graphs show the distribution of literacy scores of working individuals older than 50 in three dimensions according to the IALS 1994 study. Each score dimension ranges from 0 to 500. The 'Professional' and 'Administrative' groups correspond to the two top groups of Table 17. The third group ('Other') includes all other working individuals.

B Migration, Hospital Births and Reform Assignment

This section evaluates migration patterns and reform assignment based on the place of birth. For cohorts born in the 1920s and early 1930s the dominant mode of delivery was home births, while the norm from the mid 1930s was to give birth in an institution. As mentioned in Section 3.2 the location of the hospital, and not the place of residence of the parents, was recorded as the place of birth in Swedish register data until 1947. Figure 14 illustrates the effects arising from a hospital opening on the recorded births for the city of Motala as an example. After the opening of the hospital in 1927 the recorded births in the city grew rapidly while the parishes around the city recorded fewer newborns. Children from parents living around the city, but who were born in the newly established hospital were now recorded as being born in Motala. From 1947 onwards the place of residence of the parents is registered as place of birth and the number of births in the city sharply decreased.

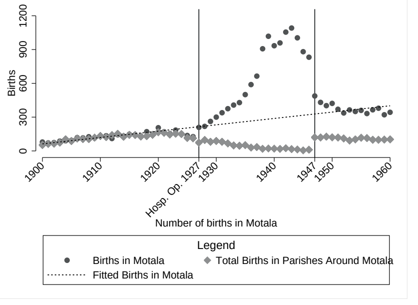


Figure 14. Recorded Births: Motala

Births around Motala refer to parishes with a direct boarder to the city of Motala. While there is an overall urbanization trend most of the excess births in Motala stem from the specific coding prior to 1947.

Whether the place of birth (PoB) is a sufficiently good proxy for the place of residence during adolescence depends on the actual information recorded as place of birth but also on migration patterns. Figure 15 shows the share of individuals in the 1950 Census for each year of birth where the *place of birth* coincides with the *place of residence in 1950*. In order to capture the effect from the hospital coding we add information from openings of birth centres across Sweden in the period of interest and split the population by the presence of a birth center in the parish of birth. For parishes without a hospital, deviations stem mostly from migration. These occur mainly during the first years of life and in the age 15-30. For children during schooling age and for older adults migration appears to be a seldom phenomena as their parents unlikely move in this period of life.

The gap between parishes with and without a hospital at the year of birth is a direct result of the recording of the place of birth prior to 1947. Figure 15 suggests that in the absence of a hospital the parish of birth coincides with the parish during schooling age in approximately 70% of all cases.

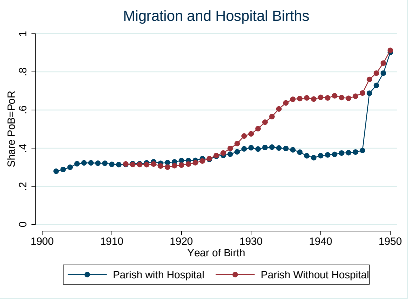


Figure 15. Hospital Births vs. Migration

Source: 1950 Swedish Census. The dotted lines present the share of individuals where the parish of birth (PoB) as coded in the Swedish Registers equals the place of residence (PoR) in 1950 differentiated by the presence of a hospital with a birthing center in the year of birth.

C Primary Education and Compulsory Schooling in Sweden

Sweden has a long-lasting tradition of compulsory education. Already in 1842, church parishes[33] were obliged to offer schooling by an approved teacher and children in *school age* had to attend the local primary schools (*Folkskola*) (Fredriksson, 1971).[34] School age was defined as the years during which children had to fulfil their compulsory curriculum. After several changes during the 19th century, the school age was finally set to the year children turned seven and lasted until the year they turned fourteen in 1897.

Still the school age neither explicitly determined the compulsory years of schooling, nor the total amount instructional time within a given school year in the late 19th century. Different local regulations with respect to term length and various options to fulfil the compulsory curriculum[35] implied great variation in the actual time spent in school by children in school age across Sweden. By the late 1860s the majority of

[33]Until the mid twentieth century the church was responsible for the Swedish primary school (*Folkskola*) and school-districts coincided almost exclusively with parishes.

[34]Long before compulsory schooling was introduced by law a large fraction of the population had basic reading and writing skills and many parishes offered schooling on voluntary basis. The main explanation to the high literacy rates was that local clerks regularly tested household members on their knowledge in Christianity and catechism (Paulsson, 1946)

[35]Term length was not fixed to a certain amount of weeks and especially rural districts initially offered half–time reading (children only attended primary school every second week day or only half of a year). By the end of the nineteenth century children could also leave *Folkskola* after an exit examination or due to poverty.

students received at least *some* amount of formal schooling (Sandberg, 1979), but the length and quality of instruction differed remarkably between regions.

The geographical variation was greatly reduced by the 1919 restructuring of the educational guidelines. The national government issued directives in so-called *normalplaner* (normal plans) stating the content of education, but initially these plans were only advisory. In 1919 the so-called *Utbildningsplanen* (the education plan) was introduced.[36] For the first time the number of compulsory years of schooling were fixed to six years by setting the minimum school leaving age to the year a student turned 13 on a nationwide level. Primary schooling was free of charge and attendance was compulsory until a student had completed the highest grade of *Folkskola* in the school district he/she was registered as a resident.[37] Parents were responsible for that their children fulfilled the compulsory school attendance and had to report to the school district board that they had a child in school age. §51 of the "Royal Decree of the Folkskola" of 1930 states that parents were legally obliged to send their children to school and that parents that did not send their children to school could get penalty payments and even lose custody.[38] The legal, administrative and pedagogical control of the school district was handled by regional school inspectors appointed by the Ministry of Ecclesiastical affairs.[39] The county governments were the highest legal instance responsible for the enforcement of regulations related to compulsory school attendance.

Following the new guidelines the 1920s were characterized by an intensive reform debate about the need for further extensions of compulsory education and the need for greater equality of opportunity within the Swedish schooling system. In 1920 a clause was introduced in the primary school code[40] that a seventh school year *could* be made compulsory in a school district (Fredriksson, 1950). However, only very few districts

[36] *Utbildningsplanen* was a governing document and included time-tables and syllabuses for compulsory education (Lindmark, 2009) and remained intact until the 1950s.

[37] Sweden has a very long tradition of national registration. Since the early 15th century until 1991 the church handled the registration of all individuals and households in a parish.

[38] The yearbook of the Supreme Administrative Court report precedents from 1935 related to this paragraph.

[39] The first school inspectors were appointed already in 1861. Their duties were to visit each school district in the inspector area on a yearly basis and to inform and make sure that the intentions and decisions made at the central level were implemented. In 1930 the school districts were divided into 52 inspection areas (Paulsson, 1946).

[40] Paragraph 47 mom. 4.

introduced a mandatory seventh year and still *Folkskola* generally covered six years.[41]

With respect to equality of opportunity, the Swedish school system in the early 20th century was very selective. Children were tracked into separate schools based on their academic achievements after spending the first four years in *Folkskola*. High achievers could follow the academic track at the 5-Year *Realskola* (junior secondary education) which could lead to *Gymnasium* (upper secondary education) and University. The remaining students stayed in *Folkskola* for at least two more years, when there was another option to enter lower secondary education. Students not enrolling to secondary education took low intensity courses six weeks per year in continuation schools for two more years after compulsory schooling. Figure 1 in the main text gives a stylized presentation of the various school types and continuation options following the basic primary education.[42]

The majority of students in the 1920s till the 1940s only completed compulsory schooling and compared to e.g. the US where more than 70 per cent enrolled in high school by 1940, relatively few students continued to secondary schooling. In 1930, less than two per cent of the adult population had upper secondary education or more (Björklund et al., 2004) and in 1940, only ten per cent of the cohort graduating from *Folkskolan* continued to junior secondary education and only five per cent of a cohort continued to upper secondary (Fredriksson, 1971). Still there was a growing demand for higher education and secondary schools became more widely spread geographically (Murray, 1988). In 1952 the share of children continuing to junior secondary education reached 38 per cent (Lindensjö, Lundgren et al., 1986).

[41] The most southern Swedish region Scania and some larger cities were early birds and introduced a compulsory seventh grade during the 1920s. Furthermore, several cities had a so-called *högre avdelning* (higher divisions of Folkskola) which covered up to three additional years after grade 6. The *högre avdelning* was however never mandatory.

[42] In a massive educational reform in the 1950s and 1960s the old primary and lower secondary school system was replaced by a high-school type comprehensive school system. The various old school types were then abolished in favour of a single 9-year comprehensive school (*Grundskola*). The reform reshaped the entire school system and included among other things a compulsory schooling extension to nine-years, the abolishment of tracking and a regional integration. In contrast to the reforms taking part during first part of the twentieth century, this reform is the subject of a number studies on the impacts of education and very well documented, see e.g. Meghir and Palme (2005); Lundborg, Nilsson and Rooth (2014); Hjalmarsson, Holmlund and Lindquist (2014); Lager and Torssander (2012). Interestingly it often went unnoticed that the comprehensive school reform only constituted the second part of an ongoing reform process of the Swedish primary and lower secondary school system which eventually culminated in the establishment of the new type of school. In fact, Sweden exhibited a continuous roll-out of extending the mandatory amount of schooling from 6 to 9 years over a period of 40 years.

C.1 A Seventh Year becomes Compulsory

Motivated by the discussion in the 1920s and the relatively low level of mandatory education compared to other European countries,[43] the national government decided that seven-year schooling should be made compulsory in 1936.[44] The law gained legal validity on July 1 the same year, and the decision to extend compulsory schooling by an extra year was taken by the school board of the school district. The reform was not implemented at the same time in all school districts. Rather it was stipulated that a seventh year had to be implemented all across the country before the school year 1948/49. The compulsory seventh year was consequently introduced during a twelve-year transition period.

The main motive for the reform was that six years was considered too short for achieving the learning objectives that were stated for the *Folkskola*.[45] In line with this motive, the reform did not come with any fundamental changes with respect to learning goals or curricula, but instead emphasized the goal of achieving more long-lasting results of schooling. The recommendation from the central administration of the Ministry for Ecclesiastical Affairs school districts should distribute the pre-reform compulsory school curricula over seven years instead of six (Ecklesiastikdepartementet, 1935*a*).[46]

[43]In the debate preceding the introduction of the seventh year, politicians often benchmarked with other Western countries, emphasizing that the number of school years was the most striking difference of compulsory education in Sweden compared with Denmark, Norway, Germany and Great Britain. This discourse was also brought forward in the contemporary educational literature, e.g. in the *Svensk Läraretidning* (the teachers' journal). According to (Ståfelt, 1930, p.823) Sweden is *lagging* behind with respect to the length of compulsory schooling compared to other countries in Europe.

[44]School districts were also allowed to introduce an eighth year of compulsory schooling if their application was accepted by the king. In 1940 0.1% of all schools in the country offered eight years of education (Fredriksson, 1950), but since these schools generally were located in urban areas there were quite some students taking eight years of compulsory schooling (Statistics Sweden, 1974).

[45]*The strengthening of teaching of the most important topics of Folkskolan, that according to the experts are utterly necessarily, can likley not be achieved in any other way than through an extension of the length of the novitiate.* Own translation of: *Den förstärkning av undervisningen i folkskolans viktigaste färdighetsämnen, som enligt de sakkunnigas mening är behövlig, torde icke kunna vinnas annorlunda än genom en utsträckning av lärotidens längd.* (Ecklesiastikdepartementet, 1935*a*)(p.49) and *Apparently the time in school has been too short for the children that leave Folkskolan at age 12–13 after only six years of education to get the amount of training and repetition that is needed to gain lasting skills.* Own translation of: *För de barn, som vid 12–13 års ålder lämna folkskolan efter endast sex års undervisning, har skoltiden uppenbarligen varit för kort för att lämna tillräckligt utrymme åt den övning, den innötning, förutan vilken bestående färdighet i berörda ämnen icke kan vinnas.* (Ecklesiastikdepartementet, 1935*a*) (p.54)

[46]*For the seventh year to fulfill its aim to generate a more thorough and deeper knowledge and understanding there is at this time no need for any extra curriculum in addition to the ones that are provided by the 1919 education plan.* Own translation of: *För vinnande av syftet med det sjunde skolåret ett grundligare och mera fördjupat inhämtande av folkskolans lärokurs torde några kursplaner*

Due to the soft transition rules, the compulsory schooling reform did not cause any major difficulties in the school districts. The implementation was also facilitated by the fact that the responsibility for funding of school buildings, teaching materials and teachers' salaries was the responsibility of the central government and not the school districts (Larsson, 2011). Moreover, since the main idea was to distribute the same courses given in six years over seven, there was no need to produce or distribute new teaching material.[47] The reform could also be implemented without any organizational difficulties since there were an oversupply of teachers in the 1930's and 1940's (Fredriksson, 1971).[48]

C.2 Extensions of the Term Length

With the 1919 Education plan there was also a harmonization with respect to the length of a school year across school districts (Paulsson, 1946). The yearly reading time was divided into an autumn and a spring semester, with the academic year starting in autumn. More or less parallel with the compulsory schooling extension, the government decided to extend the term length in *Folkskolan.* In 1937 the regular term length was approximately eight months, or minimum 34.5 weeks. The number of weekly teaching hours was recommended to 30 and could not exceed 36, and the school day could not exceed six hours (Paulsson, 1946). In conjunction with a wage reform for teachers in the *Folkskola*, the parliament of 1937 decided that the school year should be 34.5 (207 days), 36.5 (219 days) *or* 39 weeks (234 days) long, and that different wages would be paid for the different term lengths, all covered by the national government. The main motivation for the term extension was once again that Sweden was lagging behind other Western European countries and that the school year was to short to accomplish learning objectives. Following this policy, several school districts

för den sjuåriga folkskolan utöver dem, som äro upptagna i 1919 års undervisningsplan, tillsvidare icke vara erforderliga. (p.130)

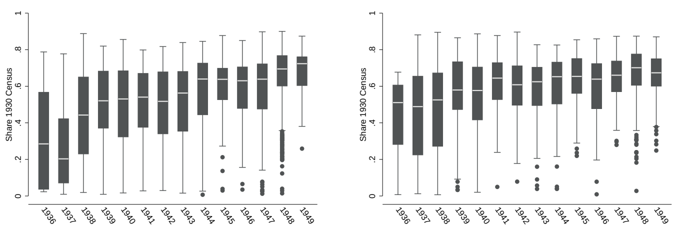
[47] The regional school inspectors stated what study materials and books could be used in *Folkskolan* (Ecklesiastikdepartementet, 1935*b*) and from 1938 the national government had an official approval scheme for examining books before they could be used as textbooks in Swedish schools (Johnsson Harrie, 2009).

[48] Following the oversupply of teachers the Ministry of Ecclasticial Affairs cut the intake to the teachers colleges in the early 1940's. The situation changed in the 1950's, but it was not until the early 1960's and the implementation of the comprehensive school system that that there was a real teacher shortage (Fredriksson, 1971). The good supply of qualified teachers is confirmed by statistics on the number of unqualified teachers in service across the period of interest. For example only 808 out of 33406 teachers working in Folkskolan did not have an appropriate teacher degree in 1955.

prolonged their school year. In 1938, about 400 districts switched to 39 weeks, and about 200 to 36.5 weeks (Örtendal, 1938). Similar to the compulsory schooling reform, the term extension did not come with any curricula changes.

In 1939, the Swedish Parliament decided to further raise the minimum school year duration to 36.5 weeks, thus removing the shortest option of 34.5 weeks. Because of the Second World War and the national savings programme that followed, a transition time was given for the implementation of this reform. The school districts providing 34 4/7 weeks were given the opportunity to wait until 1941/42 to choose either one of the longer school year durations (Weijne, 1942). Eventually, in 1953, 39 weeks was implemented as the standard term length across Sweden (Statistics Sweden, 1974).[49]

The box plots in Figures 16(a) and 16(b) show that both reforms were implemented later in school districts with high shares of employment in agriculture. This pattern is especially pronounced for the compulsory school extension. On small farms children at the age of 13 were generally a valuable source of labor which can explain a greater reluctance to implement the compulsory school extension in school districts in rural regions.



(a) Introduction of the 7$^{\text{th}}$ Grade. (b) Term Length Extension

Figure 16. Local Share Working in Agriculture in 1930 by Implementation Year

The figure shows box plots for the share of the adult population working in agriculture by year of implementation of the two extensions, respectively. The box gives the 25–75 quantile range, separated by the median. Upper and lower adjacent values add 1.5 times interquartile distance to the nearer quartile.

[49]Sweden was neutral in the Second World War and there are no sources that point to that schools were closed or that schooling was disrupted due to the war. As noted in Bhalotra et al. (2016) who use data from exam catalogues from children in grade 1 and grade 4 of *Folkskolan* between 1937–1947, there is no structural break in the number of catalogues or children during the time period. See also Fredriksson (1971) on that compulsory schools were not very affected by the war. A parliamentary decree on November 3, 1939 stated that the regional school inspectors, in the event of war or danger of war, could allow deviations from regulations concerning instruction time. The school inspector however also had to decide when the missed instruction time should be fulfilled.

C.3 Comparative International Contexts

The current literature on compulsory schooling reforms and their returns to labor market achievements tends to agree that context matters – high or low returns to compulsory schooling reforms depend on a large set of institutional parameters such as labor market structure, the educational system in general, wage distributions, etc. In order to enhance the understanding of how extensions of mandatory schooling may affect later-life outcomes it appears fruitful to compare the Swedish setting with similar developments in other countries.

The German educational system which implemented a compulsory school reform in the middle of the twentieth century that extending the basic track by one more year from 8 to 9 years of mandatory education constitutes a first reference(Pischke and Von Wachter, 2008). The Swedish school system before 1950 shared large similarities with the German educational system.[50] The German school system was (and still is) highly selective with tracking students relatively early after 4 or 6 years into three types of secondary schools based on performance. The compulsory schooling extension in Sweden and Germany was also extremely similar in spirit.[51] Both extended the lowest track by one more year and both reforms were relatively *pure* extensions in the sense that they only affected the years of education and did not reshape the entire school system. Pischke and Von Wachter (2008) find robust results of at best very low returns to the compulsory schooling reform in Germany. They explain this by that the initial stock of skills was already comparably high even in the lowest schooling track in Germany at the time of the reform implementation.

The pure extension effect is naturally connected to the absence of direct degree effects induced by the reforms in *Folkskola*. Related to this Grenet (2013) contrasts two compulsory schooling reforms in France (1967) and England and Wales (1972) and finds that only the latter increased wages due to the reform. These differences are assigned to a sharp drop in students leaving school without qualification in England and Wales. This rise in credentials appeared to not solely have a signalling effect on

[50] We here refer to the West German school system. Interestingly it appears not to be a coincidence, that the decision in 1948 to replace the educational system in Sweden with a system similar to the one of East Germany – with a comprehensive element placing strong emphasis on e.g. democratic values and educating citizens – falls exactly into the years after the Second World War.

[51] Before a name change to the current *Hauptschule* the German equivalent was called *Volksschule* which is the literal translation of *Folkskola*.

earnings, but rather increased the skill levels of school-leavers. For various institutional reasons the comparable French reform did not alter this margin.

D Data Source

Figure 17. Exam catalogue from Archive