The Long-Term Effects of Long Terms Compulsory Schooling Reforms in Sweden

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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 years of 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 sizeable 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 and the term extension reduced the gender gap in employment and earnings. (JEL: J24, J31, I28)

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 US states have increased the number of days of teaching to a minimum of 190 days (New York Times, 2012). Across Europe the number of school days in primary education varied between 162 and 200 days in 2016/2017, and many

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education reformers agree that more instructional time is a key step to improve educational outcomes (EACEA, 2017).

This paper evaluates the long-run effects of a term-length extension and compares the effects with a parallel compulsory schooling extension. Despite the ongoing discussion among educational policy makers, 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 very short school years did not significantly impact the affected cohorts' income or employment. A more recent study by using survey data from Indonesia comes to similar conclusions (Parinduri, 2014). Both 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 contributes 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 Harmon and Walker (1995) following Angrist and Krueger (1991) or Oreopoulos (2006), generally reported large *causal* effects of schooling on earnings based on instrumental variable estimates.[2] But more recent studies suggest smaller and often negligible impacts of additional schooling on economic outcomes and 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; Grenet, 2013; Chib and Jacobi, 2016). Also the large causal estimates for education and labor

1. In the economics literature term length has sometimes been considered a proxy variable for school quality (Card and Krueger, 1992; Betts, 1995). 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 et al. (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.

3. Recent contributions on the effects of education on health include Meghir et al. (2018); Lager and Torssander (2012); Mazumder (2012); Brunello et al. (2013); Clark and Royer (2013); Gathmann et al. (2015); Galama et al. (2018). Other outcomes considered in the literature are cognitive abilities (Schneeweis et al., 2014; Crespo et al., 2014), fertility (Cygan-Rehm and Maeder, 2013; Fort et al., 2016), intergenerational transmission (Chevalier et al., 2013; Piopiunik, 2014; Lundborg et al., 2014; Güneş, 2015), and crime (Hjalmarsson et al., 2015).

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 more recent findings of limited effects of compulsory schooling extensions seem to suggest that expanding compulsory instruction by term extensions would be a quite ineffective 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 (Cunha and Heckman, 2007; Tominey, 2010), 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, but with important heterogeneity regarding effect sizes and students and subjects affected (Sims, 2008; Bellei, 2009; Fitzpatrick et al., 2011; Agüero and Beleche, 2013; Lavy, 2015; Huebener et al., 2017).[5] In fact, one potential explanation of why compulsory schooling reforms are ineffective in improving labor market outcomes is that basic skills (cognitive and non-cognitive)[6] may be fixed already when a compulsory schooling extension is introduced.[7] A second important difference between term extensions and compulsory schooling

4. Several studies analyze the effect of instruction time on student performance using different identification strategies, cf. Leuven et al. (2010); Battistin and Meroni (2016); Lavy (2015); Cortes et al. (2015); Bingley et al. (2018).

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 et al., 2017)

6. We will refer to *basic skills* as skills learned during primary school. This includes basic cognitive skills in math, reading and writing but also potentially non-cognitive skills as the ability to organize and plan or interpersonal skills. We differentiate basic skills from primary education from advanced skills learned in higher education or vocational training.

7. 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.Heckman and Masterov (2007) and Chetty et al. (2011)). This raises the question whether increases in instructional time in early years, where students acquire most fundamental skills have effects on later life outcomes. Gathmann et al. (2015), use aggregate data from 18 European compulsory school reforms implemented during the twentieth century and examine the dispersion in mortality gains across time and contexts. The authors conclude that early twentieth century reforms were 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 twentieth century reforms affected younger children than later reforms.

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 20th 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, all of the 2,463 Swedish school districts were obliged to extend the annual term length from 34.5/36.5 to 39 weeks, and to increase the mandatory amount of schooling from 6 to 7 years within a period of 12 years. School districts could choose the timing of the implementation independently within the given time window, generating large variation in time spent at school across 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 education, 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 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. Our study population consists of all Swedish children born 1930–1940 and for them we have information on place of residence during schooling age. 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.

A second contribution is that we can follow our population (born 1930–1940) over an exceptionally long time period – ranging from birth until the year 2013. 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 et al., 2017). We thus add to the small literature on the returns to education in the very long term (Van Kippersluis et al., 2011; Schneeweis et al., 2014; Crespo et al., 2014; Brunello et al., 2016; Bingley and Martinello, 2017) using high-quality administrative data.

A third contribution is the detailed reform data with variation across time and local geographical units which allows 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 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 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; Black et al., 2005; Pekkarinen et al., 2009; Lundborg et al., 2014), and many historical reforms generated educational degree effects in addition to the prolongation of schooling (Grenet, 2013; Kırdar et al., 2016). 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 interpretation of results.[8]

Using purposively collected school district reform data, excerpted from exam catalogues in historical archives, and high-quality register data, we show that the term extension was very effective in increasing labor market earnings – the effect on earnings at around age 35 is 5% when scaled to a school year – especially for women. The effects are persistent over the life cycle, with significant improvements in female pension earnings. Looking into effect heterogeneity across parental socioeconomic backgrounds, the results suggest that the positive effects of the term extension benefited broad parts of the labor force, with no evidence of an SES gradient in treatment effects.

Compared to the term extension the compulsory schooling 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

8. Still, the reforms may have had an impact on the situation of affected families, to the extent that children were working. Labor laws prohibited work during compulsory schooling ages, but informal employment may still have been a source of family income. We provide an analysis of effect heterogeneity by parental SES addressing this concern.

compulsory schooling extension are relatively modest for both men and women. If at all, the compulsory schooling extension seems to have benefited mainly individuals from low-SES background.

Using administrative data we examine a number of potential mechanisms mediating the positive term extension effect on earnings and explain the observed gender differences. Treated individuals are more likely to work in relatively well-paid occupations in administration and transport. In particular, we see an inflow of women 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 can furthermore rule out that the earnings effect from the term extension is biased by selective mortality or marriage market effects. In addition, the absence of a clear SES gradient in the effect suggests that there are no relevant effects operating via a direct impact on family income while the affected children were going to school. In a separate analysis we examine a family economics channel and show that maternal labor supply does not appear to be an important mediator, nor does the term extension seem to have had any contemporary effect on household earnings.

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 (Dustmann et al., 2003; Chetty et al., 2011; Fredriksson et al., 2012), school starting age (Black et al., 2011; Fredriksson and Öckert, 2014), extra days of schooling instruction (Crawford et al., 2014; Carlsson et al., 2015; Bingley et al., 2018) or more generally additional school resources (Jackson et al., 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 enhance human capital. The term extension led 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 achieved its intended aims. With results consistently more pronounced for women, longer terms also seem to have contributed to a reduction in the gender wage gap.

The next section gives the institutional background of the Swedish educational system and the reforms, and outlines an economic model. 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, analyzes potential mediators and presents robustness checks, and Section 6 concludes.

2. Institutions, Reforms, and Schooling Decisions

In this section we provide some information on the historical and institutional context in which the two reforms were implemented, with particular focus on the school system and the transition from education into the labor market. We then present a simple economic model which aims at capturing some of the more essential behavioral implications of the reforms.

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 fall and ends in spring. In the 1930s and 1940s all children entered a primary school called *Folkskola*.[9] 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 they were registered as a resident.[10]

Figure 1 illustrates the Swedish school system at the time. The mandatory amount of schooling was six years, but was extended to seven years with the compulsory schooling reform enacted from 1936. Similar to many other countries at the beginning of the twentieth century, the system consisted of different tracks. During the first years of primary school students were kept together. After finalising grade 4 or grade 6, students could switch to an academic educational track called *Realskola* (lower secondary school), which lasted for four or five years depending on when matriculation took place. Lower secondary school generally required entrance exams, suggesting that students remaining in *Folkskola* were less able on average. Other factors, such as the availability of a secondary school nearby and costs related to schooling, constituted additional barriers to secondary and higher education. After lower secondary school, students could continue to higher secondary education (*Gymnasium*) and university.[11]

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9. Private schools never played a substantial role in Sweden. Online Appendix D and Fischer et al. (2016) provides detailed description of the primary school system.

10. 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 reports precedents from 1935 related to this paragraph. Drop-out rates were extremely low. In 1939, 0.1 per cent of all children in school age were not in Folkskola (Statistics Sweden, 1974). 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.

11. 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*.

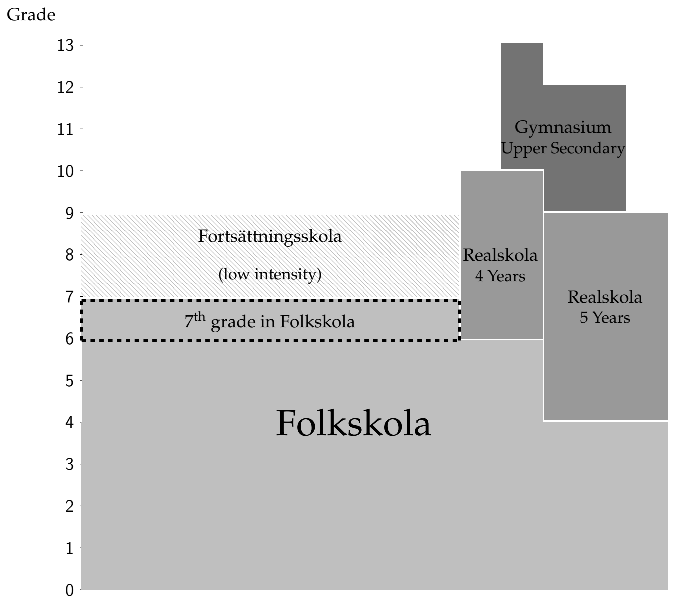
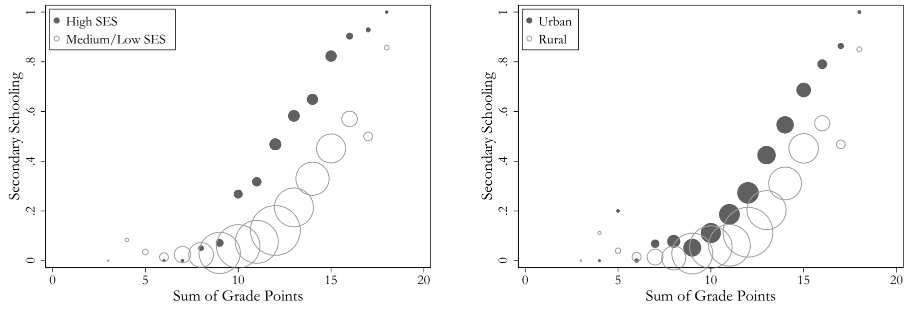


FIGURE 1. Swedish School System. The figure illustrates the Swedish school system before and after the 1936 compulsory schooling reform. Before the reform the mandatory amount of schooling was six years, and after the reform students instead had to take seven years of schooling. In the first years of primary school students were kept together. After grade 4 or grade 6 students could switch to lower secondary school (*Realskola*). Students matriculating after grade 4 followed a five-year track, and students matriculating after grade 6 followed a four-year track. After *Realskola* successful students could continue to upper secondary schooling (*Gymnasium*). The school system exhibited striking similarities with the contemporary German school system (see e.g. Pischke and Von Wachter, 2008). A majority of students did not switch to an academic track but stayed in primary education until completing the highest grade of *Folkskola* applying in the school district where they resided. Students not enrolling in secondary education took low intensity courses six weeks per year in local continuation schools (*Fortsättningsskola*) for two more years after finishing compulsory schooling.

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 (GPA) in grade 4 and background factors for a sample of individuals born 1930–34. The data are a representative sample of the entire country and thus the majority of included districts are untreated with respect to the education reforms (see Bhalotra et al., 2016, for sample details). The gap in secondary schooling enrollment between children in families of high and medium/low SES and between children residing in urban and rural areas, respectively, is partially attributable to high-SES and urban children performing better in primary school. Still there is a gap in enrollment also after conditioning on primary school performance, particularly pronounced at 29 percentage points when stratifying by father's SES.



(a) By Father's SES (b) By Place of Residence

FIGURE 2. Proportion Enrolled in Realskola by School Performance and Background. The graphs show proportion of students enrolled to *Realskola* in a sample of 25,000 individuals born 1930–34. Primary school performance ranges between 3 and 18 and is calculated as the sum of marks in math, reading and writing by 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 2,463 school districts and the reform triggered term length extensions in 600 districts already in the following two school years.[12] The 34.5 weeks option was to be discontinued by the school year 1941/42 and by the school year 1952/53, 39 weeks became the uniform standard.

The national Parliament also passed a law in 1936 extending compulsory schooling by introducing a mandatory 7$^{\text{th}}$ 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 of *Folkskola*.

The changes resulting from each reform had to be implemented uniformly in all schools within each school district. However, the term length extension and the compulsory schooling extension 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,

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

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 Fredriksson, 1971; Bhalotra et al., 2016, and Online Appendix D).

Swedish child labor laws have generally been coordinated with the compulsory schooling attendance laws. In 1931 the minimum age for manufacturing and construction work (*hard work*) was 14 years, whereas the limit for *light work* was 13 years.[14] Labor laws stated that children could not take on a full-time employment until they had completed compulsory schooling.[15]

According to the child labor law, a child was allowed to start working in the year they 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 could however have reduced labor market experience for students. If this is the case, estimates for the compulsory schooling extension constitute a net effect from an increase in formal schooling and a corresponding reduction in experience. After the implementation of the compulsory schooling extension, students left school in the middle of the year they turned 14, as compared to in the middle of the year they turned 13. Consequently, this reform reduced the time a child could spend in *light work* by one year, whereas the corresponding reduction for *hard* industrial work was 6 months. In Appendix Section A.4 we show that the effects of the compulsory schooling extension on labor market experience were small.

2.2. An Economic Assessment

We now sketch a simple economic model which highlights some of the key differences between the two reforms. Both schooling reforms increased 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

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. 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 they had fulfilled compulsory schooling (Sjöberg, 2009).

15. 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).

had virtually *perfect compliance* in the sense that it increased instructional time for everyone, while the compulsory schooling extension applied only to pupils who decided not to proceed to 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. A change in secondary schooling enrollment accompanying the reforms is thus likely mainly driven by self-selection in the case of the compulsory schooling extension – whereas such a change could also relate to improved learning outcomes in primary school in the case of the term extension.

Below we highligt some of the key aspects of our model, while Appendix A provides a more extensive description. Adapting a standard Roy model for schooling decisions (cf. Eisenhauer et al., 2015),[16] we consider the decision of enrolling in secondary school. We assume that individual i's potential earnings Y_i^j after secondary (Y^1) and primary (Y^0) schooling, respectively, are determined by

$$Y_i^1\left(T_i\right) = \mu_1 + T_i\alpha_i^1 + U_i^1 \tag{1}$$

$$Y_i^0\left(T_i, Z_i\right) = \mu_0 + T_i\alpha_i^0 + Z_i\beta_i + U_i^0 \tag{2}$$

where $\mu_j, j \in \{0,1\}$ represent population means, T_i is a variable taking a value of 1 if individual i was affected by the term length extension, α_i^j is the effect of the term extension on earnings for individual i with qualification j, and U_i^j represents unobserved heterogeneity in earnings potentials by qualification; $Z_i \in \{0,1\}$ represents the compulsory schooling extension and β_i the gains associated therewith. It should be noted that Y_i^0 is a function of both policy variables whereas Y_i^1 only is a function the term length extension T_i. This assumption is derived directly from the academic tracking system described in the previous section. We assume that students potential earnings in the academic track (*realskola*) are unaffected by extensions in the non-academic track. We also assume that the compulsory schooling extension satisfies the SUTVA (*Stable Unit-Treatment Value*; Rubin, 1990) assumption, so that no general equilibrium or spillover effects affect potential earnings of secondary schooling graduates.

The decision of whether to enroll in secondary schooling is not only governed by potential returns but also by the perceived costs. We assume that costs are given by

$$C_i\left(Z_i\right) = \left[\mu_C + U_i^C\right]\left(1 - Z_i\gamma\right) \tag{3}$$

where μ_C and U_i^C are defined analogously to the above, i.e. population mean and unobserved heterogeneity in costs. $\gamma \in (0,1)$ represents the reduction in the

16. It should be noted that in contrast to us, Eisenhauer et al. (2015) do not require the unobserved determinants of the schooling decision to follow a certain distribution.

costs of secondary schooling associated with the compulsory school extension. We thus assume that an additional year of compulsory schooling is perceived as a fraction of the cost of going to secondary school, and that this fraction is the same for everyone. A special case where $\gamma = 1/3$ implies that the costs of spending one more year in compulsory schooling are equivalent to one year of secondary schooling.[17] Without loss of generality, we assume that the costs of secondary schooling are unaffected by the term length extension.[18]

Next we consider the decision to enrol in secondary schooling, which we denote S_i:

$$\begin{aligned} S_i &= 1\left(Y_i^1\left(T_i\right) - Y_i^0\left(T_i, Z_i\right) - C_i\left(Z_i\right) \geq 0\right) && (4)\\ &= 1\left(\mu_1 - \mu_0 - \mu^C + U_i^1 - U_i^0 - U_i^C\right. && (5)\\ &\left. + T_i\left(\alpha_i^1 - \alpha_i^0\right) + Z_i\left(\gamma\left(\mu_C + U_i^C\right) - \beta_i\right) \geq 0\right) && (6) \end{aligned}$$

Hence, the observed earnings will be given by

$$\begin{aligned} Y_i &= Y_i^0\left(T_i, Z_i\right) + S_i\left[Y_i^1\left(T_i\right) - Y_i^0\left(T_i, Z_i\right)\right] \\ &= \mu_0 + T_i\alpha_i^0 + Z_i\beta_i + U_i^0 + S_i\left(T_i, Z_i\right)\left[\mu_1 - \mu_0 + T_i\left(\alpha_i^1 - \alpha_i^0\right) - Z_i\beta_i + U_i^1 - U_i^0\right] \qquad (7) \end{aligned}$$

implying that the two reforms potentially have direct and indirect effects on earnings.

In Appendix A, we formally show that it is possible to make some predictions regarding how the two reforms affect the decision to select into secondary schooling, which in turn have important implications for the interpretation of the effects on earnings. In the absence of any reform, the proportion of individuals selecting into secondary education will equal $\Psi\left(\mu_W\right)$, where $\Psi\left(\cdot\right)$ is the cdf of a normally distributed variable, and μ_W is the pre-reform mean of the argument of the selection equation (4). For other combinations of reform exposure, the probability of selecting secondary schooling is given by

$$\Pr\left(S = 1 \mid T = 1, Z = 0\right) = \Psi\left(\mu_W + \frac{\sigma_W}{\sigma_{W^{10}}}\left(\mu_{\alpha^1} - \mu_{\alpha^0}\right)\right) \qquad (8)$$

$$\Pr\left(S = 1 \mid T = 0, Z = 1\right) = \Psi\left(\mu_W + \frac{\sigma_W}{\sigma_{W^{01}}}\left(\gamma\mu_C - \mu_\beta\right)\right) \qquad (9)$$

17. This example is based on a comparison between (i) primary and secondary schooling together lasting nine years (i.e. the five-year track of Realskola) and (ii) compulsory schooling lasting six years; cf. Figure 1.

18. It is possible to imagine that the term length extension leads to improved skills which in turn reduce the ‘psychic costs’ of secondary schooling. In Appendix A we show that allowing for such a channel is completely equivalent to human capital gains (α_i) in potential earnings. Thus, for the ease of exposition, we leave that potential channel out here.

where $\sigma_W/\sigma_{W^{10}}$ and $\sigma_W/\sigma_{W^{01}}$ capture how the heterogeneity (standard deviation) of the unobserved net benefits of secondary schooling is affected by the reforms (see Appendix A for definitions). Thus, the proportion of students taking on secondary schooling after the term extension – $\Pr(S=1 \mid T=1, Z=0)$ – will be higher than before, provided that $\mu_{\alpha^1} - \mu_{\alpha^0} > 0$. A necessary but not sufficient condition for the term extension to have an impact on secondary enrollment is consequently that it leads to human capital gains ($\mu_{\alpha^1} > 0$). A sufficient condition for the term extension to increase secondary school enrollment is that there is complementarity between term length and secondary schooling so that $\mu_{\alpha^1} > \mu_{\alpha^0}$.

The situation is quite different for the compulsory schooling extension (equation 9): here increased secondary enrollment becomes *less* likely if the reform leads to human capital gains ($\mu_\beta > 0$). Thus, it is possible that the compulsory schooling reform leads to increases in secondary enrollment, and thereby earnings effects, even when the human capital gains are zero or negative on average ($\mu_\beta \leq 0$). Conversely, the term length extension can bring about an increase in earnings *only if* it has an effect on human capital.

The fact that a compulsory schooling reform can have effects on the academic degree is a feature specific to school systems with separate tracks of distinct length. In such a system, some individuals may be institutionally constrained if their optimal years of schooling is between the two lowest tracks. Among these institutionally constrained individuals there may be movement in either direction after a compulsory schooling reform, depending on whether changes in opportunity costs or productivity gains dominate. This calls for a very careful consideration of selection between tracks. The same concern is not present in an analysis of the term extension. For the term extension, changes to selection behaviour have to be driven by productivity gains, and thereby selection effects on earnings will be of second-order importance compared to the first-order effect on productivity.

3. Data and Sample Selection

3.1. 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. These are the cohorts for which reliable assignment of reform exposure is possible, and for whom links to parents are available in the 1950 Census. The 1970 Census 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.[19] To explore labor market effects we create a dummy variable indicating whether an individual was gainfully employed in 1970. Importantly, the 1970 census includes all the variables required to measure years of education.

Income statistics stem from official tax returns and are considered as highly accurate.[20] With information from 1970 this measure gives earnings for our cohorts born 1930–1940 at ages 30–40. 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, and CPI adjust incomes to SEK in 2014.[21] Furthermore, we use data on individual pensions from the years 2006–13.[22] 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).[23] Pensions can be expected to be less sensitive to fluctuations in labor supply than earnings. This is a desirable feature, especially when examining 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 1 presents summary statistics for our main outcome variables, the schooling reforms and socio-demographics for males and females.

19. 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.

20. In general all individuals aged 16 or older were liable of submitting a tax declaration in Sweden in 1970. If individual annual income or aggregated annual income in the case of married fell 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.

21. 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 source of 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.

22. 2006 is the first year we observe all relevant cohorts receiving a pension. The pension measure corresponds to the average pensions 2006–13 weighted based on the number of observations per individual.

23. Online Appendix G gives an introduction to the Swedish pension system, details on how pensions contributions were accumulated and on the relevant changes affecting cohorts born 1930–1940.

Table 1. Descriptive Statistics

	Males			**Females**		
	Mean	Std.Dev.	Obs	Mean	Std.Dev.	Obs
MAIN OUTCOMES						
Labor Earnings (1970)	241,699	132,012	315,792	75,301	90,481	294,563
Pensions (2006-2013)	230,814	157,692	1,736,834	158,561	75,345	1,857,604
Secondary School	0.23	0.42	315,792	0.27	0.44	294,563
Working	0.96	0.19	314,708	0.55	0.50	294,133
Public Sector Employment	0.18	0.39	314,708	0.25	0.43	294,133
Private Sector Employment	0.78	0.42	314,708	0.30	0.46	294,133
Managers, Profess.	0.25	0.43	314,708	0.17	0.38	294,133
Accounting, Admin.	0.04	0.20	314,708	0.12	0.33	294,133
Transport, Comm.	0.09	0.28	314,708	0.02	0.15	294,133
Other Occupation	0.58	0.49	314,708	0.24	0.43	294,133
TREATMENTS						
Average Term Length	38.46	1.02	315,792	38.50	0.98	294,563
Compulsory 7th Year	0.87	0.33	315,792	0.89	0.32	294,563
SOCIO-DEMOGRAPHICS						
Year of Birth	1935.63	3.03	315,792	1935.77	2.98	294,563
Parents High SES	0.21	0.40	314,717	0.21	0.41	293,644
Parents Academics	0.04	0.20	309,181	0.04	0.20	288,602

Notes: Sample consists of cohorts born 1930–1940. Labor earnings are measured in 2014 SEK. *Source:* Linked 1970 Census. Own calculations.

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 et al., 2017). The dataset 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.

3.2. Reform data

For reform assignment we rely on a purpose-built reform dataset extracted and digitized from historical archives. The dataset 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 are 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 confirms 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. Online Appendix H 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 the 2,463 school districts existing 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.[24]

Figure 3 plots average contemporary 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 figure confirms that reform implementation is strongly related to local characteristics and there is a clear ranking between the four groups concerning 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 are in between and indistinguishable from each other. The districts that adopted both reforms late tend to be the poorest. In Online Appendix Figures D1(a) and D1(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.

24. 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 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.

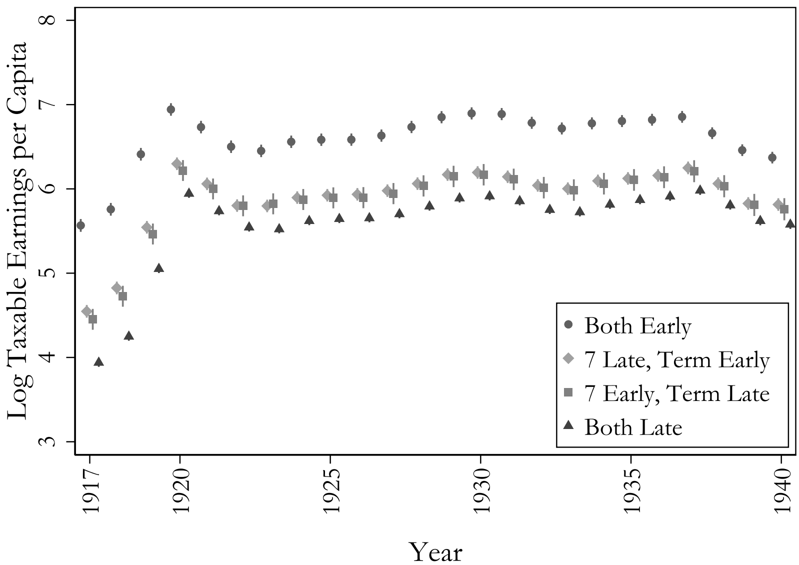


FIGURE 3. Trends in taxable earnings. The figure shows average taxable earnings per capita in four groups of school districts 1917-1941: early adopters of both reforms (before 1945), later adopters of both reforms, and district that were early adopters of one reform but not the other. *Source*: Municipal yearbooks (*Årsbok för Sveriges Kommuner* 1918–42). Own calculations.

3.3. 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. As shown in Online Appendix E, internal migration, even at school district level, is negligible between ages 6 and 16. Therefore, our reform assignment will be highly accurate for the included cohorts. Information on family structure and place of residence in 1946 comes from the 1950 Census. Family pointers are not available for the City of Stockholm due to a different registration system and therefore the city is not part of our baseline sample.[25] The parish of residence in 1946 is however possible to identify for individuals

25. It is only the city of Stockholm that is excluded in baseline regressions. In 1946 the city had 670,000 inhabitants. Many parishes in the urban agglomeration are parishes which only later became parts of the city of Stockholm (and still are), and they are indeed included in our baseline sample (e.g. Hässleby, Spånga). These areas were own school districts at the time of our analysis.

living in Stockholm, and we include all individuals from Stockholm in a robustness check in order to rule out selective migration (see Appendix C).

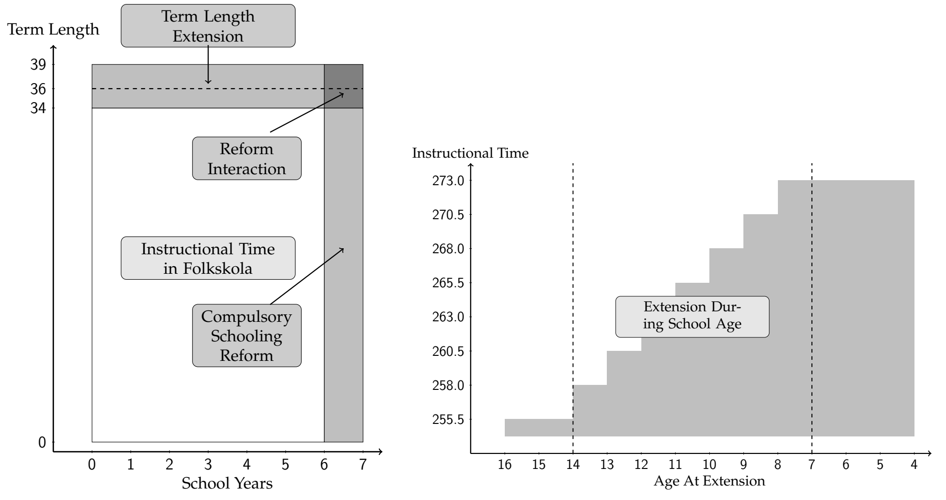
An alternative way to assign reform status would be to use information on individuals' *place of birth* as a proxy for the place of residence during schooling age. Under normal circumstances, such assignment would allow estimating an "intent-to-treat" effect. In the current context, however, the place 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.[26] 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 during childhood. We evaluate the impact of the hospital birth coding in more detail in Online Appendix E. Given the above caveats our main analysis uses the place of residence during schooling age to assign reform status.

4. Empirical Strategy

Our empirical analysis exploits exogenous variation in the amount of instructional time in primary school generated by the two 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 over the time spent in primary school.[27] The compulsory schooling reform added one complete year with 34.5, 36.5 or 39 weeks of additional instructional time. Figure 4 gives a stylized representation of the variation in instructional time generated by the two educational policies, while Figure 5 shows treatment variation with respect to the term length extension and the compulsory schooling extension across the cohorts 1930–1940 based on reform data. Clearly there is a sharp increase in treatment over time. Only about 12 per cent of all school districts had implemented 39 weeks of mandatory schooling when cohort 1930 started Folkskola, while the corresponding figure for the 1940 cohort was 70 per cent.

26. This potential problem is also mentioned by Holmlund (2008).

27. 15 weeks corresponds to an extension from 36.5 to 39 weeks and 6 years of compulsory schooling, while 31.5 weeks corresponds to an extension from 34.5 to 39 weeks and 7 years of compulsory schooling. The range 15 to 31.5 is equivalent to 1/2 to 1 full school year.



(a) Total Instructional Time

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

FIGURE 4. Total Instructional Time by Reform Status. Figure 4(a) shows the total instructional time in primary school. The instructional time varies in two dimensions – term length (measured in weeks) and length of *Folkskola* in years. Figure 4(b) shows the variation in total instructional time (measured in weeks) depending on the age at which individuals were affected by the term extension. The dashed parallel lines is what we refer to as the transition period. The numerical example here refers to a term extension moving from 36.5 to 39 weeks, and 7 years of compulsory schooling. An individual affected by this policy change already from the first year in primary school receives a total of 273 weeks of instruction (39*7), while an individual only affected in grade 7 receives a total of 258 weeks of instruction ((36.5*6)+(39*1)). School districts changed the term length simultaneously for all grades. Individuals in lower grades at the time of an extension received more instructional time in total.

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 4(a) however demonstrates that the effects of the two reforms are independently identified even without such adjustment, regardless of the correlation between them. The reason is the step-wise increase in instructional time that the term length extension causes – as shown in Figure 4(b). In contrast the compulsory schooling extension generates a sharp discontinuity between pre- and post-reform cohorts.

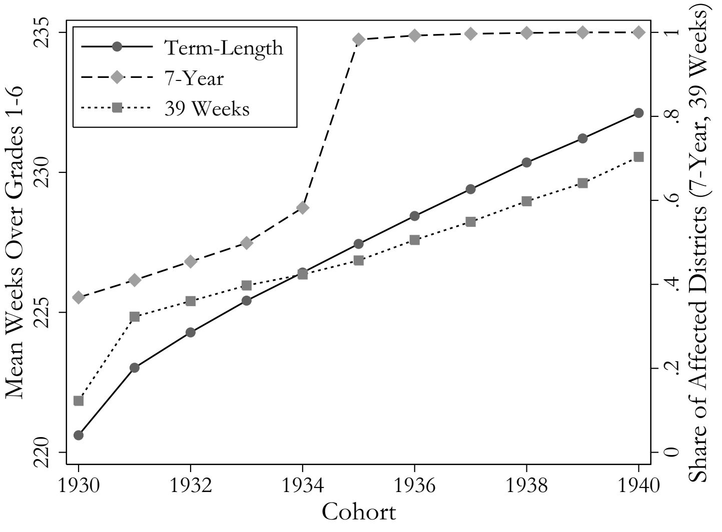


FIGURE 5. Treatment variation by cohort. The figure shows treatment variation with respect to the term length extension and the compulsory schooling extension across the cohorts 1930–1940. *Legend*: *Term length* corresponds to the average weeks of instructional time over grade 1—6 in Folkskola for each cohort. *39 weeks* shows the share of districts having implemented 39 weeks of instructional time at school starting age by cohort. *7-Year* illustrates the share of districts having implemented the compulsory schooling extension by cohort.

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

$$y_{ijk} = \beta_0 + \beta_1 T_{jk} + \beta_2 Z_{jk} + \beta_3' X_{ijk} + \nu_k + \mu_{R_k j} + C_k \cdot j + \varepsilon_{ijk} \quad (10)$$

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 cohort fixed effects $\mu_{R_k j}$ 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 linear time trends $C_k \cdot j$. Thus our empirical specification is essentially a two-way fixed effects approach, relying on within school district variation to identify our coefficients of interest

β_1 and β_2. As a robustness check, we consider an alternative specification with district-specific linear trends. X_{ijk} denotes a vector of potential additional control variables, including a gender dummy in pooled regressions and a widow indicator for regressions on pensions as outcome variable. The latter is included because widowhood has mechanical effects on pension earnings.

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 district characteristics, it is important to carefully assess the plausibility of this assumption – in comparison to a more restrictive specification which allows for divergent district trends.

Below we add a summary of balancing tests based on a large number of household and school district characteristics. Table 2 presents the reform effect relative to the mean for each specification based on 36 independent tests for the available balancing variables.[28] The table also shows the number of significant results at the 1%, 5% and 10% significance levels. Clearly, both reforms correlate with several household characteristics and school district characteristics. Adding trends reduces effect sizes significantly compared to specifications only including cohort fixed effects. Notably, our baseline specification with district fixed effects and county trends appears to balance a large number of observables – at the individual and district levels – and performs at least as well as the alternative specification with district trends. The baseline specification produces one significant test at the five per cent level and one at the ten per cent level, while the specification with district trends produces three significant results. Both outcomes are in line with what would be expected from 36 independent tests.

28. Detailed results and descriptives can be found in Appendix Tables B1 and B2.

Table 2. Balancing Test Summary.

	Term Length Extension			7-Year Extension		
Effect Relative to Mean	0.341	0.039	0.033	0.202	0.024	0.041
No. Coef. Significant at 1%	13/18	0/18	1/18	14/18	0/18	0/18
No. Coef. Significant at 5%	14/18	0/18	3/18	15/18	1/18	0/18
No. Coef. Significant at 10%	15/18	0/18	3/18	16/18	2/18	0/18
Cohort FE	✓	✓	✓	✓	✓	✓
District FE		✓	✓		✓	✓
County Trends		✓			✓	
District Trends			✓			✓

Notes: The table summarizes the results from our balancing regressions presented in Table B2. It presents the reform effect relative to the mean for each specification based on all balancing variables and the number of significant results for our balancing variables at the 1%, 5% and 10% significance level.

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. As a robustness test we therefore also implement design-based inference and randomly reassign reform years (without replacement) between school districts.

The parameters β_1 and β_2 of equation (10) 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 term length extension in primary school affected all students in the first 4 years 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 with the Reforms

The main dataset does not contain explicit information on compliance with the term extension, but we can estimate how the time actually spent in school responds to the extension using another data source including information on school attendance in first and fourth grade of primary school. Panel A of Table 3 presents results of a regression of actual attendance on the length of the school year and several control variables.[29] Attendance rates were 0.94 on average, and according to our estimates, each additional day added to the school year translated into 0.88-0.93 additional days spent in school. The results are clearly indicative of perfect compliance with the term extension reform.

For the compulsory schooling extension, we can estimate a first stage using equation (10) and the information on educational attainment in 1970.[30] Results are presented in Panel B of Table 3. Accordingly, the compulsory schooling extension increased years of schooling by 0.83 years on average; slightly more for men than women. These first-stage estimates are in close agreement with estimates based on an alternative dataset with self-reported years of schooling (cf. Fischer et al., 2018). In the absence of reform effects on secondary schooling enrollment, the first stage would equal $1 - q$, where q is the secondary schooling completion rate. Based on the means of this variable reported in Table 1, the first-stage effect would be 0.75 (0.77 for males; 0.73 for females) in such a case. The estimated first stages are slightly larger than this, which suggests there have been spillover effects on secondary education. We investigate this in Panel C of Table 3, including also results for the term extension. According to these estimates, the compulsory schooling extension had a small yet significant effect on the likelihood to complete secondary schooling. This is consistent with the reduced opportunity cost of secondary schooling imposed by the reform (cf. Section 2.2). The effects of the term extension are similar in size but less precisely estimated.

29. The data set is representative sample of the 1930-34 cohorts. For details on the dataset and available variables see Bhalotra et al. (2016).

30. As mentioned the Census 1970 makes no distinction between 6 and 7 years of *Folkskola*, but for compliers, years of education are completely determined by reform assignment.

Table 3. Effects on Schooling.

	(1) Males & Females	(2) Males	(3) Females
A. Attendance			
Term Length	0.9051***	0.9308***	0.8773***
	(0.037)	(0.035)	(0.052)
Mean	196.04	196.34	195.74
Rate	0.94	0.94	0.94
N	16,053	8,117	7,936
B. Years of Education			
Compulsory 7-Year	0.832***	0.871***	0.760***
	(0.022)	(0.025)	(0.033)
F-Statistic	2,123	1,357	743
N	498,738	303,106	195,629
Districts	2,389	2,389	2,378
C. Secondary Schooling			
Term Length	0.007	0.012	0.003
	(0.009)	(0.011)	(0.014)
Compulsory 7-Year	0.008***	0.011***	0.004
	(0.003)	(0.004)	(0.004)
Mean	0.25	0.23	0.27
N	610,352	315,789	294,559
Districts	2,391	2,389	2,384

Notes: Robust standard errors clustered at school district level are reported in parenthesis. Significance levels: $*$ 0.10 $**$ 0.05 $***$ 0.01. *Panel A:* The panel shows results from a regression of actual school attendance in grade 1 and grade 4 of Folkskola on the length of the school year. The estimation sample consists of 16,000 individuals born between 1930–34. All regressions include school district fixed effects, quarter of birth $\times$ year of birth fixed effects and control for SES. *Source*: Exam catalogues (c.f. Online Appendix H) from 130 nationally representative school districts (Bhalotra et al., 2016). *Panel B, C:* Results refer to cohorts 1930 to 1940. All regressions include rural/urban birth cohort FE, linear district trends and a gender dummy if pooled regression. Dependent Variable: *Years of Education* constructed from 1970 Census, *Secondary Schooling* based on 1970 Census.

5.2. Labor Market Outcomes

5.2.1. Earnings and Pensions Table 4 reports coefficients from regressions of log earnings and pensions on the two policy instruments term extension and compulsory schooling extension. For the compulsory extension we can also estimate a local average treatment effect using 2SLS (column 6) where we instrument years of education by the 7-year reform. Both reforms significantly increase earnings in 1970 (when the individuals are 30–40 years old). The term length extension increases earnings by 5 per cent; the effect on pensions is about half that size. These effects are almost entirely driven by females, who experience an increase in their 1970 earnings by 9.5 per cent, whereas their pensions (measured in 2006–2013) increase by 3.7 per cent. The compulsory schooling extension, in contrast, leads to smaller gains in earnings: the local average treatment effect is 2.4 per cent for 1970 earnings, and drops to 0.8 per cent when pensions in later life are considered. The gender differences are also less pronounced for this reform. Our result for the compulsory schooling extension 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, 2016).

TABLE 4. Main Results: Earnings and Pensions

	(1) *Districts*	(2) *N*	(3) *Mean Earnings*	(4) **Term Length Extension**	(5) **7-Year Extension**	(6) **7-Year 2SLS**
A. MALES & FEMALES						
Log Earnings 1970	2,389	498,741	161,394	0.050*** (0.018)	0.020*** (0.006)	0.024*** (0.006)
Log Pensions 2006-13	2,393	3,591,184	193,474	0.026*** (0.010)	0.006** (0.003)	0.008** (0.004)
B. MALES						
Log Earnings 1970	2,389	303,109	241,699	0.024* (0.014)	0.016*** (0.004)	0.018*** (0.005)
Log Pensions 2006-13	2,391	1,735,190	230,814	0.018 (0.015)	0.011** (0.004)	0.013** (0.005)
C. FEMALES						
Log Earnings 1970	2,378	195,632	75,301	0.095** (0.043)	0.026** (0.013)	0.035** (0.017)
Log Pensions 2006-13	2,386	1,855,994	158,561	0.037*** (0.013)	0.003 (0.004)	0.004 (0.005)

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 and linear county trends; pooled regression also include a gender dummy. **Dependent Variables:** *Earnings* from 1970 Tax Records. Earnings equal zero for non-working individuals. *Pensions* from 2006-2013 Tax Records. **Legend:** (1) Number of School Districts (2) Number of Observations (3) Mean Earnings/Pensions (4) and (5) Reduced Form Baseline Specification (6) 2SLS for 7-Year Extension.

As a complement to the main regression results we provide estimates where we split up the term extension effect by school year to capture the earnings and pension effects along the number of years of exposure (cf. Bingley et al., 2018). The estimates presented in Table 5 clearly suggest that the noted term length effect is driven by school years 1-4. This finding actually eases the interpretation of all results, since it implies that only the term extensions that apply to all children – and not the year 5-6 extensions that do not affect individuals selecting into secondary schooling – matter for later labor market outcomes. The regression results in Table 5 are corroborated by event studies for the term extension, see Appendix Figure B1, which also suggest that earnings and pension increases are more concentrated for children receiving the term length extension in their early school years. Conversely, effects in the later school years are small and statistically insignificant.[31] Appendix Figure B2 presents event studies for the compulsory schooling extension.

Table 5. Effects by School Year

	(1) **Term Length Extension 1-4**	(2) **Term Length Extension 5-6**	(3) **7-Year Extension**
A. Males & Females			
Log Earnings 1970	0.087***	-0.061	0.021***
	(0.023)	(0.049)	(0.006)
Log Pensions 2006–2013	0.033**	0.005	0.007**
	(0.013)	(0.026)	(0.003)
B. Males			
Log Earnings 1970	0.036*	-0.013	0.016***
	(0.020)	(0.043)	(0.004)
Log Pensions 2006–2013	0.031	-0.022	0.011**
	(0.020)	(0.041)	(0.004)
C. Females			
Log Earnings 1970	0.168***	-0.124	0.029**
	(0.053)	(0.116)	(0.013)
Log Pensions 2006–2013	0.038**	0.031	0.003
	(0.017)	(0.033)	(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 and linear county trends; pooled regression also include a gender dummy. Effects stem from baseline regression but allowing for separate effects from term length depending on grade. **Dependent Variables:** *Earnings* from 1970 Tax Records. Earnings equal zero for non-working individuals. *Pensions* from 2006-2013 Tax Records. **Legend:** (1) Term length grade 1-4 (2) Term length extension grade 5-6 (3) Reduced form 7-year extension.

5.2.2. Employment and Occupation 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 6 presents results for employment, sector of employment, and occupational group. Column 3 shows results for

31. It should be noted that the validity of event studies for the analysis of causal effects has been challenged in the recent literature (Borusyak and Jaravel, 2017; Abraham and Sun, 2018). In our case, the small increments of term length (3 weeks) added each year, and the collinearity between them, add a power issue to these concerns.

the term extension, while column 4 shows results for the compulsory schooling extension.

Starting with employment, we find a striking difference between the two reforms: the term extension increases it by 1.7 percentage points, 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 increases the probability of working in accounting/administrative and transport/communication positions, which experience comparable increases in absolute (1.3-1.4 percentage points) and relative (17-23 %) terms. The reduction in “other occupation” is primarily driven by reductions in agricultural employment; however, that estimate is not robust to the inclusion of local district trends.

Splitting the sample by gender, it is clear that the net increase in employment is driven by women, who increase it by 4.0 percentage points. This is to a great extent driven by increased employment in the public sector, and in accounting/administrative occupations. Among women, 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 women at the time (cf. Schånberg, 1993) – so 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 smaller in general, but some patterns are similar to what is noted for the term extension: the reform causes an inflow into the class “Accounting, Administrative” driven by women. In addition, there is a small but significant increase in the probability of working in a “managerial and professional” occupation driven by men – and the effect on this category is similar in size to the previously reported effect on secondary schooling completion.

Table 6. Employment and Occupation 1970

	(1) *Mean Earnings*	(2) *Mean*	(3) **Term Length Extension**	(4) **7-Year Extension**
A. Males & Females				
Working	205,639	0.765	0.017**	0.001
			(0.008)	(0.002)
Public Sector Employment	210,845	0.216	0.003	0.001
			(0.009)	(0.003)
Private Sector Employment	203,592	0.549	0.014	-0.000
			(0.010)	(0.003)
Managers, Profess.	276,484	0.211	0.009	0.007***
			(0.009)	(0.003)
Accounting, Admin.	167,600	0.080	0.014**	0.005***
			(0.005)	(0.002)
Transport, Comm.	199,009	0.055	0.013**	-0.002
			(0.005)	(0.002)
Other Occupation	177,713	0.416	-0.020*	-0.009***
			(0.010)	(0.003)
B. Males				
Working	249,203	0.963	-0.004	-0.001
			(0.005)	(0.002)
Public Sector Employment	290,771	0.183	-0.016	0.002
			(0.012)	(0.004)
Private Sector Employment	239,443	0.779	0.012	-0.002
			(0.013)	(0.004)
Managers, Profess.	339,892	0.248	0.008	0.009**
			(0.013)	(0.004)
Accounting, Admin.	255,241	0.040	0.002	0.003*
			(0.005)	(0.002)
Transport, Comm.	218,148	0.085	0.016*	-0.003
			(0.008)	(0.003)
Other Occupation	214,926	0.582	-0.031**	-0.009**
			(0.015)	(0.005)
C. Females				
Working	124,677	0.554	0.040***	0.003
			(0.015)	(0.005)
Public Sector Employment	148,563	0.251	0.024*	0.001
			(0.014)	(0.004)
Private Sector Employment	104,857	0.303	0.016	0.002
			(0.015)	(0.004)
Managers, Profess.	178,413	0.172	0.006	0.004
			(0.012)	(0.004)
Accounting, Admin.	136,917	0.123	0.031***	0.009***
			(0.010)	(0.003)
Transport, Comm.	117,862	0.022	0.010**	-0.001
			(0.005)	(0.001)
Other Occupation	80,215	0.238	-0.007	-0.009**
			(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 1930–1940. All regressions include rural/urban birth cohort FE, linear county trends and a gender dummy if pooled regression. **Dependent Variables:** Employment, sector of employment (public and private) and occupational groups according to Census 1970. **Legend:** (1) Mean Earnings: mean earnings in 1970 for each outcome group, (2) Mean: mean of dependent variable, (3) and (4) Reduced Form Baseline Specification.

Our findings regarding labor market outcomes thus provide further evidence of why the two education reforms had such a different impact on earnings: the term extension triggered a large inflow of women into the workforce, and a net increase in employment in well-paid occupations. This 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, consistent with the previous literature (cf. Hansen, 2011; Fitzpatrick et al., 2011; Agüero and Beleche, 2013; Lavy, 2015).

5.3. Heterogeneity

To gain a better understanding of the labor market returns of the two reforms we turn to an effect heterogeneity analysis. 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 about 75% 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 present two sets of heterogeneity results; one with the aim of shedding light on the gender differences in our baseline results, and one with the purpose of assessing whether the reforms contributed to reduced inequality with regard to socioeconomic background. The latter point is of particular interest, given that evaluations of subsequent schooling reforms in Nordic countries have shown that even a reform with modest effects may contribute substantially to a narrowing of the gap between children of different backgrounds (Meghir and Palme, 2005; Pekkarinen et al., 2009).

First, we consider heterogeneity by sector of employment and by marital status in Table 7. Since both variables are potentially affected by the reforms, some caution is required when interpreting results. Still, heterogeneity estimates along these lines are indicative of the subpopulations driving the results, which in turn may be informative regarding the exact mechanisms. The fact that our estimates for sector of employment in Table 6 and for marital

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.

status in Table C4 rule out large effects on either variable, suggests that the "bad control" problem may be limited in this case.

The upper panel A of Table 7 compares the reform effect on earnings in 1970 for public and private sector workers. Since wages in the private sector cannot deviate systematically from productivity, an analysis split between the sectors has been suggested as an indication of whether returns to education reflect productivity (Chevalier et al., 2004). In our case, the estimated gains are completely concentrated among private sector employees (column 1); this result holds for males and females alike. The lower panel B of Table 7 makes the corresponding comparison by marital status. In 1970, married spouses were still subject to joint income taxation in Sweden, which significantly reduced the incentives for married women to work (Gustafsson, 1992; Selin, 2014). Reform gains are concentrated among married women (column 6).

Table 7. Heterogeneity by Sector and Marital Status in 1970: Earnings

	Males & Females		**Males**		**Females**	
	Private sector/ Single (1)	*Public sector/ Married* (2)	*Private sector/ Single* (3)	*Public sector/ Married* (4)	*Private sector/ Single* (5)	*Public sector/ Married* (6)
A. Public sector employment 1970						
Compulsory 7-Year	0.019***	0.023***	0.014***	0.022***	0.035**	0.025*
	(0.005)	(0.009)	(0.005)	(0.008)	(0.014)	(0.015)
Term Length	0.045***	-0.001	0.035**	-0.008	0.076	0.007
	(0.016)	(0.030)	(0.015)	(0.031)	(0.048)	(0.050)
Observations	317,284	129,248	238,578	56,968	78,662	72,136
Mean Dep. Var.	10.085	10.024	10.298	10.478	9.440	9.665
B. Married 1970						
Compulsory 7-Year	0.011	0.015**	0.010	0.011**	0.004	0.020
	(0.009)	(0.006)	(0.011)	(0.004)	(0.016)	(0.014)
Term Length	0.002	0.062***	0.004	0.029**	0.008	0.123**
	(0.028)	(0.021)	(0.034)	(0.015)	(0.052)	(0.050)
Observations	95,742	402,974	59,173	243,881	36,378	159,085
Mean Dep. Var.	9.992	9.851	10.045	10.374	9.906	9.049

Notes: The table shows the reduced form effects stratified by public sector employment (panel A) and marital status in 1970 (panel B). *Private sector/Single* refers to private sector employment or being single, and *Public sector/Married* refers to public sector employment or being married. 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. Results based on 2,390 school districts. **Dependent Variable:** *Log-Earnings* from tax-records in 1970.

Next we explore heterogeneity with respect to family background. Table 8 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 the 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 males and females, but there is not enough power to draw a definite conclusion. Conversely, and as expected, the earnings gains from the compulsory schooling extension appear to be concentrated in lower socioeconomic groups. Still, there is not much of a 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 8. Heterogeneity by SES Background

	Males & Females		**Males**		**Females**	
	Low SES (1)	*High SES* (2)	*Low SES* (3)	*High SES* (4)	*Low SES* (5)	*High SES* (6)
A. Log 1970 Earnings						
Compulsory 7-Year	0.024***	-0.002	0.016***	0.008	0.035**	-0.011
	(0.006)	(0.014)	(0.004)	(0.011)	(0.014)	(0.028)
Term Length	0.039**	0.075	0.020	-0.000	0.080*	0.197*
	(0.019)	(0.048)	(0.015)	(0.036)	(0.047)	(0.103)
Observations	390,907	106,057	239,747	62,147	151,153	43,575
Mean Dep. Var.	9.832	10.045	10.262	10.493	9.150	9.408
B. Log Pensions 2006–2013						
Compulsory 7-Year	0.011***	-0.005	0.011***	0.012	0.011***	-0.025**
	(0.003)	(0.007)	(0.004)	(0.011)	(0.004)	(0.010)
Term Length	0.024**	0.011	0.017	-0.033	0.034**	0.054
	(0.010)	(0.025)	(0.015)	(0.043)	(0.013)	(0.034)
Observations	2,816,245	763,576	1,363,480	366,128	1,452,765	397,443
Mean Dep. Var.	12.026	12.205	12.199	12.414	11.863	12.012
C. Secondary Schooling						
Compulsory 7-Year	0.009***	0.005	0.013***	0.007	0.004	0.002
	(0.003)	(0.008)	(0.003)	(0.012)	(0.004)	(0.011)
Term Length	0.007	-0.005	0.017	-0.010	-0.003	0.001
	(0.009)	(0.027)	(0.011)	(0.035)	(0.014)	(0.039)
Observations	481,099	127,167	249,834	64,696	231,263	62,182
Mean Dep. Var.	0.176	0.524	0.160	0.498	0.194	0.551

Notes: Table shows the reduced form effects stratified by household head characteristics. Household head 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 rural/urban birth cohort FE, linear county trends and a gender dummy. Results based on 2,390 school districts. **Dependent Variables:** *Log-Earnings* from Tax-Records in 1970. *Log-Pensions* from Tax-Records 2006-2013. *Secondary Schooling* from Census 1970. **Stratification Variable:** *Low and High SES indicators* are based on household 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.

The absence of an SES gradient in effects of the term extension is interesting considering that there is an empirical literature suggesting that there is increasing complementarity between skills and investments when children age (Cunha and Heckman, 2007; Cunha et al., 2010). To the extent that parental SES is a proxy for the child's skills when entering school, we would have expected larger effects for high-SES children. Conversely, the absence of a SES gradient suggests that there was no such complementarity between skills and the term extension.

5.4. Contemporaneous Effects

Next, we consider *contemporaneous* effects of the term extension on maternal labor supply and household income. The availability of childcare and/or early education has been shown to be an important determinant of maternal employment in some studies (Cascio and Schanzenbach, 2013; García et al., 2016; Herbst, 2017; Morrissey, 2017). Likewise, maternal employment and family income have been found to affect child development (Bernal, 2008; Bernal and Keane, 2011; Del Boca et al., 2016). Thus, any later-life effects of the reforms we study may in fact be mediated by maternal labor supply and parental income.

The education reforms studied here were carried out in a context where employment rates of mothers were relatively low: in the total female working-age population, only 36 per cent worked half-time or more (including those who helped their partners in their business e.g. on a farm; Statistics Sweden, 1952). Employment among mothers of schoolchildren was at 13.6 per cent and the corresponding number for married mothers was 10.7 per cent. Figure 6 shows how maternal employment in 1950 varied with the youngest child's age, and in particular the effect of the youngest child starting school. Even though maternal employment does vary somewhat with the child's age, there is no discontinuity at the school starting age seven for any group of mothers. It thus appears that the time children spent in school was a relatively unimportant determinant of maternal labor supply. Still, in the analysis below we allow maternal labor supply and household earnings to be contemporary mediators.

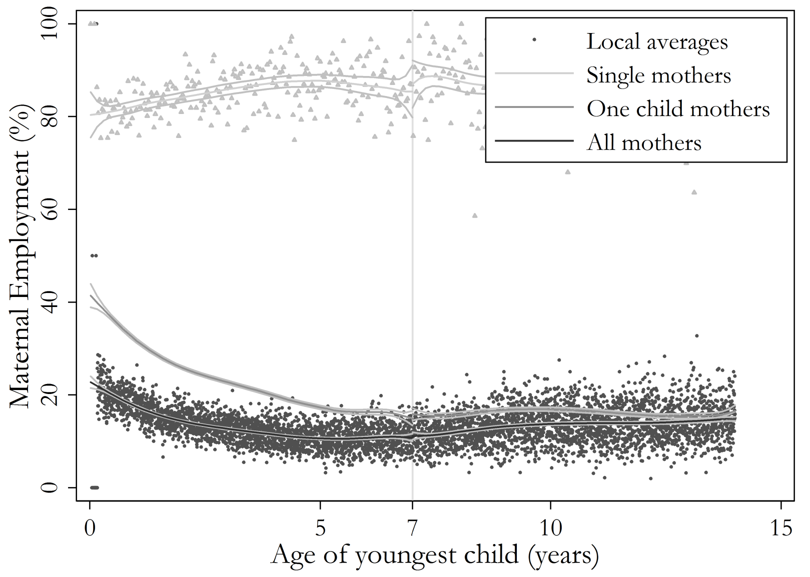


Figure 6. Maternal Labor Supply as a Function of the Age of the Youngest Child. The figure shows maternal employment in 1950 – local averages and local polynomial regression estimates – as a function of the age of the youngest child at the end of the year. Compulsory schooling attendance applies to children born before 31 December 1943 (signified by vertical line at age 7). Maternal employment is defined as working at least half-time, including employment in a family firm. Local averages are calculated by exact birth date. Sample size is 688,317 (single mothers: 21,959; one-child mothers: 300,306). *Source:* Population census 1950. Own calculations.

With information on maternal employment in 1950, when the term length extension was still ongoing, we can estimate the impact of the term length extension in a differences-in-discontinuities framework, where the discontinuity in maternal employment when the child reaches school starting age is interacted with the term length in the parish of residence at the time:

$$y_i = \beta_0 + \sum_{k\in\{37.5,39\}} \beta_k 1\left(T_i = k\right) + \delta 1\left(a_i \geq 7\right) + \sum_{k\in\{37.5,39\}} \eta_k 1\left(T_i = k\right) 1\left(a_i \geq 7\right)$$
$$+ \sum_{k\in\{36,37.5,39\}} \zeta_k 1\left(T_i = k\right) a_i + \sum_{k\in\{36,37.5,39\}} \kappa_k 1\left(T_i = k\right) 1\left(a_i \geq 7\right) a_i + \varepsilon_i \tag{11}$$

where $y_i \in \{0, 100\}$ represents the employment status of the mother, T_i represents term length applying in 1950 (taking on the three values 36, 37.5 and 39; where 37.5 corresponds to districts in transition to 39 weeks). a_i is the age of the youngest child in the family on December 31 1950, calculated based on exact day of birth (in our case a_i ranges from 6 to 8 years of age). Hence, δ picks up the effect of the child turning 7 in unreformed districts whereas η_k

represents the differential effect of the child turning 7 in transition and reformed districts. This is also the relevant causal effect in this context: whenever $\eta_k \neq 0$ for some $k \in \{37.5, 39\}$, we would be concerned that maternal labour supply is a potential mediator of the reform.

Table 9 presents results for different types of mothers (married; young; farmers; and mothers with only one child) with possibly different behaviours on the labor market. The first estimate (*Baseline*), shows the effect of the youngest child staring school in the non-reformed districts, while the second (*Transition*) is the corresponding interaction term for districts that change to 39 weeks in 1950. The third estimate (*39 weeks*) represents the interaction for districts which extended the term length before 1950. All estimates are insignificant, and effect sizes of more than six percentage points can be ruled out. Maternal labor supply does thus not seem to be an important mediator of the observed effects, nor explain heterogeneity by SES.

TABLE 9. Difference-in-Discontinuities Estimates: Maternal Employment.

	All	One Child	Married	Young	Farmer
Baseline (36 w)	-2.2701	-1.4764	-2.0597	2.0241	-1.2247
	(2.021)	(4.533)	(1.631)	(4.434)	(1.130)
× Transition	-0.6344	-6.0426	2.7479	-8.0881	0.9076
	(3.082)	(6.093)	(2.883)	(9.540)	(1.846)
× 39 weeks	2.4482	2.1043	2.2312	-1.4734	1.3901
	(2.080)	(4.602)	(1.694)	(4.533)	(1.231)
Baseline	11.19	15.77	8.61	11.88	2.38
N. of cases	92,673	38,245	88,618	19,876	19,294

Notes: Results from a local linear regression discontinuity with interaction terms. Robust standard errors clustered at school district level are reported in parenthesis. Significance levels: $*$ 0.10 $**$ 0.05 $***$ 0.01. *Baseline (36 w)* represents the effect of the youngest child staring school in the non-reformed districts. × *Transition* is the corresponding interaction term for districts that change to 39 weeks in 1950. × *39 weeks* represents the interaction for districts which extended the term length before 1950. The regression sample consists of all children between 6 and 8 years of age. *Young* mothers are mothers below the median age at the birth of their youngest child, conditional on the number of children. *Source*: Own estimates based on Census 1950.

We also consider effects on household earnings. This analysis can only be conducted at the district level, since no suitable data on individual earnings exist for the period of interest. We estimate a series of specifications where the outcome variable is log taxable earnings per household

$$y_{kt} = T_{kt} \cdot K_{kt}\beta + K_{kt}\delta + \nu_k + \mu_{R_k t} + C_k \cdot t + \varepsilon_{kt} \tag{12}$$

where y_{kt} represents total labor market income per household, $T_{it} \in [0, 1]$ represents the term length applying in district k in year t ($T_{it} = 1$ if 39 weeks have been implemented), and K_{kt} is a vector of fractions representing the proportion of households that have schoolchildren. The trends and fixed effects are equivalent to those included in our main estimating equation (10) with calendar year t replacing cohort j. In the simplest specification, K_{kt} only includes the fraction of households with children of ages 7–13. Subsequent specifications also include the fraction of farmer households with school-age children. The estimates in Table 10 are robustly insignificant and small: no confidence interval allows for an earnings effect larger than 2 per cent.

Table 10. DID Estimates: Contemporary Effect on Local Earnings.

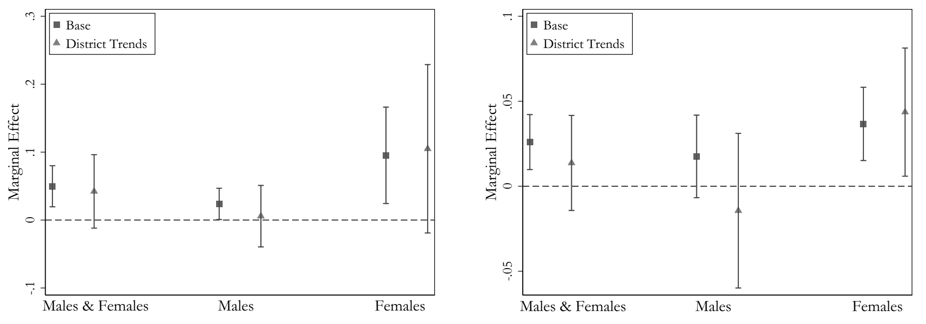
	Baseline Specification		+ Local trends	
Schoolchildren × 39 weeks	-0.0065	0.0012	0.0028	0.0048
	(0.005)	(0.006)	(0.007)	(0.009)
Farmer Schoolchildren × 39 weeks		-0.0153		-0.0085
		(0.010)		(0.014)
N. of cases	9,676	9,676	9,676	9,676

Notes: The parameter *Schoolchildren × 39 weeks* represents the interaction of reform status with the fraction of households with children between ages 7–13 (the latter variables measured in standard deviations). The parameter *Farmer schoolchildren × 39 weeks* is based on the corresponding measure for farming households. The dependent variable is log local taxable earnings per household. The panel is balanced and covers years 1946–49, which are the only years for which all variables are available. *Sources*: Population Census 1950 and Municipal yearbooks 1948–51).

5.5. Robustness checks

Balancing regressions suggest that our preferred specification takes care of potential confounders (compare Table 2). Differential trends in the outcome variables could nevertheless bias our estimates. We therefore test the robustness of our results by adding further trends to our baseline specification (which already includes rural/urban birth cohort fixed effects, linear county trends and school district fixed effects). We also run our baseline regression including everyone living in the city of Stockholm in order to rule out biases related to our sample selection. Appendix Tables C1, C2 and C3 present regression results for the various specifications. The estimates suggest that differential modelling of trends does not affect our main results. For the compulsory schooling extension, the estimated effects are small throughout: none of the pooled estimates in Table C1 (for 1970 earnings) and Table C2 (for pensions) are larger than 2.1 per cent. Including district trends reduces the estimated effect on 1970 earnings almost by half, and eliminates the effect on pensions.

For the term extension, the point estimates are relatively robust when we control for district-level trends (cf. Figure 7). The effect on female earnings is estimated at 10.5 per cent, compared to 9.5 per cent in our main specification. Also the effect on female pensions increases slightly compared to the baseline specification when adding local trends. The pooled effects, on the other hand, are somewhat reduced with this specification: the effect on earnings drops from 5.0 to 4.2 per cent, and the effect on pensions is reduced from 2.6 to 1.4 per cent. This drop in the pooled estimates follows from the male effects being unstable: including district trends eliminates the effects on pensions and earnings for males. As shown and discussed in Section 4 the covariate balance appears to be worse for the specification with district trends than for our baseline specification. In fact, the gender differences in robustness may also be explained with reference to covariate balance: as shown in Appendix Table B3, introducing local trends makes the term extension negatively correlated with the SES background of males, and positively correlated with the SES background of females.



(a) Log Earnings 1970

(b) Log Pensions 2006-2013

FIGURE 7. Robustness: Term Length Extension. The figures illustrate robustness of our main results using alternative specifications. *Base* refers to our baseline specification including rural/urban birth cohort FE, linear county trends and a gender dummy if pooled regression. *District Trends* includes linear district trends. 90% Confidence intervals included. Standard errors clustered at school district level.

Including district-specific trends decreases the efficiency of the estimated effects of the term extension – which is not surprising since the reform is also linear in cohort during the reform transition period on the district level (cf. Figure 4(b)). To investigate this collinearity problem empirically, we calculate the variance inflation factor between year of birth and treatments on school district level.[33] Appendix Figure C1 shows that collinearity is indeed an issue for the term length extension in the specification including linear district trends, but not in our baseline specification. Thus collinearity likely explains

33. For each school district we estimate

$$VIF_{T,k} = \frac{1}{1 - R^2_{T,k}}$$

where $R^2_{T,k}$ is the R^2 for the regression of term length T on regressors varying on district level: year of birth, sex and compulsory 7-year Z. As a rule of thumb inflation factors > 10 are a signal of high multicollinearity (standard errors are $\sqrt{10}$ times larger in contrast of being orthogonal).

the decrease in efficiency of our estimates using linear district-specific cohort trends. It should be noted that despite this loss in efficiency, the effect on female pensions remains significant at the 10% level ($p = 0.056$). The reduced precision and better balancing properties lead us to prefer our baseline specification, though the specification with district trends supports our main conclusion that the term extension increased earnings over the life cycle, in particular for females.

Our main focus is labor market returns to schooling, but since selective mortality and marriage market effects may represent confounding factors, Appendix Table C4 presents results for these outcomes. Previous work using aggregate data suggests that the compulsory schooling extension associated with a reduction in mortality (cf. Fischer et al., 2013). We find some evidence consistent with the previous findings, but in general our estimates do not suggest that selective mortality is a major issue. Regarding marriage, Becker's theory on the treatise on the family suggests that women with more education are less likely to marry (Becker, 1981). The term extension seems to cause a slight increase in marriage rates, driven by women, but the effect is not significant. The compulsory schooling extension increases male marriage rates and reduces female rates.

Finally, we also perform robustness tests regarding inference. Given that our analysis sample represents almost the entire population, the rationale for statistical significance testing may be questioned. As an alternative basis for inference, we conduct a permutation test. Appendix Section C.4 presents the details and the results for these robustness tests showing that the statistical inference conducted has correct size and power also according to this alternative design.

5.6. Discussion

The empirical analysis has delivered a number of striking results. First, we have shown that the term extension appears to have had much larger effects on labor market outcomes than the compulsory schooling extension: the effect is at least twice as large in the general population and among females. Taken together, our results point strongly in the direction of earlier investments being more effective than later investments: the effects of the term extension are mainly driven by years 1–4. In addition, it cannot be ruled out that the effect of the 7^{th} year on earnings is driven by increased selection into secondary schooling. As the model outlined in Section 2.2 shows, the compulsory schooling extension may provide strong incentives to select secondary education in the absence of human capital effects.

In general, these findings are hardly surprising given the large literature suggesting that earlier investments tend to generate larger returns. However, this point has rarely been made with two independent sources of exogenous variation. What is possibly less obvious is the much larger returns to the term extension for women – the effects are consistently at least twice as large as for males – and this finding merits some further discussion. In general, studies reporting gender-specific effects of education deliver mixed and partly conflicting results. While Devereux and Hart (2010) estimate effects of about 4 per cent for men but zero for women in the U.K, Black et al. (2005) (Norway), Meghir and Palme (2005) (Sweden), Grenet (2013) (France), and Stephens and Yang (2014) (U.S) find comparably-sized effects for men and women. Some studies suggest larger earning effects for women than for men (see e.g. Buscha and Dickson (2012) on returns later in life (U.K), Machin et al. (2012) (Norway) and Aydemir and Kirdar (2017) (Turkey)). Regarding early-life interventions, e.g. Anderson (2008); Havnes and Mogstad (2011) and Bhalotra et al. (2016) find that most of the effect on labor market outcomes is driven by women.

So how can the larger effects for women be explained? One possibility is of course that there are confounders biasing the results, for example if there is measurement error in the male reform assignment leading to attenuation bias. This would be the case for example if men's migration patterns were different from those of women, or if men were more likely to take schooling outside the district of residence. We have not found any evidence suggesting that the gender differences are driven by such factors: migration between ages 6 and 16 was generally negligible for men and women (cf. Online Appendix Figure E2), and the estimated effect on reform assignment on migration is small and insignificant for both sexes (cf. Online Appendix Table E1). Also, Sweden has no tradition of boarding schools.[34]

We can also effectively rule out that the larger effects for women are driven by spillover effects in educational choices. Our model in Section 2.2 shows that only in a case where there is very strong complementarity between term length and secondary schooling would we expect to see effects on secondary schooling uptake. Our empirical results clearly suggest there is no such complementarity: the estimated effects on secondary schooling are small, insignificant, and larger for males (cf. Table 3). This finding does not, however, rule out potentially large effects on skill acquisition; it is consistent with a situation where such gains are present regardless of the school track chosen.

There are a number of findings suggesting that the estimated gains in earnings are driven by an enhancement of skills, and that this effect was disproportionately felt by women. First, treated women increased their employment rates by four percentage points (cf. Table 6). This is already indicative of an improved productivity; although we would not expect men to increase their employment in a similar fashion, given that their employment rates were close to 100% already. Second, despite this expansion of labor supply,

34. In particular there have historically been almost no such alternatives for children of primary schooling ages. The only exceptions are boarding schools for blind children (Statistics Sweden, 1974) and for some children in parishes of *Lapland* (parts of the most northern county) living very far from a Folkskola (Andersson and Johansson, 2013).

female earnings per worker went up by significant amounts, which suggests that a positive wage effect dominates potentially negative selection effects. Third, the treated women disproportionately selected into relatively well-paid occupations heavily reliant on basic academic skills, whereas there is much less evidence of male occupational choices being affected by the reform. Fourth, we deliver descriptive evidence suggesting that the earnings gains were confined to the private sector – where we would expect wages to better reflect productivity – and married women, who were facing larger hurdles in their labor market decisions (cf. Table 7).

Finally, larger productivity gains for women are plausible given that it is term length in school years 1–4 that drive the effect (cf. Table 5). The large literature studying the gender gap in academic achievement (cf. Figlio et al., 2019, for a review of that literature) considers a wide range of factors affecting the increasing gender gap. It has been shown that girls tend to enter school with more advanced non-cognitive skills, which facilitates their academic performance during elementary school (DiPrete and Jennings, 2012). This would also be consistent with the findings of Bingley et al. (2018), who report that increasing the instructional time in early years disproportionately benefits girls.

6. Conclusion

Policymakers and other 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 paper 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 1930–40 and affected by the independent reforms in working and pension age.

Our analysis delivers two important results: (i) extending the average term length from 34.5/36.5 to 39 weeks had sizeable effects on earnings and female employment whereas (ii) the compulsory schooling reform which increased primary education from 6 to 7 years was largely ineffective regarding later-life returns. The term extension also seems to have contributed to the narrowing of gender gap in earnings. It affected broad parts of the population, and even though it may have had small 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. Our paper also provides a positive story as to why such reforms fail to deliver positive results: far from being "untreatable", the compliers to such reforms can benefit from investments undertaken earlier. Our results indicate that it is possible to affect 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 basic skills being 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. This also corroborates findings in the limited but growing literature evaluating the long-run effects of early childhood programs and universal child care: Heckman et al. (2010) show positive employment and earnings effects in their re-examination of the evidence from the Perry preschool program, Havnes and Mogstad (2011) find positive earning effects, especially for women, for children affected by an expansion of subsidized child care in Norway, and Bhalotra et al. (2016) note large labor market effects for women treated by infant care in the 1930s.

Another key finding of our paper is that the term extension contributed to closing the gender gap in labor market outcomes. We find that the reform had an impact on female earnings about twice as large as the effect on male earnings, and this difference is persistent over the life cycle. We are able to rule out a number of possible confounders that might lead to this result, and we also show that it is not driven by changes in educational attainment. Instead, our results suggest that the term extension did have a larger impact on female productivity, which in turn pulled married women back into the labor market and increased their chances of working in relatively well-paid occupations. Our study thus provides one piece of evidence of how Sweden became a paragon of gender equality on the labor market.

Given these large effects of early term extensions on labor market outcomes, a natural question that follows is if the reform also affected health outcomes. Evidence on this topic is scarce; one exception being Parinduri (2017) who does not find any evidence of improved health. The empirical literature on the effects of compulsory schooling extension on health is ambiguous, which may be due to their often limited impact on skills and labor market outcomes. On the other hand, there is empirical evidence suggesting that the education gradient in health is attributable to differences in skills (Bijwaard et al., 2015). Thus, the term extension which was studied here, which appears to have had sizeable effects on skills, might also have led to an improvement in health.

Appendix A: A Roy Model of the Reforms

A.1. General Setup

Adapting a standard Roy model for schooling decisions (cf. Eisenhauer et al., 2015),[35], we consider the decision of enrolling in secondary school. We assume that the potential earnings Y^j after secondary (Y^1) and primary (Y^0) schooling are determined by

$$Y_i^1\left(T_i\right) = \mu_1 + T_i\alpha_i^1 + U_i^1 \tag{A1}$$

$$Y_i^0\left(T_i, Z_i\right) = \mu_0 + T_i\alpha_i^0 + Z_i\beta_i + U_i^0 \tag{A2}$$

where $\mu_j, j \in \{0,1\}$ refers to population means, T_i is a variable taking a value of 1 if individual i was affected by the term length extension, α_i^j is the effect of the term extension on earnings for individuals with qualification j, and U_i^j represents unobserved heterogeneity in earnings potentials by qualification; $Z_i \in \{0,1\}$ represents the compulsory schooling extension and β_i the gains associated therewith.[36] We assume β_i and α_i^j to consist of a mean μ_β (and μ_α respectively) and an unobserved component U_i^β (and $U_i^{\alpha^j}$ respectively). It should be noted that Y_i^0 is a function of both policy variables whereas Y_i^1 only is a function the term length extension T_i. Thus, we assume that the compulsory schooling extension satisfies the SUTVA (*Stable Unit-Treatment Value*; Rubin, 1990) assumption, so that no general equilibrium or spillover effects affect potential earnings of secondary schooling graduates.

The decision of whether to enroll in secondary schooling is not only governed by potential returns but also by the perceived costs. We assume that costs are given by

$$C_i\left(Z_i\right) = \left[\mu_C + U_i^C\right]\left(1 - Z_i\gamma\right) \tag{A3}$$

where μ_C and U_i^C are defined analogously to the above, i.e. population mean and unobserved heterogeneity in costs. $\gamma \in (0,1)$ represents the reduction in the costs of secondary schooling associated with the compulsory schooling extension. We thus assume that an additional year in compulsory school is perceived as a fraction of the cost of going to secondary school, and that this fraction is the same for everyone. A special case would be the situation where $\gamma = 1/3$, which implies that in terms of costs, spending one more year

35. It should be noted that in contrast to us, Eisenhauer et al. (2015) do not impose a functional form on the unobserved determinants of secondary schooling.

36. In our case the school system was a tracking system where tracking took place in grade 4 or grade 6. The additional 7$^{\text{th}}$ compulsory year implied by the compulsory schooling reform happened after any decision about (and matriculation to) secondary schooling was made. Y_i^1 does not depend on Z, but Z affects the probability of enrolling in secondary school.

in compulsory schooling is equivalent to one year of secondary schooling.[37] Without loss of generality, we assume that the costs of secondary schooling are unaffected by the term length extension; we will show below that relaxing this assumption does not change the qualitative predictions of the model.

Next consider the decision to take secondary schooling, which we denote S_i:

$$\begin{aligned} S_i &= 1\left(Y_i^1\left(T_i\right) - Y_i^0\left(T_i, Z_i\right) - C_i\left(Z_i\right) \geq 0\right) && \text{(A4)} \\ &= 1\left(\mu_1 - \mu_0 - \mu^C + U_i^1 - U_i^0 - U_i^C + T_i\left(\alpha_i^1 - \alpha_i^0\right) + Z_i\left(\gamma\left(\mu_C + U_i^C\right) - \beta_i\right) \geq 0\right) && \text{(A5)} \end{aligned}$$

Hence, the observed earnings will be given by

$$\begin{aligned} Y_i &= Y_i^0\left(T_i, Z_i\right) + S_i\left[Y_i^1\left(T_i\right) - Y_i^0\left(T_i, Z_i\right)\right] \\ &= \mu_0 + T_i\alpha_i^0 + Z_i\beta_i + U_i^0 + S_i\left(T_i, Z_i\right)\left[\mu_1 - \mu_0 + T_i\left(\alpha_i^1 - \alpha_i^0\right) - Z_i\beta_i + U_i^1 - U_i^0\right] && \text{(A6)} \end{aligned}$$

implying that the two reforms potentially have direct and indirect effects on earnings.

A.2. Distributional Assumptions

The decision to enroll in secondary schooling depends on individual-level heterogeneity in the perceived benefits and costs of secondary schooling, which in turn depend on the two school reforms. We thus define random variables which represent this heterogeneity under various scenarios: V_i represents the unobserved component of net benefits in the absence of any reform, and V_i^{kl} represents the corresponding component when term length exposure equals k and exposure to the 7[th] year equals l. Exposing the argument of equation (A4) to the possible combinations of Z_i and T_i, we thus get

$$\begin{aligned} V_i &= U_i^1 - U_i^0 - U_i^C && \text{(A7)} \\ V_i^{10} &= U_i^1 - U_i^0 - U_i^C + U_i^{\alpha^1} - U_i^{\alpha^0} && \text{(A8)} \\ V_i^{01} &= U_i^1 - U_i^0 - U_i^C + \gamma U_i^C - U_i^\beta && \text{(A9)} \\ V_i^{11} &= U_i^1 - U_i^0 - U_i^C + U_i^{\alpha^1} - U_i^{\alpha^0} + \gamma U_i^C - U_i^\beta && \text{(A10)} \end{aligned}$$

Analogously, we define four variables W_i^{kl} which represent the total (observed and unobserved) perceived net benefits of secondary schooling under

37. This example is based on a comparison between (i) primary and secondary schooling together lasting nine years (i.e. the five-year track of Realskola) and (ii) compulsory schooling lasting six years; cf. Figure 1.

the various scenarios. Hence,

$$W_i = \mu_1 - \mu_0 - \mu^C + U_i^1 - U_i^0 - U_i^C \tag{A11}$$

$$W_i^{10} = \mu_1 - \mu_0 - \mu^C + \mu_{\alpha^1} - \mu_{\alpha^0} + U_i^1 - U_i^0 - U_i^C + U_i^{\alpha^1} - U_i^{\alpha^0} \tag{A12}$$

$$W_i^{01} = \mu_1 - \mu_0 - \mu^C + \gamma\mu_C - \mu_\beta + U_i^1 - U_i^0 - U_i^C + \gamma U_i^C - U_i^\beta \tag{A13}$$

$$W_i^{11} = \mu_1 - \mu_0 - \mu^C + \mu_{\alpha^1} - \mu_{\alpha^0} + \gamma\mu_C - \mu_\beta + U_i^1 - U_i^0 - U_i^C + U_i^{\alpha^1} - U_i^{\alpha^0} + \gamma U_i^C - U_i^\beta \tag{A14}$$

Finally, we make the following assumption:

$$\left(U_i^1, U_i^0, U_i^C, U_i^{\alpha^1}, U_i^{\alpha^0}, U_i^\beta\right) \sim \mathcal{N}\left(0; \Sigma\right) \tag{A15}$$

It follows from this assumption that all the variables W_i^{kl} and V_i^{kl} are jointly normally distributed; and that the V_i^{kl} variables have mean zero. We denote the cdf's of W_i^{kl} by $F^{kl}\left(\cdot\right)$ and the cdf's of the residuals V_i^{kl} by $\Psi^{kl}\left(\cdot\right)$. We denote the variance of W_i^{kl} and V_i^{kl} by $\sigma^2_{W^{kl}}$.

A.3. Effects of the Reforms on Secondary Schooling

We first consider the secondary schooling decision in the absence of any reform. Denote by $\Psi\left(\cdot\right)$ the cumulative distribution function of V_i. Clearly, the condition for taking on secondary schooling in the absence of reforms is given by the condition $V_i > -\mu_W$. Hence, the proportion of students taking on secondary schooling will be equal to

$$\Pr\left(S = 1 \mid T = 0, Z = 0\right) = 1 - \Psi\left(-\mu_W\right) = \Psi\left(\mu_W\right). \tag{A16}$$

Now, consider the same schooling decision after an extension to the term length (but no compulsory schooling extension). This condition will be governed by the behaviour of W_i^{10}. Also, it should be noted that

$$W_i^{10} = W_i + \alpha_i^1 - \alpha_i^0 = W_i + \mu_{\alpha^1} - \mu_{\alpha^0} + U_i^{\alpha^1} - U_i^{\alpha^0} \tag{A17}$$

Hence,

$$W_i^{10} \sim \mathcal{N}\left(\mu_{W_i} + \mu_{\alpha^1} - \mu_{\alpha^0}; \sigma^2_{W^{10}}\right) \tag{A18}$$

We may therefore compare the number of enrollees before and after the reform by exploiting the relationship $\frac{\sigma_W\left(W_i^{10} - \mu_{\alpha^1} + \mu_{\alpha^0}\right)}{\sigma_{W^{10}}} \sim W_i$. This allows us to formulate the secondary schooling decision in this scenario as follows:

$$\Pr\left(S=1 \mid T=1, Z=0\right) = 1 - F^{10}\left(0\right) = 1 - F\left(-\frac{\sigma_W}{\sigma_{W^{10}}}\left(\mu_{\alpha^1} - \mu_{\alpha^0}\right)\right) \tag{A19}$$

$$= 1 - \Psi\left(-\mu_W - \frac{\sigma_W}{\sigma_{W^{10}}}\left(\mu_{\alpha^1} - \mu_{\alpha^0}\right)\right) \tag{A20}$$

$$= \Psi\left(\mu_W + \frac{\sigma_W}{\sigma_{W^{10}}}\left(\mu_{\alpha^1} - \mu_{\alpha^0}\right)\right) \tag{A21}$$

Comparing this result with the pre-reform enrollment rate given by (A16), we get a simple condition for when the term length extension increases enrollment in secondary schooling: this will happen whenever $\mu_{\alpha^1} - \mu_{\alpha^0} > 0$. This result in turn implies some kind of dynamic complementarity, so that the later investment (secondary schooling) increases the returns to the early investment (term length). In addition, notably, a necessary condition for the term extension to affect selection into secondary schooling is that it has human capital effects, at least for earnings of secondary graduates ($\mu_{\alpha^1} > 0$). Finally, the effect on selection into secondary education is larger if the term extension reduces variation in the earnings gains ($\sigma_{W^{10}} < \sigma_W$).

Next, consider the opposite case where only the compulsory schooling extension is implemented. Now the secondary schooling decision is governed by W_i^{01}, characterized by

$$W_i^{01} \sim \mathcal{N}\left(\mu_{W_i} + \gamma\mu_C - \mu_\beta; \sigma^2_{W^{01}}\right) \tag{A22}$$

Thus, we get:

$$\Pr\left(S=1 \mid T=0, Z=1\right) = 1 - F^{01}\left(0\right) = 1 - F\left(-\frac{\sigma_W}{\sigma_{W^{01}}}\left(\gamma\mu_C - \mu_\beta\right)\right) \tag{A23}$$

$$= 1 - \Psi\left(-\mu_W - \frac{\sigma_W}{\sigma_{W^{01}}}\left(\gamma\mu_C - \mu_\beta\right)\right) \tag{A24}$$

$$= \Psi\left(\mu_W + \frac{\sigma_W}{\sigma_{W^{01}}}\left(\gamma\mu_C - \mu_\beta\right)\right) \tag{A25}$$

This case is quite different from the previous scenario with the term extension. For example, in the absence of human capital effects of the compulsory schooling extension ($\mu_\beta = 0$), there will always be an increase in secondary schooling uptake. Also, the effect on secondary enrolment will be larger whenever the compulsory schooling extension reduces variation in the gains from secondary schooling ($\sigma_{W^{01}} < \sigma_W$). This may happen if, for example, the potential earnings gains from the compulsory schooling extension are negatively correlated with the gains from secondary schooling.

Finally, consider the scenario where both reforms are in place. Now the secondary schooling decision is governed by W_i^{11}. This variable takes on the distribution

$$W_i^{11} \sim \mathcal{N}\left(\mu_{W_i} + \mu_{\alpha^1} - \mu_{\alpha^0} + \gamma\mu_C - \mu_\beta; \sigma^2_{W^{11}}\right) \tag{A26}$$

which in turn leads to the probability

$$\Pr\left(S = 1 \mid T = 1, Z = 1\right) = 1 - F^{11}\left(0\right) = 1 - F\left(-\frac{\sigma_W}{\sigma_{W^{11}}}\left(\mu_{\alpha^1} - \mu_{\alpha^0} + \gamma\mu_C - \mu_\beta\right)\right) \tag{A27}$$

$$= 1 - \Psi\left(-\mu_W - \frac{\sigma_W}{\sigma_{W^{11}}}\left(\mu_{\alpha^1} - \mu_{\alpha^0} + \gamma\mu_C - \mu_\beta\right)\right) \tag{A28}$$

$$= \Psi\left(\mu_W + \frac{\sigma_W}{\sigma_{W^{11}}}\left(\mu_{\alpha^1} - \mu_{\alpha^0} + \gamma\mu_C - \mu_\beta\right)\right) \tag{A29}$$

A.4. Allowing the Term Length Extension to Affect Costs

Finally, we consider the consequences of allowing for an effect of the term length extension on the costs of secondary schooling. For this extension, we introduce an additional variable which we denote δ_i.[38] The cost function will thus take on the form

$$C_i'\left(T_i, Z_i\right) = \left[\mu_C + U_i^C - T_i\delta_i\right]\left(1 - Z_i\gamma\right) \tag{A30}$$

which in turn leads to a new set of arguments W'^{ij} in the schooling decision:

$$W_i' = W_i \tag{A31}$$

$$W_i'^{10} = W_i^{10} + \delta_i \tag{A32}$$

$$W_i'^{01} = W_i^{01} \tag{A33}$$

$$W_i'^{11} = W_i^{11} + \left(1 - \gamma\right)\delta_i \tag{A34}$$

38. In what follows, we consider δ_i fixed, as the point we are making becomes obvious without any assumptions regarding the distribution of δ_i.

Hence, for a given δ_i the relationships identified above would still apply:

$$\frac{\sigma_W\left(W_i'^{10}-\mu_{\alpha^1}+\mu_{\alpha^0}-\delta_i\right)}{\sigma_{W^{10}}}\sim W_i$$
$$\frac{\sigma_W\left(W_i'^{11}-\mu_{\alpha^1}+\mu_{\alpha^0}-\gamma\left(\mu_C+\delta_i\right)\right)}{\sigma_{W^{11}}}\sim W_i$$

As a consequence, the secondary schooling decision in equation (A21) is affected as follows:

$$\Pr\left(S=1\mid T=1,Z=0\right)=\Psi\left(\mu_W+\frac{\sigma_W}{\sigma_{W^{10}}}\left(\mu_{\alpha^1}-\mu_{\alpha^0}+\delta_i\right)\right) \tag{A35}$$

and the secondary schooling decision in equation (A29) is affected as follows:

$$\Pr\left(S=1\mid T=1,Z=1\right)=\Psi\left(\mu_W+\frac{\sigma_W}{\sigma_{W^{11}}}\left(\mu_{\alpha^1}-\mu_{\alpha^0}+\gamma\mu_C-\mu_\beta+\left(1-\gamma\right)\delta_i\right)\right) \tag{A36}$$

It becomes clear from equations (A35) and (A36) that δ_i, the reduction in costs of schooling brought about by the term extension, enter the schooling decision in the same way as the human capital gains α^1. If the compulsory schooling extension is already in place, as in equation (A36), the effect is diminshed by factor $(1-\gamma)$ since the reduced costs are assumed to apply equally to both tracks of the school system. Nevertheless, it becomes clear that little is gained in terms of insights by adding δ_i to the model, and therefore we maintain our assumption that there are no relevant effects of the term extension on the costs of schooling.

Appendix B: Tables and Figures

B.1. Descriptive Statistics Aggregate Data

Table B1. Descriptive Statistics

	Mean	Std.Dev.	Obs
ELECTION DATA			
Number of voters	1,658	10,065	7,209
Election turnout	0.716	0.13	7,157
Conservative vote	0.150	0.10	7,152
Agrarian vote	0.288	0.18	7,152
Liberal vote	0.121	0.10	7,152
Labour vote	0.404	0.18	7,152
Communist vote	0.032	0.05	7,152
SCHOOL DISTRICT DATA			
Distance secondary school	21.002	19.00	27,742
Population Density	0.920	3.30	43,048
Share poor	0.052	0.04	39,859
Property per capita	3,356	1,575	41,552
taxable income per cap.	939	565	40,016
Debt/Asset ratio per cap.	0.807	14.87	28,291

Notes: Descriptive statistics for balancing variables. Own calculations.

B.2. Balancing Tests

Below we present balancing tests for household characteristics and for school district characteristics (Table B2). Panel A 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 (10), the magnitudes for these characteristics are reduced a lot and all but one turn insignificant.

Panel B shows that the reform correlates with population size (proxied by the number of voters) and political preferences. In our baseline specification, the magnitudes of these correlations are diminished and all variables lose their statistical significance but one, which remains significant at the ten per cent level. It should be noted that the specification with district trends performs *worse* than our baseline specification for these variables: differences in vote shares of the agrarian and communist parties, which are eliminated in our baseline specification, become significant when trends are added. The same goes for the variable capturing the distance to the nearest secondary school in the following panel which shows how well some key indicators extracted from municipality yearbooks are balanced.

Clearly, both reforms correlated with the yearbook indicators presented in Panel C – poverty, property values, and taxable earnings. However, our baseline specification eliminates all these differences.

Table B2. Balancing Tests

	Mean	**Term Length Extension**			**Mean**	**7-Year Extension**		
A. Individual Level								
Household Head Male	0.857	-0.046***	-0.003	-0.018	0.857	-0.000	0.003	0.005
		(0.007)	(0.008)	(0.013)		(0.002)	(0.002)	(0.003)
High SES	0.209	0.186***	0.001	0.002	0.209	0.058***	-0.000	-0.002
		(0.040)	(0.008)	(0.014)		(0.008)	(0.002)	(0.003)
Household Size	3.807	-0.154**	0.029	-0.044	3.807	-0.140***	-0.004	-0.002
		(0.069)	(0.023)	(0.040)		(0.020)	(0.007)	(0.008)
HHhead Academic	0.040	0.048***	-0.003	-0.003	0.040	0.014***	-0.002**	-0.001
		(0.010)	(0.004)	(0.006)		(0.002)	(0.001)	(0.001)
Age at Birth HHead	32.983	-2.910***	-0.231	0.170	32.983	-0.649***	-0.034	0.022
		(0.373)	(0.168)	(0.290)		(0.080)	(0.047)	(0.058)
B. Election Data								
Number of voters	1,658	1887.167***	-123.726	-21.106	1,658	1514.165***	-98.095*	48.933
		(536.127)	(96.641)	(25.974)		(535.196)	(57.878)	(39.224)
Election turnout	0.716	0.001	-0.005	-0.001	0.716	0.028***	0.009	-0.001
		(0.003)	(0.009)	(0.007)		(0.004)	(0.008)	(0.006)
Conservative vote	0.150	-0.022***	0.002	-0.002	0.150	-0.014***	-0.000	0.002
		(0.004)	(0.001)	(0.002)		(0.004)	(0.002)	(0.003)
Agrarian vote	0.288	-0.093***	0.001	0.007**	0.288	-0.049***	-0.002	-0.001
		(0.007)	(0.002)	(0.003)		(0.007)	(0.002)	(0.004)
Liberal vote	0.121	-0.007*	-0.002	-0.003	0.121	-0.011***	0.002	-0.001
		(0.004)	(0.001)	(0.002)		(0.004)	(0.002)	(0.003)
Labour vote	0.404	0.104***	0.002	0.006	0.404	0.069***	-0.000	0.002
		(0.006)	(0.002)	(0.004)		(0.007)	(0.003)	(0.005)
Communist vote	0.032	0.018***	-0.001	-0.007***	0.032	0.004*	-0.001	-0.001
		(0.002)	(0.001)	(0.003)		(0.002)	(0.002)	(0.004)
C. School District Level								
Distance sec school	21.339	-0.542	0.274	0.542***	21.339	-8.746***	-0.499	-0.413
		(0.815)	(0.286)	(0.186)		(0.862)	(0.466)	(0.497)
Population density	0.920	0.679***	-0.002	-0.007	0.883	0.561***	-0.045	-0.046
		(0.088)	(0.014)	(0.013)		(0.094)	(0.028)	(0.032)
Share poor	0.052	0.014***	-0.000	-0.000	0.051	-0.002**	-0.000	-0.000
		(0.001)	(0.000)	(0.000)		(0.001)	(0.000)	(0.000)
Property per capita	3,356	350.351***	-8.515	-18.106	3,298	435.870***	-15.058	-19.323
		(42.860)	(14.276)	(13.871)		(37.276)	(13.047)	(14.380)
taxable income per cap.	939	191.288***	1.841	-2.246	928	113.732***	6.307	1.366
		(10.819)	(5.978)	(5.715)		(9.527)	(4.305)	(4.596)
Debt/Asset ratio per cap.	0.807	0.003	0.351	0.123	1.005	0.002	-0.175	-0.532
		(0.205)	(0.456)	(0.458)		(0.275)	(0.526)	(0.826)
Cohort FE		✓	✓	✓		✓	✓	✓
District FE			✓	✓			✓	✓
County Trends			✓				✓	
District Trends				✓				✓

Notes: Robust standard errors clustered at school district level are reported in parenthesis. Significance levels: $*$ 0.10 $**$ 0.05 $***$ 0.01. *Panel A: Individual Level Data* Parental background variables from our sample cohorts 1930-1940. *Panel B: Election Data* Aggregate election data for years 1936-1944. *Panel C: School District Level* Aggregate data on school district level for 20 year window around reform year.

B.3. Balancing Sex Stratified

TABLE B3. Balancing Tests Sex Stratified

	Mean	Term Length Extension			Mean	7-Year Extension		
A. INDIVIDUAL LEVEL - MALES								
Household Head Male	0.858	-0.034***	-0.012	-0.018	0.858	0.002	0.003	0.005
		(0.006)	(0.011)	(0.019)		(0.003)	(0.003)	(0.004)
High SES	0.206	0.224***	-0.008	-0.034*	0.206	0.039***	-0.002	-0.003
		(0.032)	(0.010)	(0.019)		(0.007)	(0.003)	(0.004)
Household Size	3.815	-0.333***	0.024	-0.010	3.815	-0.093***	-0.003	0.010
		(0.050)	(0.032)	(0.060)		(0.014)	(0.009)	(0.012)
HHhead Academic	0.040	0.051***	-0.006	-0.010	0.040	0.010***	-0.001	0.000
		(0.009)	(0.005)	(0.009)		(0.002)	(0.001)	(0.002)
Age at Birth HHead	32.978	-3.007***	-0.269	-0.180	32.978	-0.391***	-0.031	-0.008
		(0.316)	(0.235)	(0.426)		(0.080)	(0.064)	(0.084)
B. INDIVIDUAL LEVEL - FEMALES								
Household Head Male	0.856	-0.035***	0.007	-0.009	0.856	0.001	0.003	0.004
		(0.008)	(0.011)	(0.020)		(0.003)	(0.003)	(0.005)
High SES	0.212	0.233***	0.009	0.042*	0.212	0.039***	0.002	-0.000
		(0.029)	(0.013)	(0.024)		(0.007)	(0.004)	(0.005)
Household Size	3.798	-0.322***	0.026	-0.100	3.798	-0.083***	-0.001	-0.011
		(0.053)	(0.035)	(0.064)		(0.014)	(0.010)	(0.014)
HHhead Academic	0.040	0.051***	-0.000	0.004	0.040	0.006***	-0.004**	-0.001
		(0.007)	(0.005)	(0.011)		(0.002)	(0.002)	(0.002)
Age at Birth HHead	32.989	-2.948***	-0.148	0.673	32.989	-0.481***	-0.050	0.092
		(0.333)	(0.225)	(0.442)		(0.081)	(0.066)	(0.088)
Cohort FE		✓	✓	✓		✓	✓	✓
District FE			✓	✓			✓	✓
County Trends			✓				✓	
District Trends				✓				✓

Notes: Robust standard errors clustered at school district level are reported in parenthesis. Significance levels: $*$ 0.10 $**$ 0.05 $***$ 0.01. *Panel A: Individual Level Data* Parental background variables from our sample cohorts 1930-1940. *Panel B: Election Data* Aggregate election data for years 1936-1944. *Panel C: School District Level* Aggregate data on school district level for 20 year window around reform year.

B.4. Event Studies

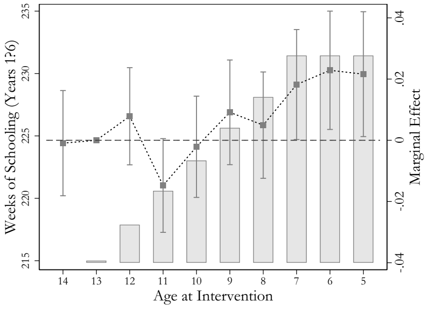
Additionally to the main regression results, we show visually that the outcome variables follow the expected trajectory around the reform year. We estimate the following event study regression

$$\begin{aligned} y_{ijk} \;=\;& \beta_0 + I(AgeAtReform_{ijk} < 5)\gamma + \sum_{a=5}^{14} I(AgeAtReform_{ijk} = a)\delta^a \\ & + I(AgeAtReform_{ijk} > 14)\xi + \beta_2 Z_{jk} + \nu_k + \mu_{R_k j} + C_k \cdot j + \varepsilon_{ijk} \end{aligned} \tag{B1}$$

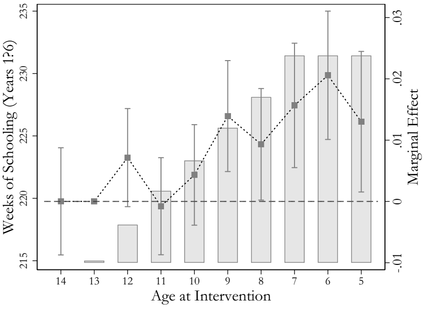
where $I(AgeAtReform_{ijk} = a)$ is an indicator equal to one for individuals being of age a at the year of the 39 weeks term length extension in school district k. We include age 5–14 with age 13 as reference category. γ captures effects for children being less than 5 years at the term length extension. ξ captures the effect of individuals being older than 14 years. The other regressors are unchanged compared to our baseline specification in equation 10.

Our event study specification allows us to investigate the effects along the number of years of exposure. Individuals aged 7 or less at the time of the reform implementation gained the full extension during their primary school attendance. Individuals born earlier and thus older at the year of the reform implementation gained fewer years of extended instructional time. Students aged 13 years or older were too old at the time of the reform implementation to receive an increased term length.

Figure B1 presents the 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. The results also tentatively suggest that increases are more concentrated for children receiving the term length extension in their early school years. On the contrary are effects in the later years small and statistically insignificant. However, it needs to be stressed out that each effect is identified by very small changes in education of about 3 weeks. Figure B2 presents event studies for the compulsory schooling extension.

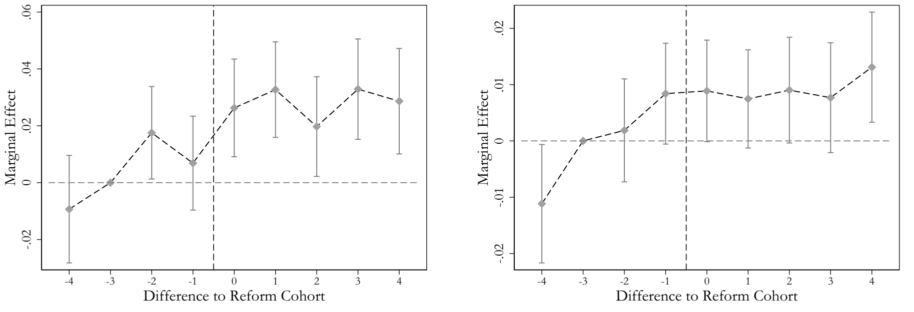


(a) Log 1970 Earnings



(b) Log Pensions 2006–2013

Figure B1. 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.



(a) Log 1970 Earnings (b) Log Pensions

FIGURE B2. Event Study Graphs: 7-Year Reform. The figure shows the coefficients and 95% confidence intervals from an event-study regression. Results refer to cohorts 1930 to 1940. Robust standard errors are clustered at school district level.

Appendix C: Robustness Checks

C.1. Alternative Specifications

Table C1. Main Results: Log 1970 Earnings

	(1) Base	(2)	(3)	(4)	(5)	(6)	(7)
A. Males & Females							
Compulsory 7-Year	0.020***	0.020***	0.020***	0.019***	0.021***	0.012*	0.020***
	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)	(0.008)	(0.006)
Term Length	0.050***	0.051***	0.046**	0.048**	0.049***	0.042	0.048***
	(0.018)	(0.018)	(0.018)	(0.019)	(0.018)	(0.033)	(0.018)
B. Males							
Compulsory 7-Year	0.016***	0.016***	0.014***	0.017***	0.016***	0.001	0.016***
	(0.004)	(0.004)	(0.005)	(0.005)	(0.004)	(0.005)	(0.004)
Term Length	0.024*	0.024*	0.025*	0.025*	0.026*	0.006	0.023*
	(0.014)	(0.014)	(0.014)	(0.014)	(0.014)	(0.028)	(0.014)
C. Females							
Compulsory 7-Year	0.026**	0.027**	0.027**	0.023*	0.029**	0.028	0.027**
	(0.013)	(0.013)	(0.013)	(0.014)	(0.013)	(0.018)	(0.013)
Term Length	0.095**	0.099**	0.084*	0.091**	0.093**	0.105	0.092**
	(0.043)	(0.043)	(0.043)	(0.044)	(0.043)	(0.075)	(0.043)
Quadratic County Trends		✓					
Np. County Trends			✓				
Fully Interacted Trends				✓			
Household Control Variables					✓		
Linear District Trends						✓	
Stockholm							✓

Notes: The 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) Linear school district trends; (7) Adding more Stockholm. *Source:* Linked 1970 Census. Own calculations.

Table C2. Main Results: Log Pensions 2006-2013

	(1) Base	(2)	(3)	(4)	(5)	(6)	(7)
A. Males & Females							
Compulsory 7-Year	0.006**	0.007**	0.007**	0.007**	0.007**	-0.000	0.006**
	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.004)	(0.003)
Term Length	0.026***	0.029***	0.026***	0.026**	0.026***	0.014	0.026***
	(0.010)	(0.010)	(0.010)	(0.010)	(0.010)	(0.017)	(0.010)
B. Males							
Compulsory 7-Year	0.011**	0.011**	0.009**	0.012***	0.012***	0.003	0.011**
	(0.004)	(0.004)	(0.005)	(0.005)	(0.004)	(0.006)	(0.004)
Term Length	0.018	0.020	0.020	0.020	0.021	-0.014	0.017
	(0.015)	(0.015)	(0.015)	(0.015)	(0.015)	(0.028)	(0.015)
C. Females							
Compulsory 7-Year	0.003	0.003	0.005	0.003	0.003	-0.003	0.003
	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)	(0.005)	(0.004)
Term Length	0.037***	0.040***	0.033**	0.033**	0.034***	0.044*	0.036***
	(0.013)	(0.013)	(0.013)	(0.014)	(0.013)	(0.023)	(0.013)
Quadratic County Trends		✓					
Np. County Trends			✓				
Fully Interacted Trends				✓			
Household Control Variables					✓		
Linear District Trends						✓	
Stockholm							✓

Notes: Table shows reduced form effects on pensions. 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 2006-2013. **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) Linear school district trends; (7) Adding more Stockholm. *Source:* Linked 1970 Census. Own calculations.

Table C3. Main Results: Working

	(1) Base	(2)	(3)	(4)	(5)	(6)	(7)
A. Males & Females							
Compulsory 7-Year	0.001	0.001	0.001	0.000	0.000	-0.000	0.001
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.003)	(0.002)
Term Length	0.017**	0.016**	0.016**	0.018**	0.014*	0.025	0.016**
	(0.008)	(0.008)	(0.008)	(0.008)	(0.008)	(0.015)	(0.008)
B. Males							
Compulsory 7-Year	-0.001	-0.001	-0.001	-0.001	-0.001	-0.002	-0.001
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
Term Length	-0.004	-0.004	-0.003	-0.002	-0.004	0.009	-0.004
	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)	(0.010)	(0.005)
C. Females							
Compulsory 7-Year	0.003	0.003	0.003	0.002	0.002	0.003	0.003
	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)	(0.006)	(0.005)
Term Length	0.040***	0.040***	0.039**	0.042***	0.037**	0.038	0.040**
	(0.015)	(0.015)	(0.016)	(0.016)	(0.016)	(0.032)	(0.015)
Quadratic County Trends		✓					
Np. County Trends			✓				
Fully Interacted Trends				✓			
Household Control Variables					✓		
Linear District Trends						✓	
Stockholm							✓

Notes: Table shows reduced form effects on working 1970. 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:** *Working* in 1970 from Census. **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) Linear school district trends; (7) Adding more Stockholm. *Source:* Linked 1970 Census. Own calculations.

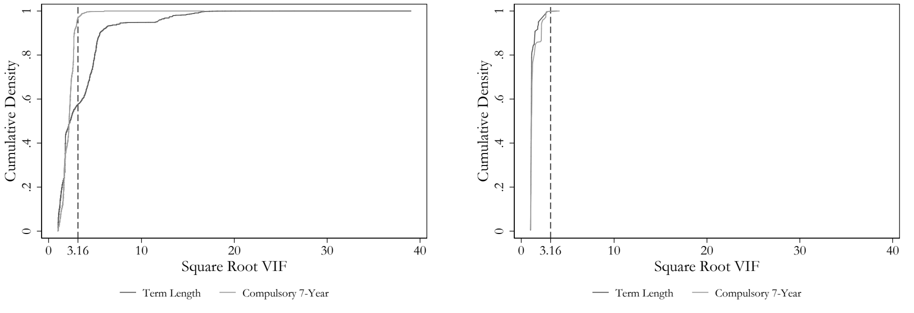
C.2. Collinearity

In order to empirically investigate the collinearity problem stemming from linear district trends, we calculate the variance inflation factor (VIF) between year of birth and treatments on school district level. For each school district we estimate

$$VIF_{T,k} = \frac{1}{1 - R^2_{T,k}}$$

where $R^2_{T,k}$ is the R^2 for the regression of term length T on year of birth, sex and Z. As a rule of thumb inflation factors > 10 are a signal of high multicollinearity (standard errors are $\sqrt{10}$ times larger in contrast of being orthogonal). Figure C1 plots the cumulative density of the $\sqrt{VIF}$ for all school districts and shows, as expected, that collinearity is a major concern for the term length extension. In more than 40% of the school districts the $\sqrt{VIF}$ exceeds the value of $\sqrt{10} = 3.16$, indicating a high degree of collinearity. However, as expected, collinearity stemming from linear district trends is not an issue for the compulsory schooling extension.

The above results likely explain the decrease in efficiency of our term extension estimates using linear district specific cohort trends. In our baseline specification, collinearity is likely less of an issue. There could however be problems of collinearity at the county level as we control for trends on this level in our baseline specification. In Figure C1(b) we calculate VIF on the level of 25 counties for rural and urban areas separately. There is no indication of collinearity problems for either of the treatment variables in our baseline specification.



(a) District specific trends (b) Baseline

FIGURE C1. Variance Inflation Factor. The figure shows the cumulative density function for the the square root variance inflation factor for both treatment variables on school district level. Variance inflation factors are estimated by $VIF_{X,k} = 1/(1 - R^2_{X,k})$ with $R^2_{X,k}$ is the R^2 for the regression of treatment $X \in \{T, Z\}$ on year of birth, sex and the other treatment.

C.3. Survival and Marriage

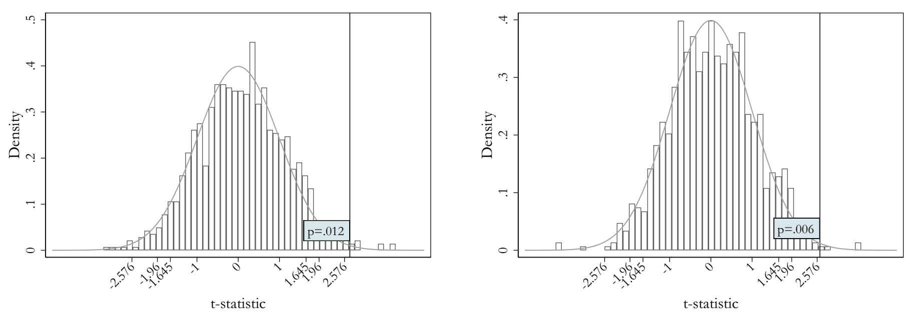
Table C4. Further Outcomes

	(1)	(2)	(3)	
	Mean		Term Length	7-Year
	Earnings	*Outcome*	Extension	Extension
A. Males & Females				
Death Prior Census 1970	-	0.017	-0.000	-0.000
			(0.003)	(0.001)
Death prior Age 73	-	0.225	0.013	-0.000
			(0.009)	(0.003)
Married	209,410	0.861	0.005	0.001
			(0.008)	(0.002)
B. Males				
Death Prior Census 1970	-	0.023	-0.001	-0.000
			(0.004)	(0.001)
Death prior Age 73	-	0.274	0.019	-0.005
			(0.013)	(0.004)
Married	260,776	0.826	-0.003	0.008**
			(0.012)	(0.003)
C. Females				
Death Prior Census 1970	-	0.011	-0.000	0.000
			(0.003)	(0.001)
Death prior Age 73	-	0.172	0.006	0.005
			(0.011)	(0.004)
Married	115,610	0.899	0.012	-0.006**
			(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.

C.4. Randomization Inference

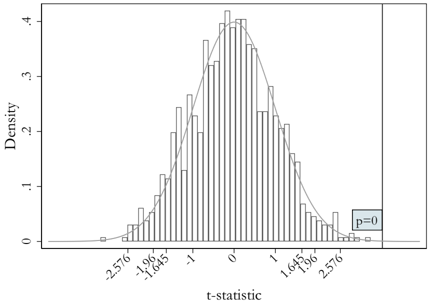
Given that our analysis sample represents almost the entire population, the rationale for statistical significance testing may be questioned. As an alternative basis for inference, we conduct a permutation test where, for each school district, the reform years of the two interventions were randomly drawn from the empirical distribution in 1,000 permutations.[39] Figure C2 shows how our main estimated t statistic, represented by the grey 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. 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 larger 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 C3).



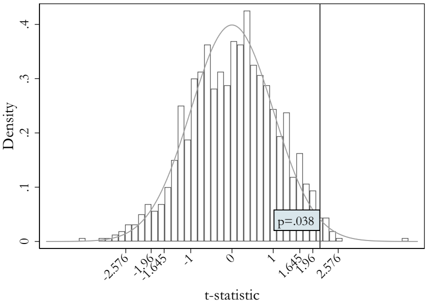
(a) Log Earnings (b) Log Pensions

FIGURE C2. Randomization Inference: Term Length Extension. 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 1930–1940. Regressions are based on our preferred specification using county trends. Robust standard errors are clustered at school district level. *Source:* Linked 1970 Census. Own calculations.

39. 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 (MacKinnon and Webb, 2016).This holds in particular if the clusters are unequally distributed (which applies in our case) or small in number (which is not the case in our study).



(a) Log Earnings



(b) Log Pensions

Figure C3. Randomization Inference: Compulsory Schooling Extension. 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 1930–1940. Regressions are based on our preferred specification using county trends. Robust standard errors are clustered at school district level. *Source:* Linked 1970 Census. Own calculations.

For Online Publication

Appendix D: Primary Education and Compulsory Schooling in Sweden

Sweden has a long-lasting tradition of compulsory education. Already in 1842, church parishes[40] were obliged to offer schooling by an approved teacher and children in *school age* had to attend the local primary schools (*Folkskola*) (Fredriksson, 1971).[41] 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[42] implied great variation in the actual time spent in school by children in school age across Sweden. By the late 1860s the majority of 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.[43] 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

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

41. 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)

42. 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.

43. *Utbildningsplanen* was a governing document and included time-tables and syllabuses for compulsory education (**?**) and remained intact until the 1950's.

he/she was registered as a resident.[44] Parents were responsible for their children's fulfillment of compulsory school attendance and had to report to the school district board that they had a child in school age. §51 of the 1930 "Royal Decree of the Folkskola" 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.[45] The legal, administrative and pedagogical control of the school district was handled by regional school inspectors appointed by the Ministry of Ecclesiastical affairs.[46] 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[47] that a seventh school year *could* be made compulsory in a school district (Fredriksson, 1950). However, only very few districts introduced a mandatory seventh year and still *Folkskola* generally covered six years.[48]

With regard 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, after which 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 (*Fortsättningsskola*) 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.[49]

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44. 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.

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

46. 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).

47. Paragraph 47 mom. 4.

48. The most southern Swedish region Scania and some larger cities were early birds and introduced a compulsory seventh grade during the 1920's. 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.

49. In a massive educational reform in the 1950's and 1960's the old primary and lower secondary school system was replaced by a high-school type comprehensive school system. The

The majority of students in the 1920s–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 and Lundgren, 1986).

D.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,[50] the national government decided that seven-year schooling should be made compulsory in 1936.[51] 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.

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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); Lager and Torssander (2012); Lundborg et al. (2014); Hjalmarsson et al. (2015). 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.

50. 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 (Stråfelt, 1930, p.823) Sweden is *lagging* behind with respect to the length of compulsory schooling compared to other countries in Europe.

51. 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).

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*.[52] 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, 1935a).[53]

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 materials.[54] 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).[55]

52. *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, 1935a)(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, 1935a) (p.54)

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

54. The regional school inspectors stated what study materials and books could be used in *Folkskolan* (Ecklesiastikdepartementet, 1935b) 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).

55. 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.

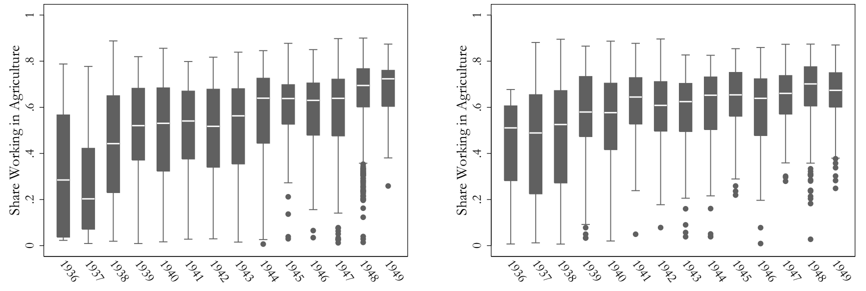
D.2. Extensions of the Term Length

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 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).[56]

The box plots in Figures D1(a) and D1(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.

56. 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.



(a) Introduction of the 7th Grade. (b) Term Length Extension

Figure D1. 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.

Appendix E: Reform Assignment

This section provides a critical assessment of the reform assignment we use in the analysis – parish of residence in 1946 – and the alternative of using parish of birth. We demonstrate that migration during schooling age is unlikely a concern for our analysis and also show why the parish/district of birth is an inferior alternative to assign our treatment. Parishes and school districts normally coincide and differ from each other only in rare cases. Thus, our findings on parishes can be directly transferred to school districts used in our empirical analysis.

E.1. Hospital Births

For cohorts born before 1930 the dominant mode of delivery was home births, while during the 1930s the norm became to give birth in an specialized institution such as a labor ward or birthing center. If the birth took place in a hospital, Swedish administrative data recorded the location of the hospital as place of birth. In 1947 this was changed to the parish of residence of the parents. Thus, before 1947 and with rising shares of institutionalized births, it is unclear whether the recorded parish of birth captures parental parish of residence or location of the hospital.

Figure E1 exemplifies this for the city of Motala. After the opening of a labor ward in the hospital in 1927 the recorded births in the city grew rapidly while the parishes around the city recorded fewer newborns. Children living around the city, but born in the newly established hospital were now recorded as born in Motala. The period between 1927 and 1947 is characterized by excess births from a general trend in births in Motala.

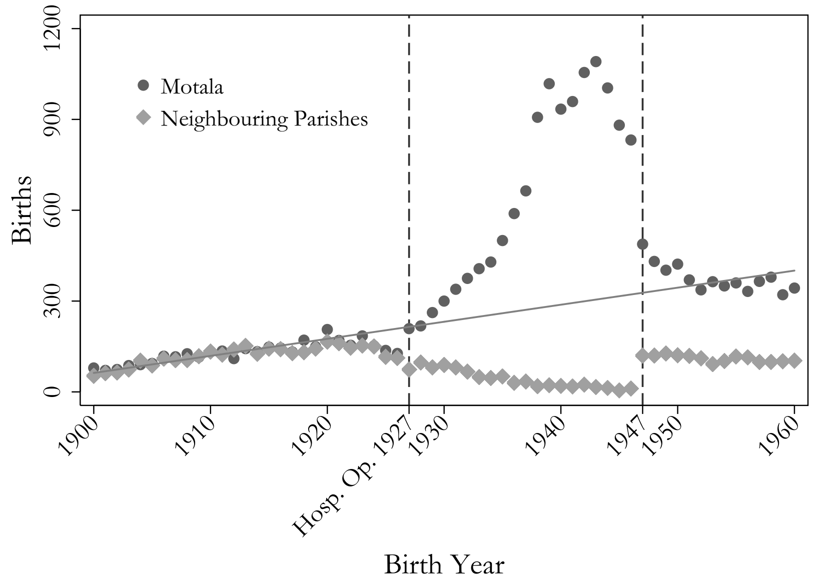


FIGURE E1. Recorded Births: Motala. Neighbouring are defined as parishes with a common boarder to the city of Motala. In 1927 a labor ward was opened in the city of Motala. In 1947 the coding in Swedish administrative data was changed to the place of residence of the mother as place of birth. Between 1927 to 1947 individuals born in the Motala labor ward but residing in places around the city were recorded as born in Motala.
The fitted line is based on births before 1927 and after 1946 and captures the trend in births in Motala without the effects of the hospital labor ward. Source: Fischer et al. (2016).

E.2. Migration During Schooling Age

We now show that migration during schooling age is a rare phenomenon and that our assignment based on the place of residence in 1946 is a reasonable choice. Migration mainly occurs during the first years of life and between ages 16–30. For children of schooling age and for older adults, migration appears to be a rare phenomenon. To underline this empirically we estimate a survival curve based on the Census 1950. We define non-migrants at age a as

$$NonMigrant(Age = a) = I(PoB = PoR(a)) \tag{E1}$$

with PoB being the parish of birth and $PoR(a)$ being the parish of residence at age a.

Figure E2 gives the estimated migration pattern. In order to isolate effects only from migration, we restrict the sample to parishes without a labor ward or birthing center (see Appendix E.1). The migration pattern between age 6–16 and for adults older than 35 is almost flat. This indicates that families with children of schooling age are unlikely to migrate. Our cohorts are between age 6–16 in 1946. Place of residence in 1946 should thus give a very reasonable approximation of their actual parish of residence during schooling age.

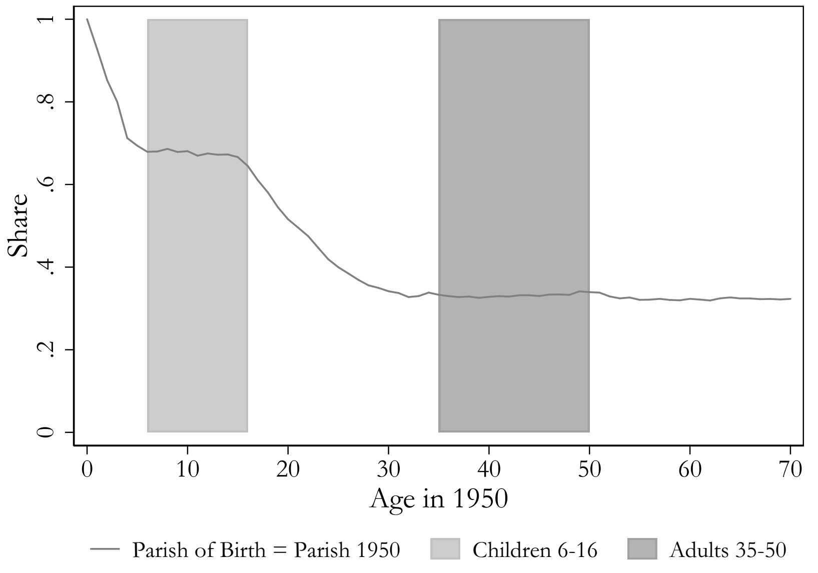


Figure E2. Migration from Parish of Birth. This graph shows the share of individuals still residing in their parish of birth in 1950. It represents a survival curve. The sample is restricted to parishes without a labor ward or birthing center.*Source*: 1950 Swedish Census.

Estimating the same survival curve for parishes with a labor ward or birthing center in Figure E3 shows the quantitative importance of the hospital recording. In addition to migration, the parish of birth is now possibly confounded by the recording of the hospital location as parish of birth. The low overlap between parish of birth and parish 1950 of only 40% clearly warrants against the use of the parish of birth for cohorts born after 1930 (age 20 or younger in 1950). The figure also includes the overall share of individuals born in parishes with a labor ward or birthing center. This share is rising up to almost 90% in 1946 (age 4 in 1950), suggesting that a majority of the population is born in a hospital at this time. Note that after 1946 (age 3 or younger in 1950) the share of hospital births drops sharply and the survival indicator of non-migration shows the exact same pattern as for parishes without a labor ward or birthing center in Figure E2.

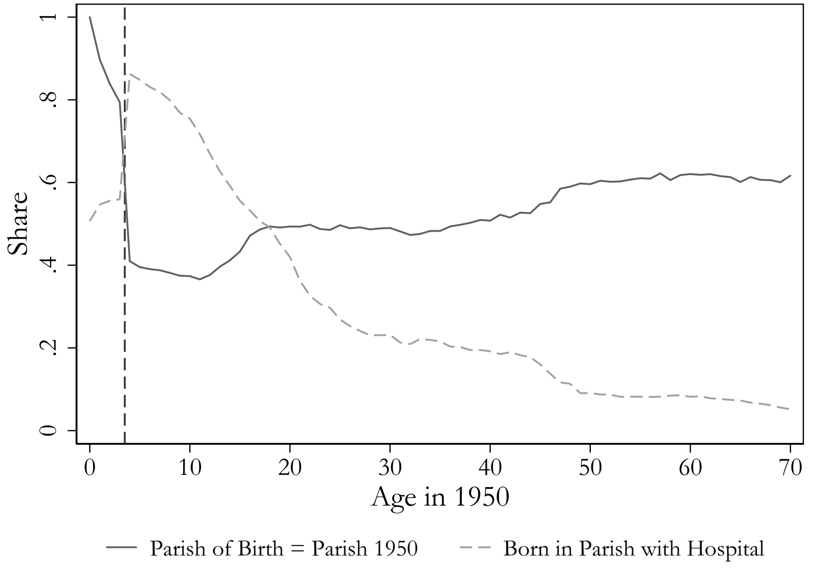


FIGURE E3. Hospital Births. The solid line shows the share of individuals still residing in their parish or birth in 1950. It represents a survival curve. The sample is restricted to parishes with a labor ward or birthing center. The dashed curve represents the overall share of individuals born in a parish with a labor ward or birthing center.
A vertical line at age 3 shows cohort for which the recording has been changed to the parish of residence of the parents at birth.
Source: 1950 Swedish Census.

We also investigate whether exposure to the reform induced migration. Therefore we estimate the effects of our treatment on the probability of migrating between birth and 1946. In this exercise we assign treatment based on the parish of birth. If migration is induced by the reform this would be the correct pre-treatment assignment. Due to the hospital registration this is only meaningful for parishes without a labor ward or birthing center. Table E1 shows that our treatment effects are not correlated with migration.

Table E1. Migration

	Pooled	Males	Females
7-Year Extension	0.003	0.008	-0.001
	(0.004)	(0.005)	(0.006)
Term Extension	-0.022	-0.032	-0.010
	(0.017)	(0.022)	(0.024)
Observations	261,345	136,480	124,809
Districts	2,150	2,123	2,104

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. The sample is restricted to parishes without a labor ward or birthing center. All regressions include rural/urban birth cohort FE, linear county trends, district fixed effects and a gender dummy if pooled regression. Treatment assignment is based on the parish of birth. **Dependent Variables:** Migration between Parish of Birth and Parish in 1946.

Appendix F: The Swedish Labor Market

F.1. Measuring Returns to Education

To estimate the returns to education of the term extension and the compulsory schooling extension, respectively, we use administrative data on annual labor earnings in 1970 and annual pensions as dependent variables. To assess the reliability of these income measures we compare our estimates to standard Mincer wage regressions using information on hourly wages from the LNU survey 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 F1 compares OLS estimates for returns to education, with hourly wage regressions as the benchmark presented in the upper panel.

Table F1. 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 F1 suggests our earnings measure gives very reasonable estimates regarding the returns to education in a Mincerian framework for males. This is expected as it is well documented that labor earnings for men in their 30s constitute an extremely good proxy for their life-cycle earnings. For women estimates for 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 the largest for women in 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 LNU survey estimates. Given that pensions are based on the *best* 15 income years it is reasonable that labor supply frictions are minimized.

F.2. Labor Market Entrance

The LNU Survey (Swedish Level of Living Survey) provides descriptive evidence indicating that opportunities to enter the formal labor market for adolescents in the ages 12 to 14 were likely low in the 1930s and 1940s and that labor market entry rules were enforced. Figure F1 shows that even before the compulsory schooling reform only a negligible share of students entered regular employment before the age of 14. It is thus unlikely that the reform induced a substantial loss in labor market experience. The graph suggests that a very small fraction (1-2%) of students increased their working start age from 13 to 14 over the reform period. Given that more than 70% of individuals took only compulsory schooling, this is a tiny fraction.

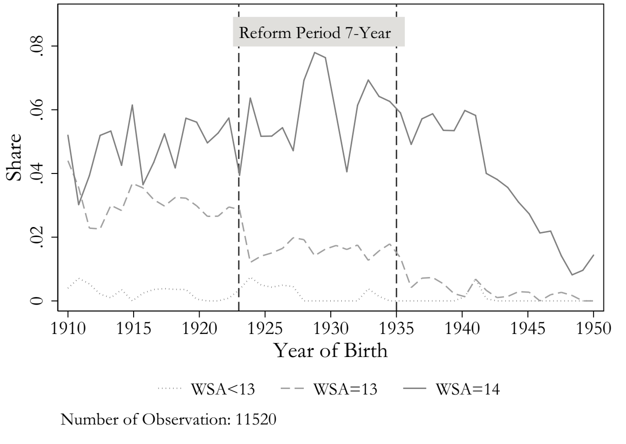
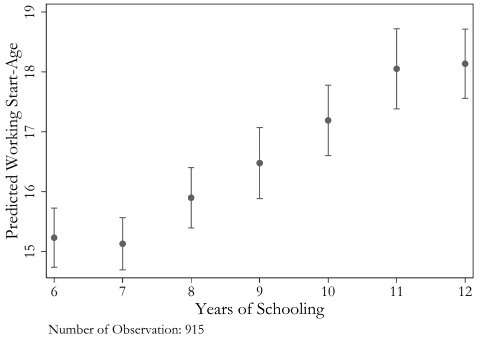


Figure F1. Working Start Age. The figure shows the share of individuals starting their first *real* job at a given age for cohorts born 1910–1950. Questionaire: *Vilket år började ni förvärvsarbeta på riktigt, alltså inte feriearbete, praktik eller tillfälligt beredskapsarbete?(Translation: In what year did you start your first real job, i.e. not summer job, internship or temporary public relief work?*) The compulsory schooling extension mainly affected individuals born between 1923-1935 (as indicated by the solid vertical lines). The solid graph refers to individuals responding that they started their first real job before age 13 (WSA<13), the dashed graph to starting by the age of 13 (WSA=13), and the dotted graph to starting by the age of 14 (WSA=14). *Source:* LNU Survey, 1981. Own calculations.

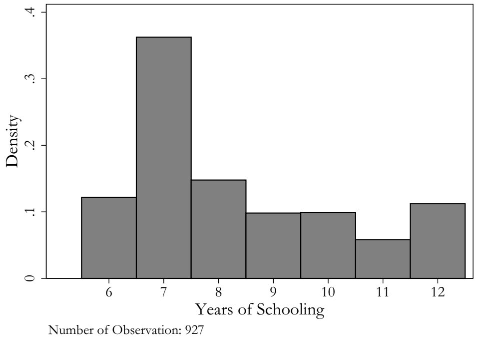
In addition to Figure F1 we also test whether an additional seventh school year shifts labor market entrance by running a regression of working starting age, wsa_i, on years of schooling:

$$wsa_i = \beta_0 + \sum_{s=6}^{12} \gamma_s I(Schooling = s) + \beta' x_i + u_i. \quad \text{(F1)}$$

In the regression we control for any education beyond any schooling. Figure F2(a) plots the predicted values. There is no significant difference at labor market entrance between having 6 or 7 years of schooling. This suggests that the opportunities for adolescents in the ages 12 to 14 to enter the formal labor market were low in the 1930s and 1940s.



(a)



(b)

FIGURE F2. Regression Years in School on Working Start Age: Figure F2(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 F2(b) shows the distribution of years of schooling. Results refer to cohorts 1930-1940. *Source:* LNU Survey, 1981. Own calculations.

Appendix G: The Swedish Old Age Pension System

Sweden implemented its first public old age pension system in 1913 and with its introduction, all citizens were entitled to a pension (Lundberg and Åmark, 2001). The 1913 version of the pension system was marginally modified in a series of reforms in the following decades, with a larger reform in 1959 and in 1998. For cohorts born 1930–1940 the system implemented in 1959, the so-called ATP system, is undoubtedly the one of major relevance.

G.1. The ATP System

The ATP system, with the first pensions paid in 1963, was a pay-as-you-go defined-benefit scheme where the mandatory pension age was 65, with the possibility to receive an early pension from age 60 and to postpone retirement to age 70 (Kridahl, 2017).[57] The pension scheme consisted of two main parts: (i) a flat benefit independent of previous income financed by the national budget and (ii) an earnings-related benefit covering all employees. The earnings-related benefit corresponded to 60 per cent (up to a ceiling) of 15 years of the highest earnings during 30 years of active labor force participation (Selén and Ståhlberg, 2007).[58] The pension right included earnings, but also social security transfers, e.g. unemployment insurance and parental benefits. Individuals with low or no earnings-related benefit received a supplementary benefit.

By the 1980's almost all employees were also covered by occupation schemes negotiated by trade unions. This implied that employees retiring also received a occupational pension corresponding to a percentage of the salary, paid as a contribution by the employer (Ståhlberg, 1985). The occupational pension is of quite limited relevance for the cohorts 1930–1940. On average this part corresponds to less than 15 per cent of total pensions for these cohorts, while the (i) flat benefit and the (ii) earnings benefit corresponds more than 70 per cent (ISF, 2017).

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

57. Mandatory pension age refers to the minimum age at which workers can take their public pension.

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

the ages ages 65–69 and ten per cent in the ages 70–74 were still employed (SOU, 2015).[59]

Married women born before 1945 whose husband passed away before 1990 could receive an additional widow pension. The widow pension corresponded to 40 per cent of the husband's earnings-related benefit and lasted until age 65 (Olofsson, 1993). After age 65 the widow pension was still 40 per cent of the husband's earnings-related benefit if the widow had never been employed, but was reduced if the widow herself had an earnings-related pension. This reduction also to some extent related to the birth year of the widow.[60]

The widow pension came to an end in 1990, but men and women having very low pensions could then get guarantee pension, starting by age 65. Also, married couples could sign a survivor pension (where pension benefits were paid to the surviving partner) and this has been frequently been signed by men than women (ISF, 2017).

G.2. The New Old Age Pension System

The Swedish parliament passed a new pension legislation in 1998 for a pay-as-you-go notional defined contribution plan. There were several reasons for the reform, among others that the old system was sensitive to economic and demographic changes and thus financially unstable (Johansson et al., 2014). The first benefits from the new system was paid out in 2001, but the system is successively being phased in with benefits drawn from both the old and the new system and it is only in 2040 that benefits will completely paid from the new system (Laun and Wallenius, 2015). As described below this new pension system is of very limited relevance to cohorts born 1930–1940.

The new system consists of three main parts and has a stronger link between contributions and benefits compared to the old system. The first part assigns to all citizens that have worked and lived in Sweden and is termed (i)the national public pension and is based on the income an individual paid tax on.

59. If an individual claimed an early pension from age 60 he or she received a reduced flat pension, and, if eligible, a supplementary pension. For individuals younger than 65 who could not perform gainful employment there was also a possibility to claim retirement through the disability insurance scheme (Johansson et al., 2014)

60. The rule was that the widow's own earnings-related pension and her widow pension together should correspond to a share of the widow's and her deceased husbands' aggregated earnings-related pension (Kridahl, 2017). For a widow born 1930 this share was 60 per cent and then declined gradually by two percentage points for each cohort until cohorts born 1935 or later for whom the share was 50 per cent. As an example, assume a woman born in 1935 whose husband passes away in 1985. Her deceased husbands earnings-related pension is €10,000 per year. Until the year 2000 when she turns 65, she receives €4,000 in widow pension per year. After the year 2000 and if she has an earnings-related pension, her survivor's pension is reduced. If the widows earnings-related pension is €7,500 per year, the combined earnings-related pension is €17,500. Given her birth year, the widow then has the right to $(0.5 \times 17500) - 7500 = e\,1250$ in widow pension per year.

Each year 16 per cent of an individual's pension-qualifying earnings and other taxable benefits (e.g. social transfers) are set aside (with an income ceiling) to the notional account, and the return follows the country's wage growth. An additional 2.5 per cent goes to the premium pension where an individual can choose what funds to invest the money. Just like in the old system, individuals with low or no earnings-related pension are entitled to a guarantee pension. The second part refers to (ii) occupational pension from the employer which varies depending on the agreement and is most important for those individuals who hit the income ceiling in the public pension. The third part corresponds to savings in (iii) private pension.

The mandatory pension age in the reformed system is flexible between age 61-67 for the earnings related pension. In agreement with the employer and individual older than 67 can continue working. As compared to the ATP system the new pension system provides larger incentives for older workers to stay active on the labor market. Additional time in paid labor, independently at what stage in life it occurs, generates higher pension benefits (Lindquist and Wadensjö, 2009). The new system also provides more flexibility as compared to the old system regarding e.g. part-time retirement.

As mentioned above the new system is phased in over time. Individuals born 1937 or earlier receive their entire pension according to the rules of the old ATP system, while individuals born 1954 will have their whole pension from the new system (Kridahl, 2017). The in-between cohorts (1938-1953) receive pensions from both systems, but to a varying degree. An individual born 1938 will receive 16/20 from the unreformed old system and 4/20 from the new system. An individual born 1939 will receive 15/20 and 5/20 and so on.

Appendix H: Data Source

The exam catalogue lists all pupils enrolled in a certain class. In addition to the name and date of birth of all pupils and their performance in various subjects, it contains information on the transition at the end of the academic year (highlighted black box in Figure H1). In the case exhibited here, all students were transferred to year 7 at the end of the academic year. Thus, by comparing this catalogue to previous editions, we can determine the academic year in which a seventh year became compulsory. The exam catalogue also contains information on the term length, thus giving an opportunity to also determine the year term extensions took place.

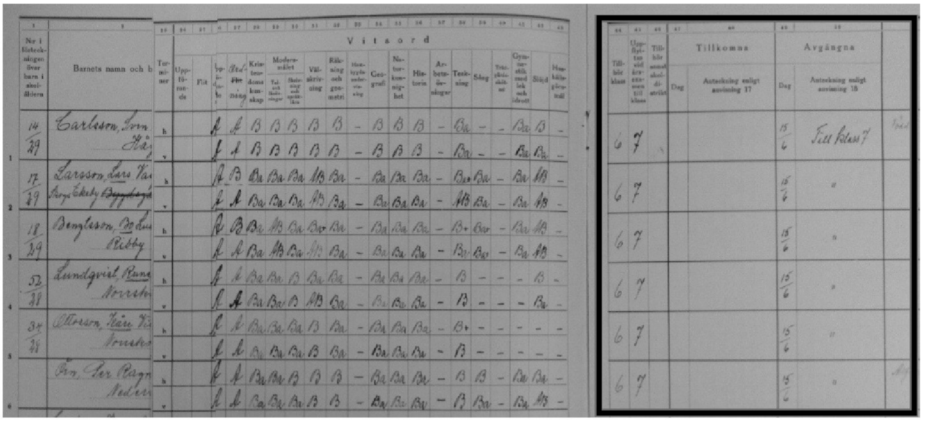


Figure H1. Exam catalogue from Archive

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