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## The Sooner the Better? Compulsory Schooling Reforms in Sweden

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## ABSTRACT

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# The Sooner the Better? Compulsory Schooling Reforms in Sweden\*

This paper evaluates the impact on earnings, pensions, and other labor market outcomes of two parallel educational reforms increasing instructional time in Swedish primary school. The reforms extended the compulsory years of schooling from 6 to 7 years and the annual term length from 34.5/36.5 to 39 weeks per year. Gradually introduced over the 1930-1950 period in more than 2,500 school districts, the extensions generated large exogenous variation in educational attainment at different points in primary school while the overall school system and curricula remained unchanged. The reforms thus constitute an ideal quasi-experimental setting for analyzing the long-run causal impact of compulsory education keeping other school characteristics fixed. With a majority of students receiving only primary schooling, both reforms affected large shares of the population and consequently had large impacts on educational attainment at the compulsory level. We find striking differences in impact between the two reforms, and between males and females. Estimated returns to compulsory schooling are robustly positive only for females, who experience a small increase in early career earnings (~ 2%) when exposed to a 7<sup>th</sup> year of schooling, and large and persistent increases in earnings (~ 4 – 5%) when exposed to an extended school year. The effects are driven by the extensive margin, in particular increased employment in the public sector.

**JEL Classification:** J24, J31, I28

**Keywords:** educational reforms, compulsory schooling, term length, returns to education

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# 1 Introduction

Policies extending the mandatory amount of education are frequently used in the economics literature to estimate the causal effect of human capital on earnings. A prominent example is compulsory schooling laws, regulating the minimum years of schooling individuals have to acquire. Early studies like Oreopoulos (2006) or Harmon and Walker (1995) following Angrist and Krueger (1991) generally presented large *causal* effects of schooling on earnings based on instrumental variable estimates. However, recent studies suggest smaller and often negligible impacts of human capital on economic outcomes or health.<sup>1</sup> In particular, recent studies on European countries suggest that changes in the number of compulsory years of schooling have rather small effects on pecuniary returns (Pischke and Von Wachter, 2008; Devereux and Hart, 2010; Chib and Jacobi, 2015). Also the large causal estimates for education and labor market returns in the US have recently been challenged as potentially spurious and sensitive to regional trends, suggesting that previous findings were driven by differences in school quality between states (Stephens and Yang, 2014).

With governments across the globe investing large shares of their budgets in education the effectiveness of alternative approaches to increase instructional time is of obvious interest. An alternative policy instrument to compulsory schooling laws regulating the years of schooling is to increase the term length. Important conceptual differences between the two types of extensions arise from the fact that the exit age is not affected by a term length extension and the age at which individuals are affected. A growing literature estimates the short-run effects of such term length extensions on student performance, and generally finds positive effects – but there is important heterogeneity with regard to the effect sizes as well as the students and subjects affected (Sims, 2008; Agüero and Beleche, 2013; Huebener et al., 2016; Bellei, 2009).<sup>2</sup> 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., 2016).<sup>3</sup>

This paper examines two policies increasing compulsory instructional time of the Swedish primary school (*Folkskola*) in the 1930s and 1940s. Implemented in the first half of the twentieth century when a 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 more

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<sup>1</sup>Recent contributions on the effects of education on health include (Gathmann et al., 2015; Brunello et al., 2013; Clark and Roayer, 2013; Meghir et al., 2012; Lager and Torssander, 2012). 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 (Piopiunik, 2014; Lundborg et al., 2014; Chevalier et al., 2013; Güneş, 2015), and crime (Hjalmarsson et al., 2014).

<sup>2</sup>Several studies analyze the effect of instruction time on student performance using other identification strategies, cf. Leuven et al. (2010); Lavy (2015); Battistin and Meroni (2013).

<sup>3</sup>In the economics literature term length has sometimes been considered a proxy variable for school quality (Betts, 1995; Card and Krueger, 1992). This is a valid interpretation in some contexts, but *ceteris paribus* changes in term length are de facto primarily changes in instructional time.

than 2,500 Swedish school districts were obliged to increase the mandatory amount of schooling from 6 to 7 years within a period of 12 years, but also to extend the annual term length in *Folk-skola* from 34.5/36.5 to 39 weeks. School districts could choose the timing of the implementation independently within the given time window generating large variation in educational attainment between cohorts and small local school districts. While the compulsory schooling reform added one additional school year (corresponding to 34.5-39 weeks) at the *end* of primary school, the term length extension corresponded to an instructional time increase of 15-31 weeks distributed over the complete course of primary school. The two educational interventions consequently increased overall instructional time by a comparable magnitude, but at different margins. In contrast to the compulsory schooling reforms investigated in previous studies, the term length extension constitutes a rare quasi-experiment for instructional time changes within a school year.<sup>4</sup>

The interpretation of compulsory school reforms as a pure increase of the amount of schooling is challenging whenever evidence is based on reform packages where changes in the instruction time only is one of many components. As an example the Scandinavian comprehensive school reforms in the 1950s and 1960s combined extensions of compulsory schooling with the abolishment of tracking and changes in curriculum and teaching practices (cf. Meghir and Palme, 2005; Lundborg et al., 2014; Pekkarinen et al., 2009; Black et al., 2005). Moreover, some historical reforms (including the Scandinavian comprehensive school reforms) generated educational degree effects in addition to the prolongation of schooling (Kırdar et al., 2015; Grenet, 2013). By comparing similar reforms in UK and France, Grenet (2013) concludes that sheepskin effects could explain diverging patterns in different European countries. These caveats do not apply in our case since the two extensions considered left all other components of the school system, including secondary and higher education, unaffected. Keeping the overall school system constant, the interventions thus allow us to isolate effects from extensions in instructional time from other school inputs such as teacher wages or class size. We consequently argue that the two reforms were *pure* schooling reforms in the sense that they only aimed at raising the *amount* of education, allowing a straightforward result interpretation.

Another common challenge when estimating human capital returns using schooling reforms is that individuals generally cannot be completely tracked over the life course. By having a rich and unique data material we can avoid several pitfalls and disadvantages of earlier studies. First, by assigning school characteristics during primary school age we avoid bias from selective migration during early childhood. Thus, compared to using the place of birth as a proxy for primary schooling location we avoid a possibly large attenuation bias in the estimates of the policy effects. Second,

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<sup>4</sup>Other studies estimating the medium- and long-run effects of term extensions include (Cortes et al., 2015) who analyze the impact of increased instructional time in math on high school graduation and college enrollment, and Pischke (2007) who analyzes the effects of short school years in Germany in 1966 and 1967.

we are able to follow our population over an unusually long time period – ranging from birth until age 71. 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., 2014). Consequently we contribute to the small literature on the returns to education in the very long term (Van Kippersluis et al., 2011; Schneeweis et al., 2014; Brunello et al., 2016; Crespo et al., 2014) using a large sample of high-quality administrative register data.

Finally, one argument to why compulsory schooling extensions could be ineffective in improving labor market outcomes is that basic skills may already be set at the age when the extension is introduced (Pischke and Von Wachter, 2008). We are aided in this by features of the reforms. Both extensions captured students at a very young age which is of particular interest given that the early years in life seem to represent a crucial period for the development of cognitive and non-cognitive skills (Cunha and Heckman, 2007). The compulsory schooling extension affected students at age 13 and the term length extension was introduced already in first grade at age seven for some individuals. In contrast to most previous studies our analysis sets in at ages where basic skills are more likely to still be formed.

Using high quality Swedish register data, we show that while the reforms by construction had large impacts on educational attainment at the mandatory level, they did not translate to additional educational attainment above the compulsory level. If anything, there appears to be a small *reduction* in years of education beyond primary school due to the average term length extension – implying a substitution effect from later to earlier education. Regarding effects on earnings, conventional OLS estimates indicate substantial returns to years of education. An additional year is associated with 7.7% higher labor earnings in 1970 for males and an even larger increase in earnings for females (13.5%). Pension income at the age of 71 suggests a growth of similar magnitude (7%) for males and females. While there do only seem to be small returns to the extension of compulsory schooling, we find substantial and persistent effects of the term length extension on earnings for females. Females experienced an increase in 1970 earnings of about 5% and estimates on female pension income are of similar magnitude. We do not find comparable effects of compulsory schooling or term length extension for males.

The returns to the two schooling reforms are very heterogeneous. Assessing quantile treatment effects it is evident that male returns are essentially flat around zero throughout, whereas female returns are concentrated in the lower parts of the distribution for both reforms. In particular the term length extension appears to benefit the lower quantiles disproportionately: effect sizes are as large as 20-30% in some parts of the distribution. When instead considering heterogeneity based on observables, a consistent picture emerges for both sexes: the term length extension appears to

have a disproportionate impact in industrialized areas with high incomes per capita and in areas where the distance to the nearest secondary school was short.

In order to gain a deeper understanding of the mechanism underlying the asymmetric impact on males and females, we examine effects on additional labor market outcomes. Our results suggest that females increased their labor market participation by 6% due to the term length extension while there are no comparable effects when it comes to compulsory schooling. Furthermore, females increased their work in municipal and governmental public sector employment while males decreased participation in higher skilled scientific, medical and technical jobs. Treated males rather sorted themselves into lower skilled jobs in transport, communication and services. These findings are in line with a recent study of Bhalotra et al. (2016) showing that the overall development on the labor market induced by the expansion of the Swedish welfare state and public policies in the 1950s especially benefited females that were exposed to a neonatal health care intervention in the early 1930s.

The next section gives the institutional background of the Swedish educational system and the reforms, and embeds them in a broader context of compulsory schooling reforms. Section 3 describes the data and sample selection, while Section 4 presents the empirical strategy. Section 5 presents the empirical results for a variety of socio-economic outcomes and Section 6 concludes.

## 2 The Swedish School System

Children in Sweden start school in the year they turn seven.<sup>5</sup> In the 1930s and 1940s children entered a primary school called *Folkskola*. The mandatory amount of schooling before the compulsory schooling reform in 1936 was 6 years, with some exceptions in the larger cities and in the most southern county of Sweden, Scania. Around 70% of students only attended primary school and just a small fraction of students completed secondary schooling and further higher education.

Similar to many other countries at the beginning of the twentieth century, the overall school system consisted of different tracks. In the first years of primary school students were kept together. After 4 or 6 years, good performing students could switch to an academic educational track. The lower secondary school (*Realskola*) generally took entrance exams, indicating that the students remaining in *Folkskola* were more likely to be less gifted children. Other factors, such as schooling fees or the availability of a secondary school nearby constituted additional constraints to higher education. After *Realskola*, students could continue to higher secondary education (*Gymnasium*) and academic education.<sup>6</sup> Figure ?? shows the distribution of students for the cohorts 1926-1940

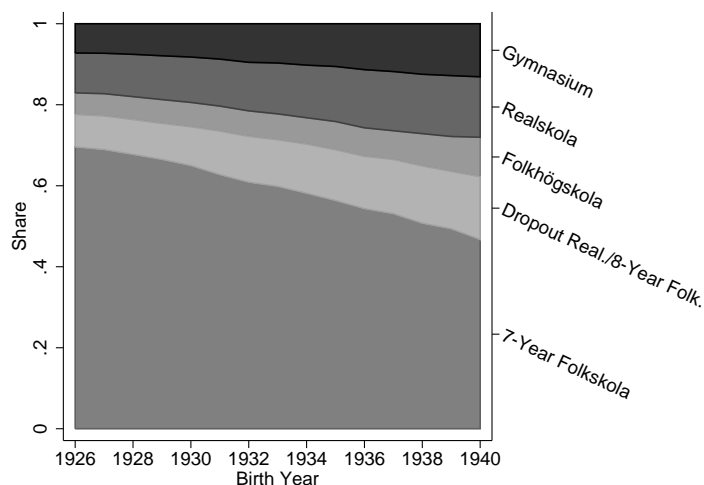
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<sup>5</sup>A more detailed description of the primary school system is given in Appendix E and in Fischer et al. (2016)

<sup>6</sup>For girls also special secondary schools existed which however were very similar to *Realskola* and girls also left



according to their highest achieved school degree. Overall the Swedish school system exhibited striking similarities with the contemporary German school system.<sup>7</sup>



**Figure 1.** Distribution of Students by School Type

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 passed a law in 1936 extending compulsory schooling by introducing a mandatory 7<sup>th</sup> year. At the time the country had approximately 2,500 school districts (which in most cases coincided with the Swedish church parishes).<sup>8</sup> It was stipulated that a seventh year had to be implemented all across the country before the school year 1948/49. As a result the reform and the additional year in the lowest track was not implemented at the same time in all school districts but over a range of 12 years.

With the same argumentation that Sweden was lagging behind other countries, the national Parliament also decided in 1937 that the school year should be either 34.5, 36.5 or 39 weeks long, which fueled term length extensions in 600 districts in the following two school years. Furthermore, the 34.5 weeks option was to be discontinued by the school year 1941/42 and by 1952/53 39 weeks became the uniform standard.

Gainful employment typically constitutes the most important alternative to schooling, but options were limited for children affected by the two reforms. Swedish child labor laws have generally been coordinated with the compulsory schooling attendance laws. In 1931 the minimum age for

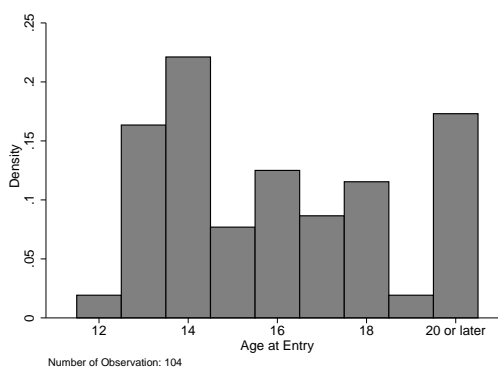
*Folkskola* after 4 years in case they attended the so called *högre flickskolas*. Also some non-degree secondary education existed (e.g. the *Högre Folkskola*). Private schools never played a substantial role.

<sup>7</sup>For a description of the Germany school system see e.g. (Pischke and Von Wachter, 2008)

<sup>8</sup>Exceptions to this rule are mostly larger cities which were generally defined as one school district but consisted of several church parishes.

manufacturing and construction work was 14 years, whereas the limit for *light work* was 13 years.<sup>9</sup> A child was allowed to work from the beginning of the calendar year in which it would reach the age limit (Sjöberg, 2009). After the implementation of the seven year compulsory school reform, most pupils left school in the middle of the year they turned 14, whereas before they would leave school the year they turned 13. Consequently, the reform reduced the time a child could spend in *light work* by one year, whereas the corresponding reduction for *hard* (industrial) work was 5-6 months.

Figure 2 shows the labor market entrance age for children with only 6 years of primary school and no secondary education. Working at a younger age than the new school leaving age of 14 was a rare phenomenon even before the compulsory schooling reform. Although the sample refers to individuals who left school in the year they turned 13, only a minority reported to start working before the age of 14. This implies that labor market experience was mostly unaffected by the compulsory schooling extension. By construction, the term length extensions in primary school also left labor market experience unaffected.



**Figure 2.** Age at Labor Market Entry

Source: LNU (1981). Restricted to sample to cohorts 1930–40 with less than seven years of education. Sample restricted to students who left school after 6 years (with regular leaving age 13) and gained no further education.

## 3 Data and Sample Selection

### 3.1 Reform data

The empirical analysis uses a purpose-built reform data set with information on several mandatory extensions of the Swedish primary school system (*Folkskolan*), extracted and digitized from historical archives. The reform data includes information on 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.

<sup>9</sup>Light work referred to work outside factories or construction sites. Notably child labor laws only applied to employed work and not to work at e.g. the family farm.

The data was collected on school district level.

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

The reform data set contains information on the year of reform implementation for more than 98% of all existing school districts at the time of interest. Fischer et al. (2016) gives a detailed background on the reforms and its connection to later school reforms, as well as information on data collection procedures and sources. Fischer et al. (2016) also provide information on various sensitivity tests and check-ups performed to assess the quality and validity of the unique reform data.<sup>10</sup>

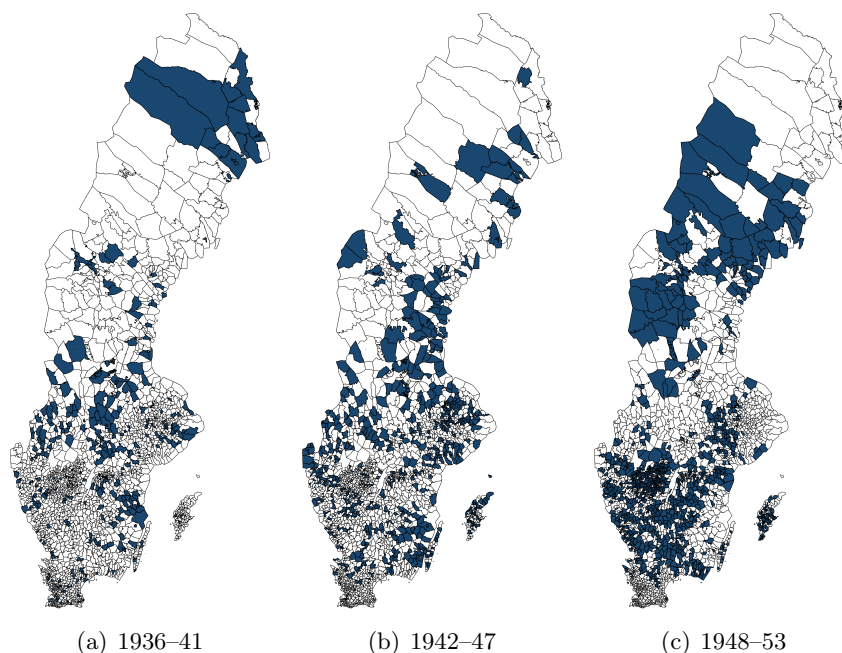
Figures 3(a) – 3(c) and 4(a) – 4(c) graphically present the spatial and temporal variation of the 7-year school extension and the term length extension respectively. While considerable spatial and temporal variation is visible, the implementation was not random. As mentioned above, the districts could choose when to implement the instruction time extensions within a certain time frame. While the box plots in Figures 5(a) and 5(b) show that both reforms were implemented later in school districts with high shares of employment in agriculture, this is especially pronounced for the introduction of the seventh grade. 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 seventh grade in more rural regions.

The empirical analysis will account for the structural differences between school districts. Balancing tests on parental background variables do not suggest any relevant differences between

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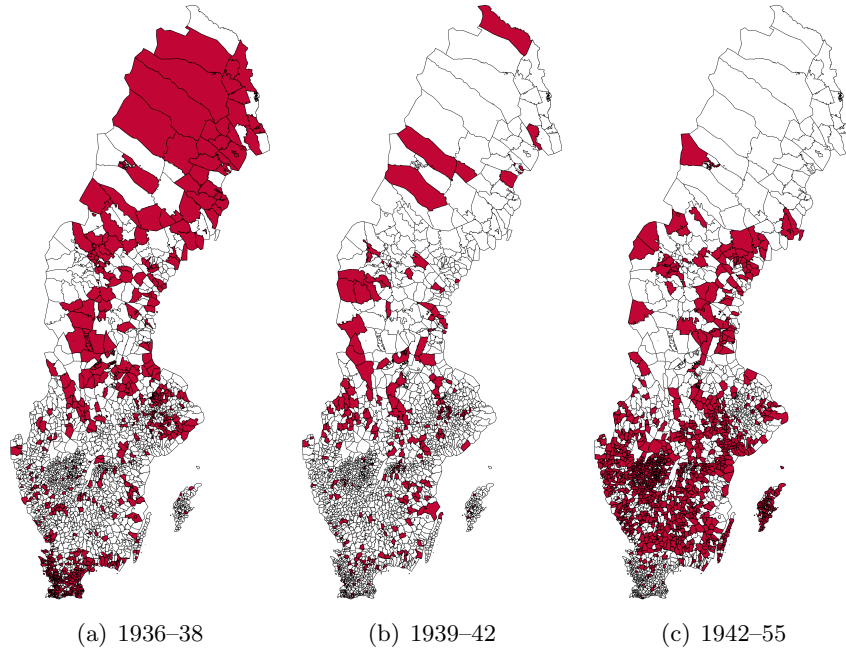
<sup>10</sup>Our way of assigning reform status to a district has been validated in various ways: e.g. by comparing our reform data with official statistics on the share of school districts that had implemented the seven year reform in each county of Sweden for the years 1938-1945 and by manual check-ups 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.

treatment and control groups after the inclusion of trends. In Appendix B we present results for maternal and paternal education, occupation and personal background information like wedlock status or place of birth. Neither for the compulsory school reform nor for the term length extension there are any significant correlations that could represent a threat to our estimation strategy and results. Thus, although the reforms were not randomly implemented we are confident that our estimates may be given a causal interpretation.



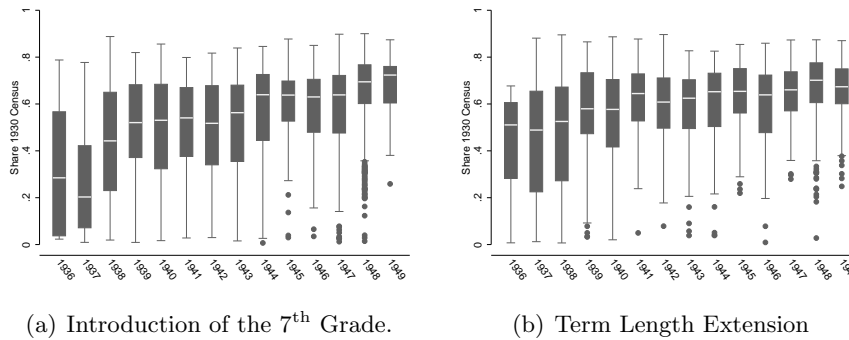
**Figure 3.** Timing of the Introduction of the 7<sup>th</sup> Grade

Geographical unit: Municipalities which coincide with school districts in most cases. Rural and small districts in the middle west and sparsely populated districts the north (with the exception of the most northern county Norrbotten) implemented the seventh grade at the latest possible date or in some rare exceptions even delayed the introduction. The southern region of Skåne has almost no reform implementation after 1936 as most districts already had a mandatory seventh grade.



**Figure 4.** Timing of the Term Length Extension

Geographical unit: Municipalities which coincide with school districts in most cases. Rural and small districts in the middle west extended the term length to 39 weeks later.



**Figure 5.** Local Share Working in Agriculture in 1930 by Implementation Year

The graph shows box plots for the share working in agriculture by year of implementation. 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.

### 3.2 Assigning Reform Status

In our main data set, the *Linked 1970 Census*, we assign reform exposure based on the 1946–50 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.

Alternatively, we can assign reform information to individuals using the *place of birth* as a

proxy for the place of residence during schooling age. This frequently employed strategy relies on the assumption that the parish of birth also captures the place of residence at the relevant age. The quality of this assignment procedure generally depends on migration patterns during the first years of life, but the approximation is further complicated in the current context due to the registration of hospital births. 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.<sup>11</sup> With a growing number of institutionalized 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 Appendix C.

Given the above caveats our main analysis uses the place of residence during schooling age to assign reform status. Only in some exercises where we use survey data, for which it is not possible to identify and match the place of residence to individuals, we rely on the place of birth as approximation.<sup>12</sup> In order to address the hospital birth issue, we moreover gather information on hospital openings and their location and include it in the analysis.

### 3.3 Individual-Level Data

#### 3.3.1 Linked 1970 Census

The base study population for our main analysis is drawn from the Swedish Census in 1970 and consists of all individuals born between 1930 and 1940. The data covers information on individuals' labor market status, occupation, income, education 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.<sup>13</sup> To explore labor market effects we apply dummy variables indicating whether an individual has been gainfully employed and whether an individual worked fulltime (more than 35 hours) in the 1970 Census week. Furthermore, we construct a set of variables capturing occupational choices and public sector employment on the lower (local) municipal level and on the higher (state) governmental level.

Educational attainment is reported by the highest completed schooling degree as well as by the highest completed post-schooling degree (e.g. vocational training or university) attained. In case

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<sup>11</sup>This potential problem is also mentioned by Holmlund (2008).

<sup>12</sup>We consequently have measurement error in the reform assignment in the exercises where we use survey data whenever the individual was born in hospital or migrated between birth and their primary schooling years – whenever there is a discrepancy in reform status between the official place of birth and the actual place of residence.

<sup>13</sup>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.

individuals were currently in training the last completed highest degree is recorded but given that all our individuals are older than 30 the clear majority had finished their education by the time of the Census in 1970. As the 1970 Census does not contain direct information on the years of education we add a constructed measure for the years of education (YoE) based on the schooling and post-schooling variables in the Census. We assign the usual years of education associated with different types of schooling and post-schooling degrees<sup>14</sup> and take the sum as an approximation for the total years of education:

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

The years of education measure allows us to estimate returns to years of education (see Appendix D for the construction outlined in detail). Unfortunately, the 1970 Census does not differentiate between the number of years spent in *Folkskola* if instruction time was less than 7 years.<sup>15</sup>

Income statistics stem from official tax returns and are considered as highly accurate.<sup>16</sup> 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.<sup>17</sup> In 1970 individuals are in their prime working age which reduces potential bias from life-cycle effects. Furthermore, we add data on pensions from the year when individuals turned 71.<sup>18</sup> 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). Consequently pensions can be expected to be less sensitive than earnings to fluctuations in labor supply. This is a desirable feature, especially when analyzing women's returns to human capital as career interruptions due to e.g. childbearing affect the pensions to a smaller degree than annual earnings.

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<sup>14</sup>The years associated with a post-schooling degree in the 1970 Census are directly drawn from the Educational Registers. We used the most frequent SUN2000 classification for each degree in the Census and assigned the length from the Educational Registers to the 1970 Census degrees.

<sup>15</sup>This caveat is universal for Swedish Register Data on education including the Swedish Educational Registers.

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

<sup>17</sup>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 sickness absent benefits which only became taxable in 1974 which should be included in income from employment. We also lack information on pensions which should be subtracted. Given that pensions are unlikely a major income in 1970 for cohorts born after 1930 and sickness benefits are only a minor part of the income, we conclude that the income measure is a very reasonable approximation of annual labor earnings.

<sup>18</sup>We have access to pension data from 2001 onwards, therefore 71 is the first age we observe all relevant cohorts receiving a pension.

### 3.3.2 Survey Data

As the 1970 Census does not differentiate between the number of years spent in primary school, we present additional analyses based on survey data including self-reported information on the years of primary schooling in order to assess the effects from the policy interventions directly on educational attainment. To demonstrate that the reform actually had bite we use the Swedish *Survey on Living Conditions* (ULF), a survey conducted on a continuous basis in Sweden since 1975 and comprising a representative sample of the Swedish population aged 16-84 years. Each respondent was randomly selected and participated in a face-to-face interview and was asked to answer questions regarding living conditions. The ULF survey covers a wide range of variables on economic and educational outcomes. Most importantly the survey incorporates detailed questions on the schooling history of individuals including how many years they actually spent in the highest attended school track. This allows us to differentiate between different lengths of compulsory schooling for each individual on a small subpopulation to illustrate the impact of the reform.

The ULF survey does not contain information on the place of residence during childhood which implies that we have to rely on the place of birth as an approximation when using this data. All individuals in the survey were assigned a place of birth regarding their birth parish based on information in the *Befolkningsregistret* (the population register) by Statistics Sweden (SCB).

## 4 Empirical Strategy

Our empirical analysis exploits exogenous variation in the amount of instructional time in primary school generated by the two above-mentioned educational policies. The first reform extended the years of education in basic track from 6 to 7 years, the second policy extended the annual instruction time from 34.5/36.5 weeks to 39 weeks. Both reforms imposed substantial changes in the amount of instructional time – both in relative and absolute terms. 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<sup>19</sup> over the time spent in primary school. The compulsory schooling reform adds one complete year with 34.5, 36.5 or 39 weeks of additional instructional time. Figure 6 gives a stylized representation of the variation in instructional time generated by the two educational policies.

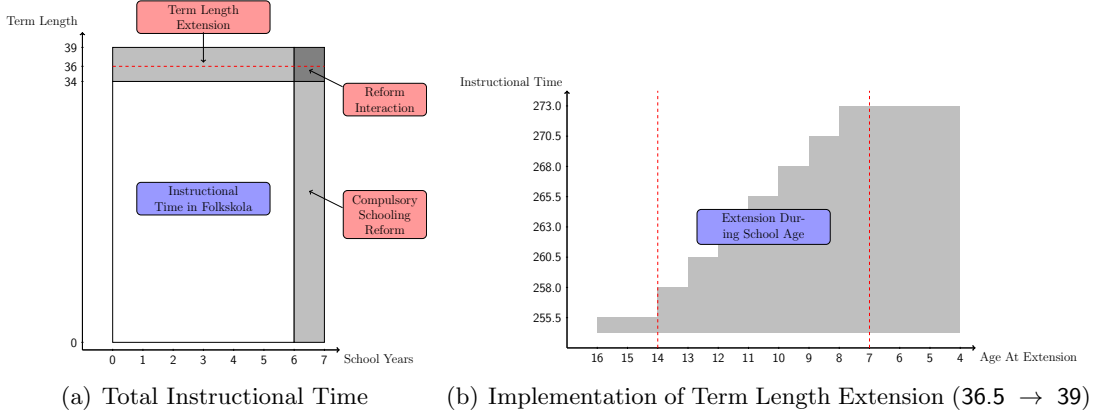
Since both reforms were decided and implemented at the local level they may be correlated. We thus use specifications including both reforms at the same time. Figure 6(a) however demonstrates that even strong correlation in the timing of the two reforms is unlikely to cause any problems

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<sup>19</sup>Equivalent to 1/2 to 1 full school year.



even without such adjustment. The reason is a direct consequence of the step-wise increase of the extension of term length as shown in Figure 6(b). In contrast the compulsory schooling extension generates a sharp discontinuity between pre- and post-reform cohorts.



**Figure 6.** Total Instructional Time by Reform Status.

Figure 6(a) compares the total instructional time in primary school. The instructional time varies in two dimensions – term length and length of Folkskola in years. Figure 6(b) shows the variation from term length depending on the age at which individuals were affected by the policy change. School districts changed the term length simultaneously for all grades. Individuals in lower grades at the time of extension received more education in total.

We estimate the causal effect of the interventions using different Difference-in-Differences (DID) specifications. Denoting the outcome variables by  $y_{igc}$ , our main regression equation is given by

$$y_{igc} = \beta_0 + \beta_1 Z_{gc} + \beta_2 T_{gc}/6 + \beta_3 g_c + \mu_c + \nu_g + \epsilon_{igc} \quad (1)$$

where  $c$  indicates the cohort,  $g$  the school district,  $\mu_c$  denotes cohort fixed effects and  $\nu_g$  school district fixed effects. In some specifications we also allow for diverging cohort trends between school districts.  $Z_{gc}$  is an indicator of whether an individual belonging to cohort  $c$  and residing in parish  $g$  has been exposed to the extension of compulsory schooling from 6 to 7 years. The continuous variable  $T_{gc}$  is the average term length measured in annual weeks of instruction in the school district for cohort  $c$ . We re-scale the average term length by the factor 6 in order to enhance a comparison to estimates for the returns to years of education.<sup>20</sup> Both treatment variables vary on school district level.

The parameters  $\beta_1$  and  $\beta_2$  capture intention-to-treat effects, i.e. the effect of the policy on the population. However, with both interventions affecting large shares of the population, effects should be relatively close to the average treatment effect in the overall population. 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

<sup>20</sup>An increase of 1 week in average term length translates to 6-7 weeks over the whole period in primary school. Up-scaling by the factor 6 extrapolates the effect size to a full year of 36-39 weeks of instruction.

otherwise have finished school after 6 years, i.e. 70% of all children. The extension of term length in primary school generally affected all students in the first 4 years in primary school and later only those not attending the academic track which again was the majority of children. In addition, we estimate quantile treatment effects in order to investigate potential heterogeneity along the earnings distribution.

## 5 Results

### 5.1 Educational Attainment

First we present evidence that the reforms did in fact increase educational attainment. Based on the ULF survey we construct an indicator of whether a person had more than the old compulsory level of 6 years of schooling. Since the survey does not contain information on term length this data can only be explored for the 7 year extension. Table 1 reports coefficients from a regression of the reform indicator on having more years of schooling than the old compulsory level. The results clearly indicate that the compulsory schooling reform had bite. The estimates present a lower bound and suggest that schooling increased by about 0.3-0.4 years. As discussed extensively in the data section and in Appendix C, the actual impact of the reform based on survey data is attenuated due to measurement error from the reform assignment based on place of birth due to migration and hospital codings in the survey data.<sup>21</sup>

**Table 1.** First Stage: Compulsory Schooling Reform (Survey)

	(1)	(2)	(3)
	<b>Indicator more than 6 Years of Schooling</b>		
<b>Compulsory 7-Year</b>	0.421 [0.373,0.468]	0.334 [0.269,0.399]	0.277 [0.212,0.342]
<i>F</i> Statistic	302.199	102.506	69.198
N	5,326	5,326	5,326
School Districts		1,286	1,286
<b>Cohort FE</b>	✓	✓	✓
<b>Cluster FE</b>		✓	✓
<b>County Cohort Trends</b>			✓

Estimates are based on the ULF survey, cohorts born 1920–40. Standard errors are clustered on school district level and 95% CI are added. All regressions control for sex and survey year. Estimates are based on survey waves 1981, 1982, 1995 and 1996 which covered self-evaluated information on the highest school attended within the tracking system and how many years they attended this type of school. From this we can construct a measure based on information whether an individual visited only *Folkskola* and for how many years.

Effects on educational attainment above 7 years of education can be inferred from the 1970 Census. Table 2 presents results on further educational attainment indicated by years of education beyond primary school. With access to the full population and precise location of individuals

<sup>21</sup>As the results indicate trend sensitivity we also estimate the indicator of having more than six years of schooling on lags and leads of the treatment indicator. Results can be found in Appendix A

during schooling age, the estimates do not suffer from a small sample or serious attenuation bias. The compulsory schooling reform had only very small and negligible impacts on the total years of education for males and females, respectively. Estimates for the average term length extension however suggest that male and female students substitute later educational attainment if earlier education is expanded. Without linear trends the effects are sizeable as estimates suggest a reduction of 0.3 years for males and 0.5 years for females – but smaller than the increase in compulsory schooling. The results are however sensitive to the inclusion of trends. This is hardly surprising considering Figure 6, which shows that the term length is approximately linear in age during the implementation period.

Overall we conclude that the two extensions of instructional time did not increase education beyond the compulsory level. If anything, results suggest a small substitution effect. Notably, we do not find empirical evidence for increasing attendance of secondary education due to the compulsory schooling reform.

**Table 2.** Educational Attainment: More than 7 Years (Register)

	(1)	(2)	(3)	(4)
<b>Men</b>				
<b>Years of Education (Mean = 9.044 )</b>				
<b>Compulsory 7-Year</b>	-0.026 [-0.062,0.010]		0.010 [-0.029,0.048]	0.037 [-0.015,0.089]
<b>Average Term Length</b>		-0.257 [-0.369,-0.144]	-0.269 [-0.391,-0.147]	-0.134 [-0.399,0.131]
	413,031 2,400	412,964 2,398	412,964 2,398	412,964 2,398
<b>Women</b>				
<b>Years of Education (Mean = 8.818 )</b>				
<b>Compulsory 7-Year</b>	-0.134 [-0.179,-0.090]		-0.081 [-0.126,-0.035]	-0.033 [-0.082,0.016]
<b>Average Term Length</b>		-0.515 [-0.642,-0.387]	-0.411 [-0.542,-0.279]	-0.084 [-0.332,0.164]
	401,591 2,401	401,519 2,399	401,519 2,399	401,519 2,399
<b>Cohort FE</b>	✓	✓	✓	✓
<b>District FE</b>	✓	✓	✓	✓
<b>Linear District Cohort Trends</b>				✓

Estimates based on Linked 1970 Census. Cohorts 1930-1940. 95% CI based on clustered SE (cluster unit: school districts). Average Term Length is rescaled so that estimates are comparable to a full year in additional instruction time. **Years of Education:** Measure based on years of schooling and post-schooling from Census 1970. Measures years of education after compulsory level; no variation if *Folkskola 7* or less years of schooling. Specifications: (1) Only 7-Year Reform; (2) Only Average Term Length; (3) 7-Year Reform and Average Term Length; (4) + linear cohort trends within school districts.

## 5.2 Earnings

### 5.2.1 1970 Earnings

Table 3 reports coefficients from regressions of log annual labor earnings on the two policy instruments. In order to compare the magnitude to general returns to education, we add conventional estimates for the years of education on earnings as a benchmark. The conventional results show substantial returns to years of education on earnings. For males an additional year of education associates with an increase of 7.7% in earnings. For females the effect is even larger with a 13.5% increase in earnings. The size of the effect for females is unsurprising given that annual earnings capture effects from both labor supply and productivity increases. For males the labor market participation rate was close to 100% with most males working full-time, implying that the returns on annual earnings should be close to returns on hourly wages. Labor market effects will be explored further in Section 5.3. Our results are in line with previous studies on the returns to education for Sweden before the 1970s suggesting an average return of about 8% to an additional year of education on hourly wages based on survey data (Edin and Topel, 1997).

The reduced form estimates in Table 3 for one additional compulsory year of education are smaller. The difference-in-difference estimate suggests a 2% increase in earnings. If linear trends are taken into account the effect size decreases for females and becomes basically zero for males. Even a simple rescaling of the effect by the high complier rate of 70% delivers much smaller estimates than the measured OLS returns to education.

In contrast, results for the effects from average term length extension are large and suggest a sizeable increase in earnings, especially for females. Controlling only for school district fixed effects, estimates for average term length are economically meaningful for both males and females. When local trends are included, the estimate for females hardly changes, whereas the estimated effect for males disappears.

**Table 3.** Main Results: Log Earnings 1970

	(1)	(2)	(3)	(4)
<b>Men</b>				
<b>Log-Earnings 1970</b>				
<b>Years of Education</b>		0.077 [0.076,0.078]		0.077 [0.076,0.078]
<b>Compulsory 7-Year</b>	0.026 [0.018,0.034]		0.021 [0.013,0.029]	-0.001 [-0.010,0.009]
<b>Average Term Length</b>		0.065 [0.040,0.089]	0.038 [0.015,0.061]	-0.021 [-0.070,0.028]
	395,863 2,400	395,802 2,398	395,802 2,398	395,802 2,398
<b>Women</b>				
<b>Log-Earnings 1970</b>				
<b>Years of Education</b>		0.136 [0.131,0.140]		0.135 [0.131,0.140]
<b>Compulsory 7-Year</b>	0.025 [0.007,0.043]		0.018 [-0.001,0.038]	0.016 [-0.011,0.043]
<b>Average Term Length</b>		0.075 [0.020,0.131]	0.052 [-0.008,0.112]	0.051 [-0.072,0.174]
	271,094 2,399	271,043 2,397	271,043 2,397	271,043 2,397
<b>Cohort FE</b>	✓	✓	✓	✓
<b>District FE</b>	✓	✓	✓	✓
<b>Linear District Cohort Trends</b>				✓

Estimates based on Linked 1970 Census. Cohorts 1930-1940. 95% CI based on clustered SE (cluster unit: school districts). Regressions are run separately for years of education and reforms. Specifications: (1) Returns to 7-Year Reform; (2) Returns to Average Term Length; (3) Returns to both; (4) + linear cohort trends within school districts.

### 5.2.2 Pensions at Age 71

Table 4 presents results using pensions at age 71 as an outcome. They offer an alternative measure for earnings as they are calculated on the basis of the 15 years with the highest income and thus less prone to labor supply fluctuations than annual earnings. OLS returns to education underline this argument. With 7% the returns to years of education for females are basically identical to males and of similar magnitude as labor market returns to education in the 1970 Census. The results for the instructional time extensions are also quantitatively similar to results on earnings in 1970. While the compulsory schooling extension had almost no effect, estimates for the average term length imply an increase in pensions for females of between 4-5%. The slight reduction in the point estimate compared to the results in Table 3 could be due to labor supply effects contributing to the 1970 estimate.

**Table 4.** Main Results: Log Pension (Age 71)

	(1)	(2)	(3)	(4)
<b>Men</b>				
<b>Log-Pensions</b>				
<b>Years of Education</b>		0.070 [0.069,0.070]		0.070 [0.069,0.070]
<b>Compulsory 7-Year</b>	0.022 [0.015,0.029]		0.017 [0.009,0.024]	0.008 [-0.002,0.018]
<b>Average Term Length</b>		0.060 [0.038,0.082]	0.039 [0.016,0.062]	-0.018 [-0.070,0.033]
	300,131 2,397	300,087 2,395	300,087 2,395	300,087 2,395
<b>Women</b>				
<b>Log-Pensions</b>				
<b>Years of Education</b>		0.071 [0.071,0.072]		0.071 [0.071,0.072]
<b>Compulsory 7-Year</b>	0.014 [0.008,0.020]		0.008 [0.001,0.014]	-0.004 [-0.013,0.004]
<b>Average Term Length</b>		0.060 [0.039,0.081]	0.050 [0.028,0.072]	0.042 [-0.003,0.088]
	323,943 2,401	323,882 2,399	323,882 2,399	323,882 2,399
<b>Cohort Dummies</b>	✓	✓	✓	✓
<b>Cluster FE</b>	✓	✓	✓	✓
<b>Linear Trends</b>				✓

Estimates based on Linked 1970 Census. Cohorts 1930-1940. 95% CI based on clustered SE (cluster unit: school-districts). Specifications: (1) Returns to 7-Year Reform; (2) Returns to Average Term Length; (3) Returns to both; (4) + linear cohort trends within school districts.

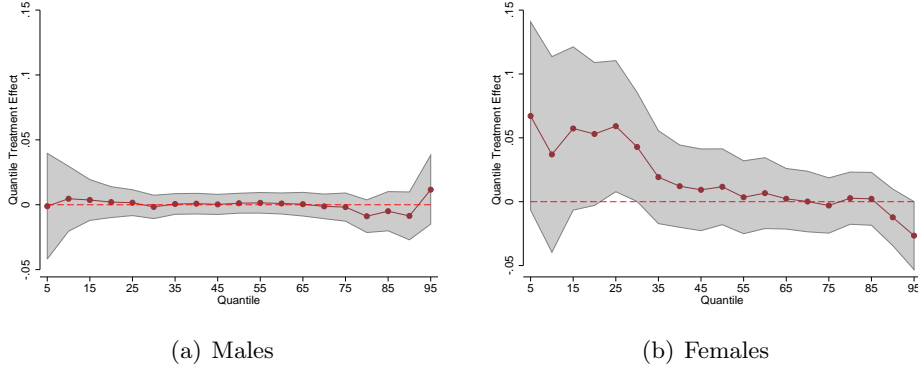
### 5.2.3 Heterogeneity

It has been argued that different compositions of compliers are a potential explanation of the low returns to education in Europe compared to the US and Canada (Oreopoulos, 2006). Northern American studies have estimated returns to compulsory education for a small group of drop-outs that may be very prone to be positively affected by extensions of mandatory education. In the current setup about 70% of the population were affected by the extension of *Folkskola*. Therefore the study design is much closer to compulsory schooling reforms 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.<sup>22</sup>

In order to investigate potential heterogeneity along the whole earnings distribution we present quantile treatment effects. If students of lower ability and social background rank lower in the income distribution but only the least gifted benefit from mandatory education we would expect gains from the reforms in the bottom of the earnings distribution. The overall results are in line with the mean effects. Figure 7 presents quantile treatment effects for the compulsory schooling extension including trends. For males the effect is homogeneously zero and precisely estimated.

<sup>22</sup>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.

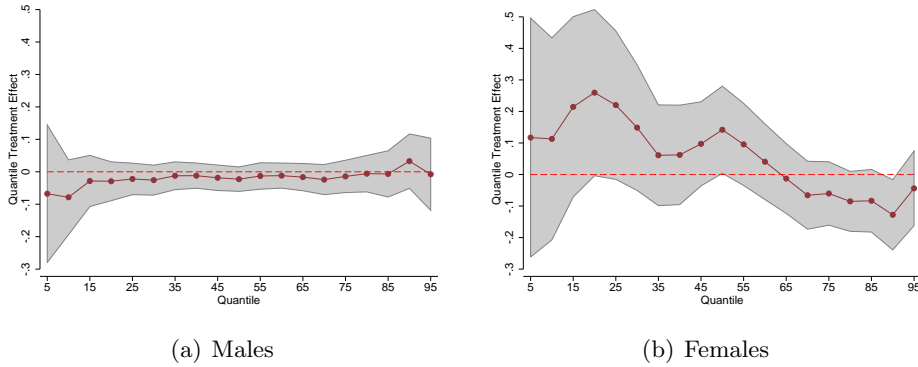
For females the small increase in average earnings is driven by lower income quantiles. These disproportionate effects possibly arise from the uptake of low-income and part-time occupations – an issue we return to in Section 5.3.



**Figure 7.** Quantile Treatment Effects: 7-Year Reform

Estimates based on Linked 1970 Census. Cohorts 1930-1940. 95% CI based on clustered SE (cluster unit: school-districts). Outcome Variable: **Log Earnings**: Natural logarithm of combined income from employment, self-employment and agriculture. (Conditional) Quantile Regression controlling for district fixed effects, local linear trends and average term length.

Figure 8 shows the analogue estimates for average term length. The general patterns are similar though at lower percentiles the size of the estimates for females is much larger than overall mean effects. For some percentiles, the estimates are as large as 20-30%, albeit imprecisely estimated. Again we find no impact on males along the earnings distribution.



**Figure 8.** Quantile Treatment Effects: Average Term Length

Estimates based on Linked 1970 Census. Cohorts 1930-1940. 95% CI based on clustered SE (cluster unit: school-districts). Outcome Variable: **Log Earnings**: Natural logarithm of combined income from employment, self-employment and agriculture. (Conditional) Quantile Regression controlling for district fixed effects, local linear trends and compulsory 7th year.

We also explore heterogeneity among different school districts by stratifying them according to their structural composition. We split school districts according to their pre-reform occupational structure based on the 1930 Census and the presence of a lower secondary school. First we group

districts into having a high share of manufactural workers if their school district exceeds the median share of manufactural workers in 1930. Second, we do the same for income per capita in 1930. Finally, we create an indicator of whether there is no secondary school (*Realskola*) within range of 10km. Table 5 gives the results from a regression with an interaction effect for all three indicators.

**Table 5.** Effect Heterogeneity: Log Earnings

	Manufacturing > Median		Income per capita > Median		Next <i>Realskola</i> > 10km	
	Males	Females	Males	Females	Males	Females
<b>7-Year</b>						
<b>Treatment</b>	0.001	0.009	-0.001	0.021	0.007	0.014
	[-0.011,0.012]	[-0.022,0.041]	[-0.012,0.009]	[-0.008,0.051]	[-0.009,0.023]	[-0.034,0.062]
<b>Treatment × Variable</b>	-0.005	0.026	0.004	-0.017	-0.010	0.004
	[-0.023,0.014]	[-0.032,0.084]	[-0.017,0.025]	[-0.079,0.046]	[-0.029,0.008]	[-0.050,0.059]
<b>Average Term Length</b>						
<b>Treatment</b>	-0.052	0.031	-0.057	0.019	0.025	0.104
	[-0.109,0.004]	[-0.121,0.183]	[-0.110,-0.005]	[-0.133,0.171]	[-0.050,0.101]	[-0.080,0.288]
<b>Treatment × Variable</b>	0.103	0.067	0.122	0.096	-0.065	-0.081
	[0.013,0.193]	[-0.156,0.289]	[0.028,0.216]	[-0.125,0.316]	[-0.149,0.020]	[-0.296,0.133]

Estimates based on Linked 1970 Census. Cohorts 1930-1940. 95% CI based on clustered SE (cluster unit: school-districts). All regressions control for cohort fixed effects, school district fixed effects and linear school district cohort trends and dummy variables indicating the interaction term.

In this exercise we do not detect substantial heterogeneity for males and females from the compulsory schooling reform. The effects are small and close to zero across groups. The effects of the term length extension however differ by the structural composition of the districts. Females living in more urban areas, implied by a higher share in manufacturing, experienced an increase in earnings about 10 % larger and those living in wealthier areas experienced an increase about 12% larger compared to districts below the median. Also those individuals residing in districts close to a *Realskola* had substantial benefits from the term length extension reform. For males the effects suggest a similar pattern. While it is difficult to disentangle the most important contributor to the effects we interpret this as evidence that the term length extension was more positively effective on earnings in more urban areas.

### 5.3 Labor Supply and Occupational Choice

In this section we explore whether the compulsory schooling reform and the weeks extension had effects on labor market and occupational choices which might explain why we only find income effects for females (especially at the bottom of the earnings distribution) and no effects for males. We construct dummy variables indicating whether an individual was working at all in 1970 and whether working full-time i.e. more than 35 hours per week. In order to examine occupational sorting we set up a range of variables for broader occupational groups and public sector employment in 1970. Table 6 shows results for the 7 year compulsory extension. As for earnings, effects are small and suggest only minor increases in females' labor force participation. The small positive



effect on earnings of about 2% for females therefore likely relate to the small increase in their labor market participation. We do not see any evidence of occupational sorting related to this reform.

**Table 6.** Labor Supply Outcomes (7-Year Extension)

	(1) Mean	(2) OLS YoE	(3) 7-Year	(4) Extensions
<b>Men</b>				
<b>Labor Supply</b>				
<b>Working</b>	0.961	0.002 [0.002,0.003]	0.002 [-0.001,0.004]	-0.000 [-0.004,0.003]
<b>Working Fulltime</b>	0.947	-0.001 [-0.002,-0.001]	0.002 [-0.001,0.005]	0.000 [-0.004,0.005]
<b>Public Sector Employment</b>				
<b>Governmental</b>	0.104	0.022 [0.021,0.023]	0.005 [0.000,0.009]	0.002 [-0.005,0.008]
<b>Municipal</b>	0.080	0.028 [0.027,0.029]	0.000 [-0.003,0.004]	0.002 [-0.004,0.007]
<b>Occupational Group</b>				
<b>Scientific, Medical, Technical</b>	0.216	0.074 [0.073,0.075]	0.004 [-0.002,0.010]	0.007 [-0.001,0.016]
<b>Transport, Communication</b>	0.087	-0.013 [-0.014,-0.013]	-0.002 [-0.007,0.002]	-0.005 [-0.011,0.001]
<b>Service</b>	0.043	0.004 [0.003,0.005]	0.002 [-0.001,0.005]	0.001 [-0.003,0.006]
<b>Women</b>				
<b>Labor Supply</b>				
<b>Working</b>	0.569	0.033 [0.031,0.035]	0.006 [-0.001,0.014]	0.007 [-0.004,0.017]
<b>Working Fulltime</b>	0.320	0.030 [0.029,0.032]	0.004 [-0.002,0.011]	0.005 [-0.005,0.015]
<b>Public Sector Employment</b>				
<b>Governmental</b>	0.047	0.007 [0.007,0.007]	0.002 [-0.001,0.005]	-0.002 [-0.006,0.002]
<b>Municipal</b>	0.206	0.052 [0.050,0.054]	0.001 [-0.005,0.007]	0.006 [-0.002,0.014]
<b>Occupational Group</b>				
<b>Scientific, Medical, Technical</b>	0.162	0.077 [0.077,0.078]	0.000 [-0.005,0.005]	0.005 [-0.002,0.013]
<b>Transport, Communication</b>	0.023	-0.002 [-0.002,-0.001]	0.000 [-0.002,0.002]	-0.001 [-0.004,0.002]
<b>Service</b>	0.105	-0.020 [-0.021,-0.020]	-0.001 [-0.006,0.004]	0.001 [-0.006,0.008]
<b>Cluster FE</b>		✓	✓	✓
<b>Linear Trends</b>				✓

Estimates based on Linked 1970 Census. Cohorts 1930-1940. 95% CI based on clustered SE (cluster unit: school-districts). All regressions control for Cohort FE and District Term Length. Outcome Variables: **Working**: Whether an individual was economically active (> 1 hour paid work) in the Census Week or up to 4 months earlier if temporarily on leave. **Working Fulltime**: Working more than 35 hours in Census week. **Governmental**: Working in higher (state) level of government. **Municipal**: Working in lower (local) level of government. Specifications: (1) sample average of outcome variables, (2) OLS regression coefficient for years of education, (3) Reform Effect with school district FE, (3) Reform Effect with school district FE and linear school district trends.

In contrast the week extension had large and positive effects for females. Section 5.2 indicated an increase in earnings for females of about 5%. As shown in Table 7 this effect is driven by an increase in females' labor market participation. Females exposed to the reform were about 3-4 percentage points (about 6% in comparison to the baseline) more likely to be working in 1970 and 1-2 percentage points (about 4% in comparison to the baseline) more likely to be working full-time.

During the 1950s and 1960s when our cohorts were about to join the labor market the Swedish

welfare state greatly expanded. Since the expansion offered a lot of new jobs with women-friendly working conditions (Stanfors, 2003), we also consider governmental and municipal public sector employment and occupational choices of males and females in order to deepen our understanding of the income effects. Our results suggest that the weeks extension encouraged females to work in the public sector as implied by a 11% increase in municipal and a 21% increase in governmental employment. They especially started to work in the transport and communication, and service branches which generally include lower income jobs. Results also correspond well to the quantile treatment effects, which show income gains at the lower part of the distribution, since we do not see any positive impact on higher skilled jobs in e.g. the scientific, medical and technical branch. Females enhanced their labor market participation at the extensive margin and apparently selected into minor employment. This is in line with the above results on educational attainment which show no further educational attainment beyond 7 years of compulsory schooling which could had lead to higher skilled occupations. Our findings are also in line with the recent findings of Bhalotra et al. (2016), who show that the overall development of the labor market induced by the expansion of the Swedish welfare state and public policies in the 1950s especially benefited females.

For males on the other hand, there are no positive effects on income and employment when including trends. It is not surprising that there are no real findings for working and fulltime employment in 1970 since almost 100% of males already worked fulltime during this time. But results indicate that males experienced a negative development of income which can relate to occupational choices. Just like females, males were more likely to work in the transport and communication, and service branches but at the same time they reduced employment in the scientific, medical and technical branch. This implies that treated males shifted their focus from higher skilled jobs to lower skilled jobs resulting in a lower and even negative income effect. While females benefited from the weeks extension by enhanced employment and the take up minor work, males decided to leave school earlier and work in lower skilled jobs instead. With the expansion of the Swedish welfare state starting in the 1950s females gained a lot of employment possibilities while males were left behind due to disadvantageous occupational choices.

**Table 7.** Labor Supply Outcomes (Average Term Length)

	(1) Mean	(2) OLS YoE	(3) Average Term Length	(4)
<b>Men</b>				
<b>Labor Supply</b>				
<b>Working</b>	0.961	0.002 [0.002,0.003]	0.000 [-0.007,0.008]	-0.005 [-0.023,0.012]
<b>Working Fulltime</b>	0.947	-0.001 [-0.002,-0.001]	0.002 [-0.007,0.011]	-0.011 [-0.032,0.010]
<b>Public Sector Employment</b>				
<b>Governmental</b>	0.104	0.022 [0.021,0.023]	-0.003 [-0.018,0.012]	0.007 [-0.025,0.038]
<b>Municipal</b>	0.080	0.028 [0.027,0.029]	-0.002 [-0.013,0.009]	-0.009 [-0.034,0.015]
<b>Occupational Group</b>				
<b>Scientific, Medical, Technical</b>	0.216	0.074 [0.073,0.075]	-0.023 [-0.040,-0.005]	-0.059 [-0.100,-0.018]
<b>Transport, Communication</b>	0.087	-0.013 [-0.014,-0.013]	0.020 [0.007,0.033]	0.038 [0.011,0.065]
<b>Service</b>	0.043	0.004 [0.003,0.005]	0.010 [0.001,0.020]	0.012 [-0.008,0.031]
<b>Women</b>				
<b>Labor Market Outcome</b>				
<b>Working</b>	0.569	0.033 [0.031,0.035]	0.044 [0.020,0.067]	0.035 [-0.016,0.086]
<b>Working Fulltime</b>	0.320	0.030 [0.029,0.032]	0.021 [0.000,0.042]	0.012 [-0.034,0.057]
<b>Public Sector Employment</b>				
<b>Governmental</b>	0.047	0.007 [0.007,0.007]	0.010 [0.000,0.019]	0.012 [-0.008,0.032]
<b>Municipal</b>	0.206	0.052 [0.050,0.054]	0.023 [0.003,0.042]	0.023 [-0.018,0.065]
<b>Occupational Group</b>				
<b>Scientific, Medical, Technical</b>	0.162	0.077 [0.077,0.078]	0.003 [-0.014,0.020]	0.003 [-0.032,0.038]
<b>Transport, Communication</b>	0.023	-0.002 [-0.002,-0.001]	0.010 [0.003,0.017]	0.012 [-0.003,0.028]
<b>Service</b>	0.105	-0.020 [-0.021,-0.020]	0.011 [-0.005,0.026]	0.020 [-0.014,0.053]
<b>Cluster FE</b>		✓	✓	✓
<b>Linear Trends</b>				✓

Estimates based on Linked 1970 Census. Cohorts 1930-1940. 95% CI based on clustered SE (cluster unit: school-districts). All regressions control for Cohort FE and compulsory 7th year in primary school. Outcome Variables: **Working**: Whether an individual was economically active ( $> 1$  hour paid work) in the Census Week or up to 4 months earlier if temporarily on leave. **Working Fulltime**: Working more than 35 hours in Census week. **Governmental**: Working in higher (state) level of government. **Municipal**: Working in lower (local) level of government. Specifications: (1) sample average of outcome variables, (2) OLS regression coefficient for years of education, (3) Average Term Length controlling for school district FE, (4) + linear school district trends.

## 6 Conclusion

This paper examines two Swedish school reforms which increased the total amount of education at different margins. Our analysis suggests that the compulsory schooling reform which increased primary education from 6 to 7 years was largely ineffective regarding later life returns, while the extension of the average term length from 34.5/36.5 to 39 weeks had sizeable effects on employment and earnings for females. Especially the lower part of the female distribution benefited from the term length extension suggesting a reduction in male–female and socio-economic inequalities. We

argue that females gained from the overall development of the labor market, while males made disadvantageous occupational choices leading to zero returns.

Overall, our results are in line with the recent literature arguing that there are only minor or zero returns to compulsory education. However, our results also indicate that it is fruitful to address childrens' cognitive development through instructional time at younger ages – the sooner the better. 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) estimates zero returns to education in Germany and interpret this as a consequence of that basic skills are already fixed in grade 8 which was the year of schooling when the German reform was introduced. The extension of one more year of academic training left students basic skills unaffected. Our results suggest that skill formation is crucial at an even earlier point in time.

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## A First Stage (Survey)

An empirical analysis of the 7-year extension based solely on register data cannot identify educational effects of the reform as Swedish register data does not contain information on the actual length spent in the old Swedish primary school or students with 7 or less years spent in school.<sup>23</sup> In order to assess the impact of a compulsory schooling extension from 6 to 7 years we present empirical evidence based on survey data including self-reported information on years of schooling. This exercise illustrates that the reform increased educational attainment substantially. Regression results are given in Table 8. Adopting a standard difference-in-difference estimation approach we can establish an increase of educational attainment of a magnitude between 0.25-0.35 years of schooling due to the reform. It should be noted that estimates based on survey material are downward biased as we assigned reform status by place of birth. Migration during childhood and the potentially erroneous place of birth coding due to hospital births induce measurement error in the exposure variable. The actual increase can be expected to be considerably higher given relatively high migration rates. Adding estimated effects of the compulsory schooling extension on a binary indicator for more schooling than the old compulsory educational level of 6 years gives a more precise estimate for the reform effects. This stems from the fact that the schooling variable from the self-evaluated survey is relatively noisy. Estimates nevertheless lie extremely close to the estimates for the years of schooling, suggesting that the reform did not extend schooling beyond the additional year in the basic primary track. This confirms results based on the Linked Census on educational spill-overs (see Table 2).

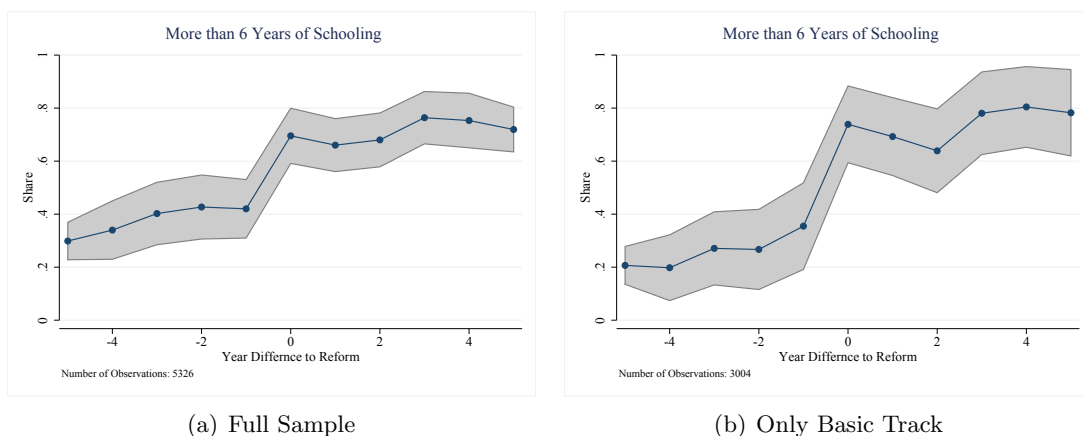
**Table 8.** First Stage: Effects on Schooling

	(1)	(2)	(3)
	<b>Years of Schooling</b>		
<b>7-Year Extension</b>	1.111 [0.800,1.422]	0.361 [0.131,0.591]	0.260 [0.008,0.513]
<i>F</i> Statistic	49.106	9.506	4.096
N	5326	5326	5326
School Districts		1286	1286
	<b>Indicator more than 6 Years of Schooling</b>		
<b>7-Year Extension</b>	0.421 [0.373,0.468]	0.334 [0.269,0.399]	0.277 [0.212,0.342]
<i>F</i> Statistic	302.199	102.506	69.198
N	5326	5326	5326
School Districts		1286	1286
<b>Cohort Dummies</b>	✓	✓	✓
<b>Survey Year Dummies</b>	✓	✓	✓
<b>Cluster FE</b>		✓	✓
<b>County Cohort Trends</b>			✓

Estimates are based on the ULF survey, cohorts born 1920-1937. Standard errors are clustered on school district level and 95% CI are added.

<sup>23</sup>As mentioned in the data section the Census 1970 only identifies students with 7 or less years in *Folkskola*. The Educational Registers are even less informative in this respect as they do not differentiate between any length in *Folkskola* and do not capture the highest attended school for those with post-schooling degrees.

Column (3) in Table 8 indicates that estimates are somewhat trend sensitive. Therefore, we estimate the indicator for education beyond compulsory level on lags and leads of the treatment indicator. Figure 9(a) shows a clear jump around the cutoff when school districts introduced a seventh grade. The development before the reform year is smooth with no further discontinuities. We also restrict the sample to students with *Folkskola* as highest attended school, which increases estimates as expected. Measurement error in the instrument and the presence of always takers<sup>24</sup> explain why we cannot see a sharp discontinuity for students in the basic track only.



**Figure 9.** Event Study for Compulsory Schooling Extension

Event Study Graphs: The graphs plots estimates for lags and leads of the 7-Year reform indicator with 95% CI based on clustered SE (cluster unit: school districts). Zero indicates the year when a school district introduced a seventh grade in *Folkskola*. All regressions include school district FE, birth cohort and survey dummies. Figure 9(b) excludes individuals attending more than primary school and districts with a hospital at the year of birth.

<sup>24</sup>In some districts prior to the 7-Year extension higher forms of *Folkskola* existed (*Högre Folkskola, högre avdelning*. Grade repeaters are another source of always takers, though grade retention was rather seldom.)

## B Balancing Tests

**Table 9.** Balancing Test (Compulsory Schooling)

	(1)	(2)	(3)	(4)	(1)	(2)	(3)	(4)
	Mean	Treatment			Mean	Treatment		
	Mothers				Fathers			
<b>Socio-Demographics</b>								
<b>White Collar Worker</b>	0.037	0.010*** (0.002)	0.002 (0.002)	0.002 (0.003)	0.234	0.040*** (0.008)	-0.006 (0.005)	-0.009 (0.007)
<b>Academic Education (1960)</b>	0.011	0.003** (0.001)	-0.001 (0.001)	-0.001 (0.002)	0.053	0.011*** (0.003)	-0.005 (0.003)	0.001 (0.004)
<b>Parent's age at child's birth</b>	29.394	-0.213** (0.090)	0.054 (0.078)	0.110 (0.097)	33.355	-0.357*** (0.117)	0.171* (0.090)	0.195* (0.112)
<b>Parent born abroad</b>	0.015	0.005*** (0.001)	0.000 (0.002)	-0.001 (0.002)	0.009	0.002** (0.001)	-0.000 (0.001)	-0.000 (0.001)
<b>Unmarried 1950</b>	0.011	-0.003* (0.002)	-0.002 (0.001)	-0.002 (0.002)	0.000	-0.000 (0.000)	0.000 (0.000)	0.000 (0.000)
<b>Occupational Group (1950)</b>								
<b>Scientific, Medical, Technical</b>	0.014	0.002* (0.001)	0.001 (0.001)	0.001 (0.002)	0.084	0.015*** (0.004)	-0.004 (0.004)	-0.004 (0.004)
<b>Administrative</b>	0.000	0.000 (0.000)	-0.000 (0.000)	0.000 (0.000)	0.026	0.006** (0.003)	-0.000 (0.002)	-0.002 (0.003)
<b>Accounting</b>	0.004	0.002*** (0.001)	-0.000 (0.001)	-0.000 (0.001)	0.019	0.005*** (0.002)	-0.002 (0.002)	-0.001 (0.002)
<b>Sales</b>	0.011	0.006*** (0.001)	0.001 (0.001)	0.001 (0.002)	0.055	0.015*** (0.003)	0.003 (0.003)	0.004 (0.004)
<b>Agricultural</b>	0.008	-0.007*** (0.002)	-0.002* (0.001)	-0.001 (0.001)	0.348	-0.145*** (0.020)	-0.019*** (0.005)	-0.006 (0.006)
<b>Mining</b>	0.000	-0.000 (0.000)	-0.000 (0.000)	-0.000 (0.000)	0.008	0.001 (0.002)	0.001 (0.001)	0.000 (0.001)
<b>Transport, Communication</b>	0.006	0.000 (0.001)	0.000 (0.001)	-0.001 (0.001)	0.067	0.016*** (0.003)	0.003 (0.003)	-0.001 (0.004)
<b>Crafts</b>	0.023	0.007** (0.003)	0.000 (0.002)	-0.003 (0.002)	0.361	0.078*** (0.013)	0.017*** (0.006)	0.009 (0.007)
<b>Service</b>	0.027	0.006** (0.002)	-0.002 (0.002)	-0.004 (0.003)	0.028	0.009*** (0.003)	0.000 (0.002)	0.001 (0.003)
<b>No occupation</b>	0.907	-0.016*** (0.005)	0.002 (0.004)	0.007 (0.005)	0.003	-0.001 (0.001)	-0.000 (0.001)	-0.000 (0.001)
<b>Cluster FE</b>			✓	✓			✓	✓
<b>Linear Trends</b>				✓				✓

Estimates based on Linked 1970 Census. Cohorts: 1933-1940. Cohorts: 1933-1940. Robust Standard Errors in parenthesis. All regressions control for Cohort FE and District Term Length. Significance levels: \* 0.10 \*\* 0.05 \*\*\* 0.01. Specifications: (1) Sample average of outcome variables, (2) baseline without school district controls (3) + school district FE, (4) + linear school district trends.

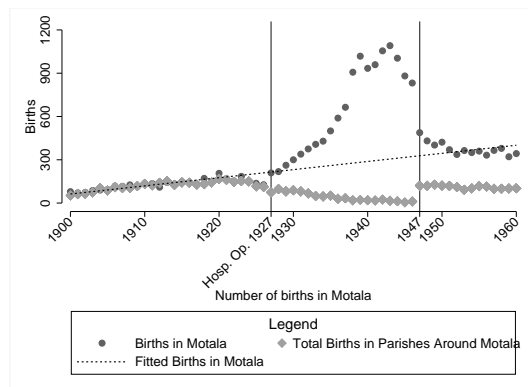
**Table 10.** Balancing Test (Average Term Length)

	(1)	(2)	(3)	(4)	(1)	(2)	(3)	(4)
	Mean	Treatment			Mean	Treatment		
	Mothers				Fathers			
<b>Socio-Demographics</b>								
White Collar Worker	0.037	0.063*** (0.009)	0.009 (0.007)	0.001 (0.024)	0.234	0.274*** (0.035)	0.011 (0.016)	-0.042 (0.053)
Academic Education (1960)	0.011	0.019*** (0.004)	-0.004 (0.004)	0.015 (0.014)	0.053	0.092*** (0.013)	-0.021** (0.009)	0.030 (0.029)
Parent's age at child's birth	29.394	-2.696*** (0.235)	-0.283 (0.240)	-1.270 (0.776)	33.355	-4.125*** (0.367)	-0.238 (0.276)	-0.849 (0.893)
Parent born abroad	0.015	0.015*** (0.004)	0.001 (0.005)	0.006 (0.015)	0.009	0.005** (0.002)	0.003 (0.004)	0.019 (0.012)
Unmarried 1950	0.011	0.003 (0.003)	0.002 (0.004)	-0.010 (0.013)	0.000	0.001** (0.000)	0.001 (0.001)	0.003 (0.002)
<b>Occupational Group (1950)</b>								
Scientific, Medical, Technical	0.014	0.020*** (0.003)	0.002 (0.005)	0.013 (0.015)	0.084	0.131*** (0.014)	-0.011 (0.011)	-0.011 (0.035)
Administrative	0.000	0.001*** (0.000)	0.000 (0.001)	0.003 (0.002)	0.026	0.034*** (0.007)	0.005 (0.006)	0.017 (0.020)
Accounting	0.004	0.011*** (0.002)	-0.003 (0.002)	-0.010 (0.008)	0.019	0.042*** (0.006)	0.001 (0.005)	-0.021 (0.018)
Sales	0.011	0.027*** (0.005)	0.006 (0.004)	-0.008 (0.013)	0.055	0.067*** (0.012)	-0.005 (0.009)	-0.003 (0.029)
Agricultural	0.008	-0.010*** (0.003)	-0.010*** (0.004)	0.019* (0.011)	0.348	-0.765*** (0.084)	-0.034** (0.015)	0.090* (0.050)
Mining	0.000	0.000 (0.000)	0.000 (0.000)	0.000 (0.000)	0.008	0.003 (0.004)	-0.002 (0.003)	0.007 (0.011)
Transport, Communication	0.006	0.005** (0.002)	0.007** (0.003)	0.013 (0.009)	0.067	0.051*** (0.010)	0.008 (0.010)	-0.001 (0.032)
Crafts	0.023	0.031*** (0.011)	0.017*** (0.006)	-0.031* (0.019)	0.361	0.380*** (0.040)	0.039** (0.018)	-0.085 (0.059)
Service	0.027	0.044*** (0.007)	0.023*** (0.006)	0.005 (0.020)	0.028	0.053*** (0.007)	-0.002 (0.007)	0.005 (0.021)
No occupation	0.907	-0.128*** (0.024)	-0.043*** (0.011)	-0.002 (0.036)	0.003	0.003*** (0.001)	0.003 (0.002)	0.002 (0.007)
Cluster FE			✓	✓			✓	✓
Linear Trends				✓				✓

Estimates based on Linked 1970 Census. Cohorts: 1933-1940. Robust Standard Errors in parenthesis. All regressions control for Cohort FE and compulsory 7th year in primary school. Significance levels: \* 0.10 \*\* 0.05 \*\*\* 0.01. Specifications: (1) Sample average of outcome variables, (2) baseline without school district controls (3) + school district FE, (4) + linear school district trends.

## C Migration, Hospital Births and Reform Assignment

In this section we evaluate migration pattern and reform assignment based on the place of birth. For cohorts born in the 1920s and early 1930s the dominant mode of delivery was home births, while the norm from the mid 1930s was to give birth in an institution. As mentioned in Section 3.2 the location of the hospital, and not the the place of residence of the parents, was recorded as the place of birth in Swedish register data until 1947. Figure 10 illustrates the effects arising from a hospital opening on the recorded births for the city of Motala as an example. After the opening of the hospital in 1927 the recorded births in the city grew rapidly while the parishes around the city recorded fewer newborns. Children from parents living around the city, but who were born in the newly established hospital were now recorded as being born in Motala. From 1947 onwards the place of residence of the parents is registered as place of birth and the number of births in Motala sharply decreased.



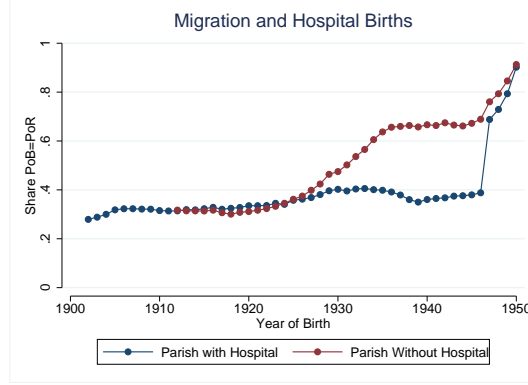
**Figure 10.** Recorded Births: Motala

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

Whether the place of birth (PoB) is a sufficiently good proxy for the place of residence during adolescence depends on migration patterns during adolescence and the actual information recorded as place of birth. Figure 11 shows the share of individuals in the 1950 Census for each year of birth where the *place of birth* coincides with the *place of residence in 1950*. In order to capture the effect from the hospital coding we add information from openings of birthing centers across Sweden since 1900 and split the population by the presence of a birthing center in the parish of birth. For parishes without a hospital deviations stem mostly from migration. These occur mainly during the first years of life and in the age group of 15-30. For children during schooling age and for older adults migration appears to be a seldom phenomena as their parents unlikely move in this period

of life.

The gap between parishes with and without a hospital at the year of birth is a direct result of the recording of the place of birth prior to 1947. Until 1947 the location of the hospital instead of the parental place of residence was recorded. Figure 11 suggests that in the absence of a hospital the parish of birth coincides with the parish during schooling age in approximately 70% of all cases.



**Figure 11.** Hospital Births vs. Migration

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

## D Measuring Years of Education in the Swedish Register

The following sections discuss ways to approximate years of educational attainment when using Swedish register data. The standard approach adopted in the empirical literature is to assign years of education to individuals is based on the highest attained educational degree. In the following we will distinguish between *schooling* and *post-schooling* as the two parts of education. By *schooling* we refer to education which does not lead to any form of higher degree but rather the basic education in school (*Folkskola*, *Grundskola*, *Realskola*, *Gymnasium* in the Swedish context). *Post-schooling* then refers to all further education after attending school (vocational training, apprenticeship or university). We argue that in certain applications the standard procedure based on the highest degree might miss important variation in the actual years of education stemming from variation in schooling. We will suggest an alternative measure for the years of education based on information on schooling from then Swedish Census in 1970 and the length of the highest degree from the Educational Registers (*Utbildningsregistret*).

### D.1 The Standard Approach

Most empirical work based on Swedish register data related to education builds upon the Swedish Educational Registers which capture information on the highest degree achieved (*level and type of*



*education*) or, since the latest revision of the classification, information on the latest incomplete degree.<sup>25</sup> The empirical literature on effects of years of education on labor market outcomes and various other socio-demographic characteristics using Swedish register data usually relies on a transformation of the highest degree achieved into an average length in years associated with a certain degree. Some authors have based their transformation by linking the educational registers to the Swedish Level of Living Survey (LNU for *Levnadsnivåundersökningen*) which contains self-evaluated information on the length of total education (see e.g. Meghir and Palme (1999) or Isacson (1999)).<sup>26</sup> Meghir and Palme (1999) e.g. estimate the mean years of education based on the self-evaluated question in the LNU for each of the seven main categories of the highest degree classification and apply these averages as years of education. From this perspective, the standard transformations are empirically well-grounded and generally deliver a reasonable approximation of the actual length of education (see e.g. Antelius and Björklund (2000)).

In the following we will argue that the standard ways to approximate total years of education can miss important variation in the actual amount of schooling and that these differences matter quantitatively. By construction, years of education based on the highest degree are (*ceteris paribus*) insensitive towards the amount of schooling below the highest achieved level of education except where the schooling degree coincides with highest degree.

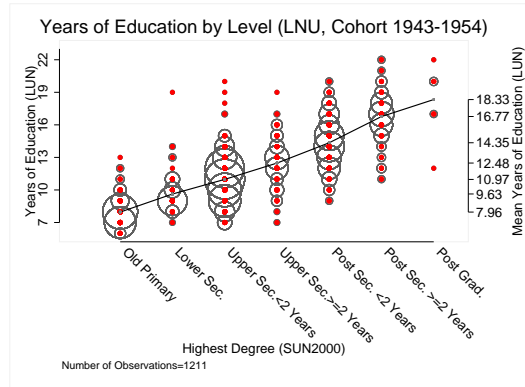
For example, following the standard transformation all individuals with a vocational degree (as indicated by their highest degree in the Educational Registers) receive an equivalent number of years of education - corresponding to the average length of schooling - irrespective of their amount of schooling prior to the vocational training. This concerns a substantial part of the population for cohorts born prior to 1960 and consequently affects, among other things, evaluations of long-term effects of education using Swedish register data. Before the strong unification of the primary and lower secondary educational system in the 1950s and 1960s, the diversity of various tracks within the Swedish school system lead to substantial variation in the amount of education before acquiring the highest degree. Figure 12 exemplifies this variation for the levels of education according to the SUN2000 classification<sup>27</sup> in the LNU Survey of 2000.

<sup>25</sup>The SUN2000 Classification was introduced in October 1999 by Statistics Sweden (SCB). The main changes constituted a closer relation to the international standard education classification (ISCED97) and the construction of a more user-friendly classification system than the former SUN classification. For a detailed description see *SUN 2000 Svensk utbildningsnomenklatur MIS 2000:1 (Swedish standard classification of education)*, ISSN 1402-0807, SCB Sweden.

<sup>26</sup>Questionnaire 2000: *Hur många år har Din sammanlagda skol- och yrkesutbildning på heltid varat? (Från första klass och uppåt.)*

<sup>27</sup>The broadest usual classification of levels of education are given by

- Level 1: 7 Years, Old Primary
- Level 2: 9 Years, (New) Lower Secondary, 9.5 Years (Old) Lower Sec.
- Level 3: 11 Years, Upper Sec. < 2 Years
- Level 4: 12 Years, Upper Sec. ≥ 2 Years



**Figure 12.** Years of Education for Each SUN Class Based on LNU (Survey Year 2000)

The left y-axis shows the self-evaluated years of education based on the LNU survey. The x-axis gives the different broad levels of the SUN-classification. The right y-axis presents the estimated average years of education for each SUN-class based on the self-evaluated years of education. The circles are proportional to the group size associated with each SUN-classification/Self-Evaluated Years of Education combination.

The graph clearly indicates a sizable *within-group* variation across the educational classes. This variation is mainly attributable to two main factors<sup>28</sup>:

1. Different length of the educational programmes leading to the highest degree.
2. Different length of schooling before attending post-schooling education.

The first point can be addressed by using finer SUN2000 degree classifications. The higher levels 3-7 in the SUN2000 codes can be subdivided by their specific length, i.e. more detailed information on the length of post-schooling can be derived directly from the Educational Registers.

Differences in the length of schooling before post-schooling education however cannot be extracted directly from the information in the educational registers. These differences are hidden below the highest degree.

## D.2 An Alternative Years of Education Measure

To overcome shortcomings of the standard approach we suggest an alternative approximation which combines information on schooling from the highest school-degree in the Swedish Census from 1970 and the highest degree from the Educational Registers to gain information on post-schooling. In the Swedish Census in 1970 individuals born between 1911-1954 were asked to report their highest attended school-type. The upper part of Table 11 gives a broad classification of the different

- Level 5: 14 Years, Post Sec. < 2 Years
- Level 6: 15.5 Years, Post Sec.  $\geq$  2 Years
- Level 7: 19 Years, Post Grad.

with a usual transformation of years of education associated with the specific degree (see e.g. in Holmlund (2008)).

<sup>28</sup>Additional possible sources of variation are class repetition, multiple post-degrees and misreporting in the self-evaluated variable.

categories from the Census 1970 and the years of education assigned to the different types of school degree.<sup>29</sup>

As a measure of post-schooling attainment we use detailed information from the Educational Registers. The SUN2000 classification captures the usual associated length of an educational programme in the second digit of the three digit code. The associated years of post-schooling for different SUN2000 codes can be found in the lower part of Table 11. In order to avoid double counting we exclude highest degrees which are associated with theoretical oriented programmes which enable to study at university in the category of higher secondary education (Codes 300-340). In most cases these codes capture information for individuals which went to *Gymnasium* but never acquired further post-schooling education. As the Census accounts already for *Gymnasium* we do not add additional years of education for the same degree.

**Table 11.** Sources for New Education Variable

Census 1970	
Years of Schooling	Schooling Group
7	Folkskola 7 Years or Less
8	Folkskola 8 Years and Dropouts Realskola
9	9 Years Folkskola, Grundskola and Folkhögskola
9.5	Realskola, Flickskola and Dropouts Gymnasium
11	Short Gymnasium
12	Gymnasium
Educational Registers	
Years of Post-Schooling	SUN2000 Group
1	310/313/317 400-430
2	320/323/327 520-529
3	330/333/337 530-539
4	540-549
5	550-559
7	600-620
9	640

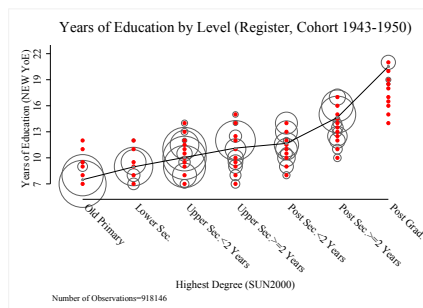
The combination of the years of schooling derived from the Census 1970 and the length of the highest degree as a measure for post-schooling gives an alternative measure for years of education (we will refer to this in the following as *New YoE*):

$$\text{New YoE} = \text{Years of Schooling (Census1970)} + \text{Years of Post Schooling (SUN2000)}.$$

This measure is sensitive to changes within the school system and is able to capture the variation

<sup>29</sup>The associated years of education are identical to the standard approach in case the school degree matches the highest degree. Furthermore, the years coincide with a self-evaluated variable in the ULF survey and are therefore empirically backed up. Unfortunately, some groups are indistinguishable in the Census (e.g. 9-Year *Folkskola*, *Folkhögskola* and *Grundskola* are in the same category). The difference between long and short *Gymnasium* is based on the SUN2000 classification. In case individuals only had *Gymnasium* as highest degree the SUN2000 Classifications indicate the length of associated *Gymnasium* degree. We used the most-frequent combination to classify different types of *Gymnasium* in the Census in short and long *Gymnasium*.

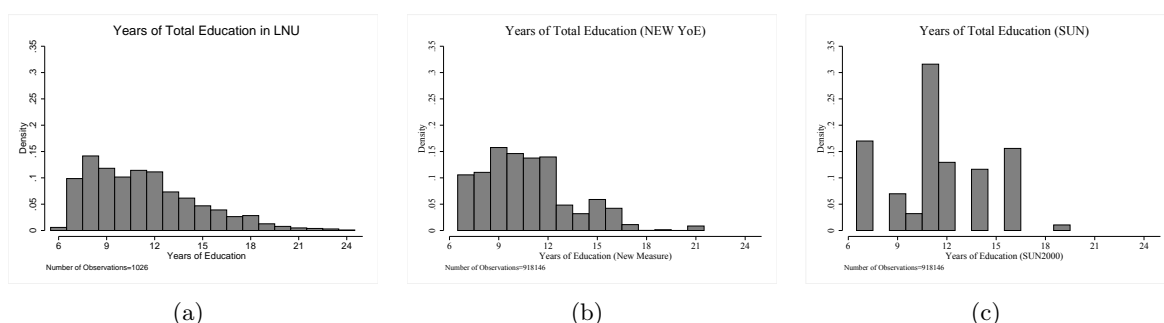
in education within highest degree groups in the registers. Figure 13 shows that the new measure indeed reflects a similar pattern as the self-evaluated years of education variable from the LNU-Survey in Figure 12 with respect to the SUN2000 classification. Especially it reflects the variation in total years of education within highest degree groups.



**Figure 13.** Years of Education for Each SUN Class Based on New YoE

The left y-axis shows New YoE. The x-axis gives the different broad levels of the SUN-classification. The circles are proportional to the group size associated with each SUN-classification/New YoE combination. The sample is restricted to cohorts 1943-1950 for comparability as individuals born after 1950 attending *Gymnasium* have in most cases not finished their schooling in 1970.

Figures 14(a), 14(b) and 14(c) compare the distributional features of the new education measure compared to the self-evaluated variable from the LNU survey and the standard transformation based on the highest degrees. The new alternative measure generally matches the distributional features of the self-evaluated years of education variable from the LNU survey well. It is slightly more skewed to the left as the new alternative measure does not capture multiple degrees, prolonged indirect educational paths or grade repetition.<sup>30</sup> Therefore, the total amount of education is slightly underestimated. In stark contrast the distributional features of the standard transformation based on the Educational Registers deviate substantially from the two other measures, even though the overall average length of education is well captured (as it is based and validated in the LNU survey).



**Figure 14.** Comparison Distribution Years of Education: (a) Self-Evaluated YoE LNU Survey; (b) New YoE Measure; (c) Standard YoE Measure (SUN2000).

<sup>30</sup>E.g. many students left regular *Gymnasium* (3-Years) after 13 Years. In the school system before the introduction of the comprehensive school they generally had two alternative ways leading to *Gymnasium*: A 10-year route (6 years *Folkskola* / 4 years *Realskola*) and a 9-year route (4 years *Folkskola* / 5 years *Realskola*).

## E Primary Education and Compulsory Schooling in Sweden

Sweden has a long-lasting tradition of compulsory education. Already in 1842, church parishes<sup>31</sup> were obliged to offer schooling by an approved teacher and children in *school age* had to attend the local primary schools (*Folkskola*) (Fredriksson, 1971).<sup>32</sup> 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. Different local regulations with respect to term length and various options to fulfil the compulsory curriculum<sup>33</sup> 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.<sup>34</sup> 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. The legal, administrative and pedagogical control of the school district was handled by regional school inspectors appointed by the Ministry of Ecclesiastical affairs.<sup>35</sup>

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<sup>36</sup> that a seventh school year *could* be made compulsory in a school district (Fredriksson,

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<sup>31</sup>Until the mid twentieth century the church was responsible for the Swedish primary school (*Folkskola*) and school-districts coincided almost exclusively with parishes.

<sup>32</sup>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)

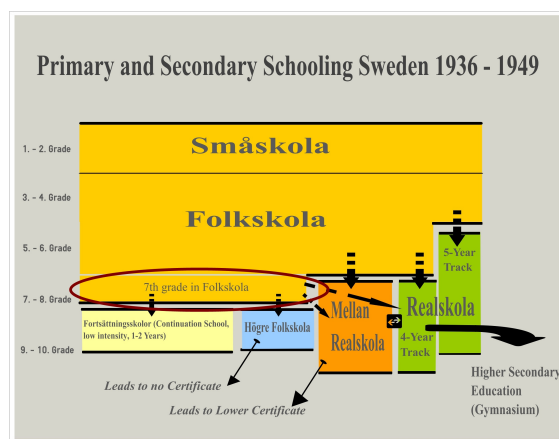
<sup>33</sup>Term length was not fixed to a certain amount of weeks and especially rural districts initially offered half-time reading (children only attended primary school every second week day or only half of a year). By the end of the nineteenth century children could also leave *Folkskola* after an exit examination or due to poverty.

<sup>34</sup>*Utbildningsplanen* was a governing document and included time-tables and syllabuses for compulsory education (Lindmark, 2009) and remained intact until the 1950s.

<sup>35</sup>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).

<sup>36</sup>Paragraph 47 mom. 4.

1950). However, only very few districts introduced a mandatory seventh year and still *Folkskola* generally covered six years.<sup>37</sup> With respect to equality of opportunity, the Swedish school system in the early 20th century was very selective. Children were tracked into separate schools based on their academic achievements after spending the first four years in *Folkskola*. High achievers could follow the academic track at the 5-Year *Realskola* (junior secondary education) which could lead to *Gymnasium* (upper secondary education) and University. The remaining students stayed in *Folkskola* for at least two more years, when there was another option to enter lower secondary education. Students not enrolling to secondary education took low intensive courses six weeks per year in continuation schools for two more years after compulsory schooling. Figure 15 gives a stylized presentation of the various school types and continuation options following the basic primary education.<sup>38</sup>



**Figure 15.** Swedish Primary School System

The majority of students in the 1920s till the 1940s only completed compulsory schooling and compared to e.g. the US where more than 70 per cent enrolled in high school by 1940, relatively few students continued to secondary schooling. In 1930, less than two per cent of the adult population had upper secondary education or more (Björklund et al., 2004) and in 1940, only ten per cent of the cohort graduating from *Folkskolan* continued to junior secondary education and only five

<sup>37</sup>The most southern Swedish region Scania and some larger cities were early birds and introduced a compulsory seventh grade during the 1920s. Furthermore, several cities had a so-called *högre avdelning* (higher divisions of *Folkskola*) which covered up to three additional years after grade 6. The *högre avdelning* was however never mandatory.

<sup>38</sup>In a massive educational reform in the 1950s and 1960s the old primary and lower secondary school system was replaced by a high-school type comprehensive school system. The various old school types were then abolished in favour of a single 9-year comprehensive school (*Grundskola*). The reform reshaped the entire school system and included among other things a compulsory schooling extension to nine-years, the abolishment of tracking and a regional integration. In contrast to the reforms taking part during first part of the twentieth century, this reform is the subject of a number studies on the impacts of education and extremely well documented, see e.g. Meghir and Palme (2005); Lundborg et al. (2014); Hjalmarsson et al. (2014); Lager and Torssander (2012). Interestingly it often went unnoticed that the comprehensive school reform only constituted the second part of an ongoing reform process of the Swedish primary and lower secondary school system which eventually culminated in the establishment of the new type of school. In fact, Sweden exhibited a continuous roll-out of extending the mandatory amount of schooling from 6 to 9 years over a period of 40 years.

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 et al., 1986).

## E.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,<sup>39</sup> the national government decided that seven-year schooling should be made compulsory in 1936.<sup>40</sup> The law gained legal validity on July 1 the same year, and the decision to extend compulsory schooling by an extra year was taken by the school board of the school district. The reform was not implemented at the same time in all school districts. Rather it was stipulated that a seventh year had to be implemented all across the country before the school year 1948/49. The compulsory seventh year was consequently introduced during a twelve-year transition period.

The main motive for the reform was that six years was considered too short for achieving the learning objectives that were stated for the *Folkskola*.<sup>41</sup> 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).<sup>42</sup>

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<sup>39</sup>In the debate preceding the introduction of the seven year reform, politicians often benchmarked with other Western countries, emphasizing that the number of school years was the most striking difference of compulsory education in Sweden compared with Denmark, Norway, Germany and Great Britain. This discourse was also brought forward in the contemporary educational literature, e.g. in the *Svensk Läraretidning* (the teachers' journal). According to (Ståfelt, 1930, p.823) Sweden is *lagging* behind with respect to the length of compulsory schooling compared to other countries in Europe.

<sup>40</sup>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).

<sup>41</sup>*The strengthening of teaching of the most important topics of Folkskolan, that according to the experts are utterly necessarily, can likely 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)

<sup>42</sup>*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*

Due to the soft transition rules, the reform did not cause any major difficulties in the school districts. The implementation was also facilitated by the fact that the responsibility for funding of school buildings, teaching materials and teachers' salaries was the responsibility of the central government and not the school districts (Larsson, 2011). Moreover, since the main idea was to distribute the same courses given in six years over seven, there was no need to produce or distribute new teaching material. 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).

Child labor laws and compulsory schooling attendance laws have generally been coordinated in Sweden. According to the 1931 Labor Act, the minimum age for manufacturing and construction work was 14 years, whereas the limit for *light work* was 13 years.<sup>43</sup> A child was allowed to work from the beginning of the calendar year in which she would reach the age limit (Sjöberg, 2009). After the implementation of the seven year compulsory school reform, most pupils left school in the middle of the year they turned 14, whereas before they would leave the year they turned 13. Consequently the reform reduced the time a child could spend in *light work* by one year, whereas the corresponding reduction for *hard* (industrial) work was only 5-6 months.<sup>44</sup>

## E.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. Simultaneous with the extension of the number of compulsory years of schooling, 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 reform was once again that Sweden was lagging behind other Western European countries. Following this

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*undervisningsplan, tillsvidare icke vara erforderliga.* (p.130)

<sup>43</sup>Light work referred to work outside factories or construction sites. Notably child labor laws only applied to employed work and not to work at e.g. the family farm.

<sup>44</sup>The 1949 Labor Act increased the lower age limit by one additional year, in turn harmonizing the age when a majority of students finished schooling and started to work. Notably, the legal documents generally regulated full-time work, but not the part-time work of young people (Sjöberg, 2009).



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).

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).<sup>45</sup>

### E.3 Comparative International Contexts

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

The German educational system which implemented a compulsory school reform in the middle of the twentieth century that extending the basic track by one more year from 8 to 9 years of mandatory education constitutes a first reference. (Pischke and Von Wachter, 2008). The Swedish school system before 1950 shared large similarities with the German educational system.<sup>46</sup> The German school system was (and still is) highly selective with tracking students relatively early after 4 or 6 years into three types of secondary schools based on performance. The compulsory schooling extension in Sweden and Germany was also extremely similar in spirit.<sup>47</sup> Both extended the lowest track by one more year and both reforms were relatively *pure* extensions in the sense that they only affected the years of education and did not reshape the entire school system. Pischke and Von Wachter (2008) find robust results of at best very low returns to the compulsory schooling reform in Germany. They explain this by that the initial stock of skills was already comparably

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<sup>45</sup>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.

<sup>46</sup>We here refer to the West German school system. Interestingly it appears not to be a coincidence, that the decision in 1948 to replace the educational system in Sweden with a system similar to the one of East Germany – with a comprehensive element placing strong emphasis on e.g. democratic values and educating citizens – falls exactly into the years after the Second World War.

<sup>47</sup>Before a name change to the current *Hauptschule* the German equivalent was called *Volksschule* which is the literal translation of *Folkskola*.

high even in the lowest schooling track in Germany at the time of the reform implementation.

The pure extension effect is naturally connected to the absence of direct degree effects induced by the reforms in *Folkskola*. Related to this Grenet (2013) contrasts two compulsory schooling reforms in France (1967) and England and Wales (1972) and finds that only the latter increased wages due to the reform. These differences are assigned to a sharp drop in students leaving school without qualification in England and Wales. This rise in credentials appeared to not solely have a signalling effect on earnings, but rather increased the skill levels of school-leavers. For various institutional reasons the comparable French reform did not alter this margin.

## F Data Source

Figure 16. Exam catalogue from Archive

No i listan över barn i skolorna	Barnets namn och h	Termin	Uppförd	Vitsord														Tillkomna	Avgångna
				1	2	3	4	5	6	7	8	9	10	11	12	13	14		
14	Carlsson, Linn	h		A	A	B	B	B	B	-	B	B	B	-	Ba	-	Ba	B	-
29	Carlsson, Linn	v		A	A	B	B	B	B	-	B	B	B	-	Ba	-	Ba	Ba	-
17	Larsson, Lars Te	h		A	B	Ba	Ba	Ba	Ba	Ba	-	Ba	Ba	Ba	-	Ba	Ba	-	Ba
29	Larsson, Lars Te	v		A	B	Ba	Ba	Ba	Ba	Ba	-	Ba	Ba	Ba	-	Ba	Ba	-	Ba
18	Bergström, Bertha	h		A	B	Ba	Ba	Ba	Ba	Ba	-	Ba	Ba	Ba	-	Ba	Ba	-	Ba
29	Bergström, Bertha	v		A	B	Ba	Ba	Ba	Ba	Ba	-	Ba	Ba	Ba	-	Ba	Ba	-	Ba
52	Lundqvist, Ragn	h		A	B	Ba	Ba	Ba	Ba	Ba	-	Ba	Ba	Ba	-	B	-	-	B
29	Lundqvist, Ragn	v		A	B	Ba	Ba	Ba	Ba	Ba	-	Ba	Ba	Ba	-	B	-	-	Ba
34	Olsson, Karin Te	h		A	B	Ba	Ba	Ba	Ba	Ba	-	Ba	Ba	Ba	-	B	-	-	-
29	Olsson, Karin Te	v		A	B	Ba	Ba	Ba	Ba	Ba	-	Ba	Ba	Ba	-	B	-	-	-
	Jon Lar, Olga	h		A	B	Ba	Ba	Ba	Ba	Ba	-	Ba	Ba	Ba	-	B	B	-	Ba
	Jon Lar, Olga	v		A	B	Ba	Ba	Ba	Ba	Ba	-	Ba	Ba	Ba	-	B	Ba	-	Ba