UNIVERSITÄT DUISBURG-ESSEN

DISSERTATION

The Long-Term Effects of Education on Health and Labor Market Outcomes

Evidence from Historical School Reforms in Sweden and Germany

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"Man kan inte piska in något i barn, men man kan smeka fram mycket ur dem."
(There is very little you can beat into a child, but no limit to what you can hug out of it.)

Astrid Lindgren

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Chapter 1

Introduction

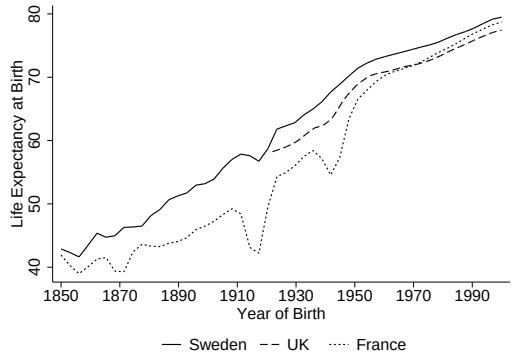
1.1 Motivation of the Thesis

Among the many remarkable developments that have deeply influenced societies during the 20th century, two of the most outstanding are the advances in human longevity and health, and the increases in educational attainment. Since 1900, life expectancy at birth in Europe and Northern America rose from 40 to more than 70 years in 2000 (Riley, 2005). This rise in longevity is by no means concentrated only to industrialized countries and similar developments are observable worldwide. Almost simultaneously most countries have experienced astounding growth in human capital. Over the course of the last century, industrialized countries went from generally low levels of schooling with large fractions of the population barely literate, to a situation where a majority of the population graduates from at least lower secondary school (Mincer, 1996; Goldin and Katz, 2009). Similar developments of increased levels of education are by now also observed in developing countries. Figures 1.1(a) and 1.1(b) exemplify these trends for the European countries Sweden, France and the UK. These figures suggest converging patterns in life expectancy with converging patterns in attendance to secondary education.

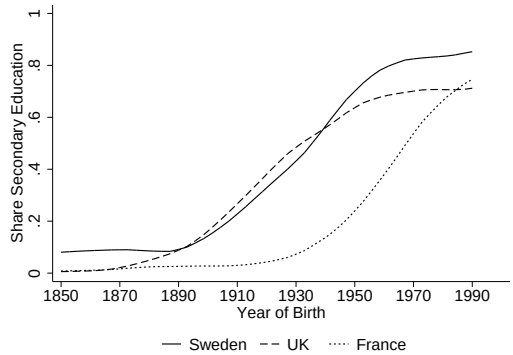
These general trends impose some immediate questions about how education and the demographic shifts that have been observed since the beginning of the 20th century are interrelated. What are the consequences for an ageing society? Is the observed substantial increase in longevity partially caused by the increases in human capital? Are increased levels of education also causing better health in the older population, i.e. implying a healthier ageing?

In general, the existence of an educational gradient in health is one of the most stable and well-documented stylized facts in the literature of health economics and public health. Numerous studies document this positive association between education and health. Better educated individuals tend to live longer. For the United States, education has become a stronger predictor for mortality than race or sex by today (Kitagawa and Hauser, 1973; Feldman et al., 1989; Meara, Richards and Cutler, 2008). A similar development is observable in Europe (see e.g. Huisman et al., 2005). Higher educated individuals are also healthier while alive (see e.g. Cutler and Lleras-Muney, 2006, 2012). This relationship can be documented for a wide range of health measures, including various morbidity measures and self-reported

health measures (Cutler and Lleras-Muney, 2012). With respect to health in the older population, some recent trends in dementia suggest that the risk of Alzheimer and other forms of dementia is decreasing over the last decade (Jones and Greene, 2016). The trend is paralleled by a continuously better educated older population. The evidence supports the idea that increased education not only increases survival to old age but also later life well-being and health (Larson, Yaffe and Langa, 2013).



(a)



(b)

FIGURE 1.1: (**a**) Life Expectancy; (**b**) Share with Secondary Education.

Source: Human Mortality Data Base, Lee and Lee (2016). Own calculations

If education indeed has such a large positive impact on longevity and health in general, policies supporting educational attainment should constitute an important tool to increase general welfare. This also applies to the older population. Education could have the potential to preserve late-life independence, which substantially increases individual welfare as well as those of potential carers. As promising as this may sound, for such a policy recommendation it is ultimately key to know whether education actually *causes* better health and to understand how education affects health.

There are several possible theoretical explanations why the observed positive association between education and health could indeed be causal. The

most prominent economic theoretical foundation of a causal effect of education on health is provided by Grossman (1972). The model views health as a form of human capital as well as a consumption good and that health is produced by a *health production function* (Cutler and Lleras-Muney, 2012). Individuals consume health in the sense that being healthy increases utility directly. Treating health additionally as a form of human capital, interrelates health closely with earnings and labor market outcomes. Healthier individuals are more productive, less ill and earn higher wages. Thus health affects individual utility directly and indirectly through income and occupation.

Education can affect health through multiple ways in this model. First, education can affect health through earnings, though the theoretical predictions from increased earnings are ambiguous. While e.g. increased earnings should increase the consumption of health via an income effect, increased wages as a result of higher productivity from better health also increase the opportunity costs of healthy behavior such as exercise instead of working. Education also potentially affects the production of health via more efficient use of available resources. A further important factor how education potentially affects health is through social networks (Cutler and Lleras-Muney, 2006). Participation in educational programmes likely influences the circle of individuals we interact with. This again could reshape our own behavior, also related to health.

All before mentioned theories suggest a causal relationship between education and health. Alternative explanations however exist, suggesting that the educational gradient in health and mortality might only reflect a spurious correlation. Other factors might be the driving force between both, increased levels of education and better health in the population. Such factors might be observable or unobservable. In this context of health and education, a frequently mentioned candidate for such an underlying factor are different time preferences (Farrell and Fuchs, 1982; Van Der Pol, 2011) between individuals. More patient individuals are more likely to invest in education and in healthy behavior. This view is closely related to Grossman's perspective on health as a form of human capital. Similarly, innate abilities, socio-demographic and environmental factors are potential explanatory variables underlying the association between health and education.

While several theories support the idea that a better education leads us to live longer and age healthier, overall there is no clear guidance from theory alone. It is ultimately an empirical question to find support for a causal relationship and test theoretical hypotheses on the causal link between health and education. Randomized controlled trials (RCTs) are often considered being a *gold standard* to uncover and test causal relationships. Whether that claim is justified or merely a rhetorical device to justify a certain type of analysis is far from undisputed (Cartwright, 2007; Deaton, 2010). More importantly, running randomized controlled trials to analyze the causal effect of education is for ethical as well as simply practical reasons rarely an option. Tossing a coin in order to decide who enters an educational programme is often difficult to justify. Furthermore, the few existing RCTs in the health education nexus such as the Perry Preschool Program and the Abecederian

(ABC) Program in the US are plagued by extremely low sample sizes.

Instead of relying on RCTs, most of the literature on the causal effect of education on health focusses on quasi-experimental evidence. Many of those studies evaluate policy changes leading to exogenous shifts in educational attainment. In stark contrast to the overwhelmingly strong association between education and health reported in observational studies, recent overview articles on the empirical evidence for a causal relationship between health and education based on quasi-experiments find it difficult to draw a definite conclusion (Grossman, 2015; Galama, Lleras-Muney and van Kippersluis, 2018). While some differences across studies may stem from methodological insufficiencies, a substantial part of the heterogeneity seems to be genuine and suggest that education affects health, but only under certain circumstances. Cross study comparisons e.g. suggest that longevity is positively affected by education if labor market outcomes such as earnings are increased. There is a tendency that the magnitude of mortality effects from quasi-experiments are larger for older cohorts (Gathmann, Jürges and Reinhold, 2015). In contrast, smoking, as one major health behavior leading to preventable diseases, is apparently mainly affected via changes in peers.

The already challenging analysis is additionally complicated by the necessity of a sufficient long follow up time to observe outcomes, when focussing on long-term outcomes such as longevity or general health in the older population. A quasi-experimental research design, generating exogenous variation in education to identify such long-term causal effects, has to rely on suitable historical events leading to substantial changes in educational attainment many decades ago. Furthermore, theoretical considerations suggest that a compelling analysis of later life health should include short, medium and long-term outcomes. Health behaviors and income across the life cycle are important to consider when analyzing health of the older population.

The purpose of this thesis is to attempt an encompassing life cycle perspective to draw conclusions on the causal effect of education on late-life and mortality. The overall aim of this thesis is to deliver new insights on the causal effects of education in the very long-run and to answer questions such as whether education increases longevity or decreases the risk of developing dementia. In order to disentangle causal effects of education from mere correlations, all studies in this thesis take a quasi-experimental approach in order to gain exogenous variation in education. All chapters rely on school policies affecting the primary and lower secondary educational levels.[1]

In order to allow an in-depth analysis of circumstances and a life-cycle perspective, the thesis is restricted to a relatively narrow institutional context. The thesis covers institutional changes in the Swedish and German school system for cohorts born between the 1920s to 1950s. Both countries shared remarkable similarities in their educational systems in the first half of the 20th century until Sweden reformed its school system in 1948. Both school systems tracked individuals based on scholastic achievement after a common primary school of 4 or 6 years into academic and vocationally oriented

[1]The whole thesis concentrates only on formal learning/education, i.e. institutionalized education in forms of school, university or vocational training (Eraut, 2000).

extended primary and lower secondary school forms. Only small fractions of the population attended higher secondary education at the time. Thus, the thesis mainly concentrates on the long-term impacts of education from institutional changes in an environment of relatively restricted and unequal access to education.

1.2 Contribution of the Thesis

All chapters add historical educational policies from Sweden and Germany to the existing empirical literature in order to identify the causal effects of education on long-term outcomes. Most of the school reforms covered in this thesis are yet fully unexplored in the empirical literature. All of them have in common that they affected individuals born in the first half of the 20th century such that their long-term socio-demographic outcomes are observable.

Chapters 2 and 4 exploit changes in the length of compulsory education in the Swedish primary school in the period 1919–1948 and evaluate their long-run effects. The two reforms included an extension of the annual term length as well as increases in the overall years of compulsory schooling. The policies were introduced independently across more than 2,500 school districts over a period of 12 years. These reforms deliver a vast number of small scale quasi-experiments across Sweden and the long follow-up time allows for an analysis of outcomes over the whole life-cycle.

In 1948 Sweden introduced a comprehensive school. In connection with this new school form Sweden extended the length of compulsory education, abolished academic tracking and essentially reshaped the whole primary and lower secondary school system. In an attempt to disentangle effects from changes in the length of education from other parts of the reform, we compare the comprehensive school to a yet fully unexplored parallel compulsory school reform in the old Swedish primary school system in chapter 3 and 5. This reform in the old primary school system lead to increases in the years of education but left the school system otherwise fully intact. IT was was enacted almost simultaneously with the comprehensive school reform and thus affected essentially the same cohorts. The direct comparison allows to identify channels how certain school policies differently affect individuals. The analysis is thus complementary to already existing research on the long-term impact of the Swedish comprehensive school reform on health and labor market outcomes.

Finally, chapter 6 analyses the long-term impact of the first school entrance regulation in the largest former German province Prussia from 1922 which previously has not been examined in the empirical literature. The long time horizon enables me to investigate late-life cognitive disease risks from consequences of an earlier school start. The study is also complementary in the sense that it allows contextual comparison with empirical evidence on effects of school starting age regulations on mid-life outcomes for more recent birth cohorts in Germany. This allows additional insights on when school entrance regulations have long-lasting consequences.

All studies in this thesis combine historical interventions with high quality administrative and survey data, in order to allow the investigation from changes in the school environment on health and longevity in the older population and further life-cycle outcomes. As mentioned earlier, it is pivotal to carefully assess the historical background and analyze potential mediating factors of the particular school policy in order to understand potential effects on long-term health outcomes. Thus, an important part of the thesis is the detailed documentation of the specific contextual aspects of the historical school interventions. Furthermore, mid-life outcomes such as earnings or occupation constitute potentially important mediating factors for the effects of education on health in the older population. To explain the mechanisms behind long-term effects or the absence of such, it is crucial to study effects of education at different stages in life. As the thesis introduces new quasi-experiments, a substantial part of the thesis is devoted to the evaluation of potential mediating factors, especially labor market outcomes.

Finally, the thesis also adds some smaller but nevertheless relevant insights for further research using Swedish administrative data. In the absence of an exact measure for the years of education, many studies rely on approximations based on the highest level of education. We suggest an alternative measure to this standard approach of approximating years of education in Swedish administrative data. This measure does not only improve accuracy compared to the standard procedure in terms of measurement properties, but also substantially alters the interpretation on a wide range of previous research analysing the effects of education on socio-economic outcomes in Sweden. Our results also likely apply to numerous studies outside Sweden. We also explore and quantify the consequences from changes in the birth place registration in Swedish administrative data. The place of birth from birth certificates is often used as a reasonable proxy for the place of residence during very early childhood. Such information is clearly of interest when evaluating the long-run consequences from the early childhood environment. We demonstrate empirically that due to specific registration procedures this is ill-guided for many individuals born before 1947 in Sweden and leads to sizable misclassification. We investigate alternative procedures and introduce new data sources to address this problem.

1.3 Summary of Chapters

Summary of Chapter 2

The second chapter investigates heterogeneity in the returns to education on earnings from different sources of instructional time changes in primary education. The literature on estimating returns to education has traditionally focussed on variation in years of education. However, instructional time in school can also be extended within a school year. We evaluate the long-term impact on earnings, pensions, and further labor market outcomes of two parallel educational reforms increasing instructional time in the Swedish primary school during the 1930s and 40s. The reforms extended the annual term

length from 34 to 39 weeks and increased the compulsory years of schooling from 6 to 7. The overall extension in instructional time from each reform is of comparable magnitude. We find striking differences in the effects of the two reforms: at 5%, the returns to the term length extension were at least half as high as OLS returns to education and benefited broad ranges of the population. The compulsory schooling extension had small (2%) albeit significant effects, which were possibly driven by an increase in post-compulsory schooling. Both reforms led to increased sorting into occupations with heavy reliance on basic skills.

Summary of Chapter 3

Following the changes within the existing school system in the 1930s and 1940s covered in the previous chapter, Sweden undertook a massive educational reform during the 1950s and 1960s which reshaped the whole school system. Sweden extended compulsory schooling from 7 to 9 years and simultaneously introduced a comprehensive school system, replacing the former selective two-tier system. Meghir and Palme (2005) evaluate the impact of this comprehensive school reform. They find small returns to earnings on average, but sizable increases in earnings for individuals with low-educated parents and substantially lower earnings for individuals with high-educated parents. The latter finding has been explained by a decrease in efficiency of the school system due to the abolishment of academic tracking.

Extending their analysis to the full population and using an improved education measure, we find substantially larger (up to 100%) increases in educational attainment than previously reported due to the reform. We can confirm small average effects on earnings but do not find any evidence of differential reform effects across socio-economic groups. In particular, we do not find any adverse effects for individuals from a high educational background, suggesting that effects of de-tracking were moderate at most. We corroborate this finding by evaluating a parallel school reform, which only increased compulsory education from 7 to 8 years without affecting the tracking system, leading to very similar results.

Summary of Chapter 4

Turning to long run health outcomes, we examine the effect of the compulsory schooling reform from 2 on mortality. Existing evidence on the education mortality relationship either uses information of fairly recent schooling reforms, implying that health outcomes are observed only over a limited time period or truncated and incomplete mortality data. We combine the historical educational reform with mortality data covering the full life cycle. Taking advantage of the variation in the timing of the implementation across school districts, by using county-level proportions of reformed districts, census data and administrative mortality data, we find that the extra compulsory school year reduced mortality. In fact, the mortality reduction is discernible already before the age of 30 and then grows in magnitude until the age of 55–60. The

evidence on occupational sorting given in chapter 2 is a potential explanation for the finding of reduced mortality.

Summary of Chapter 5

This chapter focusses on understanding the heterogeneity in findings for the causal relationship between education and health. Potential reasons for this lack of clarity include estimation applying different methods, analysis of different populations and school reforms that are different in design. Comparing the Swedish comprehensive school reform with a pure compulsory schooling reform we assess whether the type of school reform, the instrument and therefore subgroup identified and the modelling strategy impact the estimated health returns to education. To this end we apply both Regression Discontinuity and Difference-in-Differences to the reforms. The reforms are different in design but were implemented across overlapping cohorts born between 1938 and 1954 and follow them up until 2013. We find small and insignificant impacts on overall mortality and its common causes. The results are robust to the choice of the regression method, identification strategy and type of school reform. Extending the analysis to hospitalisations or self-reported health and health behaviours, we find no clear evidence of health improvements due to increased education. Based on the results we find no support for a positive causal effect of education on health. Our results in chapter 3 also indicate the absence of substantial labor earning effects for any subgroup for both school reforms. The absence of both, health and labor market effects is thus also in line with the recent literature stressing out the importance of labor market outcomes as a mediator for a causal impact of education on later-life health.

Summary of Chapter 6

This chapter focusses on the potential protective effects of education on cognitive disease risk in older people. Using the German 1970 Census and administrative health insurance data I evaluate the very long run-effects of a historical school starting age regulation in Germany on educational attainment and cognitive disease risk. From 1922 onwards, children in the northern parts of Germany born before the 1st of July entered school the year they turned six, Children born after the 1st of July entered school the year they turned seven, creating a discontinuity in school starting age. Starting school one year later lead to a significant increase in educational attainment and income. Likely explanations why effects persisted until adulthood are a highly selective school system, a strict enforcement of the regulation and the lack of proper school entrance examinations. Effects were potentially manifested or even exaggerated by exposure to World War II. However, I cannot detect effects on the risk of getting diagnosed with dementia for individuals at age 75+. Instead a later school starting age is associated with a significant reduction in overall mortality, potentially driven by the persistent effects of an

earlier school starting age on educational attainment and associated socio-demographics.

Chapter 2

The Long-Term Effects of Longer School Terms on Earnings: Evidence from Extended Compulsory Education in Sweden[1]

2.1 Introduction

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

Despite this ongoing discussion, the evidence on the effects of term length on later-life outcomes is extremely limited. Pischke (2007) analyzes the effects of short school years in Germany in 1966 and 1967. While the reduction in instructional time by 26 weeks over a time period of two years increased grade repetition and decreased higher secondary schooling attendance, the short years did not significantly impact the affected cohorts' income or employment. A recent study using survey data from Indonesia (Parinduri, 2014) comes to similar conclusions. Both these studies have in common that the considered extensions were not planned as such; the term length change was the accidental consequence of another policy change. Our paper aims to contribute to this literature by evaluating the long-run effects of a structural policy on term length extension affecting the entire population of Sweden. Our analysis is based on a clear quasi-experimental research design with a well-defined control group.[2]

[1] This Chapter was co-authored by Martin Karlsson, Therese Nilsson and Nina Schwarz.

[2] 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.

The limited number of studies on the long-term effects of term extensions is in sharp contrast to the large literature exploiting changes in compulsory schooling laws, regulating the minimum years of schooling that an individual has to acquire, to estimate the causal effect of human capital. Early studies evaluating this alternative policy measure, like Oreopoulos (2006) or Harmon and Walker (1995) following Angrist and Krueger (1991), generally reported large *causal* effects of schooling on earnings based on instrumental variable estimates.[3] However, more recent studies suggest smaller and often negligible impacts of additional schooling on economic outcomes or health.[4] In particular, recent studies on European countries suggest that changes in the number of compulsory years of schooling have rather small effects on pecuniary returns (Pischke and Von Wachter, 2008; Devereux and Hart, 2010; Chib and Jacobi, 2015). Also the large causal estimates for education and labor market returns in the US have recently been challenged as potentially spurious and possibly driven by differences in school quality between states (Stephens and Yang, 2014).

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

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

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

[5]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 (2016); Cortes, Goodman and Nomi (2015).

2017; Bellei, 2009; Fitzpatrick, Grissmer and Hastedt, 2011).[6] In fact, one potential explanation of why compulsory schooling extensions are ineffective in improving labor market outcomes is that basic skills may be fixed already when an extension is introduced.[7] A second important difference between term extensions and compulsory schooling extensions is that the former do not affect school leaving age. In contrast, compulsory schooling extensions often affect both the amount of schooling and potential labor market experience. Finally, term extensions and compulsory schooling extensions may have different effects on the opportunity costs of secondary education: in a system with tracking, extensions applying to the lowest track will have implications also for the decision of which track to take. For these reasons, it seems highly desirable to improve our knowledge about the long-term effects of term extensions and to understand how these effects compare with those induced by alternative policy instruments.

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

[6]Some recent studies suggest that the effects of an extension may depend on the school system and that instructional time is complementary to other inputs (Rivkin and Schiman, 2015; Cattaneo, Oggenfuss and Wolter, 2017)

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

a rare quasi-experiment for instructional time changes within a school year.

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

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

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

reforms in the sense that they only aimed at raising the *amount* of education, allowing a straightforward result interpretation.[8]

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

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

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

In conclusion, our study provides additional empirical support for an important tool in the toolbox of school reformers throughout the World. Just like the recent studies suggesting class size (Fredriksson, Öckert and Oosterbeek, 2012; Dustmann, Rajah and Soest, 2003; Chetty et al., 2011), school starting age (Fredriksson and Öckert, 2014; Black, Devereux and Salvanes, 2011), extra days of schooling instruction (Carlsson et al., 2015; Crawford,

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

Dearden and Greaves, 2014) or more generally additional school resources (Jackson, Johnson and Persico, 2016; Hyman, 2017) may be levers to consider to improve student achievement and adult outcomes, term extensions seem to represent quite an effective instrument to improve human capital. While the long-term effects of the compulsory schooling extension are disappointing, the term extension lead to persistent improvements in labor market outcomes and seems to have benefited the majority of children. By investing in children from the very first year in school the term extension clearly seems to have fulfilled the intended aims of the reform.

The next section gives the institutional background of the Swedish educational system and the reforms. Section 2.3 describes the data and sample selection, while Section 2.4 presents the empirical strategy. Section 2.5 shows the empirical results for labor market outcomes and analyzes potential mediators. Section 2.6 presents robustness checks and Section 2.7 concludes.

2.2 Background

2.2.1 The School System and the Reforms

Since 1897 children in Sweden start school in the year they turn seven. The school year begins in the fall and ends in the spring. In the 1930s and 1940s children entered a primary school called *Folkskola*.[9] Primary education was free of charge and attendance was compulsory until a student had completed the highest grade of *Folkskola* offered in the school district where he/she was registered as a resident.[10] The mandatory amount of schooling before the compulsory schooling reform in 1936 was 6 years, with some exceptions in the larger cities and in Scania, the southernmost county of the country.

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

[9]Private schools never played a substantial role in Sweden. A more detailed description of the primary school system is given in Appendix A.1 and in Fischer et al. (2016)

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

After *Realskola*, students could continue to higher secondary education (*Gymnasium*) and university.[11] Figure 2.1 gives a stylized presentation of the various school types following basic primary education.

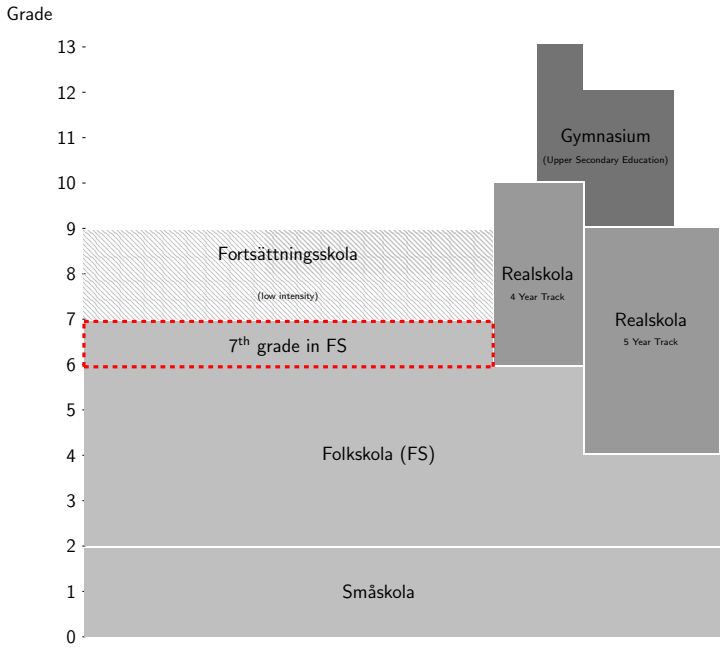
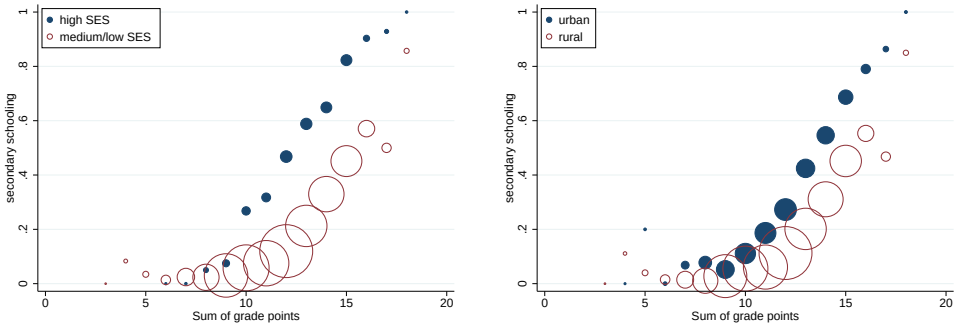


FIGURE 2.1: Swedish School System

Figure 2.2 illustrates the selective nature of admission to lower secondary school by plotting the proportion of students taking secondary schooling by primary school performance and background factors for a sample of individuals born 1930–34 (see Bhalotra et al., 2016, for details) for details. The gap in secondary schooling enrollment between children in families with high and medium/low SES and children residing in urban and rural areas, respectively, is to some extent attributable to high-SES and urban children performing better in primary school. Still there is a gap also after conditioning on primary school performance, particularly pronounced at 29 percentage points when stratifying by the father's SES (Figure 2.2(a)).

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



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

FIGURE 2.2: Proportion Admitted to Realskola by School Grades and Background

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

Compared with the US and other European countries, Sweden had a relatively low level of compulsory education in the early 1930s. In an attempt to catch up with international standards, the national Parliament decided in 1937 that the school year should be either 34.5, 36.5 or 39 weeks long. At the time the country had approximately 2,500 school districts and the reform triggered term length extensions in 600 districts already in the following two school years.[12] The 34.5 weeks option was to be discontinued by the school year 1941/42 and by 1952/53 39 weeks became the uniform standard.

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

Figure 2.3 shows the share of individuals with only primary education as highest school degree and those with having only 6 years of primary education between 1915 and 1945.[13] At the beginning around 80% of all students only attended primary school and just a small fraction of students completed secondary schooling and higher education. The share of students only attending primary school decreased to about 60% in the following decades. With the introduction of the 7 year reform, also the share of students attending only 6 years of primary school decreased substantially over time.

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

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

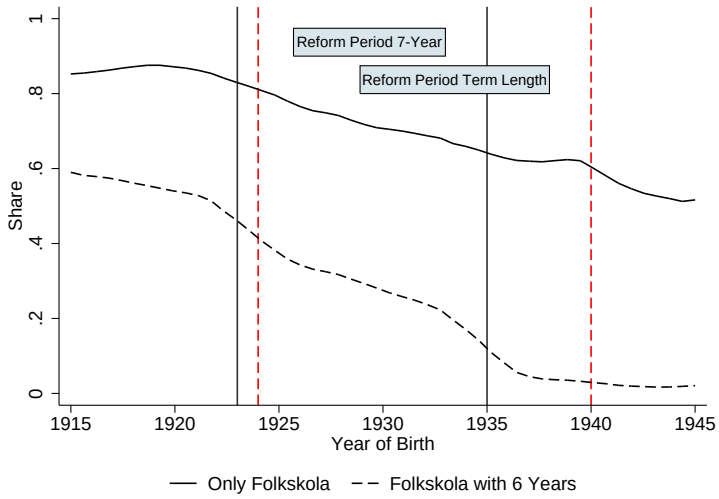


FIGURE 2.3: Share of Students with only Primary Education

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

The two extensions were independent and did not have to be implemented at the same time. Due to the soft transition rules, the extensions did not cause any major difficulties in the school districts. Their implementation was also facilitated by the fact that the funding of school buildings, teaching materials and teachers' salaries was the responsibility of the central government and not the school districts (Larsson, 2011*b*). In addition, the transition period coincided with an oversupply of teachers in the 1930's and the 1940's (Fredriksson, 1971).[14] Also no sources suggest that schools were closed or that schooling was disrupted in other ways due to the Second World war in the neutral country Sweden (see Bhalotra et al., 2016; Fredriksson, 1971, and appendix A.1).

The two education reforms increased the total instructional time by similar amounts, but there are three important differences between them. First, the term extension affected children at much younger ages and not only at the end of compulsory schooling. Second, the term extension had virtually perfect compliance,[15] while the compulsory schooling extension did not affect the minority of pupils who proceeded to lower secondary education. Third, the compulsory schooling extension changed the opportunity cost of secondary schooling: for someone considering enrollment in secondary

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

[15]All pupils were affected in school years 1–4, and the vast majority of pupils (those not proceeding to lower secondary schooling) were affected in years 5–6.

schooling, the alternative after the reform was to spend one additional year in the lower track. For the term extension, there were no such changes in opportunity costs. Related to this, a change in secondary schooling enrollment accompanying the reforms is likely driven mainly by self-selection in the case of the compulsory schooling extension – whereas such change could also relate to improved learning outcomes in primary school in the case of the term extension.

2.2.2 Labor Market Entry

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

Swedish child labor laws have generally been coordinated with the compulsory schooling attendance laws. In 1931 the minimum age for manufacturing and construction work was 14 years, whereas the limit for *light work* was 13 years.[16] The labor laws stated that children could not take on a full-time employment until he/she had fulfilled compulsory schooling. In addition to the labor protection legislation, the main decree of the Folkskola (*Folkskolestadgan*) included specific rules on how much students were allowed to work while taking on compulsory schooling (Sjöberg, 2009).

According to the child labor law, a child was allowed to start working in the year she would reach the above age limits (Sjöberg, 2009). The term length extensions in the first 6 years of primary school left labor market experience unaffected as children were below the age limit. The compulsory schooling extension however could have reduced labor market experience for students. After the implementation of the compulsory school extension, pupils left school in the middle of the year they turned 14, whereas before they would leave school the year they turned 13. Consequently, the reform reduced the time a child could spend in *light work* by one year, whereas the corresponding reduction for *hard* (industrial) work was 5-6 months.

Based on the LNU Survey (Swedish Level of Living Survey) we present descriptive evidence that supports our view that opportunities for adolescents between the ages 12 to 14 to enter the formal labor market were probably low in the 1930s and 1940s and labor market entry rules were enforced. Figure 2.4 shows that even before the reform only a negligible share of students entered regular employment before the age of 14. It is unlikely that the reform induced a substantial loss in labor market experience. The graph suggests that a very small fraction of students of 1-2% did in fact increase their

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

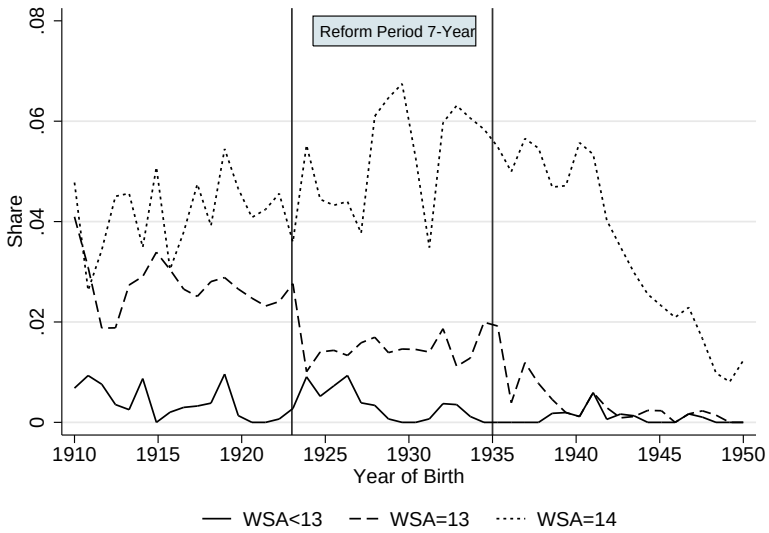


FIGURE 2.4: Working Start Age

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

working start age from 13 to 14 over the reform period. This fraction is tiny given a compliance rate of more than 60%.[17]

2.2.3 The Swedish Labor Market

There is general agreement that the Swedish labor market had the following main characteristics in 1970: a highly compressed wage structure, influential unions, high employment protection, and generous unemployment benefits (Adermon and Gustavsson, 2015). Some of the stylized facts associated with the Swedish labor market apply less to people born in the 1930s than to later cohorts; for example, family background appears to have mattered more to these cohorts, and their returns to schooling were slightly higher (Björklund, Jäntti and Lindquist, 2009). Table 2.1 provides some descriptives on how the individuals in our analysis sorted into classes of occupations. The classification is based on the 1970 census, and strongly relate to the ISCO 68 classification.

[17] Also see additional evidence in Appendix B.1.2

TABLE 2.1: Tasks for Occupational Groups

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

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

The top panel includes all individuals in our analysis sample and reports average earnings, educational attainment, task profiles (adapted from Autor, Levy and Murnane, 2003),[18] and primary school performance (normalized to have mean zero and standard deviation one, cf. Bhalotra et al., 2016). We see that individuals with secondary schooling are overrepresented in the classes "Managers & Professionals" and "Accounting, Administrative" and underrepresented in "Crafts", "Agriculture", "Transport/Communication" and "Service". The former group of classes tends to have above-average requirements in cognitive tasks, but also in routine manual tasks. The latter group, on the other hand, tends to have above-average requirements in non-routine manual tasks. These tasks profiles appear to be matched by skills, since the occupations with higher demands in cognitive and routine manual tasks also performed better in primary school on average.

Table 2.1 also shows that secondary schooling enrollment was not the only individual decision determining labor market performance. In the lower panel we report the corresponding statistics for individuals with only compulsory schooling. Also in this subsample, the first two groups are characterized by high demands in routine cognitive and manual tasks. The occupations in this group also belonged to the better-paid – but this characteristic is muddled by a compositional effect, since it was completely dominated by females.[19] Likewise, we find evidence of positive selection – based on primary school GPA – into the three highest groups, in particular "Accounting, Administrative". In Appendix B.1 we show that a similar skill profile of occupational groups is visible in adult literacy data.

2.3 Data and Sample Selection

2.3.1 Reform data

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

The primary data source for identifying the institutional features and changes of the primary school system comes from standardized exam catalogues that every school had to file. The exam catalogue is an annual documentation that provides individual information on each student, e.g. their

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

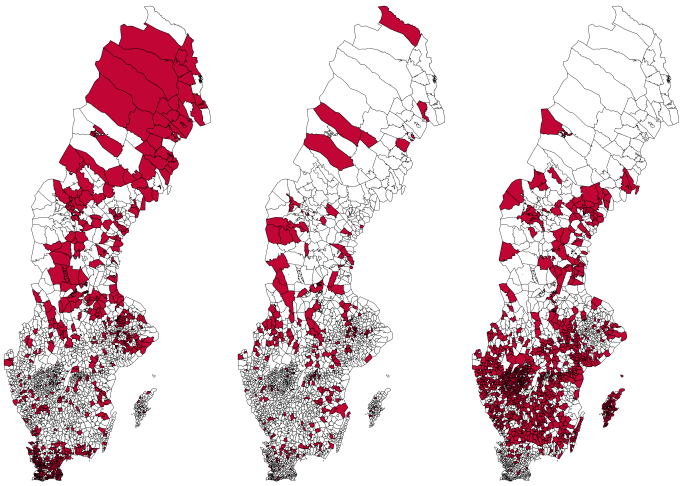
[19] Table B.2 in Appendix B.1 presents a breakdown by sex

attendance and their grades in various subjects, but also information on the name of the school, the school type, term length and information on the number of years of education provided by the school. The historical exam catalogues are publicly available in local archives across the country. By systematically reviewing the exam catalogues for a school district and each school year we exactly identify the timing of changes in term length and compulsory years of education. We thus obtained two independent reform years, for the term length and the compulsory schooling reform, within each school district. The systematic evaluation of exam catalogues also validate that changes were implemented simultaneously in all schools within the same school district. As the exam catalogues are student based it is also possible to directly infer that the compulsory schooling extension had *bite*, i.e. that students followed the newly implemented rules and did not defy. Appendix B.3 provides an example of an exam catalogue.

The reform data set contains information on the year of reform implementation for more than 98% of all existing school districts at the time of interest. Fischer et al. (2016) give detailed background on the reforms and their connection to later school reforms, and detailed information on sources and data collection procedures. Fischer et al. (2016) also provides information on various sensitivity tests and check-ups performed to assess the quality and validity of the reform data.[20]

Figures 2.5(a) – 2.5(c) and 2.6(a) – 2.6(c) graphically present the spatial and temporal variation of the term length extension and the compulsory schooling extension, respectively. While there is considerable spatial and temporal variation, the implementation was not random.

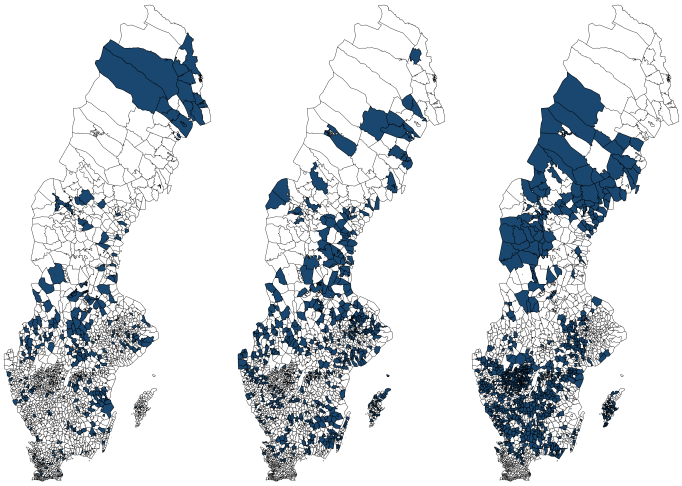
[20]Our way of assigning reform status to a district has been validated in various ways: e.g. by comparing our reform data with official statistics on the share of school districts that had implemented the seven year reform in each county of Sweden for the years 1938-1945 and by manual check-ups for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



(a) 1936–38 (b) 1939–42 (c) 1942–55

FIGURE 2.5: Timing of the Term Length Extension

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



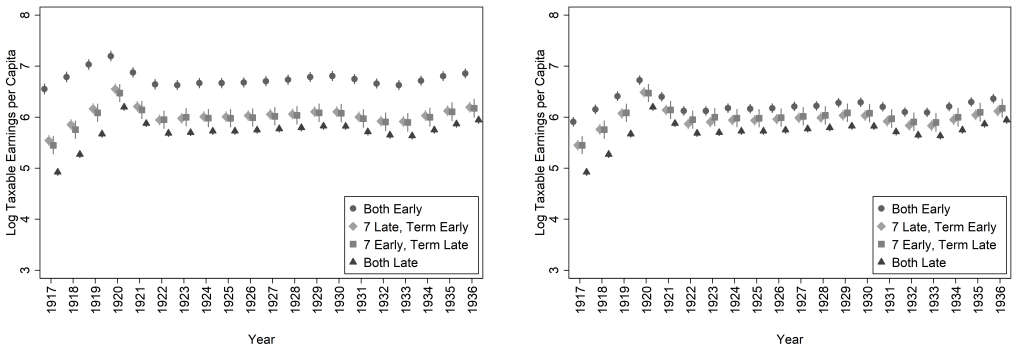
(a) 1936–41 (b) 1942–47 (c) 1948–53

FIGURE 2.6: Timing of the Introduction of the 7th Grade

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

Figure 2.7 plots average taxable earnings for four groups of school districts: districts that were early adopters of both reforms (with early adoption defined as having implemented the reforms before 1945), late adopters of both reforms, and two groups including districts that were early adopters of

one reform and not the other. The figures confirm that reform implementation is strongly related to local characteristics and there is a clear ranking between the four groups considering average taxable earnings. Early adopters are generally richer than the other groups – and this is to a great extent due to these areas being disproportionately urban. The two intermediate groups which implemented one of the reforms early and the other one late represent an intermediate case and are indistinguishable from each other. The districts that adopted both reforms late tend to be the poorest. In Appendix Figures A.1(a) and A.1(b) we also show that both reforms were implemented later in school districts with high shares of employment in agriculture. We conclude that even though all districts appear to follow a common time trend, it seems desirable to take systematic differences between urban and rural areas into account in the empirical analysis.



(a) All areas (b) Rural areas

FIGURE 2.7: Trends in taxable earnings.

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

2.3.2 Assigning Reform Status

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

An alternative way to assign reform status would be to use information on individuals' *place of birth* as a proxy for the place of residence during schooling age. Under normal circumstances, such assignment would allow estimating an "intent-to-treat" effect. In the current context, however, the place of birth variable contains severe measurement error which introduces bias of unknown sign and magnitude. The source of this measurement error is institutional deliveries. For cohorts born in a hospital before 1947 the

location of the hospital is recorded as the place of birth instead of the place of registration/residence of the parents.[21] With a growing number of institutional births, the place of birth becomes increasingly uninformative as a measure of place of residence at birth and consequently also as a proxy for place of residence during childhood. We evaluate the impact of the hospital birth coding in more detail in Appendix B.2. Given the above caveats our main analysis uses the place of residence during schooling age to assign reform status.

2.3.3 Individual-Level Data

The base study population for our main analysis is drawn from the Swedish Census in 1970 and consists of individuals born between 1930 and 1940. The data covers information on individuals' labor market status, occupation, income, education, marital status and place of residence in 1970. Information on living conditions and individual characteristics are based on self-enumeration and refer to the first week of October 1970 when the Census took place. With respect to labor force participation, persons are classified as economically active if they reported themselves as gainfully employed.[22] To explore labor market effects we create a dummy variable indicating whether an individual was gainfully employed in 1970.

Educational attainment is reported by the highest completed level of education in school and the highest post-schooling qualification (e.g. vocational training or university degree) attained. In case individuals were currently in training the last completed highest qualification is recorded. But given that all our individuals are older than 30 the clear majority had finished their education in 1970. As the 1970 Census does not contain direct information on the years of education we add a constructed measure for the years of education (YoE) based on the schooling and post-schooling qualifications from the Census. We assign the usual years of education associated primary, secondary and tertiary education[23] and take the sum as an approximation for the total years of education:

$$\begin{aligned}\textsc{Years of Education} &= \textsc{Years of Primary/Lower Secondary}\\ &\quad + \textsc{Years of Upper Secondary}\\ &\quad + \textsc{Years of Tertiary}.\end{aligned}$$

In appendix C.3.2 we give a detailed description on the construction of the years of education measure. The years of education measure allows us to

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

[22] 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.

[23] The years associated with a post-schooling qualification in the 1970 Census are directly drawn from the Educational Registers. We used the most frequent SUN2000 classification (Swedish adaptation of International Standard Classification of Education (ISCED 1997)) for each level of education in the Census and assigned the length from the Educational Registers to the 1970 Census level of education.

estimate returns to years of education. Unfortunately, the 1970 Census does not differentiate between the number of years spent in *Folkskola* if instruction time was less than 7 years; i.e. we can not distinguish between 6 or 7 years of primary education here.[24]

Income statistics stem from official tax returns and are considered as highly accurate.[25] 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.[26] Furthermore, we add data on pensions from the year when individuals turned 73.[27] Both measures are CPI adjusted to SEK in 2014. For the cohorts born 1930-1940 full pensions require thirty years of contributions and the level of the pension is based on the fifteen highest income years (Sundén, 2006). Pensions can be expected to be less sensitive to fluctuations in labor supply than earnings. This is a desirable feature, especially when analyzing women's returns to human capital as career interruptions due to e.g. childbearing affect the pensions to a smaller degree than annual earnings. Table 2.2 presents summary statistics for our main outcome variables, the schooling reforms and socio-demographics for males and females.

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

2.4 Empirical Strategy

Our empirical analysis exploits exogenous variation in the amount of instructional time in primary school generated by the two above-mentioned educational reforms. Depending on the compulsory years in *Folkskola* and

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

[25]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.

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

[27]73 is the first age we observe all relevant cohorts receiving a pension.

TABLE 2.2: Descriptive Statistics

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

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

pre-reform status, the term length extension led to a maximum cumulative increase in instructional time ranging between 15 and 31.5 weeks[28] over the time spent in primary school. The compulsory schooling reform added one complete year with 34.5, 36.5 or 39 weeks of additional instructional time. Figure 2.8 gives a stylized representation of the variation in instructional time generated by the two educational policies.

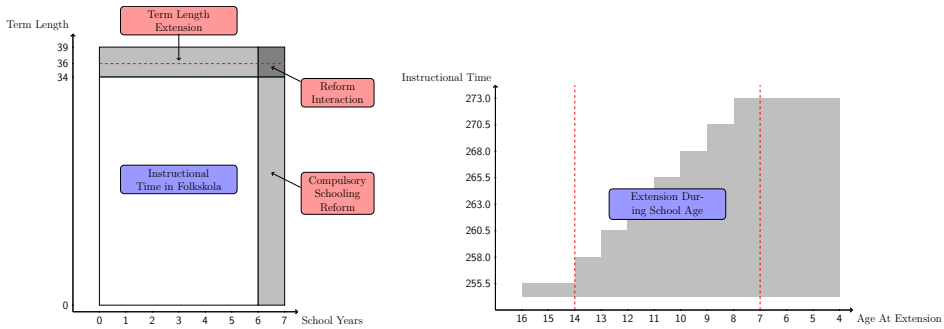
Since both reforms were decided and implemented at the local level they may be correlated. We thus use specifications including both reforms at the same time. Figure 2.8(a) however demonstrates that even strong correlation in the timing of the two independent reforms is unlikely to cause any problems even without such adjustment. The reason is a direct consequence of the step-wise increase of the extension of term length as shown in Figure 2.8(b). In contrast the compulsory schooling extension generates a sharp discontinuity between pre- and post-reform cohorts.

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

$$y_{ijk} = \beta_1 T_{jk} + \beta_2 Z_{jk} + \nu_k + \mu_{R_k j} + C_k \cdot j + \epsilon_{ijk} \tag{2.1}$$

where Z_{jk} is an indicator of whether an individual belonging to cohort j and residing in parish k has been exposed to the extension of compulsory schooling from 6 to 7 years. The continuous variable T_{jk} is the average term length measured in annual weeks of instruction in the school district for cohort j. To

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



(a) Total Instructional Time (b) Implementation of Term Length Extension (36.5 $\rightarrow$ 39)

FIGURE 2.8: Total Instructional Time by Reform Status.

Figure 2.8(a) compares the total instructional time in primary school. The instructional time varies in two dimensions – term length and length of Folkskola in years. Figure 2.8(b) shows the variation from term length depending on the age at which individuals were affected by the policy change. School districts changed the term length simultaneously for all grades. Individuals in lower grades at the time of extension received more education in total.

enhance a comparison to estimates for the returns to years of education we re-scale the average term length to a full year. By dividing the average term length by the factor 39, the interpretation of the coefficient is equivalent to an additional year of compulsory schooling instruction with 39 weeks. Both treatment variables vary on school district level. We include school district fixed effects (ν_k) and a non-parametric cohort trend $\mu_{R_k j}$ which we allow to differ between urban ($R_k = 0$) and rural ($R_k = 1$) districts. In addition, we allow for regional divergence by including the 24 county-specific time trends $C_k \cdot j$.

The identifying assumption is that in the absence of the intervention, earnings of individuals from treated and untreated districts would have followed a common cohort trend inside each cell defined by county and urban/rural status. Since section 2.3 showed that the reform implementation was related to several parish characteristics, it is important to carefully assess the plausibility of this assumption. Below we present balancing tests for household characteristics (Table 2.3) and for school district characteristics (Table 2.4). Table 2.3 shows that reform implementation correlates with several household characteristics: the affected children are more likely to have a high SES background and educated parents. But when we condition on the various fixed effects and trends specified in equation (2.1), the magnitudes for these characteristics are reduced a lot and all but one turn insignificant. Table 2.4 shows that the reform correlates with population size (proxied by the number of voters), political preferences and distance to the nearest secondary school. However, in our preferred specification, all of them are diminished and most lose statistical significance. Overall, out of 26 tests carried out in the balancing tables, only one turns out statistically significant at the 5 per cent level. We interpret this as evidence supporting our main specification.

TABLE 2.3: Balancing Test; Household Level

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

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

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

TABLE 2.4: Balancing Test; School District Level

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

2.5 Results

2.5.1 Compliance

The main dataset does not contain explicit information on compliance with the reforms, but we are able to estimate the "first stage" – i.e. how the time spent in school responds to the two treatments – using other data sources. Table 2.5 presents results on how the total days spent in school respond to the term length extension. Attendance rates were 0.94 on average, and according to our estimates, each additional day added to the school year translated into 0.86-0.99 additional days spent in school. Using log attendance as the main outcome variable, we also get elasticities very close to the observed share of the school year actually spent in school. The results are clearly indicative of

perfect compliance with the term extension reform.

TABLE 2.5: Attendance

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

The estimation sample consists of 16,000 individuals born between 1930–34. Source: Exam catalogs (cf. Appendix B.3) from 130 school districts (Bhalotra et al., 2016).

An empirical analysis of the compulsory schooling extension based solely on register data cannot identify educational effects of the reform as Swedish register data only states whether the highest schooling degree was primary.[29] To assess the impact of the compulsory schooling reform from 6 to 7 years, we use the Swedish *Survey on Living Conditions* (ULF)[30] which includes self-reported information on years of schooling. Table 2.6 presents regression results which suggest that the reform substantially increased educational attainment.

The ULF survey does not contain information on the place of residence during childhood which implies that we have to rely on the place of birth as an approximation when using this data.[31] Given the assignment by parish of

[29] 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.

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

[31] 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).

birth, we expect estimates for the first stage based on a misclassified instrument to be downward biased. For a first stage

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

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

$$\begin{aligned} \text{plim } \hat{\alpha}_1 &= \mathbb{E}[S_i|Z_i = 1] - \mathbb{E}[S_i|Z_i = 0] \\ &= \alpha_1 \underbrace{\{\mathbb{P}(Z_i^*|Z_i = 1) - \mathbb{P}(Z_i^*|Z_i = 0)\}}_{=\delta}. \end{aligned}$$

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

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

This is a split-sample IV with measurement error in the instrumental variable. We derive standard errors by applying the delta method.[32]

In the absence of substantial increases in educational attainment beyond primary school we would expected a first stage estimate for α_1 roughly equal to the share of individuals only visiting primary school. Our descriptives suggest that 60 – 70 % of the population only visited primary school. This corresponds well with our ILS estimates for α_1 in Table 2.6.

2.5.2 Earnings

1970 Earnings

Table 2.7 reports coefficients from regressions of log annual labor earnings in 1970 on the two policy instruments term extension and compulsory schooling extension. In order to compare the magnitude to general returns to education, we add conventional estimates for the years of education on earnings as a benchmark. The conventional results show substantial returns to years of education on earnings. For males and females combined, each year of education associates with an increase in earnings by 9.9 per cent; when we estimate the correlation separately by sex, the coefficient is almost twice as large for females as for males. The 14-per cent coefficient for females is unsurprising given that annual earnings capture effects from both labor supply

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

TABLE 2.6: First Stage: Effects on Schooling

	MALES AND FEMALES	MALES	FEMALES
OUTCOME: YEARS OF SCHOOLING (ILS)			
$\hat{\alpha}_1^{ILS}$	0.607	0.503	0.804
	(0.143)	(0.240)	(0.237)
OUTCOME: YEARS OF SCHOOLING			
$\hat{\alpha}_1$	0.440	0.365	0.582
	(0.103)	(0.174)	(0.171)
F-Statistic	18	4	12
Observations	6,292	3,108	3,184
Districts	1,333	912	951
Mean Outcome	7.799	7.881	7.719
OUTCOME: REFORM INDICATOR Z^*			
$\hat{\delta}$	0.725	0.726	0.724
	(0.014)	(0.015)	(0.014)
Observations	574,856	297,549	277,307
Districts	2,398	2,383	2,383

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

and wage increases. Only 66% of females were working in 1970. For males the labor market participation rate was close to 100% with most males working full-time, implying that the returns on annual earnings should be close to the returns on hourly wages. Labor market effects are explored further in Section 2.5.3. 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 2.7 for the term extension (which are scaled to correspond to an average school year) of 4.8 per cent are at around half the level as the estimated OLS returns to schooling. There are important differences by sex: a year of additional term length increases female earnings by 9 per cent when the school year is extended; male earnings increase by 2.5 per cent only.

In contrast, estimates for the effects from an additional compulsory year of schooling are small. For males and females combined, the estimate is 2.0 per cent, and this effect is also more pronounced for females. Even a simple rescaling of this estimate by the complier rate of 70% delivers much smaller estimates than the measured OLS returns to education. Our result is thus

TABLE 2.7: Main Results: 1970 Earnings

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

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

well in line with the recent economics literature suggesting that the returns to additional years of compulsory schooling are small (see e.g. Pischke and Von Wachter, 2008; Devereux and Hart, 2010; Chib and Jacobi, 2015).

Pensions at Age 73

Table 2.8 presents results using pensions at age 73 as an outcome. As pensions are calculated on the basis of the 15 years with the highest income and less prone to labor supply fluctuations than annual earnings, they offer an alternative measure for earnings. The OLS returns to education corroborate this point. At 7 %, the returns to years of education for females are basically identical to those of males and of similar magnitude as males' labor market returns to education with data on earnings. Interestingly, the term extension appears to have affected peak earnings less than early career earnings: the overall effect is reduced to 2.4 per cent, mainly driven by an effect on female earnings by 4.1 per cent. A comparison of results regarding 1970 earnings and pensions suggests that the term extension increased earnings throughout working life, but also led to a flattening of the age profile of earnings. For the compulsory schooling extension, we find small but significant effects of around 1% on pensions.

2.5.3 Mechanisms

We have seen that the term extension led to a substantial increase in earnings, whereas the compulsory schooling extension had only moderate effects. It seems likely that this difference relates to the age at which the additional instructional time is administered: pupils are affected by the term extension

TABLE 2.8: Main Results: Pensions at Age 73

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

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

from age 7 onward, while the compulsory schooling extension only becomes effective at age 13. the reforms may also have affected intermediate educational outcomes differently. Below we analyse various mediators in order to shed further light on this issue.

Educational Outcomes

As shown in section 2.5.1, both reforms had large immediate effects on time spent in school: the term extension applied to all children within a school district, and the compulsory schooling extension to seven years had around 70 per cent compliance rates. In order to understand the mechanisms responsible for the reported increases in earnings, it is important to examine whether the reforms had spillover effects affecting other educational choices. Table 2.9 provides a summary of results.

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

TABLE 2.9: Secondary Education

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

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

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

In summary, there seems to be spillover effects on further schooling for both reforms. For the term length extension, the spillover effects are so small that it is unlikely that they are responsible for the comparably large earnings effect. But for the compulsory schooling extension we cannot rule out that the increase in secondary schooling enrollment is responsible for a considerable share of the reform's moderate effects on earnings. As indicated in Table 2.9, mean earnings were around 60 per cent higher among secondary school graduates. If those who were induced to take secondary schooling by the introduction of the seventh year increased their earnings by the same amount,

the overall effect on mean earnings would be slightly below 1 per cent, which is around half of the estimated effect in Table 2.7.

Employment and Occupation

Despite substantial effects of the term extension on earnings, the effects we observe for other educational outcomes are very modest. This suggests that the labor market experience of the individuals affected by the term extension was different, even conditional on completed education. We now turn to an analysis of labor market outcomes for which we construct a number of dummy variables to capture labor market performance in 1970, when the individuals in our sample were between 30 and 40 years old. Table 2.10 presents results for employment and occupational group. The left part of the table shows results for the term extension, the right part for compulsory schooling extension.

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

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

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

effect on this category is similar in size to the previously reported effect on secondary schooling completion.

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

Migration

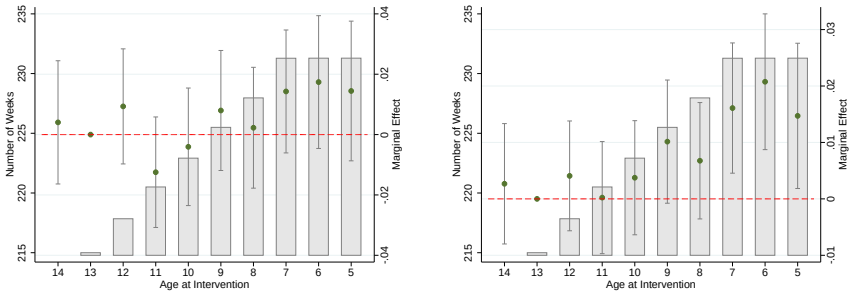
The outflow from agriculture and the corresponding inflow into more well-paid occupations for males is also mirrored in migration patterns. We construct indicators for across parish migration, across county migration and migration to an urban location by comparing the place of residence during schooling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

2.5.4 Event Study Analysis

Additionally to the main regression results, we show visually that the outcome variables follow the expected trajectory around the reform year. Figure 2.9 presents event study graphs for the term extension showing how our earnings measures evolve. The estimates suggest that earnings (represented by dots with confidence bands) increase along the increase in instructional time (represented by bars), until they level out at a higher level at the end of the reform window. The proportional increase is more pronounced for pensions where we see an increase of about 2 percentage points at the end of the window.

2.5.5 Heterogeneity

It has been argued that different compositions of compliers are a potential explanation of the low returns to education in Europe compared to the US



(a) Log 1970 Earnings (b) Log Pensions

FIGURE 2.9: Event Study Graphs: Term Extension

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

and Canada (Oreopoulos, 2006). Northern American studies have estimated returns to compulsory education for a small group of drop-outs that may be very prone to be positively affected by extensions of mandatory education. In the current setup, virtually all students were affected by the term extension and 70% of the population were affected by the compulsory schooling extension. Therefore the study design is much closer to compulsory schooling reforms that took place across Europe, such as in Germany (Pischke and Von Wachter, 2008) or the UK (Oreopoulos, 2006; Devereux and Hart, 2010) affecting larger shares of the population than reforms in the US and Canada.[33] Having such broad compliance facilitates the analysis of effect heterogeneity.

We explore heterogeneity with respect to family background. Tables 2.12 and 2.13 reports the effects of the term extension and the compulsory schooling extension on our main educational and labor market outcomes. The leftmost column shows our main estimate, while following columns report how the effect depends on the SES of the household head. For the term extension, there is no evidence supporting a SES gradient in the 1970 earnings effect. The SES gradient in the pension effect appears to be different for males and females, but there is not enough power to draw a definite conclusion. For secondary schooling dropouts, effects are concentrated in lower socioeconomic groups. Conversely, and as expected, the earnings gains from the compulsory schooling extension appear to be concentrated in lower socioeconomic groups. Still, there is no SES gradient in the response to this reform in terms of secondary schooling (even though the increase is much larger in *relative* terms for the low-SES group).

[33]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.

TABLE 2.10: Occupation 1970

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

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

TABLE 2.11: Migration

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

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

TABLE 2.12: Heterogeneity by SES Background (Income)

	Males and Females			Males			Females		
	All	*Low SES*	*High SES*	*All*	*Low SES*	*High SES*	*All*	*Low SES*	*High SES*
	(1)	(2)	(3)	(1)	(2)	(3)	(1)	(2)	(3)
DEPENDENT VARIABLE: LOG 1970 EARNINGS									
Term Length	0.048***	0.042**	0.055	0.025*	0.024	-0.002	0.090**	0.083*	0.156
	(0.019)	(0.020)	(0.049)	(0.014)	(0.015)	(0.037)	(0.043)	(0.048)	(0.102)
Compulsory 7-Year	0.020***	0.024***	-0.003	0.015***	0.015***	0.008	0.028**	0.038***	-0.009
	(0.006)	(0.006)	(0.014)	(0.004)	(0.004)	(0.011)	(0.013)	(0.014)	(0.029)
Observations	476,792	375,937	100,855	290,023	230,754	59,269	186,769	145,183	41,586
Districts	2,389	2,386	2,275	2,388	2,386	2,184	2,381	2,380	2,113
Mean Dep. Var.	9.877	9.832	10.046	10.309	10.261	10.493	9.207	9.148	9.409
DEPENDENT VARIABLE: LOG PENSIONS									
Term Length	0.024**	0.025**	0.005	0.010	0.025	-0.054	0.041***	0.029**	0.059*
	(0.010)	(0.011)	(0.029)	(0.016)	(0.017)	(0.050)	(0.013)	(0.014)	(0.035)
Compulsory 7-Year	0.011***	0.016***	-0.008	0.018***	0.018***	0.011	0.006*	0.013***	-0.028***
	(0.003)	(0.003)	(0.008)	(0.004)	(0.005)	(0.012)	(0.004)	(0.004)	(0.010)
Observations	440,291	347,655	92,636	214,378	169,707	44,671	225,913	177,948	47,965
Districts	2,390	2,387	2,250	2,388	2,385	2,112	2,384	2,383	2,123
Mean Dep. Var.	12.040	12.002	12.183	12.223	12.177	12.398	11.867	11.836	11.983

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

TABLE 2.13: Heterogeneity by SES Background (Eduation)

	Males and Females			Males			Females		
	All	*Low SES*	*High SES*	*All*	*Low SES*	*High SES*	*All*	*Low SES*	*High SES*
	(1)	(2)	(3)	(1)	(2)	(3)	(1)	(2)	(3)
DEPENDENT VARIABLE: SECONDARY SCHOOLING									
Term Length	0.008	0.009	-0.010	0.009	0.017	-0.018	0.008	0.001	0.002
	(0.009)	(0.009)	(0.027)	(0.012)	(0.011)	(0.037)	(0.014)	(0.014)	(0.041)
Compulsory 7-Year	0.008***	0.009***	0.005	0.011***	0.012***	0.009	0.005	0.006	-0.001
	(0.003)	(0.003)	(0.008)	(0.004)	(0.003)	(0.011)	(0.004)	(0.004)	(0.012)
Observations	583,716	462,790	120,926	302,045	240,396	61,649	281,671	222,394	59,277
Districts	2,390	2,387	2,298	2,388	2,386	2,195	2,386	2,385	2,194
Mean Dep. Var.	0.247	0.175	0.523	0.228	0.158	0.497	0.268	0.193	0.551
DEPENDENT VARIABLE: DROPOUTS									
Term Length	-0.033***	-0.038***	-0.006	-0.040***	-0.046***	-0.008	-0.025**	-0.031**	-0.001
	(0.012)	(0.012)	(0.018)	(0.014)	(0.014)	(0.022)	(0.013)	(0.013)	(0.024)
Compulsory 7-Year	-0.003	-0.005	0.005	-0.004	-0.005	0.001	-0.002	-0.005	0.008
	(0.004)	(0.004)	(0.005)	(0.004)	(0.004)	(0.007)	(0.004)	(0.005)	(0.007)
Observations	583,716	462,790	120,926	302,045	240,396	61,649	281,671	222,394	59,277
Districts	2,390	2,387	2,298	2,388	2,386	2,195	2,386	2,385	2,194
Mean Dep. Var.	0.115	0.112	0.125	0.111	0.106	0.130	0.119	0.118	0.120

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

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

2.6 Robustness checks

2.6.1 Marriage and Survival

Our main focus is labor market returns to schooling, but since marriage market effects and selective mortality may represent confounding factors Table 2.14 presents results for these outcomes. The term extension seems to associate with a slight increase in marriage rates, driven by females, but the effect is not significant at conventional levels. The compulsory schooling extension associates with an increase in male marriage rates and a reduction in female marriage rates.

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

TABLE 2.14: Further Outcomes

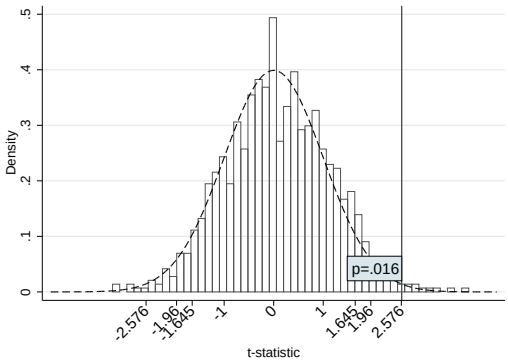
	(1)	(2)	(3)	(4)	
	Mean		**OLS YoE**	**Term Length**	**7-Year**
	Earnings	*Outcome*		**Extension**	**Extension**
A. MALES AND FEMALES					
Death Prior Census 1970	-	0.016	-	-0.000	-0.000
				(0.003)	(0.001)
Death prior Age 73	-	0.223	-	0.013	-0.001
				(0.009)	(0.003)
Married	209,150	0.861	0.004***	0.005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

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

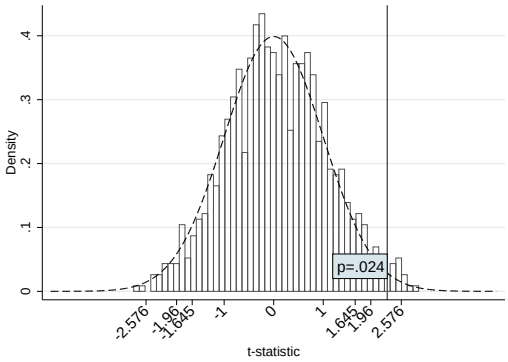
2.6.2 Randomization Inference

As an alternative basis for inference, we conduct a permutation test where, for each school district, the reform years of the two interventions were randomly drawn from the empirical distribution in 1,000 permutations. Figure 2.10 shows how our main estimated t statistic, represented by the red line, compares to the distribution of t values arising from the permutation test. For the sake of comparison, we also plot the t distribution which forms the basis of the statistical inference. Interestingly, the two distributions coincide almost perfectly – which implies that the statistical inference we conduct has correct size and power also according to this alternative design. Consequently we also get our main results confirmed: the large effects of the term extension on earnings and pensions are significant ($p < 0.025$) and the effects

of the compulsory schooling extension attain even greater statistical significance ($p < 0.001$) effects on earnings and pensions (Figure 2.11).



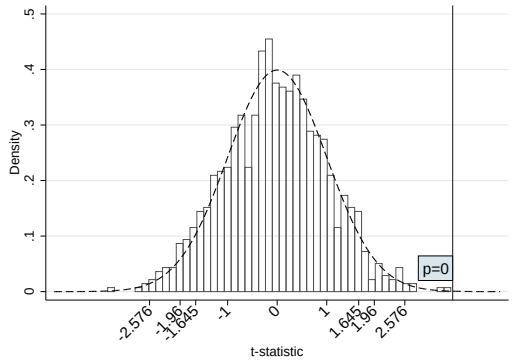
(a) Log Earnings



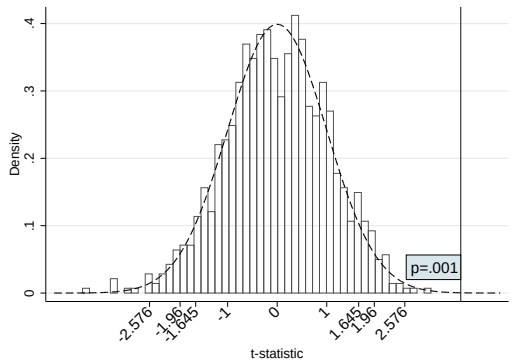
(b) Log Pensions

FIGURE 2.10: Randomization Inference, Term Length Extension.

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



(a) Log Earnings



(b) Log Pensions

FIGURE 2.11: Randomization Inference, Compulsory Schooling Extension

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

2.6.3 Alternative specifications

We also test for robustness of our results by controlling for different trends and socio-economic control variables in our baseline specification (which already includes rural/urban birth cohort fixed effects, linear county trends and school district fixed effects). Table 2.15 presents results for earnings 1970 and Table 2.16 presents results for pensions at age 73. Overall, our estimates are robust to additional controls and different trends. We also include sibling fixed effects which control for a range of observable and unobservable confounders related to family background. The effects are slightly larger than what we get with the main specification. Since sibling FE specifications may introduce biases via parental investment and birth spacing (which becomes collinear with our term extension variable), we prefer to interpret our main specification as measuring the causal effect of the reforms. Our sibling FE

estimates are also less precisely estimated, which is unsurprising given that we lose many observations when performing the sibling analysis.

TABLE 2.15: Main Results: Log 1970 Earnings

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

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

TABLE 2.16: Main Results: Log Pensions (Age 73)

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

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

2.7 Conclusion

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

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

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

While several studies already explore the health effects of compulsory schooling reforms (Clark and Royer, 2013; Gathmann, Jürges and Reinhold, 2015; Kemptner, Jürges and Reinhold, 2011; Albouy and Lequien, 2009) future research could focus on the health effects of term-extensions. Evidence on this topic is scarce; one exception being Parinduri (2014) who does not find any evidence of improved health. Since findings of compulsory schooling reforms are ambiguous, research on term-length extensions as another source of exogenous variation in education could shed further light on the health education nexus.

Chapter 3

Did Sweden's Comprehensive School Reform reduce Inequalities in Earnings?[1]

3.1 Introduction

Sweden undertook a massive educational reform extending compulsory schooling and simultaneously introducing a comprehensive school system, replacing a selective two-tier system, during the 1950s and 1960s. The seminal work by Meghir and Palme (2005) (MP hereafter) was the first exploiting this reform in a credible quasi-experimental design to study the causal effects of education on labor earnings. Their work has been been followed by numerous studies using the same reform and empirical strategy to examine the causal effects of education on various socio-economic outcomes, including health, crime and the intergenerational transmission of education.[2] Several of these studies use the reform mainly as an instrumental variable for years of education.

The results in MP are also a central contribution to the ongoing *efficiency vs. equity* debate regarding consequences of sorting students into academic and non-academic tracks (Betts et al., 2011; Holmlund, 2016).[3] The introduction of a comprehensive school system provides a setting to investigate

[1]This Chapter was co-authored by Gawain Heckley, Martin Karlsson and Therese Nilsson.

[2]The series of empirical work following MP in analyzing the Swedish comprehensive school reform includes work on Lager and Torssander (2012); Meghir, Palme and Simeonova (2017) and Palme and Simeonova (2015) on health, Hjalmarsson, Holmlund and Lindquist (2015); Meghir, Palme and Schnabel (2012) on crime, Lundborg et al. (2018) on financial literacy and Lundborg, Nilsson and Rooth (2014); Holmlund, Lindahl and Plug (2011); Lundborg and Majlesi (2018) on intergenerational effects.

[3]Many OECS countries *track* students into different types of school based on their scholastic achievement. The age at which students are tracked differs cross countries. Germany e.g. tracks students as early as age 10 while Scandinavian countries postpone tracking till age 16. Opponents of an early tracking argue that inequalities prior to school entry are manifested by an early selection. Socio-economic background is more important the earlier individuals are tracked. Proponents argue that earlier tracking helps to sort students into groups which better suit their specific needs and increases the overall efficiency of the school system. The discussion is widely referred as the *efficiency vs. equity* debate (Betts et al., 2011; Pekkarinen, 2014).

effects of postponed tracking on later-life outcomes.[4] The reform shifted differentiation into academic and vocational tracks from age 11 or 13 to age 16. The findings in MP suggest an increase in equality of opportunity which is difficult to explain only by the compulsory schooling part of the reform. While the average returns to the reform were modest, the introduction of the comprehensive school allegedly had a large positive effect on earnings and educational attainment for students with low educated fathers, while students with highly educated fathers experienced a substantial (6%) reduction in earnings and females additionally decreased their educational attainment by almost 0.5 years. The negative effects for students from high socio-economic background have been seen as the result of a decrease in the quality of education for this group due to the abolishment of the academic secondary school track. This finding sits in contrast to the most recent empirical literature on tracking, which does not find large adverse long term effects from postponed tracking on any subgroup (Betts et al., 2011; Pekkarinen, 2014; Dustmann, Puhani and Schönberg, 2017).

We contribute to the literature by extending the analysis in MP to the full population. While the original study uses a random 10 percent sample of the cohorts born 1948 and 1953, we examine the reform using administrative data for the total population. We also contribute to the literature by introducing a new education measure, taking variation in years of education below the highest level of education into account. We show that this improved measure gives a very different picture of the impact of the reform.

Our results support the finding in MP that the reform increased educational attainment and that it had small effects on labor earnings on average. However, we cannot confirm the substantive conclusions in MP regarding increasing equality of opportunity and earnings. Our reduced form estimates based on the full population indicate that the effects of the reform on labor earnings were uniformly small and positive: there is no evidence of differential effects across socio-economic groups. Especially, we can rule out negative effects on individuals from a high socio-economic background. If anything, we find evidence of an *increase* in earnings driven by an increase in secondary education for this advantaged group. Overall, our analysis does not support the previous findings of reduced inequalities in labour market outcomes by family background.

The moderate reform effects on labor earnings for individuals from a low socio-economic background are especially surprising as the new education measure that we introduce shows that education effects were substantially

[4]The comprehensive school reform was a policy experiment taking place in Sweden 1949 to 1969. The implementation of the reform came at different points in time across municipalities, generating random variation in exposure to school types. The reform increased compulsory education from 7 or 8 to 9 years and abolished a selective tracking system which streamed students into a vocational oriented or a more academically oriented track. As described in Marklund (1989) the reform also implied a unification of the national curriculum and various pedagogical changes. While this makes it difficult to exactly pin down effects of de-tracking, the reform nevertheless can give important insights how the change to comprehensive school system affects students when subgroup analysis or cross-country comparisons are included in the analysis.

larger than previously reported. Taking all compliers of the compulsory schooling extension into account, education estimates double. This has consequences for any research applying the reform as an instrumental variable for years of education. The correction implies a considerably stronger instrument but at the same time local average treatment effects decrease by approximately factor 2. The implications of the upward bias in the instrumental variable estimates reach well beyond the setting considered here. Many other empirical studies based on administrative and survey data construct education measures based on the highest level of education and do not take variation in years of education below the highest level of education into account.[5] These studies are likely also prone to overstate effects based instrumental variable estimates using compulsory schooling extensions as instruments.

In line with the recent literature on compulsory schooling extensions, our empirical evidence suggest that labor market returns to compulsory education have been around 3% in Sweden. Our results also suggest that the comprehensive education reform had no negative effects on economic outcomes later in life. It is unlikely that the introduction of the comprehensive school system substantially affected later earnings through changes in school quality or efficiency losses due to the lack of academic sorting. To further corroborate these results, we examined a parallel school reform which increased compulsory education by one year for the same cohorts, without affecting academic tracking.[6] The estimates for the comprehensive and the parallel school reform are strikingly similar, which suggests de-tracking was not an important determining factor of the comprehensive school reform.

[5]Scandinavian administrative data is likely prone to the problem in general. The Danish Education Register also captures the overall highest level of education. Years of education are approximated *by minimum duration of study required for obtaining the highest qualification by the fastest route* (Bingley and Martinello, 2017). Consequently, changes below the highest level of education are not captured by this measure. Norwegian studies on the introduction of a comprehensive school in the 1960s report extremely low rates of compliance in the 1960s (10%) suggesting similar problems as we report for the Swedish reform (see e.g. Black, Devereux and Salvanes, 2005). With respect to survey data, many empirical studies use SHARE data to evaluate compulsory schooling reforms in Europe. The first wave does not contain self-reported years of education. Researchers usually approximate years of education based on the highest level of education (cf. Brunello, Fort and Weber, 2009; Brunello, Fabbri and Fort, 2013).

[6]The extension was implemented independently across municipalities 1941 – 1962 and increased compulsory schooling from 7 to 8 years. In contrast to the comprehensive school reform it did not imply any change in curricula nor any pedagogical changes. The main motivation for the reform was that skills were low and that Sweden was lagging behind in terms of compulsory education compared to other European countries. Although implemented in parallel with the comprehensive school reform and affecting the same cohorts the reform is rarely discussed in the literature, but in-fact a majority of municipalities had rolled out a compulsory schooling length of eight years before the comprehensive school was introduced. See Heckley et al. (2018) on the relationship between education and health for the first and so far only evaluation of the reform.

3.2 Data

3.2.1 Data Sources and Sample Selection

The first data source we use is the original sample used by MP.[7] The data contains individuals born on the 5th,15th and 25th of each month (i.e. roughly 10% of the population) of the cohorts born 1948 and 1953 linked to administrative data, including their labor earnings reported in 1985–1996 from tax registers and their highest level of education. It further contains information on individual reform status, their municipality of schooling and their parental background.

The second and main data source we use is full population administrative data drawn from the *Swedish Interdisciplinary Panel* (SIP).[8] The SIP provides information on various socio-economic outcomes from a number of Swedish administrative databases, such as earnings from tax reports, educational attainment from educational registers and various censuses. The panel covers the entire Swedish population born between 1930–1980 and includes information on each individual's parents connected via the Swedish Multigenerational Register.

To measure the introduction of the new comprehensive school system we rely on a dataset as used in Hjalmarsson, Holmlund and Lindquist (2015), described in detail in Holmlund (2008).[9] The dataset contains information on the year a specific school district introduced the new comprehensive school. We also gathered information on the extension of the old primary school from 7 to 8 years from the Swedish National Archives. For each year we have digitized information on whether a school district had seven or eight years of mandatory primary school.

To identify individuals as exposed or unexposed to the reforms, we proxy the place during schooling age by the municipality of residence in the 1960 and 1965 Census.[10] As the place of residence is measured one year before children enter lower secondary school for the cohorts born 1948 and 1953, the place of residence should capture reform exposure very accurately. This is a small deviation from the original study as MP have access to the actual municipality of school attendance. In section 3.5 we briefly discuss reasons as to why municipality of school attendance is not necessarily an optimal reform assignment.

To examine effects on earnings we use labor earnings from official tax registers 1968–2012 which allow us to estimate effects over the whole life

[7] Data and description is available at American Economic Review: https://www.aeaweb.org/articles?id=10.1257/0002828053828671.

[8] Administered at the Centre for Economic Demography, Lund University, Sweden. The present study was approved by the *Lund University Regional Ethics Committee, DNR 2013/288*.

[9] We thank Helena Holmlund for generously sharing her education reform dataset and code.

[10] For cohorts older than 16 at the time of the census we follow the suggestion of Holmlund (2008) and use the place of residence of the parents in 1960.

cycle.[11] In order to investigate equality of opportunity effects we follow MP and stratify the sample by paternal education. The sample is split into two groups: individuals with a father having only compulsory education ($g = 1$, *low education*) and more than compulsory education ($g > 1$, *high education*). Information on father's education is taken from the Census 1970.

We extend the 10% sample used in MP in two steps. First we restrict the analysis to the very same cohorts born in 1948 and 1953 and earnings reported in 1985 – 1996 but, rather than use the 10% sample we use the full population born in 1948 and 1953.[12] In a robustness check we then extend the analysis to all cohorts born between 1938 and 1954, covering the whole comprehensive school reform period.

3.2.2 Measuring Years of Education

Swedish administrative data do not contain direct information on years of education. Previous studies have therefore approximated years of education based on the highest level of educational attained according to the Swedish adaptation of International Standard Classification of Education (ISCED 1997), the SUN2000 classification. As we will see, this traditional approach of proxying years of education based on the highest level of education can severely underestimate effects of compulsory schooling extensions on years of education. Below we provide a summary of the main insights, while Appendix C.3 gives a detailed derivation of the results.

The Traditional Approach

According to the SUN2000 classification all available education programmes and related qualifications are aggregated into seven broader levels of education, sorted from the lowest level of education (1, compulsory primary education) to the highest (7, post-graduate doctoral degree). Let D_i denote the highest level of education achieved by individual i. The traditional years of education measure $\hat{S}^{TRAD}$ is simply a function of D_i, assigning the *typical length* of education associated with the highest completed level of education D_i. An obvious imputation choice is to use the average years of education given each highest completed level of education:

$$\hat{S}_i^{TRAD} = E(S|D_i). \tag{3.1}$$

[11] For the years 1968–1977 no direct measure for labor earnings (*arbetsinkomst*) is available. We construct a measure for labor earnings according to the suggestions in Edin and Fredriksson (2000): For the years 1968–1973 we use total income from employment and self-employment (*sammanräknad förvärvsinkomst*). From 1974 onwards we substract unemployment benefits (*dagpenning vid arbetslöshet and KAS*, become taxable and part of income from employment in 1974) and pensions (only separately available from 1974 onwards) from income from employment and self-employment.

[12] Appendix C.1.1 provides descriptive statistics.

As the actual years of education S are not included in the administrative data (the reason for the imputation), the conditional expectation has to be evaluated based on external data sources, e.g. survey data including both years of education and highest level of education. The average years are imputed for each possible level of educational attainment. Alternatively, the imputation can be based e.g. on the total prescribed period of study.[13] Independently of the imputation method, it is important to notice that *by construction* any education below the highest level is irrelevant for the imputed length of education according to equation (3.1). Denote with $\mathfrak{J}_i$ the set of all educational programmes finished by individual i, then $\hat{S}_i^{TRAD} \perp \mathfrak{J}_i | D_i$.

This has consequences for the evaluation of the Swedish comprehensive school reform. It made lower secondary education mandatory ($D_i \geq 2 \ \forall \ i$). Adopting a potential outcome notation, we can see directly from equation 3.1 that $\hat{S}_i^{TRAD}$ only changes due to an educational reform if the highest educational level also changes, i.e. $D_i^1 > D_i^0$. The traditional measure implicitly assumes that the reform only affected students who otherwise would quit education completely after finishing compulsory primary education ($D_i^0 = 1$). This was typically not the case: the reform also affected many students taking vocationally oriented secondary education after finishing compulsory primary education. Their compliance with the comprehensive school reform is not identifiable by the traditional measure.[14]

An Alternative Approach

We suggest a simple alternative measure which is able to capture the full effects of the reform on years of education. We therefore first split all available education programmes and related qualifications into three broader categories: primary/lower secondary level of education (Level 1–2), upper secondary level (Level 3) and post-secondary/tertiary (Level 4–7). For individual i, let G_i denote the highest primary and lower secondary level of education, U_i the highest upper secondary programme and C_i the highest post-secondary and tertiary programme. We then assign the length of study L needed for completion of the highest programme to each of the three categories separately. Our alternative measure for years of education $\hat{S}_i^{NEW}$ is

[13]The imputation in Meghir and Palme (2005) is based on average years for each highest level of education. For other approximations see e.g. Hjalmarsson, Holmlund and Lindquist (2015); Lager and Torssander (2012); Lundborg, Nilsson and Rooth (2014). Appendix includes an overview of the most frequently applied imputations.

[14]We define *compliers* as individuals who increase their years of education when exposed to the reform (i.e. $S^1 > S^0$). Individuals who proceed to vocational training could alternatively be viewed as *always-takers*. However, we find empirical evidence that the choice and length of vocational training are unaffected by the length of primary or lower secondary school, so that these individuals contribute to estimates of reform effects to the same extent as compliers in a more narrow sense (i.e. those that only take compulsory schooling).

defined as

$$\begin{aligned}\hat{S}_i^{NEW} &= \underbrace{E(L|G_i)}_{\text{a) Primary/Lower Sec.}} + \underbrace{E(L|U_i)}_{\text{b) Upper Secondary}} + \underbrace{E(L|C_i)}_{\text{b) Tertiary}} \\ &= l(G_i) + l(U_i) + l(C_i) \qquad (3.2)\end{aligned}$$

Imputation of the length of education can be based again either on conditional expectations or prescribed period of study to complete a certain educational programme. Importantly, changes in the length of compulsory schooling are now captured by changes in $l(G_i)$ in equation 3.2 for *all* individuals. The changes are identifiable independently of the overall highest level of education D_i.

We implement our measure by combining information for primary and lower secondary educational programmes from the Swedish 1970 Census and for upper secondary and tertiary education from the highest achieved level of education recorded in the Educational Registers. Appendix C.3 provides a detailed description of the construction of the alternative measure for years of education. We also analytically derive the bias in estimated effects of a compulsory schooling extension based on the traditional measure.[15]

In order to demonstrate the relevance, we compare compliance rates for the traditional and the new education measures in figure 3.1. The solid line represents the share of individuals visiting the old primary school system and not attending the academic lower secondary track. Those represent the compliers to the compulsory schooling extension. The dashed curve represents individuals with the old primary school system as their overall highest level of education. The gap between the two curves gives the share of missed compliers. Most of them are missed because they attended vocational secondary education after graduating from school.[16] Based on the traditional years of education measure, 10-15% of the population are not identified as compliers, leading to severe downward bias when evaluating the reform effects on educational attainment.[17]

[15]Our implementation requires information on education from the Swedish 1970 Census, restricting our implementation for cohorts born 1911-1954 for which educational attainment was reported in the Census. The annual records of the Swedish Educational Registers starting in 1990 can be used to construct a similar measure for more recent cohorts. They allow to follow educational transitions for each individual from basic compulsory education to the highest completed level of education. The records on educational attainment are updated each year. A years of education measure can be based on exact individual trajectories and taking into account within variation for the highest completed level of education.

[16]As discussed by Håkansson and Nilsson (2013) vocational education expands rapidly in Sweden from the late 1950's until 1970. In 1955 the government decides to increase state grants for this kind of education (that normally implied 1-2 year of training, on a full- or part-time basis). The rapid increase in the number of women and men in vocational education should also be seen in light of increased mechanization in the industry sector which increased demand for workers with specialized skills that could be provided by this kind of relatively short training, but also in light of an expanding public sector.

[17]Note that the systematic exclusion of compliers due to coding is independent of further concerns related to measurement error in the instrument of reform exposure. Holmlund (2008) bounds the first stage on years of education due to measurement error in reform

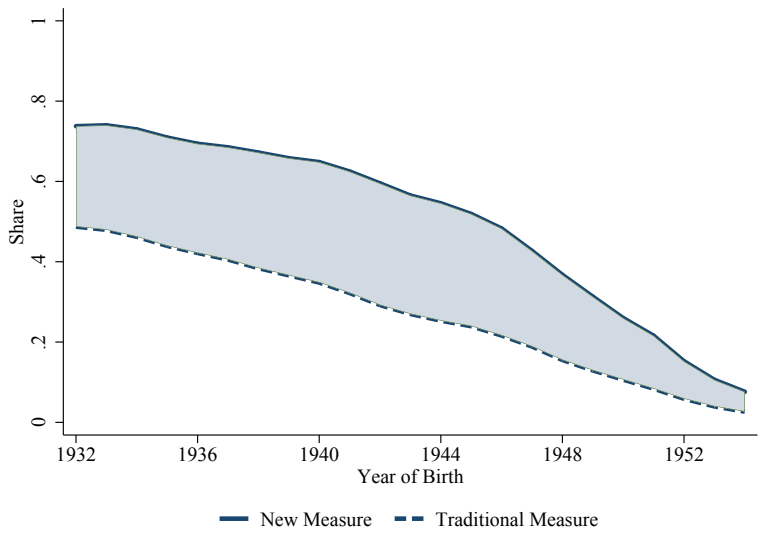


FIGURE 3.1: Reform compliers according to different measures

Notes: The figure plots the share of individuals having only primary education based on the 1970 Census and the educational registers. The solid line shows the share based on the 1970 Census. The dashed line is based on the highest level of education from the Educational Registers. The shaded gap between the two lines represents individuals with primary education in the old system but attending education beyond primary school. Those are compliers to the reform but not identified by information from the Educational Registers. *Source:* SIP. Own calculations.

3.3 Empirical Strategy

We rely on standard reduced form estimates from a regression of the reform indicators Z_{mc} on years of education S_{imc} and the log-labor-earnings $\ln Y_{imct}$:

$$S_{imc} = \alpha_0 + \alpha_1 Z_{mc} + \mu_c + \nu_m + \varepsilon_{imc} \tag{3.3}$$

$$\ln Y_{imct} = \gamma_0 + \gamma_1 Z_{mc} + \mu_c + \nu_m + \delta_t + \epsilon_{imct} \tag{3.4}$$

where c indicates the cohort, m the municipality of residence, t the year earnings are measured, μ_c denotes cohort fixed effects, ν_m municipality fixed effects and δ_t a period fixed effect.[18] We assume conditional mean independence of Z_{mc} after controlling for municipality, cohort and year fixed effects.

MP add further control variables to their specification. With the main goal to replicate their findings using the entire population, we have to exclude certain control variables which are only available in the MP sample – most importantly test scores at age 13. To compare our estimates based on the total population with MP, we re-estimate the main regressions based on

exposure. Potential sources are migration or unclear reform status of certain municipalities in the source material. Such attenuation bias from measurement error in the instrument would come *on top* to the underestimation from the systematic underreporting of compliers. The new coding would lead to an upward scaling of both upper and lower bound.

[18]In regressions including all cohorts affected by the comprehensive school reform, we follow Hjalmarsson, Holmlund and Lindquist (2015) and restrict observations to a 5-year window around the pivotal cohort for each municipality.

their original 10% sample without additional covariates. We can show that the exclusion of further regressors does not substantially affect the results and conclusions in MP. In Appendix C.1.2 we also provide empirical evidence[19] for the full population, which strongly supports that the assumption of conditional mean independence holds without conditioning on additional covariates.

The comprehensive school reform is commonly used as an instrumental variable for years of education.[20] The reform likely satisfies the criteria of *relevance* and *conditional independence*, but there exists no consensus in the literature whether the *exclusion restriction* holds. The abolishment of tracking and other parts of the reform that also came with the compulsory schooling extension represent threats to this assumption (Hjalmarsson, Holmlund and Lindquist, 2015; Meghir, Palme and Simeonova, 2017).

Our new education measure points to an additional problem with using the comprehensive school reform for instrumental variable estimation based on the traditional measure $\hat{S}^{TRAD}$ for years of education. The reduced form estimate for γ_1 in equation 3.3 is unaffected by different approaches to measuring education. The first stage α_1 based on the traditional measure $\hat{S}^{TRAD}$ is reduced by a factor proportional to the missed compliers. For an IV estimator $\hat{\beta}_1^{IV,\hat{S}^{TRAD}}$ for the returns to education, the systematic underreporting of compliers leads to an upward bias in IV estimates:

$$\begin{aligned}
\text{plim}\ \hat{\beta}_1^{IV,\hat{S}^{TRAD}} &= \frac{\mathbb{E}[Y_i|Z_i=1]-\mathbb{E}[Y_i|Z_i=0]}{\mathbb{E}[\hat{S}_i^{TRAD}|Z_i=1]-\mathbb{E}[\hat{S}_i^{TRAD}|Z_i=0]} \\
&= \underbrace{\frac{\mathbb{E}[Y_i|Z_i=1]-\mathbb{E}[Y_i|Z_i=0]}{\mathbb{E}[S_i|Z_i=1]-\mathbb{E}[S_i|Z_i=0]}}_{\text{plim}\ \hat{\beta}_1^{IV}} \cdot \underbrace{\frac{\mathbb{E}[S_i|Z_i=1]-\mathbb{E}[S_i|Z_i=0]}{\mathbb{E}[\hat{S}_i^{TRAD}|Z_i=1]-\mathbb{E}[\hat{S}_i^{TRAD}|Z_i=0]}}_{=\delta>1} \\
&= \frac{\beta_1}{\gamma_1}\cdot\delta.
\end{aligned}$$

Our alternative measure for years of education $\hat{S}^{NEW}$ takes the entire impact of the reform into account. We can estimate the bias factor as the ratio of the first stages $\hat{\delta} = \hat{\alpha}_1^{\hat{S}^{NEW}} / \hat{\alpha}_1^{\hat{S}^{TRAD}}$.

In the following analysis, we present 2SLS estimates for illustrative purposes. We see two main rationales for using the reform as an IV for education in the current setting: first, to highlight the potential problems related to measurement error in years of education. Second, it is of interest to understand whether there are systematic differences in returns to schooling between socioeconomic groups. Still, 2SLS estimates need to be interpreted with caution since the exclusion restriction is controversial. Whenever it is violated, 2SLS based on the improved measure of education will converge to (cf. Nevo and

[19] The empirical evidence includes balancing regressions, event-study graph analysis and robustness to the inclusion of municipality-specific cohort trends.

[20] Hjalmarsson, Holmlund and Lindquist (2015); Lundborg et al. (2018); Lundborg, Nilsson and Rooth (2014); Lundborg and Majlesi (2018)

Rosen, 2012)

$$\text{plim } \hat{\beta}_1^{IV,\hat{S}^{NEW}} = \beta_1 + \frac{\sigma_{z\epsilon}}{\sigma_{zS}} \tag{3.5}$$

where $\sigma_{z\epsilon}$ represents the covariance between the instrument and the error term in the earnings equation, and σ_{zS} represents the covariance between the instrument and the residual variation in schooling after partialling out all the other right-hand side variables. To the extent that the compulsory schooling reform led to a reduction in school quality for high-SES individuals (a claim made by MP and others) – which would imply $\sigma_{z\epsilon} < 0$ for this group – then the 2SLS estimate $\hat{\beta}_1^{IV,\hat{S}^{NEW}}$ represents a *lower bound* for the true effect in that group. Conversely, if low-SES individuals experienced improved schooling quality – e.g. due to de-tracking and peer effects in general – then $\hat{\beta}_1^{IV,\hat{S}^{NEW}}$ is *upward biased*. Both conclusions require that $\sigma_{zS} > 0$ so that the reform actually increased the years of schooling.

3.4 Results

As a first exercise we replicate the main results in MP using the same 10% random sample. The original estimates of MP are given in the first column of table 3.1. In order to allow a comparison with estimates for the full population we run the same regressions with municipality, year and cohort fixed effects, but without further control variables. Omitting further control variables does not qualitatively change the results. These results presented in the first and second columns of table 3.1. Omitting the controls yields results that are very close to the original estimates in MP (column one) with near zero and insignificant reform effects on earnings and an increase in education of about 0.2 years.[21] We expanded the analysis, focusing on the very same two cohorts and tax years (1985 – 1996) as MP, but using data for the full population (see in column 3 of table 3.1) and we find essentially the same results as based on the 10% random sample.

[21] The effects on education are slightly smaller without additional control variables though confidence intervals are overlapping.

TABLE 3.1: Replication: Main results Meghir and Palme (2005)

	(1)	(2)	(3)
	MP SAMPLE		FULL POPULATION
FIRST STAGE:	Years of Education ($\hat{S}^{TRAD}$)		
Reform	0.298***	0.184**	0.260***
	(0.075)	(0.086)	(0.057)
N	19,311	19,311	215,888
Municipalities	929	929	1,026
REDUCED FORM:	Log Labor Earnings		
Reform	0.014	0.009	0.014***
	(0.009)	(0.009)	(0.005)
N	209,683	209,683	2,464,540
Municipalities	925	925	1,026
Further Controls	✓		

Notes: Results refer to cohorts 1948 and 1953. Labor Earnings are given for years 1985–1996. Robust standard errors clustered at municipality level are reported in parenthesis. Significance levels: $*$ 0.10 $**$ 0.05 $***$ 0.01. All regressions control for municipality FE, birth cohort FE and gender. Reduced form estimates for labor earnings additionally control for year fixed effects.
Column (1) shows the original estimates with additional control variables added to the regression. Control variables include test scores and school grades obtained when the pupils were in sixth grade and indicators for the county of residence. Column (2) shows a standard difference-in-differences specification without further control variables. Column (3) shows the standard DiD specification for the total population.
Source: SIP. Original data from Meghir and Palme (2005), AER (10% sample of total population). Own calculations.

Table 3.2 presents results stratified by father's education (low vs. high). The original estimates from MP given in column 1–3 of table 3.2 show striking differences between individuals from different socio-economic backgrounds. Students with highly educated fathers *reduced* their educational attainment as a response to the reform. Turning to labor earnings in the lower panel, an increase of 3–4% in earnings for individuals from low socio-economic background is mirrored by a substantial decrease of 5–6% for individuals with highly educated fathers. Columns 4–6 in table 3.2 show that the results are robust to modelling choice. Without additional control variables the differential effect between socio-economic groups is even slightly more pronounced.

This conclusion changes dramatically when the sample is extended to the full population. As expected, individuals from lower socio-economic backgrounds experienced a larger increase in education also for the full population of the two cohorts. But there is no evidence of a decrease in educational attainment for individuals from higher socio-economic backgrounds. The effects for high-SES students are positive for both outcomes and also statistically different from zero for labor earnings.

Table 3.3 presents results based on the new education measure. First, with an average increase between 0.45–0.5 years of education, the effect size is approximately double that of the results using the traditional measure found

in 3.2. As the new measure takes the full compliance with the reform into account, the regression estimates suggest that half of the compliers to the reform are not identified when only using information from the highest educational level. Furthermore, based on the new measure we also find a statistically significant increase in educational attainment for students from a high socio-economic background. To investigate the source of this increase, we use educational levels as binary outcome variables in regressions. Figure 3.2 reveals that the increase in years of education for students with high educated fathers stems mainly from individuals finishing upper secondary school (leading to 2–3 years of upper secondary education). At the same time the proportion of students with only 8 years in school is reduced in this group.

These results suggest that if anything, students with highly educated fathers benefited from the comprehensive school reform in terms of educational attainment and labor market returns. This is in striking contrast to the results reported based on the 10% random sample which suggested sizable earnings *losses* as a result of the reform. The lower panel in Table 3.3 presents the 2SLS estimates based on the new education measure. The estimates suggest moderate returns to education of 3% for individuals with a low socio-economic background, while IV estimates for the high socio-economic background are large and positive though statistically insignificant. The point estimates suggest potential large heterogeneity in the returns to education across groups. If the earnings increase would transmit solely through years of education (i.e. the exclusion restriction holds), the causal effect of education would be substantially larger (17% vs. 3%) in the high socio-economic background group.

Finally, table 3.4 directly compares 2SLS estimates for both education measures to demonstrate the consequences of the choice of the educational variable for instrumental variables estimations. As reduced-form estimates are unaffected by the coding of years of education, only the first- and second-stage results change. If we are willing to assume that the exclusion restriction holds, instrumental variables estimates based on our new measure $\hat{S}^{NEW}$ suggest a local average treatment effect almost twice as large compared to the traditional education measure $\hat{S}^{TRAD}$.

Appendix C.2 presents additional regression estimates showing that our main results concerning heterogeneity and the first stage of the reform are robust to changes of the sample and across gender. This includes separate specifications for males and females, extending the analysis to the whole reform period 1938–1954 and using approximations for life-cycle earnings. Across all specifications and samples we find substantially larger increases in years of education, and no adverse effects for students from high socio-economic backgrounds. As a final exercise we compare the results for the comprehensive school reform to results for the parallel compulsory schooling extension in the old primary school system. Table C.10 shows that the pure compulsory schooling extension, which did not affect the tracking system, had equally small and mainly statistically insignificant effects on earnings – on average and across socio-economic groups and gender.

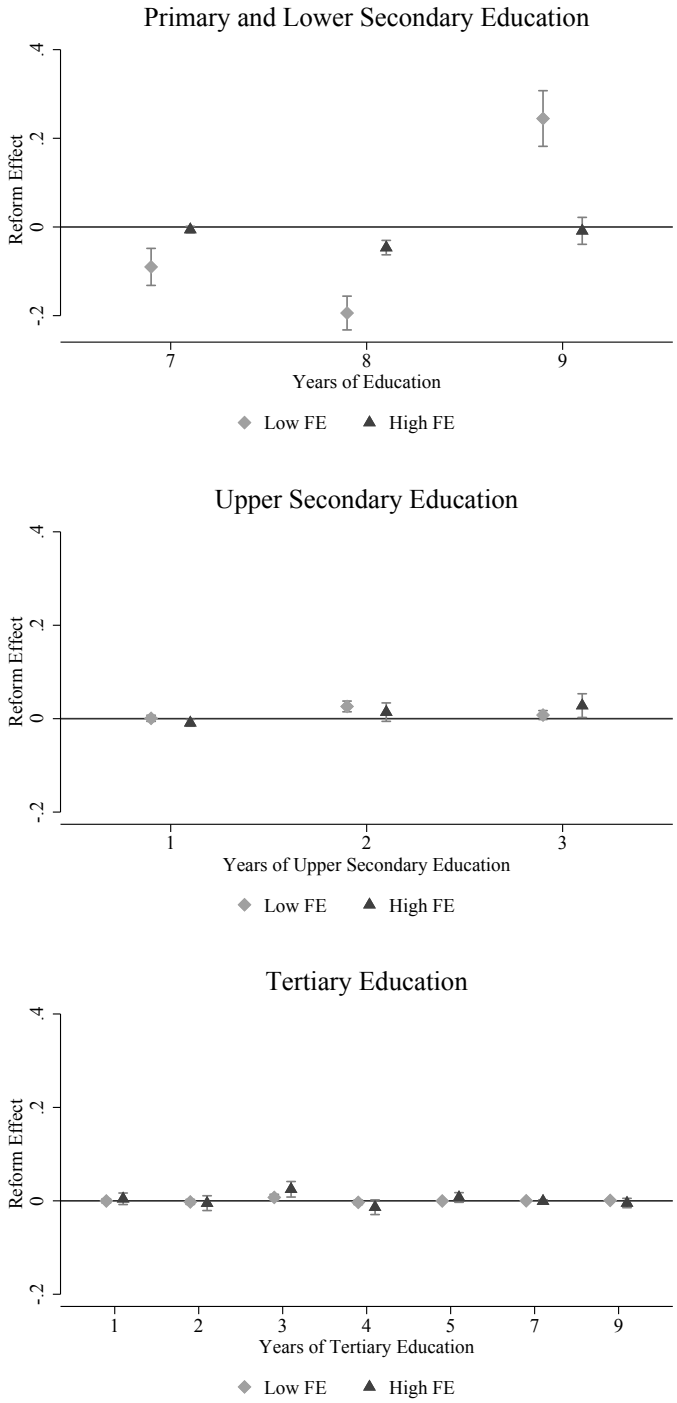


FIGURE 3.2: Reform Effects on Levels of Education

Notes: Reduced form regressions with educational levels as dependent variable. For each level of schooling and post-schooling we run a separate regression split by fathers education. Results from all figures refer to cohorts 1948 and 1953. 95% CI based on robust standard errors clustered at municipality level. All regressions control for municipality FE, birth cohort FE and gender.
Source: SIP. Own calculations.

TABLE 3.2: Heterogeneity SES and Old First Stage

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	MP SAMPLE						FULL POPULATION		
	All	Father LE	Father HE	All	Father LE	Father HE	All	Father LE	Father HE
FIRST STAGE:	Years of Education ($\hat{S}^{TRAD}$)								
Reform	0.298***	0.405***	-0.130	0.184**	0.364***	-0.300**	0.260***	0.292***	0.116
	(0.075)	(0.070)	(0.124)	(0.086)	(0.072)	(0.145)	(0.057)	(0.052)	(0.075)
N	19,311	15,985	3,326	19,311	15,985	3,326	215,888	141,118	25,197
Municipalities	929	929	575	929	929	575	1,026	1,026	958
REDUCED FORM:	Log Labor Earnings								
Reform	0.014	0.034***	-0.056***	0.009	0.033***	-0.074***	0.014***	0.013**	0.025**
	(0.009)	(0.009)	(0.019)	(0.009)	(0.009)	(0.018)	(0.005)	(0.006)	(0.013)
N	209,683	173,435	36,248	209,683	173,435	36,248	2,464,540	1,613,809	288,859
Municipalities	925	925	573	925	925	573	1,026	1,026	958
Further Controls	✓	✓	✓						

Notes: Results refer to cohorts 1948 and 1953 (sample chosen accordingly to Meghir and Palme (2005)). Robust standard errors clustered at municipality level are reported in parenthesis. Significance levels: $*$ 0.10 $**$ 0.05 $***$ 0.01. All regressions control for municipality FE, birth cohort FE and gender. Estimates are stratified by father's having only compulsory education (LE) or more than compulsory education (HE).
Source: SIP. Original data from Meghir and Palme (2005), AER (10% sample of total population). Own calculations.

TABLE 3.3: New First Stage and 2SLS

	(1)	(2)	(3)
		FULL POPULATION	
	All	Father LE	Father HE
NEW FIRST STAGE:	Years of Education ($\hat{S}^{NEW}$)		
Reform	0.458***	0.496***	0.160**
	(0.111)	(0.076)	(0.073)
N	208,486	136,461	24,354
Municipalities	1,026	1,026	949
2SLS:	Log Labor Earnings		
Years of Education	0.031***	0.029**	0.170
	(0.010)	(0.012)	(0.104)
N	2,381,434	1,561,806	279,154
Municipalities	1,026	1,026	949

Notes: Results refer to cohorts 1948 and 1953 (sample chosen accordingly to Meghir and Palme (2005)). Robust standard errors clustered at municipality level are reported in parenthesis. Significance levels: $*$ 0.10 $**$ 0.05 $***$ 0.01. All regressions control for municipality FE, birth cohort FE and gender.
Estimates are stratified by father's having only compulsory education (LE) or more than compulsory education (HE).
Source: SIP. Own calculations.

TABLE 3.4: 2SLS: Comparison $\hat{S}^{TRAD}$ and $\hat{S}^{NEW}$

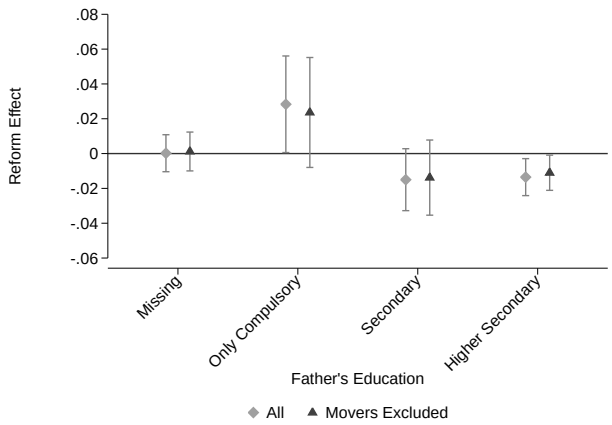
	(1)	(2)	(3)
	$\hat{S}^{TRAD}$		$\hat{S}^{NEW}$
FIRST STAGE:			
Reform	0.184**	0.260***	0.458***
	(0.086)	(0.057)	(0.111)
N	19,311	215,888	208,486
Municipalities	929	1,026	1,026
2SLS:	Log Labor Earnings		
Years of Education	0.056***	0.053***	0.031***
	(0.020)	(0.018)	(0.010)
N	206,983	2,461,495	2,381,434
Municipalities	929	1,026	1,026
MP Sample	✓		
Full Population		✓	✓

Notes: Results refer to cohorts 1948 and 1953. Robust standard errors clustered at municipality level are reported in parenthesis. Significance levels: $*$ 0.10 $**$ 0.05 $***$ 0.01. All regressions control for municipality FE, birth cohort FE and gender.
Source: SIP & original data from Meghir and Palme (2005), AER (10% sample of total population). Own calculations.

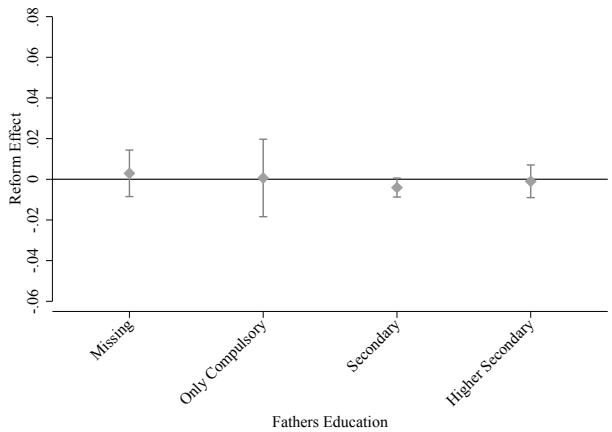
3.5 Discussion

Below we elaborate on two things that stands out in our extended analysis. First, our analysis contradicts the heterogeneity findings in MP as we do not reveal a socio-economic gradient in the effects of the comprehensive school reform. In order to explain why heterogeneity only appears in the original 10% sample, we consider paternal education as a left-hand side variable. Using the 10% sample, figure 3.3(a) reveals a negative association between fathers education and reform status, indicating a bad control variable. The correlation is modest but stratifying on a bad control variable could exacerbate a small bias. Figure 3.3(b) shows no association between paternal education and reform status when using data for the full population.

Endogeneity of paternal education could be the result of a small-sample bias or selective non-response in the original data. Furthermore, in the original study MP condition on the school district a student actually visited instead of the place of residence. If parents from higher socio-economic backgrounds sent their children away from the newly introduced comprehensive school system to a neighboring municipality where the child still could take on lower secondary education within the old system, both the actual school district and parental background become endogenous variables. Sending children to another district for secondary school appears much more likely than migrating with the whole family to another municipality in order to avoid the reform. A robustness check in MP addresses migration by excluding individuals where school district and municipality of birth/residence does not coincide, i.e. movers. Figure 3.3(a) shows that excluding those 2,000 movers leads to less precision, but point estimates hardly change and still indicate a negative correlation between paternal education and reform assignment.



(a)



(b)

FIGURE 3.3: Reform Effects on Fathers Education: (**a**) 10% Sample (AER) ; (**b**) Full population (SIP)

Notes: Reduced form regressions with fathers educational levels as dependent variable. Results from both figures refer to cohorts 1948 and 1953. 95% CI based on robust standard errors clustered at municipality level. All regressions control for municipality FE, birth cohort FE and gender.
Source: SIP, Data from Meghir and Palme (2005), AER. Own calculations.

Second, our results support the general finding in MP on that the comprehensive school reform increased educational attainment, but using a new and improved education measure we find substantially larger education effects. This suggests that the reform is a stronger instrument for education than previously reported, but also that IV estimates using the traditional years of education measure $\hat{S}^{TRAD}$ based on the educational registers are upward biased.[22] This is a direct consequence of a downward biased first stage and an unaffected reduced form. Table 3.5 gives estimates for the bias factor δ for the cohorts 1948 and 1953:

[22]Our results also imply a bias in previous measures of intergenerational persistence in education, and in the estimated effects of the reform on these intergenerational elasticities.

TABLE 3.5: Upward Bias IV, Traditional Education Measure $\hat{S}^{TRAD}$

	All	Father LE	Father HE
$\hat{\delta}$	1.76	1.70	1.38

Notes: Results refer to cohorts 1948 and 1953, full population. The table shows the ratio of the first stages based on the alternative measure $\hat{S}^{NEW}$ and the traditional measure $\hat{S}^{TRAD}$. Estimates are stratified by father's having only compulsory education (LE) or more than compulsory education (HE).
Source: SIP. Own calculations.

3.6 Conclusion

Replicating the work of Meghir and Palme (2005) using data for the full population we found several important deviating results compared to the original study. First, we cannot confirm the presence of heterogenous effects by socio-economic background for the Swedish comprehensive school reform. Extending the original analysis to the total population, reduced form effects are uniformly small and there is no indication of an efficiency decrease caused by the postponement of academic tracking. The result is highly policy relevant for the *efficiency vs. equity* debate related to academic tracking. Meghir and Palme (2005) is one among very few studies indicating a negative effect from delayed tracking for students with high socio-economic background, suggesting that early sorting may increase efficiency in the school system (Betts et al., 2011; Holmlund, 2016). Our analysis does not support such a conclusion and reveals no harmful effects from delayed academic tracking for any specific sub-group.

Furthermore, using a new education measure, our results show that the Swedish comprehensive school reform lead to substantially larger increases in years of education than previously reported. Earlier studies have relied on an education measure based on the highest level of education. This misses important variation induced by the extended compulsory schooling and leads to a sizeable upward bias in instrumental variable estimates. In the context of the Swedish comprehensive school reform, we show that instrumental variable estimates decrease by approximately factor 2 after a correction for this upward bias. Our results very likely apply to similar studies outside Sweden.

Overall, our results indicate that effects of the comprehensive school reform on labor market earnings were modest. If we assume that effects are driven solely by the increase in years of education, the estimated returns to education in Sweden from the compulsory schooling extension lie around 3%. This is in line with recent estimates based on compulsory schooling laws in Europe (Pischke and Von Wachter, 2008; Devereux and Hart, 2010).[23] Our findings are corroborated by the results from a separate analysis of a parallel

[23]We purposefully only compare estimates with studies which are not subject to the inflation of instrumental variable estimates described earlier. Pischke and Von Wachter (2008) mainly focusses on reduced form estimations and uses self-reported years of education. Devereux and Hart (2010) relies on school leaving age.

school reform in Sweden that only increased compulsory education without affecting the tracking system.

Chapter 4

Compulsory Education and Longevity in Sweden: Evidence from Aggregated Statistics[1]

4.1 Introduction

There is a well-known empirical association between education and health. It has been observed in several countries and time periods and for a wide range of health measures. For example, the life expectancy of male American university graduates at age 25 was 56 years in 2006, compared to 47 years for individuals without a high school degree. For females, the corresponding figure was 60 for graduates and 52 for high school dropouts. Besides, there is evidence suggesting that recent improvements in life expectancy have been concentrated in higher educational groups: since 1996, university graduates have increased their life expectancy at 25 by 1–2 years, whereas no improvement has been observed for the lowest educational group U.S. Department of Health and Human Services (2012).

In Sweden, pecuniary returns to education appear to be much smaller than in the U.S. An American university graduate can expect to earn 60–85% more than someone with secondary education only, and individuals with educational attainment below the secondary level have earnings 30–35% below those of high school graduates. In Sweden, the premium is much lower for both levels, and the college premium is as low as 13% for individuals aged 25–34 (OECD, 2012); income taxes even out these smaller differences even further.

Despite these relatively small differences in disposable income between educational groups, Sweden, however, just like the U.S., exhibits a striking education gradient in life expectancies, and these differences show no tendency of abating. In the year 2000, life expectancy at 30 was 51.5 years for males with a university degree; for individuals with less than a secondary education, it was 46.6 years. Ten years later, university graduates had increased their life expectancy to 53.2 years, compared to 48.0 years for the group with the lowest educational attainment (Statistics Sweden, 2011).

[1]This Chapter was co-authored by Martin Karlsson and Therese Nilsson. See Fischer, Karlsson and Nilsson (2013) for a published version of this chapter.

The study of financial returns to education have a long history in economics, and over the years, a consensus has emerged that an additional year in secondary education is associated with a substantial increase in wages, in particular for individuals from disadvantaged backgrounds (Oreopoulos and Salvanes, 2011). When non-pecuniary outcomes, such as health, are concerned, there is much less evidence and, also, much less agreement between different contexts. It remains unclear to what extent the disparities in life expectancies mentioned above reflect a causal impact of education on health. An education gradient would also arise if there were reverse causation, so that childhood health—which is known to be a strong predictor of adult health—affects the number of years of schooling. Likewise, there may be a third factor affecting both education and health—such as family background, time preferences or cognitive abilities. Cutler and Lleras-Muney (2006), however, conclude that there are reasons to be skeptical about these two explanations: the widening of disparities can hardly be driven by child health, which has been improving over time, and controlling for potential confounders tends to only slightly mitigate the correlation between education and health.

If education has a causal effect on health, it is important to consider potentially relevant mechanisms. Traditionally, the main attention of economists has been devoted to the role of education in facilitating the production of health: individuals with higher human capital likely combine inputs in the health production process in a more efficient manner (Grossman, 1972). However, alternative explanations seem equally plausible: education may also affect an individual's working conditions, preferences, rank in society and social networks—all of which are instrumental to maintaining good health (Cutler and Lleras-Muney, 2006).

Identification problems and potential mechanisms aside, one reason for the lack of a consensus regarding the effects of education on health is probably the wide variety of health outcomes that have been considered in the literature: previous studies have considered self-reported health (SAH), hospitalizations, weight and BMI, long-term illness, biomarkers, mortality and health-related behaviors (Arendt, 2008; Kemptner, Jürges and Reinhold, 2011; Jürges, Reinhold and Salm, 2011; Clark and Royer, 2013). However, also in cases where the same exogenous variation is used to estimate the impact on the same type of outcome, researchers disagree about the effect. For example, Oreopoulos (2006) and Silles (2009) both use extensions of compulsory schooling in Britain to estimate the effect of schooling on SAH. Both studies claim to find substantial positive effects of the reforms on SAH. In a recent study, however, Clark and Royer (2013) challenge this view and report effects that are small and insignificant—and attribute the effects found in previous studies to methodological weaknesses. In an online corrigendum, Oreopoulos later, however, presents evidence suggesting that there is no effect on SAH. This debate clearly highlights the importance of carefully scrutinizing the identification strategy.

In this paper, we use a change in compulsory schooling legislation in Sweden to estimate the effect of schooling on mortality. In 1936, the national government decide that a seventh school year should be introduced in all school districts before 1949. The reform was typically implemented earlier in urban districts than in rural districts, but there is also general variation in the timing of implementation between districts, which facilitates the identification of the effect.

The reform we consider has not been used before to estimate the effects of schooling, but there are several reasons why we think it is very suitable for analyzing the effects of education on health. First, it represents a relatively large expansion of compulsory schooling, so it can be expected that the estimated effects (if any) are relatively large. Second, whereas the next reform that followed in Sweden (implemented in 1949–1962) reshaped the entire school system, the reform we consider kept the school system relatively intact. Thus, there is much less ambiguity concerning the mechanisms driving the results. Third, the reform was implemented around 70 years ago, implying that mortality outcomes can be studied over a very long time horizon. Fourth, the rate of compliance with the reform was very high: at the time, an overwhelming majority of Swedes would only receive the minimum level of education.

There are a number of empirical studies that use similar methods to uncover the effects of schooling on health. The study most similar in spirit to ours is Lleras-Muney (2005), which considers compulsory schooling reforms in the U.S. in the early 20th century. Using a host of compulsory education laws from the 1915–1939 period in instrumental variable (IV) estimations, Lleras-Muney concludes that an additional year of schooling led to reductions in 10-year mortality rates by as much as 60% (six percentage points). This effect is relatively large compared to what has been found in subsequent studies. Clark and Royer (2013) study the effects of two British reforms on mortality and a host of other health outcomes. They find that small effects on mortality and coefficients often even have the "wrong" sign. A recent paper by Meghir, Palme and Simeonova (2012*b*) considers the effect of the Swedish 1949–1962 expansion of compulsory schooling on mortality, hospitalizations and labor market transitions. Their estimates for mortality suggest that there was a substantial reduction in male mortality up to the age of 50, but that these gains were erased by elevated mortality at higher ages. Similar patterns are observed for hospitalizations. Another recent paper is Lager and Torssander (2012), also considering the Swedish 1949–1962 expansion of compulsory schooling. Separating between death causes, Lager and Torssander (2012) find lower mortality in the treated group from death causes generally seen as related to more and better education, such as cancer and accident mortality. Lower mortality is also found among the least educated not continuing to senior secondary or tertiary schooling. The estimates are, however, generally small, and the authors conclude that there does not seem to exist any impact of this later schooling reform on all-cause mortality in all ages.

In an overview of the previous literature, Mazumder (2012) concludes

that the overall evidence of an effect of education on mortality is weak and that it may have different effects in different countries. Evidence is stronger for the U.S., Germany and the Netherlands than for the UK, Sweden and France. However, the issue of external validity does not only relate to the country context: there may also be important differences between different time periods. Gathmann, Jürges and Reinhold (2015) compare reforms from different countries and argue that the most consistent reductions in mortality (for both shorter and longer-run mortality) are found (for men) for a 1919 reform in Belgium and for women for a 1928 reform in the Netherlands. Thus, it appears that earlier schooling reforms have a stronger impact on mortality than those implemented after WWII. To the extent that these differences are down to education having a different effect at different levels of economic development, they imply that our findings are particularly relevant in a low- or medium-income context and not necessarily for inferring the possible effects of further extensions to compulsory schooling in present-day Sweden. Table 4.1 presents findings, data, and methodological strategies in recent contributions to the literature on the causal effects of compulsory schooling on mortality. It highlights the fact that the institutional setting and the timing of the reform matters.

Our results suggest that the relative reduction in mortality brought by an additional year of compulsory schooling is much larger than the one found by Lleras-Muney (2005). However, the discrepancy appears to be attributable to the higher rate of compliance in Sweden. According to our preferred specification, a reduction in mortality is discernible already before the age of 30. The effect then grows in magnitude and reaches its maximum around the age of 55–60, after which it remains constant or declines slightly. Thus, we do not observe the reversal of effects reported by Meghir, Palme and Simeonova (2012*b*). Our general findings appear to fit in line with the results shown in Table 4.1 and the meta analysis of Gathmann, Jürges and Reinhold (2015) that reforms implemented in the early 20th century are probably the most effective concerning a reduction in mortality. A major advantage compared to other studies is the possibility that we can actually also estimate short-run effects on mortality for an early implemented reform. The previous studies can usually only fit models conditional on survival up to a certain point in time.

The rest of the paper is organized as follows. Section 4.2 provides a brief history of the Swedish school system, with particular emphasis on reforms during the first half of the 20th century. Section 4.3 gives an overview on the data sources and sample selection. Section 4.4 presents our empirical strategy and section 4.5 provides the results. Section 4.6 concludes.

TABLE 4.1: Literature overview: Causal effects of compulsory schooling on mortality

AUTHORS	COUNTRY / DATA SOURCE	YEAR/CONTENT OF THE REFORM	IDENTIFICATION STRATEGY	MAIN RESULTS
Albouy and Lequien (2009)	France / Longitudinal data: Echantillon Demographique Permanent Census data (1968,1975,1982,1990,1999) Register Data of Deaths from 1968-2005	1936 (Zay Reform) / 6→7 1967 (Berthoin Reform) / 7→9	Regression Discontinuity Design on birth cohorts	Zay Reform: Survival till 82 for those survived till 1968 increased by 6% (Wald-estimate). Berthoin Reform: Survival till 52 for those survived till 1968 increased by 1%(Wald-estimate). Effects statistically insignificant.
Gathmann, Jürges and Reinhold (2015)	Various European Countries / Human Mortality Database European Social Service International Social Survey Programme Survey of Health Ageing and Retirement	19 different Reforms	Regression Discontinuity Design on birth cohorts Meta Analysis (for pooled estimate over the 19 reforms)	Substantial heterogeneity in time and space: Effects probably larger for reforms implemented earlier in the 20th century. Gender differences: No effects for women; reduction of 2.8% in 20 years male mortality from age 18 (reduced form).
Van Kippersluis, O'Donnell and van Doorslaer (2011)	Netherlands / Dutch Cross-sectional General Household Survey (1997-2005) Tax Records (1998) Cause-of-Death register (1998-2005) Dutch Municipality Register	1928 / 6→7	Regression Discontinuity Design on date of birth (individual data)	2-3% decrease in mortality till the age of 89 for those survived till the age of 81 (reduced form). Reduced form similar to two stage least squares estimates as raise in education between 0.6-1 depending on specification.
Clark and Royer (2013)	England and Wales / Mortality Data from the Office for National Statistics: All deaths for the years 1970 to 2007.	1947 / 8→9 1972 / 9→10	Regression Discontinuity Design	Hardly any evidence for a reduction of mortality. Some estimates even with positive sign.
Meghir, Palme and Simeonova (2012*b*)	Sweden Swedish population censuses; all individuals born in Sweden between 1946 and 1957	Implemented by municipalities between 1949 and 1962. From 1962 nationwide. / (7 or 8)→9	Reduced Form Difference in Difference / IV	Short-lived gain in expected male years of life from a shift in mortality from ages 45-50 to ages 50-55. Overall life expectancy not significantly affected. Heterogeneity with respect to social background.
Lleras-Muney (2005)	U.S. / Census (1960, 1970, 1980) National Health and Nutrition Examination Survey	1915 - 1939 Various U.S. States with different extensions.	Difference in Difference / IV Regression Discontinuity Design	Extension of one year of education decreases 10 year mortality for those surviving till 1960 by 3.6% (IV) relative to a baseline mortality of 10%. Estimates challanged by Mazumder (2012): Sensitive to state specific time trends; effects mainly due to earliest cohorts.
Lager and Torssander (2012)	Sweden / Swedish population censuses All individuals born in Sweden between 1943 and 1955.	Implemented by municipalities between 1949 and 1962. From 1962 nationwide. / (7 or 8)→9	Reduced Form Difference in Difference / IV	Overall all-cause mortality not significantly affected. Lower mortality from causes related to education (e.g. cancer and accidents). Socioeconomic heterogeneity with lower mortality among least educated

4.2 Background on the Educational System and the Reform

Compulsory school attendance was introduced in Sweden in 1842 and applied to all resident children. The compulsory school attendance implied both the right to cost-free schooling in compulsory schools and the obligation to take part in the schooling offered. The central management of the education system was practiced by the *Ecklesiastikdepartementet*, the Ministry for Ecclesiastical affairs. The country was divided into more than 2,000 school districts, and the local administration of compulsory education in these school districts was the responsibility of a school board (education policies were consequently not designed at the county (regional) level, which is the level we use in our empirical analysis). With the exception of girls' schools, private schools were always insignificant in Sweden (Ecklesiastikdepartementet, 1935*a*).

At the beginning of the 20th century, the Swedish educational system was highly selective. Schooling started at the age of seven and was compulsory for six years. The vast majority ended school after these six years in *Folkskolan*. In 1918, it was further decided that sixth grade pupils that did not continue to the non-compulsory secondary school also had to take two years of vocational courses (Ecklesiastikdepartementet, 1935*a*). These were all practically oriented one- or two-year courses in domestic science, craft or manufacturing given at local schools. Importantly, the vocational courses were taught with a very low intensity. As described by Fredriksson (1950), the time for vocational courses was only 180 h per year.

Students who chose to follow an academic path continued to lower secondary school to pass the so-called *realexamen* (although rare, students could choose to continue onto lower secondary school already at the age of 11, *i.e.*, leaving compulsory school after the fourth grade, implying that the Swedish educational system had parallel structures). However, secondary schools became widely spread geographically only between the mid-1940s and the early 1960s (Murray, 1988), and very few students continued to secondary schooling before the 1950s. In 1940, only 10% of the cohort graduating from *Folkskolan* continued in secondary school (Fredriksson, 1971). After realexamen, students either left school or entered the upper secondary level and had to sit in the prestigious *studentexamen* after three more years of studies. This was comparable to the French BAC, German *Abitur* and the English A-levels and a prerequisite for matriculating into university (Jonsson, 1996). With a highly selective school system, only 5% of a cohort continued to upper secondary schooling in 1940 (Fredriksson, 1971).

Students in *Folkskolan* were attending full-time schooling approximately eight months per year. In the rural areas, it was, however, also possible for school districts to offer half-time reading, so that children attended school only certain days of the week. This school form only existed in the rural areas of Sweden, where children often helped out in the agrarian sector. The existence of half-time reading was heavily debated in the 1920s, and the extent of

half-time reading was reduced during this decade, although still permitted. In 1933, 93% of all pupils took part in full-time schooling (Fredriksson, 1950).

For a long time, local conditions decided the format and content of primary education, and at the beginning of the 20th century, there was large variation across school districts. The national government issued its directives for the curriculum in so-called *normalplaner* (normal plans), but this document was only advisory. In 1919, however, the so-called *Utbildningsplanen* (the education plan) was introduced, which came to restructure the school's work according to the central guidelines of Ecklesiastikdepartementet. Utbildningsplanen was a governing document and included time-tables and syllabuses for compulsory school (Lindmark, 2009). It is, of course, difficult to know about the quality of the education across school districts. Completion rates were, however, high: 90% of all pupils finished Folkskolan with full curriculum (Fredriksson, 1950). The implementation of the Utbildningsplan was the first instance in which local autonomy had to give way for national standards, and the Government's edict was subject to financial compensation (Ecklesiastikdepartementet, 1935*a*).

On 1 July 1936, the national Government decided that seven-year school should be compulsory. Already, in 1925, a clause had been introduced in the primary school code that a seventh school year could be made compulsory (Fredriksson, 1950). School districts were also allowed to introduce eight year compulsory schooling, but this was a very rare event, both in urban and rural areas. In 1940, only 0.1% of all schools in the country offered eight years of education (Fredriksson, 1950). To extend compulsory schooling with an extra year was then the decision of the school board in a school district, and already, in 1936, several school districts in the urban areas had introduced an extra year of schooling. Furthermore, in the southernmost and mainly rural region of Scania, several school districts had implemented seven years of schooling, but, still, only 16% of all children in rural areas of Sweden attended seven years of schooling in 1936.

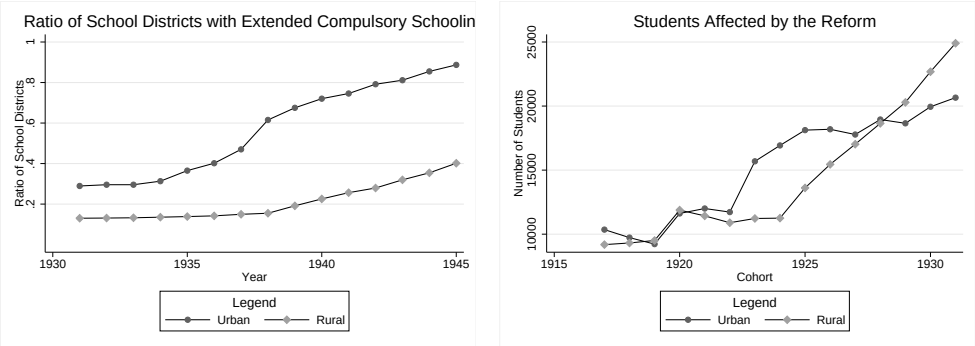
In Sweden, child labor laws and compulsory attendance laws have generally been coordinated. One basic principle has been that the right to education takes precedence over the demands of the labor market—so that educational requirements with respect to knowledge and time should determine if and when the young were allowed to work. Compulsory school attendance regulations have consequently reinforced the protective labor legislation, and as discussed by Sjöberg (2009), Swedish authorities have generally relied on double protection—age limits and compulsory school attendance. 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. A child was allowed to work from the beginning of the calendar year in which they would reach the age limit. After the implementation of the compulsory school reform, most pupils left school in the middle of the year they turned 14, whereas before they would have left the year, they turned 13. This means that the reform reduced the time a child could spend in "light work" by one year, whereas the corresponding reduction for "hard" (industrial) work would have been 5–6 months only. The 1949 Labor Act increased

the age limit by one 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).

The main motive for the reform was that six years was considered too short for achieving the learning objectives that were stated for the *Folkskolan*. This motive was also mentioned in the decision allowing school districts to implement seven years of compulsory schooling in 1925. Additional arguments on the importance of a change in the compulsory schooling legislation mentioned in various investigations were the increasing youth unemployment among those who had just finished elementary school, but also that another year of education was of importance to maintain a democratic society (Ecklesiastikdepartementet, 1935*a*). In the debate preceding the introduction of the reform, politicians were also often benchmarking with other Western countries, and it was noted that the number of school years was the most striking difference of compulsory education in Sweden compared with Denmark, Norway, Germany and Great Britain.

In line with the underlying motive for the parliamentary decision, the reform did not require any fundamental changes with respect to learning outcomes to be achieved or curricula, but instead emphasized the goal of achieving more long-lasting results of schooling. The recommendation from the central administration was that the school districts should distribute the pre-reform compulsory school curricula over seven years instead of six (Ecklesiastikdepartementet, 1935*a*).

The reform was not implemented at the same time in all school districts, but, instead, it was stipulated that it had to be implemented in all school districts before 1949. The compulsory seventh year was consequently introduced during a twelve-year transition period. In 1936–1941, an implementation of an extra year was completely at the discretion of the school district. From 1942 and onwards, a school district could be assigned to implement the reform, but according to official sources, this only happened once. The national school authority *Skolöverstyrelsen*, monitored the implementation. Initially, the reform led to a relatively rapid transition. In 1940, 33% of the rural and 80% of the urban schools had implemented a seventh year (Fredriksson, 1950). After 1940, the implementation rate, however, seems to have decreased somewhat. Figure 4.1(a) shows the trends in implementation and reports the proportion of school districts that had at least seven years of compulsory schooling at the end of each school year, and Figure 4.1(b) shows the number of students affected by the reform by birth cohort.



(a) (b)

FIGURE 4.1: (**a**) Proportion of school districts with seven years of compulsory schooling; (**b**) number of students affected.

Due to the soft transition rules, the reform does not seem to have caused any major difficulties in the school districts, and 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 and Westberg, 2011).

From 1936 until the 1950s, not much else other than the implementation of the seven year reform changed in the Swedish educational system. In 1948, a parliamentary school committee proposed a new school reform that implied nine-year compulsory and comprehensive schooling. Additionally, this reform, starting in 1949 and evaluated, e.g., in Meghir and Palme (2005) and Meghir, Palme and Simeonova (2012*b*), was gradually implemented across school districts over time. The major consequences of this reform were additional years of schooling, but also a reshaping of the entire school system.

As regards the wider institutional context, we are unaware of any reform relevant to public health that might have coincided with the school reforms at the local level. Such a coincidence is unlikely, since decisions related to healthcare were taken at either the county level or at the health district level, both of which typically consisted of several school districts. Moreover, most of the expansion of the Swedish healthcare system came after the introduction of uniform social health insurance in 1955 (Ståhlberg, 2008). Thus, access to services was limited and did not change much during the period we consider. For example, a survey made by the National Board of Health (*Medcinalstyrelsen*) in 1927 concluded it was very rare that pregnant women had been examined by any kind of medical staff, including midwifes, before delivery (SOU, 1929). This lack of coverage was perceived as a problem and led to a national roll-out of preventative services for pregnant women and newborns from 1937 onwards (Ström, 1942). Moreover, as discussed by Heidenheimer, Elvander and Hultén (1980), the physician density in Sweden was significantly lower compared to, e.g., Norway and Denmark, in the 1930–1950 period, and official statistics suggest that the number of hospitals and hospital beds *per capita* remained more or less constant (Statistics Sweden, 1938–46). One trend that did, however, affect the cohorts included in this study was the transition to institutional delivery, which became the standard

between the years 1920–1940 (Wisselgren, 2005). Even though this general trend could theoretically confound our analysis, it is very unlikely that it does so in practice. The expansion in hospital births was very smooth and mainly driven by individual-level demand-side decisions.

Another potential concern relates to World War II (1939–1945) and the fact that compulsory schooling generally protects individuals from getting involved in armed conflicts. If Swedes joined armed forces and died in battle, this could generate a misinterpretation of the results as an effect of education, while rather capturing an education gradient in enlistment. Notably, however, Sweden was neutral in World War II, and less than 9,000 individuals (almost all of them men and all older than 18 years) fought in the war as volunteers (Wolke, 1996), suggesting this should be a minor problem in our analysis.

In summary, the reform in 1936 introduced an exogenous change in the extent of compulsory schooling in Sweden. The timing of implementation in individual school districts was based on a mixture of local and national decisions; and there is no reason to suspect that the implementation of the reform was driven by health differences. Thus, it will be our working assumption that the reform was exogenous from the individual point of view so that it can be used to identify the effects of schooling on health. As regards the exact definition of treatment, the above account has made clear that affected pupils faced no significant increase in the curriculum to be covered. Thus, effects, if present, will be driven by changes in the amount of time spent in education.

4.3 Data and Sample Selection

4.3.1 Sources

We combine three different datasets in our analysis. Our school reform indicator is based on unpublished material taken from the archive of the national school authority (Skolöverstyrelsen, 1945). After the national government had decided to make a seventh year compulsory in 1936, the national school authority, *Skolöverstyrelsen*, monitored the implementation. Data are available from 1931 onwards for rural areas and from 1938 onwards for urban areas. These tables were based on reports from individual school districts, and they provide the number of districts that had introduced seven (and in some rare cases, eight) years of schooling. In a small number of cases, the classification of districts was challenging, since the seventh year was introduced at different times for different schools within the district. In the original data, such districts have been coded as having seven years of schooling. Given the low number of school districts where this is an issue, this is unlikely to lead to measurement error.

A more serious concern is that we do not have the *identity* of the school districts that have extended compulsory schooling—only in exceptional cases (*i.e.*, when there is only one city in the county) is it possible to unambiguously identify the reform year. Thus, we use the proportion of reformed districts as our treatment variable.

Our second source of data is the census of 1935 (Statistics Sweden, 1937). Traditionally, Sweden had a complete census every ten years; however, in 1935 and 1945, there were additional censuses, which provided basic demographic information on the whole population and, then, studied some parts of the country in more detail. The 1935 census gives us information on cohort sizes, which are used for calculating mortality rates at different ages. For each of the 25 counties, we collect cohort sizes of males and females from urban and rural areas separately—the exception being the City of Stockholm, which was the only county without rural areas. Thus, the total number of cells in each year is 98.

Our third data source consists of the Swedish Death Index, which is a digital data source provided by Federation of Swedish Genealogical Societies (2014). The dataset is based on official records, such as church books, and it is complete from 1947 onwards. For earlier years, 25% of deaths are missing at random throughout the country—with the sole exception of the county of Värmland, which is complete. Since our cohort size data give cohort sizes after infancy, the missing deaths, thus, apply to ages at which mortality rates approach their lifetime minimum. For the cohorts included in our dataset, the cumulative mortality rates between the baseline year and the first year with complete records (*i.e.*, for the period 1936–1946) vary between 1.2% and 2.2%, according to the life tables of the period (our calculations from the census tables). Therefore, the measurement error due to the missing information is likely to be small, but in order to correct for it we multiplied annual cohort-specific mortality rates in relevant years by a factor of 1.33 (*i.e.*, the factor necessary to compensate for the missing deaths).

The Swedish Death Index contains information on gender, date of birth, date of death, parish of birth and parish of death. Since all parishes can be attributed to either the urban or the rural category (and some of them were recategorized during the time covered), we can add them up for each region to get the numerator of the mortality rate. The Swedish Death Index has the advantage that we are able to calculate death rates for the short run, as we can practically observe all deaths in the whole of Sweden for each year.

However, a slight complication arises, due to the coding of birth parishes. Prior to 1946, it was common to attribute hospital births to the place of birth (*i.e.*, the hospital parish) and not to the place of residence (*cf.*, (Holmlund, 2008)). This is a serious concern for cohorts born in the 1940s, when institutional delivery became the norm. However, during the period we consider, hospital births were still exceptional and, also, predominantly affected children from the cities. Consequently, the measurement error arising is limited, but we circumvent this problem by aggregating urban and rural areas in our baseline specification—which brings the number of cells down to 50 per year.

4.3.2 Variable Definitions

We now turn to a detailed description of how the main variables in our analysis have been constructed. We start with our outcome variable—mortality

rates at different ages—and, then, turn to the school reform variable. Summary statistics of all main variables are provided in Table 4.2.

TABLE 4.2: Summary statistics.

	MEAN	STAND. DEV.	MIN	MAX	OBS.
10 Year Death Rate	0.007	0.004	0.000	0.027	731,791
20 Year Death Rate	0.017	0.007	0.000	0.047	731,791
30 Year Death Rate	0.026	0.009	0.004	0.073	731,791
40 Year Death Rate	0.048	0.016	0.010	0.111	731,791
50 Year Death Rate	0.097	0.035	0.025	0.218	731,791
60 Year Death Rate	0.193	0.069	0.050	0.427	731,791
70 Year Death Rate	0.373	0.118	0.105	0.746	731,791
Male	0.509	0.500	0.000	1.000	731,791
Treatment	0.406	0.272	0.000	1.000	731,791
Urban	0.269	0.223	0.040	1.000	731,791
Age at Census 1935	7.622	2.299	4.000	11.000	731,791

Calculated based on 400 cells defined by gender, cohort and county of birth. All statistics are calculated using weights. Weights are given by the number of observations in each cell. ** at the 5% level and *** at the 1% level.

Computing Mortality Rates

As mentioned above, we calculate mortality rates before certain ages combining two data sources, the Swedish Death Index and the 1935 census. The population at risk is defined by cohort-county-gender-urban cells for the cohorts 1924–1931—who were between four and 11 years old at the time of the census. The number of cohorts we can include is restricted by our treatment variable—which is complete only for the years 1938–1945. The choice of the census enables us to count individuals before they finish compulsory schooling. Therefore, we can circumvent moving induced by the treatment of one more year of schooling, a factor that appears possible, as education is often associated with mobility. The choice of using the census, however, lead to the possibility that treatment-induced selection into regions with better healthcare access are picked up as part of the treatment.

The mortality rates have been calculated as $x-$years mortality rates for each cohort-county-gender cell:

$$d_{gcs}^{1935,x} = \frac{\sum_{t=1936}^{1935+x} D_{gcs}^{t}}{N_{gcs}^{1935}}$$

where N_{gcs}^{1935} denotes the size of cohort c of sex s in county g at the end of 1935 and D_{gcs}^{t}, the number of deaths of the corresponding cell during year t. The incomplete deaths between 1935 and 1946 have been imputed under a *missing at random* assumption. The imputed death rates roughly match death rates from official statistics. Computing death rates from a later census to circumvent the missing values in the Death Index was not possible, due to migration into cities. The cell-specific population in 1950 deviates from the population 20 years earlier. This would be problematic with respect to the treatment assignment. As students in cities are more likely to have experienced extended schooling, many more individuals would be assigned as treated if a later census would have been used.

Treatment Indicator

As mentioned above, information to construct the treatment indicator was taken from Skolöverstyrelsen (1945). There were more than 2,300 rural school districts and around 120 urban school districts in total. If we denote by q_{gt} the number of school districts in region g that had extended compulsory schooling in year t and the total number of school districts in the same year as Q_{gt}, we may define our treatment indicator as a fraction:

$$\widetilde{Z}_{gt} = \frac{q_{gt}}{Q_{gt}}$$

If the variation in school district size within a county is relatively small, this variable will approximate the proportion of individuals in county g who are exposed to compulsory schooling beyond the age of 13 in year t. Thus, in order to get the assignment of the treatment variable right, we need to take into account that the cohort, c, which started school in the fall of year $c+7$, will be affected by the extension of compulsory schooling in year $c+13$, and if so, they may leave school no earlier than year $c+14$. Thus, cohort c will be exposed to the treatment indicator, $\widetilde{Z}_{g,c+14}$. As we combine rural and urban areas for reasons mentioned in Section 4.3.1, the final treatment share is constructed as a weighted average between the two areas in every region, g. The weights are given by the specific cohort sizes from the census.

4.4 Empirical Strategy

In our basic specification, the probability of dying within the next x-years from 1935 for an individual $\mathbb{P}(D_{igcs}^{1935,x} = 1)$ is assumed to be given by a linear probability model:

$$D_{igcs}^{1935,x} = \beta_0 + \beta_1 Z_{igs,c+14} + \beta_2' X_{igcs} + \delta_s + \mu_c + \nu_g + f_g(c) + \epsilon_{igcs} \qquad (4.1)$$
$$i = 1, \ldots, N_{gcs}^{1935}; \quad g = 1, \ldots, G; \quad c = 1924, \ldots, 1931; \quad s = 1, 2$$

where c indicates the cohort, g the region, $Z_{igs,c+14}$ is an indicator of whether an individual has been affected by the extension of compulsory schooling, X_{isgc} is a vector of covariates, including a dummy of whether an individual resides in an urban area in 1935, μ_c and ν_g are fixed cohort and regional effects, δ_s is a gender-specific constant and $f_g(c)$ is a region-specific cohort trend. Given the small number of cohorts, we approximate the regional trend by polynomials of the first and second order. N_{gcs}^{1935} gives the number of observations within a specific gender-cohort-region cell. In the basic regressions without the inclusion of cohort trends, β_1 is identified by deviations from a statewide cohort trend and regional specific intercepts and, therefore, constitutes a simple difference-in-difference estimate. This specification has been shown to be sensitive concerning the inclusion of regional specific trends (see Mazumder (2012)).

As mentioned before, we do not observe the treatment indicator for the individual, but only the share of schools with extended compulsory schooling within a region, g, for a specific cohort, c. Averaging Equation (4.1) over the observations in each gender-cohort-region cell produces:

$$d_{gcs}^{1935,x} = \beta_0 + \beta_1 \bar{Z}_{g,c+14} + \beta_2 \bar{X}_{gcs} + \delta_s + \mu_c + \nu_g + f_g(c) + \bar{\epsilon}_{gcs} \qquad (4.2)$$
$$g = 1, \ldots, G, \quad c = 1924, \ldots, 1931$$

The treatment is now given by the ratio of students affected by extended compulsory schooling, $\bar{Z}_{gs,c+14} = 1/N_{gcs}^{1935} \sum Z_{igs,c+14}$. In general, the constructed treatment share, $\widetilde{Z}_{g,c+14} \neq \bar{Z}_{gs,c+14}$, as $\widetilde{Z}_{g,c+14}$, does not distinguish between gender and weighs all school districts equally by construction. This creates a (possibly non-classical) measurement error in the instrument. In the following, we will carefully state the assumptions that are necessary for inference on the effects of the treatment, β_1.

Assuming (conditional) random assignment of extended compulsory schooling to school districts (or schools) within counties, the measurement error in our instrument has the expectation of zero. The estimates then represent a lower bound for β_1. This is unproblematic with respect to the gender aggregation. However, as we do not know the size of the school districts, non-random assignment with respect to the number of students could occur. Larger districts could, for example, introduce the extended minimum schooling first. If the state trends and the covariates insufficiently control for this possibility, the measurement error would not necessarily lead to an attenuation bias. Instead, the sign of the bias is unclear.

Most importantly, inference on individual behavior, as described by Equation (4.1), is achieved from aggregates. Therefore, we are subject of committing an *ecological fallacy*. The problems associated with inferring individual behavior from aggregates is well-known Robinson (1950) (in his seminal work, Robinson investigated the relationship between the ratio of foreigners and the literacy rate in U.S. states. The positive relationship on the aggregate level is a spurious correlation from foreigners living mostly in areas with higher literacy rates and disappears if confronted with individual data (Freedman (1999))). The problem occurs, as the (conditional) joint distribution of the mortality rates and the treatment is the aim of inference, while only the marginal distributions are observed. We do not know the exact combinations of mortality and schooling for each individual. This makes regressions on aggregate levels especially prone to spurious results.

Without imposing strong assumptions, it is not possible to draw conclusions about the joint distribution using only marginal distributions from aggregated data. Though this issue cannot be solved without individual level data, the reliability of the estimates can be nevertheless strengthened. Given that we use variation between cohorts within a county, we can reasonably correct, to some extent, for possible confounding. Prior studies indeed confirmed that the aggregation bias can be sufficiently relaxed in this way. For example, Lleras-Muney (2005) uses the same specification to model the effects of education on mortality rates. Comparing analysis from individual

survey data and aggregated data from official statistics, her results only differ slightly. This indicates the absence of a serious aggregation bias.

Note that we face only partial compliance, as not all students change their educational decision, due to an extension of the years of compulsory schooling. Therefore, Equation (4.1) can be interpreted as a reduced form for the effects of education on mortality. Under random assignment of the instrument and the absence of an aggregation bias, the estimation yields a lower bound for an intention to treat a parameter of compulsory schooling extensions. It is a lower bound, due to the measurement error in the share of districts treated relative to the number of students.

In Meghir and Palme (2005), for cohorts born between 1940 and 1957, the share of fathers having more than compulsory schooling can be found. These fathers could potentially be treated in our application. The proportion of men having more than compulsory schooling is less than 25% in their sample. This would indicate a large compliance rate for the reform. Without the reform, it is very likely that most of the individuals would have experienced less schooling. The estimates should, therefore, be close to the effects on those kept longer in school, only due to the extension of compulsory schooling.

4.5 Results and Discussion

Under valid assumptions, the estimates yield a conservative bound for the direct effect of the changes in compulsory schooling on mortality rates. Several x-year death-rates are regressed on the share of school districts within a county treated by an extended compulsory schooling. Table 4.3 presents the results of our main specification, which includes cohort dummies, county dummies and time trends (bottom part). Additional specifications have been included in Table 4.4 to assess the robustness of the baseline results.

All specifications indicate a reduction of mortality rates by extended minimum years of schooling. Adding possible confounders, such as state effects, the share of urban population within a county of the gender composition does not qualitatively change the result. However, with respect to the relevant baseline mortality, the magnitude of the effects appears rather large. Estimating the effects of the treatment on the 30-year death rate controlling for cohort and county dummies, as well as urban status and gender leads to a reduction of about 30% at the mean. Further, inserting linear trends even increases the effect size. Given the greater flexibility, we prefer the specification with linear trends. The additional variation explained is low. The effects from compulsory schooling are, nevertheless, for some death rates sensitive to the inclusion of the state-specific trends. Standard errors are clustered on the county-cohort level and are robust against heteroscedasticity – which is inherent in the linear probability model.

It should be noted that our results cannot be directly linked to the effects arising from increases in years of schooling, as we only observe compulsory schooling, but not education itself. Comparing our results to the reduced form estimates in Lleras-Muney (2005) using the same identification strategy,

but without measurement error in the instrument, the estimates for the 10-year death rates are of comparable size. However, in stark contrast, Lleras-Muney's 10-year death rates refer to a much older population. The baseline mortality is much larger for her sample, indicating much smaller *relative* effects.

TABLE 4.3: Estimation results: Main specification

DEATH RATE:	10 YEARS	20 YEARS	30 YEARS	40 YEARS	50 YEARS	60 YEARS	70 YEARS
NO TRENDS							
Treatment	−0.004**	−0.006**	−0.008**	−0.011***	−0.010**	−0.013	−0.028**
	(0.002)	(0.002)	(0.003)	(0.004)	(0.005)	(0.008)	(0.013)
Urban	−0.004	−0.015	−0.026	0.001	−0.009	−0.086	−0.034
	(0.012)	(0.018)	(0.021)	(0.028)	(0.051)	(0.083)	(0.111)
Male	0.002***	0.006***	0.012***	0.022***	0.051***	0.102***	0.164***
	(0.000)	(0.000)	(0.000)	(0.001)	(0.001)	(0.002)	(0.002)
R^2	0.659	0.734	0.827	0.894	0.933	0.951	0.973
LINEAR COUNTY TRENDS							
Treatment	−0.005	−0.014***	−0.025***	−0.023***	−0.029***	−0.016	−0.026
	(0.003)	(0.005)	(0.006)	(0.008)	(0.010)	(0.012)	(0.019)
Urban	−0.003	−0.010	−0.015	0.008	−0.001	−0.092	−0.012
	(0.012)	(0.019)	(0.022)	(0.030)	(0.054)	(0.086)	(0.115)
Male	0.002***	0.006***	0.012***	0.022***	0.051***	0.102***	0.164***
	(0.000)	(0.000)	(0.000)	(0.001)	(0.001)	(0.002)	(0.002)
R^2	0.682	0.760	0.848	0.901	0.936	0.954	0.975
LINEAR AND QUADRATIC COUNTY TRENDS							
Treatment	−0.008**	−0.020***	−0.028***	−0.023**	−0.034***	−0.024*	−0.019
	(0.004)	(0.005)	(0.008)	(0.009)	(0.011)	(0.013)	(0.021)
Urban	−0.005	−0.006	−0.014	−0.002	0.001	−0.099	−0.039
	(0.014)	(0.020)	(0.025)	(0.033)	(0.060)	(0.095)	(0.124)
Male	0.002***	0.006***	0.012***	0.022***	0.051***	0.102***	0.164***
	(0.000)	(0.000)	(0.000)	(0.001)	(0.001)	(0.002)	(0.002)
R^2	0.705	0.773	0.856	0.907	0.939	0.957	0.977
G	400	400	400	400	400	400	400
Baseline Mortality	0.007	0.017	0.026	0.048	0.097	0.193	0.373

Notes: Dependent variable: average number of deaths within x-years. Additional controls: cohort dummies, county dummies. Standard errors (in parentheses) are robust against heteroscedasticity and clustered on the cohort state level. G corresponds to the number of cells defined by gender, cohort and county of birth. All regressions are calculated using weights. Weights are given by the number of observations in each cell.
Significance levels: $*$ 0.10 $**$ 0.05 $***$ 0.01.

TABLE 4.4: Estimation results: Further specifications

SPECIFICATION:	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
DEATH RATE:											
10 YEARS	−0.001*	−0.004**	−0.004**	−0.004**	−0.004**	−0.006	−0.005	−0.005	−0.005	−0.007**	−0.007**
	(0.001)	(0.002)	(0.002)	(0.002)	(0.002)	(0.003)	(0.004)	(0.003)	(0.003)	(0.003)	(0.003)
20 YEARS	−0.003**	−0.006***	−0.006**	−0.006***	−0.006**	−0.016***	−0.014***	−0.015***	−0.014***	−0.016***	−0.016***
	(0.001)	(0.002)	(0.002)	(0.002)	(0.002)	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)	(0.004)
30 YEARS	−0.004***	−0.008***	−0.008**	−0.009***	−0.008**	−0.029***	−0.026***	−0.027***	−0.025***	−0.028***	−0.028***
	(0.002)	(0.003)	(0.003)	(0.003)	(0.003)	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)	(0.005)
40 YEARS	−0.006*	−0.012***	−0.011***	−0.012***	−0.011***	−0.030***	−0.023***	−0.027***	−0.023***	−0.026***	−0.026***
	(0.003)	(0.004)	(0.004)	(0.004)	(0.004)	(0.008)	(0.008)	(0.008)	(0.008)	(0.008)	(0.008)
50 YEARS	−0.009	−0.013**	−0.010**	−0.014**	−0.010**	−0.045***	−0.029***	−0.037***	−0.029***	−0.029***	−0.030***
	(0.008)	(0.005)	(0.005)	(0.006)	(0.005)	(0.011)	(0.010)	(0.011)	(0.010)	(0.010)	(0.010)
60 YEARS	−0.018	−0.017**	−0.012	−0.020**	−0.013	−0.052**	−0.018	−0.033*	−0.016	−0.022*	−0.022*
	(0.016)	(0.008)	(0.008)	(0.010)	(0.008)	(0.020)	(0.012)	(0.018)	(0.012)	(0.012)	(0.012)
70 YEARS	−0.036	−0.035**	−0.028**	−0.040**	−0.028**	−0.080**	−0.026	−0.054*	−0.026	−0.031	−0.029
	(0.028)	(0.014)	(0.013)	(0.016)	(0.013)	(0.034)	(0.020)	(0.032)	(0.019)	(0.019)	(0.018)
URBAN				✓	✓			✓	✓	✓	✓
MALE			✓		✓		✓		✓	✓	✓
COUNTY FE		✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
COHORT FE		✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
LINEAR COUNTY TRENDS						✓	✓	✓	✓	✓	✓
GENDER TREND										✓	
RESTRICTED SAMPLE											✓
G	400	400	400	400	400	400	400	400	400	400	368

Notes: Standard errors (in parentheses) are robust against heteroscedasticity and clustered on the cohort state level. G corresponds to the number of cells defined by gender, cohort and county of birth. All regressions are calculated using weights. Weights are given by the number of observations in each cell. The gender trend is a cohort gender interaction. Specification (11) drops *Stockholm Stad*, *Uppsala län* and *Stockholm län*.
Significance levels: $*$ 0.10 $**$ 0.05 $***$ 0.01.

In comparing the results, one has to keep in mind that the compliance rate in our case is much higher. If nobody in the population would experience more schooling than compulsory and the laws are effective, an extension by one year would ultimately lead to an increase in education of about one year. In Lleras-Muney (2005), compulsory schooling laws only raised education by about 0.045 years. Similarly, also Oreopoulos (2006) finds relatively small effects of about 10% on education by the extension of compulsory schooling for the U.S. This either means that the laws have been ineffective—or many individuals would have extended schooling anyway. Given the high rate of individuals only receiving compulsory schooling in our data, we assume that the effects of the years of education are much larger. If the extension of the compulsory schooling raises education by slightly less than one year, then the effects of the reduced form estimated here are more comparable with the instrumental variable (IV) estimates in Lleras-Muney (2005). The IV estimates of the effects of education are instead of comparable size: Lleras-Muney observes a relative reduction of 60% from a baseline mortality rate of 10%; we get a 57–71% reduction in 10-year mortality.

Results for the long-run death rates give more plausible effect sizes. They also appear more robust with respect to the inclusion of linear county trends. Given the high mean 70-year death rate, the compulsory schooling extensions reduces mortality risk at the mean of approximately 7% in the specification with trends. Different specifications listed in Table 4.4 give similar results. The results are sensitive to the inclusion of county-specific trends, which tend to increase the effect sizes. Differentiating the overall trend by gender did not alter the results. Furthermore, three counties have been dropped in the last specification. Due to a transition of parishes between the city of Stockholm, Stockholm County and Uppsala County in 1971, death rates are incorrectly measured. However, omitting the three did not alter the results.

In Table 4.5 and 4.6, we further investigated treatment effect heterogeneity by gender with a parsimonious parametrization and by splitting the sample. Neither interacting only the treatment and the constant by gender nor estimating the whole model separately for men and women changes the general results. Given that the separated model can be seen as a fully interacted regression, the decrease in precision of the estimates is due to the additional amount of parameters estimated.

As an additional robustness check, Table 4.7 shows the estimates for a logistic regression. Due to the binary structure of the occurrence of death for the individual, the model is inherently nonlinear. The same applies for the shares in the aggregated regressions, which are bounded between zero and one. Addressing the nonlinearity by estimating a logistic regression instead of a linear model, however, does not change the results. The precision of the estimates is very similar, and marginal effects do not differ to a large extent from the linear specification.

As regards the mechanisms that might drive the results, the data at hand do not allow for a very detailed analysis. However, two striking results which appear to be relatively robust are that (a) most of the effect appears already within the interval of 10–20 years—*i.e.*, between ages 14 and 31, and

(b) there are no striking differences between males and females, even if the effect seems to operate at slightly more advanced ages for females. It is thus useful to investigate the leading death causes for males and females in the relevant ages. According to death cause statistics from 1940 (Statistics Sweden, 1943), the leading death cause in the relevant age group was infectious disease for males and females alike. This death cause was responsible for 40% of deaths for males and more than 50% of deaths for females. The second most prevalent death cause was external causes, which account for 20–30% of male deaths and 6–7% of female deaths, and the majority of deaths within this category are due to accidents, in particular, drowning. Thus, it is indeed plausible that some of the effect is driven by compulsory schooling laws protecting young individuals from a risky working life. However, as was explained at the outset, the entry into physically demanding work was delayed by at most six months, due to the reform. Hence, it seems quite unlikely that this mechanism is the dominating one. Indeed, if the effect is related to occupational hazards, a more plausible mechanism appears to be a treatment-induced selection into physically less demanding jobs.

Given the relatively strong assumptions we had to impose, due to the data limitations, we tend to be very cautious in interpreting the results as clear evidence that compulsory schooling extensions in the beginning of the 20th century in Sweden reduced mortality risk. Given the direction of the results and their robustness to changes in the empirical setup, we nevertheless think that they can be seen as a first hint that effects do exist, even if the effect sizes might not be taken at face value from the aggregate regressions. Our findings should be corroborated with studies using district-level reform information and individual-level data.

TABLE 4.5: Estimation results: gender differences (Part I).

DEATH RATE:	10 YEARS	20 YEARS	30 YEARS	40 YEARS	50 YEARS	60 YEARS	70 YEARS
PANEL A: INTERACTION TREATMENT × MALE							
NO TRENDS:							
Treatment	−0.004**	−0.005**	−0.007**	−0.011***	−0.010*	−0.012	−0.031**
	(0.002)	(0.002)	(0.003)	(0.004)	(0.006)	(0.011)	(0.015)
Interaction	−0.000	−0.001	−0.002	0.000	−0.001	−0.003	0.007
	(0.001)	(0.001)	(0.002)	(0.002)	(0.005)	(0.009)	(0.010)
LINEAR COUNTY TRENDS:							
Treatment	−0.005	−0.013***	−0.024***	−0.023***	−0.028***	−0.014	−0.029
	(0.003)	(0.005)	(0.006)	(0.007)	(0.010)	(0.013)	(0.020)
Interaction	−0.000	−0.001	−0.002	−0.000	−0.001	−0.003	0.006
	(0.001)	(0.001)	(0.002)	(0.002)	(0.005)	(0.009)	(0.010)

Notes: Dependent variable: average number of deaths within x-years. Additional controls: cohort dummies, county dummies. Standard errors (in parentheses) are robust against heteroscedasticity and clustered on the cohort state level. G corresponds to the number of cells defined by gender, cohort and county of birth. All regressions are calculated using weights. Weights are given by the number of observations in each cell.
Significance levels: $*$ 0.10 $**$ 0.05 $***$ 0.01.

TABLE 4.6: Estimation results: gender differences (Part II).

DEATH RATE:	10 YEARS	20 YEARS	30 YEARS	40 YEARS	50 YEARS	60 YEARS	70 YEARS
PANEL B: MALES							
NO TRENDS:							
Treatment	−0.003	−0.008**	−0.011**	−0.013**	−0.013*	−0.010	−0.030
	(0.002)	(0.003)	(0.004)	(0.005)	(0.007)	(0.011)	(0.019)
LINEAR COUNTY TRENDS:							
Treatment	−0.010*	−0.024***	−0.029***	−0.020	−0.026*	−0.012	−0.040
	(0.005)	(0.007)	(0.010)	(0.013)	(0.015)	(0.021)	(0.028)
PANEL C: FEMALES							
NO TRENDS:							
Treatment	−0.004	−0.003	−0.005	−0.009*	−0.008	−0.016	−0.025*
	(0.002)	(0.003)	(0.004)	(0.005)	(0.006)	(0.010)	(0.013)
LINEAR COUNTY TRENDS:							
Treatment	0.002	−0.004	−0.021***	−0.028***	−0.032**	−0.019	−0.011
	(0.006)	(0.007)	(0.008)	(0.011)	(0.014)	(0.018)	(0.025)

Notes: Dependent variable: average number of deaths within x-years. Additional controls: cohort dummies, county dummies. Standard errors (in parentheses) are robust against heteroscedasticity and clustered on the cohort state level. G corresponds to the number of cells defined by gender, cohort and county of birth. All regressions are calculated using weights. Weights are given by the number of observations in each cell.
Significance levels: $*$ 0.10 $**$ 0.05 $***$ 0.01.

TABLE 4.7: Logistic regression: main specification.

DEATH RATE:	10 YEARS	20 YEARS	30 YEARS	40 YEARS	50 YEARS	60 YEARS	70 YEARS
NO TRENDS:							
Treatment	−0.584**	−0.354***	−0.334***	−0.278***	−0.167**	−0.115**	−0.142**
	(0.243)	(0.128)	(0.119)	(0.078)	(0.064)	(0.055)	(0.058)
Marginal effects	−0.004	−0.005	−0.008	−0.012	−0.014	−0.017	−0.031
LINEAR COUNTY TRENDS:							
Treatment	−0.473	−0.764**	−1.037***	−0.565***	−0.350***	−0.105	−0.121
	(0.586)	(0.300)	(0.239)	(0.177)	(0.121)	(0.079)	(0.085)
Marginal effects	−0.003	−0.012	−0.026	−0.025	−0.030	−0.016	−0.027
G	400	400	400	400	400	400	400

Notes: Dependent variable: logistic transformation of the average number of deaths within x-years. Additional controls: cohort dummies, county dummies. Standard errors (in parentheses) are robust against heteroscedasticity and clustered on the cohort state level. G corresponds to the number of cells defined by gender, cohort and county of birth. All regressions are calculated using weights. Weights are given by the number of observations in each cell.
Significance levels: $*$ 0.10 $**$ 0.05 $***$ 0.01.

4.6 Conclusion

Theoretically, there are several factors suggesting education will have a positive effect on health outcomes: education may improve an individual's economic situation, enabling health investments and health-related consumption, and education likely also affects health behavior by increasing knowledge or affecting time preferences. Furthermore, empirically, there is a well-known association between education and a wide range of health outcomes, but there is still disagreement with respect to the causal effects of education on health. To some extent, this ambiguity relates to the variety of health outcomes that have been used in previous analyses, but also to methodological challenges and poor identification strategies. Moreover, existing evidence mainly uses information on educational changes quite recent in time, implying that health outcomes are studied over a short time horizon.

We study the mortality effects of a nationwide Swedish reform, where compulsory schooling in primary school was extended by one year in 1936, from six to seven years of schooling. The reform was implemented at the school district level over a transition period of 12 years from 1936–1948. Taking advantage of the general variation in the timing of the implementation of the reform across school districts, we believe it is possible to identify health effects of the reform.

Using data from three different data sets: information on the number of school districts that had introduced seven years of schooling and from the national school authority, the census of 1935 covering all people in the country and the Swedish Death Index, we estimate the effect of the proportion of reformed districts on various crude death rates. Results clearly suggest a robust reduction of mortality rates by extended schooling. Baseline findings come out significant and rather large in magnitude, but controlling for cohort

and county dummies, urban/rural status and gender reduce the effect sizes. Considering long-run death rates also gives plausible effects.

Our results are quite similar to existing studies using similar methods to uncover the effects of schooling on health (see, e.g., (Lleras-Muney, 2005; Clark and Royer, 2013)). A recent study by Meghir, Palme and Simeonova (2012*b*) examines the mortality effect of a later expansion of compulsory schooling in Sweden in 1949–1962—a school reform not only extending the compulsory number of years in school, but also abolishing tracking in secondary school and vocational courses. They find that additional education reduces male morality up to the age of 50, but that mortality is elevated at later ages—a reversal effect we do not find when analyzing the seven-year compulsory school reform.

Indeed, our results suggest that increases in compulsory education have a large and persistent effect on survival prospects. For example, we estimated that the 50-year survival rate increased by 3.2% due to the reform. A simple back-of-the envelope calculation suggests that 0.8 life years were gained during this period. Thus, considering the fact that the reform only affected those who left school at the earliest possible age, the treatment effect on the treated may well have been an extension to their life expectancy, which goes beyond their extension of schooling. The causal effect of education and schooling might thus explain a large chunk of the difference in life expectancy between educational groups that was reported in the introductory Section.

In the time period studied, very few students continued to secondary schooling, and compliance with the reform was very high. The negative effect on mortality thus suggests that the true effect is partly mediated by the extra year of compulsory schooling itself, although our results rely on relatively strong assumptions, due to data limitations. Exactly what is mediated in the association is, however, difficult to assess. Importantly, however, the reform did keep the school system intact and did not imply any major changes in learning outcomes or curricula. The instructions to the school districts were to distribute the pre-reform compulsory school curricula in seven years instead of six. The major consequence with the reform was one further year of compulsory schooling.

If more education is causally linked to a reduced risk in premature mortality, this is an argument in favor of longer education for the individual. Our findings give a first indication that compulsory schooling reduced mortality risk. Future research should try to identify the mechanisms in this relationship. It would also be of great interest to separate between death causes to fully understand how education improves health outcomes.

Chapter 5

The Long-Term Impact of Education on Mortality and Health: Evidence from Sweden[1]

5.1 Introduction

The existence of an education gradient in health has been documented across various countries and for a variety of health measures including mortality, disability and various measures of morbidity (see e.g. Mackenbach et al., 2003, 2008; Marmot et al., 2012; O'Donnell, Van Doorslaer and Van Ourti, 2013) . A range of theories has been posited to explain the existence of this gradient including the suggestion that education has a causal effect through its impact on health production, or through impacting differences in financial resources, preferences or self-empowerment, understanding of information or better access to information (for an overview see e.g. Cutler and Lleras-Muney, 2006; Grossman, 2006; Mackenbach, 2012). To test these theories we need credible instruments. A substantive part of the more recent empirical literature has used differences in compulsory schooling as a source of exogenous variation for years of education. Two recent reviews of the literature on the impact of education on health (Cutler and Lleras-Muney, 2012; Grossman, 2015) have found the literature hard to summarise.

In this paper we address this lack of clarity in the literature looking at the causal effect of education on health. We do this by pinning down many of the potential reasons that have been given to explain the differences in the findings in the literature. Suggestions for differences in results across studies include estimation on different populations and for different periods (Cutler and Lleras-Muney, 2012) or that the instruments used affect different groups (Grossman, 2015; Clark and Royer, 2013). Clark and Royer (2013) arguably provide the most convincing evidence so far from Britain's compulsory schooling law changes using month of birth and Regression Discontinuity (RD) design and find small or zero impacts of education on health. However, the reforms were implemented overnight, nationwide. The reforms were also enacted during very different periods (1947 and 1972). It

[1]This Chapter was co-authored by Gawain Heckley, Ulf-G Gerdtham, Martin Karlsson, Gustav Kjellsson and Therese Nilsson.

may be that overnight implementation of the reforms reduced the wage returns to schooling because of the sudden change in supply of more educated workers, and this could impact a key potential channel for education to influence health. There is also cause for concern that the cohorts of 1947 and 1972 are not comparable. For example, Gathmann, Jürges and Reinhold (2015) provide indicative evidence that results based on pre WWII cohorts show larger impacts on mortality than results based on post WWII cohorts and this may be due to different base level mortality rates.

In this paper we consider the long-term impact of education on mortality and wider measures of health. We want to know whether the type of school reform has an impact on the results, holding other variables constant. To this end we use two school reforms in Sweden that increased years of schooling for cohorts born very close together in time but different in nature. The first reform increased minimum years of schooling from 7 years to 8 years but only for those who were not eligible or unwilling to take the academic track. This was rolled out to about half of Sweden's municipalities. The second reform increased minimum years of schooling from 7 or 8 years (depending on municipality) to 9 years and introduced a new national comprehensive school system involving a change in peer groups and the introduction of a new national curriculum. The 9 year reform was rolled out nationally but phased in across municipalities and has been found to have had a sizeable impact on years of schooling (Meghir and Palme, 2005; Holmlund, 2008; Lundborg, Nilsson and Rooth, 2014; Hjalmarsson, Holmlund and Lindquist, 2015). In fact by utilising a methodological improvement in the measurement of years of schooling we show that the 9 year reform had impacts on years of schooling twice that of previous estimates. This paper is the first paper to use the 8 year reform. It was rolled out extensively across Sweden and we show that it had a sizeable impact on years of education.

The particular set-up assessed in this paper is rather unique because the reforms are overlapping in the sense that they occur on average 7 years apart within municipalities. This means the reforms impacted individuals from similar backgrounds who entered similar labour market structures under a similar health system. That is, under similar labour market structures any returns to education should be similar for similarly aged cohorts. For each reform, the nature of their phased roll out means we can compare two groups born in the same year but who received different amounts of compulsory schooling based on where they were born. An advantage of this paper is therefore that we are able to pin down the impacts of the reforms separate of cohort effects. Also, because the reforms were rolled out over time, the impacts of the reforms are less likely to suffer the same level of general equilibrium effects that may be a concern if the reforms were rolled out nationwide overnight.

To help further isolate potential variables that explain differences across studies we employ both difference-in-differences (DiD) and RD design to identify the causal impact of the reforms on health. RD uses the cut-off for the school year, the 1st of January, combined with reform year for each municipality. Using birth date in years and months, those too old and therefore

born before the reform year cut-off are not assigned reform status and those born on or after are. There are as many cut-offs as there are years of reform implementation and we average over these to estimate the overall impact of the reform. Our DiD strategy compares across cohorts in municipalities that didn't implement the reform to those that did. To assess if modelling approach matters, we perform analysis of mortality using both linear regression and Cox proportional hazard regression for lifetime duration analysis.

Our data is based on the universe of Swedes born between 1932 and 1959. We link our various administrative records together using each individual's unique personal identification number enabling us to assess the mortality and health outcomes of about 1.2 million individuals. We consider the reform status of individuals born between 1938 and 1954. Mortality data is provided by the Swedish Cause of Death Database and our observation period follows our individuals up to the end of 2013 which means our oldest cohort born in 1938 and subject to reform is 75 years old when our period of observation ends. We also consider leading causes of death. We complement this analysis using Swedish hospital administrative data and a large survey (The Swedish Health and Living Standards Survey) considering self-reported health and health related behaviours. The survey data allows us to consider both contemporaneous health and health related behaviours and therefore gives us the potential to pick up effects using a more sensitive measure of health and also explain the pathways of any effects we find. Compared to previous Swedish studies which only consider the 9 year reform using DiD (Spasojevic, 2010; Lager and Torssander, 2012), the data we use has a much longer follow-up with more up to date data, more outcome variables and a larger sample size and we analyse this data using RD in addition to DiD. Meghir, Palme and Simeonova (2017) also make the same contributions as we do over and above those of prior Swedish studies. In comparison to Meghir, Palme and Simeonova (2017), our major contribution is that we introduce a new reform which has the novelty of allowing us to perform instrumental variable analysis of the causal effect of education on health, compare two different types of school reform across over-lapping cohorts. Further, we utilise a better measure of schooling that captures the full effect of the reform which dramatically changes the interpretation of the 9 year reform and we introduce new survey data measuring self-reported health and health behaviours.

In the following section we describe the two Swedish compulsory schooling reforms. In section 5.3 we introduce the data and in section 5.4 we outline our empirical strategy. In section 5.5 we present the results. Our findings show that there was a sizeable increase in years of education due to both reforms but these increases did not lead to an improvement in life expectancy or health. The results are robust to reform, modelling choice, identification strategy and health outcome. We then discuss the results in section 5.6 and argue that the results are not only robust internally, they also have validity beyond the Swedish context. Finally, we conclude in section 5.7.

5.2 The Swedish compulsory schooling reforms

During the 1950s and 1960s in Sweden, a large number of municipalities raised the minimum years of compulsory schooling from 7 to 8 years gradually over a period between 1941 to 1962 (affecting birth cohorts born between 1927 to 1948). This is illustrated in figure 5.1 (See "Old primary school 8-Year"). We call this the 8 year reform. From 1948 to 1969 municipalities then gradually replaced the old system with a new comprehensive school system that also raised the minimum years of schooling to 9 years (affecting birth cohorts born between 1938 and 1958). Again, this is illustrated in figure 5.1 (See "New Compulsory school"). We call this the 9 year reform. This section provides some background information on these reforms, leaving a more detailed description to Appendix A.

Prior to both reforms, students attended a common compulsory primary school (*Folkskola*) up to and including the 6th grade. After the sixth grade, good performing students (defined by an assessment) had the option to switch to an academic educational track and study at a three year junior secondary school (*Realskola*).[2] Those who continued on at primary school studied up to seventh grade. Attending junior secondary school allowed students to continue to higher secondary school (*Gymnasium*) and later university, an option denied to those who stayed on at primary school. Only a small minority went on to junior secondary school, the vast majority of students stayed on and completed compulsory education at primary school.[3]

The 8 year reform was a simple extension of the minimum years of schooling within the municipality for those students who did not choose to go to junior secondary school. The reform was seen as an opportunity to give more time for the students to learn without any specific changes to the curriculum. It is therefore a credible instrument for years of education. The 9 year reform introduced the Swedish comprehensive school system and was very different in character to the 8 year reform, as not only did it increase the minimum years of schooling within the municipality to 9 years (from 7 or 8 years) it also postponed tracking of students with the aim of fostering greater equality of opportunity (Holmlund, 2008). The reform also introduced a new national curriculum. The removal of early tracking is likely to have broadened the peer group mix in the new comprehensive school system as higher ability students who would have gone to junior secondary school now shared the same class as their lower ability peers for longer compared to students under the old system. The curriculum changes may well have led to quality changes in schooling.

Both reforms were rolled out at municipality level over time and this phased roll out was at the discretion of the municipalities. The number of municipalities who had implemented an 8th compulsory year gradually increased from 33 in 1946/47 to 207 in 1958/59. The early birds in this development tended to be more urban and included most of the larger cities. Smaller municipalities followed and in the end more than half of Sweden's

[2]Students could also start junior school after the 4th grade and study a four year track.

[3]For the cohorts we consider the share was 16% (cohort 1938) and 30% (cohort 1951).

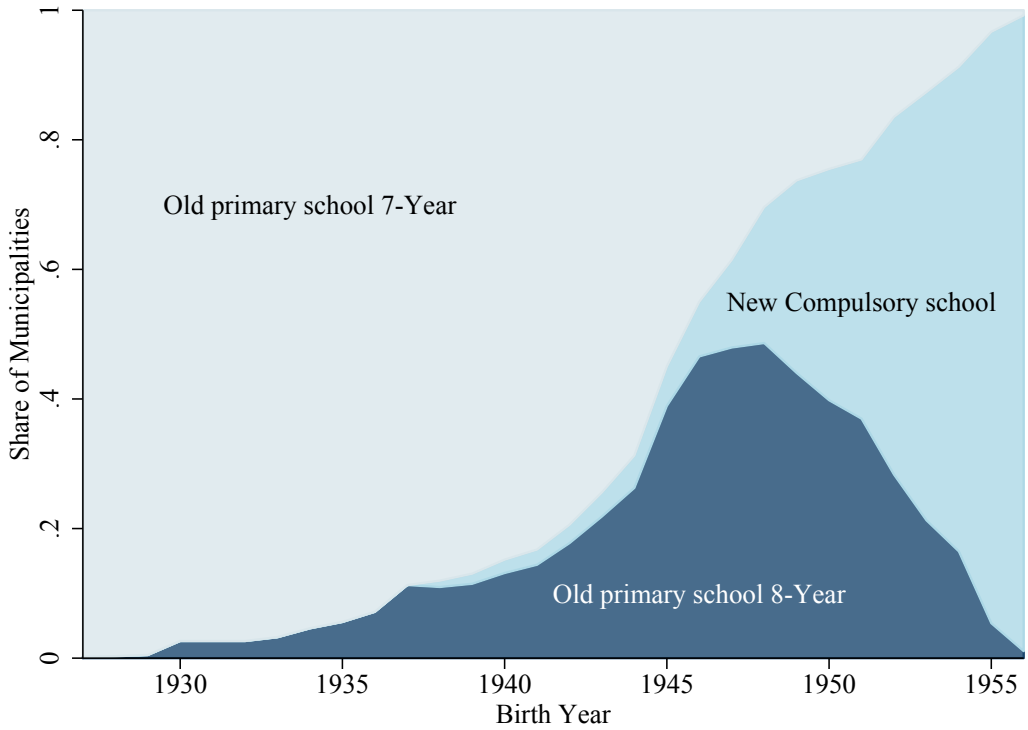


FIGURE 5.1: Share of municipalities by length of compulsory education

Notes: This figure shows the proportion of municipalities in Sweden who have the 7 year old primary school system, the 8 year old primary school system and the new 9 year compulsory school system by birth cohort.

more than 1,000 municipalities had introduced a mandatory 8th grade before implementing the comprehensive 9 year school reform. The previous literature referred to the 8 year extension as a rare phenomenon mainly occurring in the largest cities (Holmlund, 2008). In fact the majority of municipalities had rolled out a compulsory schooling length of eight years before the 9 year comprehensive school was introduced. Figure 5.1 illustrates this development.[4] Both reforms were rolled out in such a way that within the same school older students were under the old regime and younger students were under the reformed compulsory schooling regime. For the 8 year reform this simply meant that younger cohorts studied a year longer than older cohorts. For the 9 year reform this meant two curricula were being taught within the same school.

The roll out of the reforms was not random. The 8 year reform roll out was chosen by the municipalities themselves, both whether or not to implement

[4]In the early years of the introduction of the 9 year comprehensive school reform (between 1949 and 1962), the reform was introduced as a *social* experiment in certain areas (Marklund, 1982). The National School Board chose the areas from a group of applicants to form a representative set based on observable municipality characteristics (Holmlund, 2008). In 1962 the Swedish parliament finally decided that all municipalities should be obliged to offer the new comprehensive school system and by 1969 all municipalities were to have the new system in place.

at all and the timing. The 9 year reform in its early phase was introduced with explicit intention to evaluate the policy reform. The process became less strict later on. In section 5.4 we set out how we control for this to identify the exogenous variation in schooling we are after.

5.3 Data

To quantify the impact of the two education reforms on health outcomes we employ both population based administrative data and survey data. The full population administrative data is drawn from the Swedish Interdisciplinary Panel (SIP)[5]. We consider the universe of those born in Sweden between 1932 and 1959, who survived to 16 years of age, and had not emigrated from Sweden by 2012.[6] These cohorts consist of 2,789,494 individuals.

To identify individuals as exposed or unexposed to the reforms we assign treatment status based on year of birth and place of residence. Information on the timing of the 8 year reform in each municipality was gathered from the Swedish National Archives. For the introduction of the 9 year reform we rely on a dataset as used in Hjalmarsson, Holmlund and Lindquist (2015), of which an earlier version is described in detail in Holmlund (2008). Information on municipality of residence which we use to infer reform assignment is obtained from the 1960 and 1965 censuses.[7] In the empirical analysis we consider the reform status of cohorts born between 1938 and 1954 and use cohorts born before and after only as control groups. Our endpoint for the analysis, the cohort born in 1954, is chosen because this is the last cohort questioned in the 1970 census for which we have enough years to measure

[5]This is based upon Statistics Sweden's Multiple Generation dataset to which all other datasets are then linked using personal identifiers. It is administered at the Centre for Economic Demography, Lund University, Sweden. The present study was approved by the *Lund University Regional Ethics Committee, DNR 2013/288*.

[6]Very limited intergenerational information is available before 1932 explaining our chosen start point

[7]For cohorts born between 1943 and 1948, we assume that place of residence in 1960 is also the municipality where they went to school. For cohorts born on or after 1949 we follow the suggestion of Holmlund (2008) and use the place of residence as recorded in the 1965 census. For those born before 1943 we use place of residence of the mother (father if information is missing for the mother and if both parents are missing we use own place of residence in 1960) as recorded in the 1960 census. An alternative approach would be to use the *place of birth* as an approximation of place of residence during childhood. In theory this approach has a nice intention to treat interpretation and avoids being susceptible to potential parental choice of reform assignment, itself linked to the child's ability. However, for the cohorts we consider births were increasingly occurring at a hospital and before 1947 the hospital was recorded as the place of birth which often did not coincide with the place of residence of the parents. Up to 1947 therefore, the place of birth becomes increasingly uninformative as a measure of place of residence for the child at birth. From 1947 this practice changed and the place of birth of the child was recorded as the place of residence of the mother (Holmlund, 2008). In figure D.1 in the appendix we show that the misclassification from hospital recording is sizeable. As near half of our cohorts are born before 1947 and Meghir and Palme (2005) provide evidence that inter-municipality migration is very small (and therefore parental response to the reforms is small if there is one at all), we do not follow this approach.

their years of schooling. Both reforms were rolled out in different parts at different times within the cities of Stockholm, Gothenburg and Malmo and therefore we exclude those resident in these cities. From the original sample we have reform assignment for both reforms for 2,108,696 individuals.

Data on schooling is obtained from the 1970 census and this is combined with post schooling attainments from the Education administrative database. We derive a measure of years of education by assigning the years typically associated with different types of schooling (from the census in 1970) and post-schooling qualifications from Education administrative database and take the sum as an approximation for the total years of education. This approach is an important innovation in how to measure years of education in Sweden as it allows us to capture the effect of the 8 year reform on years of education and also better captures the effect of the 9 year reform.

Previous Swedish population administration data based studies have approximated years of education by the average length associated with the highest educational qualification (Hjalmarsson, Holmlund and Lindquist, 2015; Lager and Torssander, 2012; Lundborg, Nilsson and Rooth, 2014). We cannot use this approach for the 8 year reform because there is no information on whether an individual went to 7 year or 8 year primary school. These are clumped together in the same category in the variable capturing highest educational qualification. Using just administrative based information on highest qualification also means the impact of the 9 year reform is underreported as this approach cannot distinguish between someone who attained more than compulsory schooling but had different amounts of compulsory schooling e.g. two people who received vocational training but received different amounts of compulsory schooling would be given the same number of years of education using just the information from the administrative data on highest educational qualification. Our new method captures the impact of the 9 year reform more accurately as it distinguishes schooling and post schooling achievements when calculating years of schooling.[8] How we construct our years of education variable is a key contribution of the paper, as it not only allows us to capture the impact of the 8 year reform, it also improves the measurement of the impact of the 8 year reform as we shall show later.[9] A detailed description on the construction of the different education measures and its impact on the empirical analysis of compulsory schooling reforms is

[8]We still have downward measurement error for cohorts born after 1951 because the years of schooling measure in the census measures schooling up to age 19. For cohorts born 1952-1954 we impute years of schooling using the modal years of schooling measured in the census corresponding to each level of further education achieved as measured in the Education administrative data from earlier cohorts (1948-1950). For cohorts after 1954 we assign 7 years of schooling for those with post schooling Swedish Education Nomenclature 2000 (SUN2000) classification of less than 200 and 9 years with a SUN2000 classification of 200-300. This adjusts for the downward bias of measuring individuals' schooling before they have reached 19 but will still underestimate the reform effect.

[9]This is particularly important for interpretation of prior studies that used the 9 year reform as an Instrumental Variable for years of schooling as they will have biased their causal estimates upwards using years of education based on highest qualification from the Education administrative database.

given in C.3. Our final sample size for those we also have information on years of education is 2,022,174.

Data on cause of death and cause of inpatient care at hospital is obtained from the Swedish Cause of Death database and the Hospital Patient database (stays at a hospital of more than 24 hours) and merged using a personal identifier to our main dataset. The mortality data covers the years from 1964 and cause specific information is from 1969 and runs up to 2013. We consider the whole observation period and therefore measure the impact on death up to age 75 for the oldest cohort.[10] The inpatient data covers the years 1987 up to 2012. We therefore consider the impact on hospitalisations up to age 74. Underlying cause in both datasets is recorded according to the 7th, 8th, 9th and 10th versions of the International Classification of Diseases (ICD) depending on year of death/hospital admission. The data also includes date of death, date of hospital visit (admission and discharge) and length of hospital visit. We consider the most common causes of death and hospital visits and also the number of hospital days from inpatient records (see appendix D.1 table D.1 for variable ICD codings). In table 5.1 we present the means of our administrative data based outcomes variables by reform and whether treated or untreated. We include only those born within 10 years of reform implementation as this is the population we use in our main analysis. We observe that on average for our 8 year reform sample the untreated have 9.4 years of schooling, 19% have died by 2013 and predominantly of cancer and have spent 30 days in hospital whereas the treated population have 10.9 years of schooling, only 9% have died by 2013 and have spent 21 days in hospital. For the 9 year reform sample on average the untreated are again less educated, more have died by 2013 and are more likely to have had a hospital visit and for longer compared to those treated. For both reforms, the treated are younger and this explains a large part of the differences in health outcomes between our treated and untreated groups. We control for this in our analysis.

[10] We consider death by 2013 so that we capture as much data as possible. We could consider death by a certain age, but then we would lose a lot of information given most deaths are for older ages.

TABLE 5.1: Descriptive statistics - administrative data

VARIABLE:	8 YEAR REFORM		9 YEAR REFORM	
	Untreated	*Treated*	*Untreated*	*Treated*
Years of education	9.4	10.9	10.1	11.5
Dead	0.199	0.090	0.128	0.063
Proportion dead due to:				
Cancer	0.080	0.035	0.050	0.021
Circulatory Disease	0.050	0.019	0.029	0.012
External Causes	0.010	0.011	0.011	0.012
Other	0.059	0.026	0.038	0.019
Days at hospital	29.7	21.3	24.0	20.3
Proportion who had a hospital visit due to:				
Cancer	0.111	0.066	0.084	0.051
Circulatory Disease	0.156	0.083	0.113	0.062
External Causes	0.121	0.092	0.106	0.092
Other	0.313	0.269	0.284	0.293
OBSERVATIONS	215,846	318,557	640,093	607,715

Notes: This table shows the means for education and health outcomes for those treated and not treated by each reform and born within 10 years of the first cohort impacted by the reform.
Source: SIP. Own calculations.

The survey data stems from the Swedish survey on living standards (ULF) (Statistics Sweden, 2008).[11] The survey includes self-reported health and health behaviour variables which we consider as valuable complements to population based administrative data on cause of death. We consider binary indicators for smoking behaviour, obesity, self-reported anxiety or worry and self-reported fair or bad health (in contrast to good).[12] In table 5.2 we present the means of our education and health outcomes. The years of education means correspond to those for the administrative data in table 5.1 which suggests the sampling frame of the survey data does well to represent the population estimate. Whilst self-reported *fair or bad health* and obesity are more common amongst the untreated samples, *smoke daily* and *anxiety, concern* are actually more common amongst the treated population. The survey itself is carried out by face-to-face interviews of a randomly selected sample of the population. The sample size is about 7,500 individuals per year and data is reported for years 1980 through to 2012. We therefore have 32 years of data. Information from different Swedish censuses and on education attainment from the Education administrative data is linked to individuals in the survey.

[11] The ULF survey is a well respected survey used for a wide range of research topics and in recent years has formed the Swedish part of the European Union Statistics on Income and Living Conditions (EU-SILC).

[12] We define a binary variable *bad or fair health* equal to one if self-reported health is reported as fair or poor. Smoke Daily is a binary indicator, indicating one if smoked daily in the past 30 days prior to interview, zero otherwise. Anxiety is a binary variable, one indicating whether the individual self-reported having heightened anxiety, concern or worry, zero otherwise. Obese is a binary indicator derived from information on height and weight creating a Body Mass Index (BMI), one indicating a BMI of 30 or more, zero otherwise.

TABLE 5.2: Descriptive statistics - survey data

VARIABLE:	8 YEAR REFORM		9 YEAR REFORM	
	Untreated	*Treated*	*Untreated*	*Treated*
Years of education	9.5	10.9	10.2	11.5
N	3,360	4,840	9,847	9,306
Fair or bad health	0.261	0.183	0.213	0.166
N	3,349	4,832	9,830	9,294
Smoke daily	0.226	0.276	0.255	0.268
N	3,325	4,813	9,776	9,257
Obese	0.129	0.090	0.105	0.083
N	2,139	2,857	6,004	5,510
Anxiety, concern etc	0.149	0.150	0.146	0.149
N	2,256	3,132	6,437	6,050

Notes: This table shows the means for education and health outcomes for those treated and not treated by each reform and born within 10 years of the first cohort impacted by the reform.
Source: ULF-Survey. Own calculations.

5.4 Empirical Strategy

5.4.1 Identifying the impact of the reforms

In this section we outline the two empirical identification strategies we use to identify the impact of the reforms on health outcomes: DiD and RD design. The purpose of using two identification strategies is that it provides a sense of how robust the findings are. The two methods rely on different identifying assumptions to identify the causal impact of the reforms and potentially estimate the impact for different populations of compliers. We are therefore able to assess if this is important to the conclusions we draw.

The impacts of the reforms on education, mortality, hospitalisations and self-reported health outcomes are modelled in a linear setting, using either OLS or a Linear Probability Model (LPM) depending on the outcome variable, as is standard for both DiD and RD. Our DiD empirical strategy utilises the fact that the education reforms were introduced slowly over time across municipalities in Sweden. Two individuals born in the same year but one resident in a reform municipality and the other not have different exposures to compulsory schooling. This provides us with variation in reform exposure both over time and across municipalities. However, the implementation was not random as discussed in Holmlund (2008). To control for this we difference across municipalities and across birth cohorts using dummy variables for both. Our linear DiD model is:

$$H_{i,c,m} = \beta_0^{DiD} + \beta_1^{DiD} Z_{c,m} + \beta_2^{DiD} C + \beta_3^{DiD} M + \beta_4^{DiD} trend_m + \epsilon_{i,c,m}; \quad (5.1)$$

where i indicates an individual, c the birth cohort, m the municipality, and $Z_{c,m}$ is a variable equal to one for individuals assigned as exposed to the reform using their date of birth and place of residence, zero otherwise. $H_{i,c,m}$

is our outcome of interest, C is a vector of birth year cohort dummies, M is a vector of municipality dummies, *trend* is a vector of municipality specific trends and β_0^{DiD} is a constant term. The coefficient β_1^{DiD} measures the impact of the reforms on our outcome measures. We estimate equation (5.1) separately for each reform.

The empirical strategy utilising RD design involves identifying the reform effect within municipalities based on the year the school reform was introduced and the cut-off date for the school year, which is the 1st of January. Individuals born before the reform year cut-off are not assigned as exposed to the reform and those born on or after the cut-off are assigned as exposed. The forcing variable, $T_{i,m}$, is birth date measured in years and months from the reform cut-off date in their municipality. Our linear RD model takes the form:

$$H_{i,c,m} = \beta_0^{RD} + \beta_1^{RD} Z_{c,m} + f(T_{i,m}) + \mu_{i,c,m}; \tag{5.2}$$

where i, c, m, $Z_{c,m}$ are as for equation (5.1). The coefficient β_1^{RD} captures the average impact of the reforms across all of the municipality level cut-offs. The identifying assumption is that the outcome variable is a smooth function of our forcing variable and that after adequately modelling this function, $f(T_i)$, any jump found at the cut-off is due to the education reform and not some other unobserved variable. To capture the birth cohort relationship with the outcome variable, we model $f(T_i)$ using a polynomial in years-months from the cut-off, T_i, estimated separately either side of the cut-off combined with dummies for gender and month of birth, to control for seasonality effects.[13] To choose our preferred function for T_i we followed the approach of Imbens and Lemieux (2008) and included progressively higher order polynomials in T until the additional polynomials were insignificant. At the same time care was taken to not over-fit the model, a concern raised by Gelman and Imbens (2017). We found that a second order polynomial was sufficient for all outcomes.

It is potentially of concern that we include individuals much older or younger than the first cohorts impacted by the reform within a municipality. To deal with this we use a bandwidth of up to 10 years for both our DiD and RD regressions so that only those born up to 10 years before or after the first cohort impacted by the reform are included in the analysis.[14] We also test the sensitivity of bandwidth choice and whether municipality level trends are important.

We consider mortality as our main health outcome. In addition to modelling mortality using an LPM we also consider time until death during the observation period using a Cox proportional hazard model (Cox and Oakes, 1984) together with our DiD and RD identification strategies. In this way, we model the conditional probability of dying in the next period given survival

[13]For analysis of the survey data we also include survey year dummies to control for age and year effects.

[14]We say up to ten years because for the earliest cohort we only have cohorts up to 6 years older to compare to and for later cohorts we only have cohorts up to 5 years younger to compare to.

to the current period. By considering the survival nature of our data we use more information, potentially increasing efficiency. It also allows us to deal with censoring because of survival beyond the sample period of 2013 and is also a natural choice when considering causes of death. If a particular cause of death is reduced by the reforms, by construction this means other causes of death will be increased for a given level of mortality. Cox models, under the independent competing risks assumption, deal with this. In our application of the Cox model we estimate duration until death, d, and using DiD we stratify on municipality of residence and include dummies for birth cohort which gives us:

$$I_{1,i,c,m}(d|X) = I_{0,m}(d)exp[\delta_0^{DiD} + \delta_1^{DiD} Z_{c,m} + \delta_2^{DiD}\gamma_c + \delta_3^{DiD} trend_z]; \quad (5.3)$$

where $I_{0,m}$ is the baseline hazard stratified by municipality, cohort specific fixed effects are given by γ_c, time trends for municipalities that roll out the reform in the same year are given by $trend_z$, and subscripts i, c and m, and $Z_{c,m}$ are as for equation (5.1).[15] δ_1^{DiD} is the reform impact on mortality. For our RD design, the Cox model takes the form:

$$I_{1,i,c,m}(d|X) = I_0(d)exp[\delta_0^{RD} + \delta_1^{RD} Z_{c,m} + f(T_{i,m})]; \quad (5.4)$$

where I_0 is the baseline hazard and subscripts i, c and m, and variables $Z_{c,m}$ and $f(T_{i,m})$ are as for equation (5.2). The coefficient δ_1^{RD} is the impact of the reform on mortality within each municipality averaged over all municipalities.

All of the models we have outlined above are reduced form models. We also apply Two Stage Least Squares (2SLS) using our linear equations (5.1) and (5.2) as the first stages with years of education YE in place of H as the dependent variable and instrumented with reform status. Our second stages are:

$$H_{i,c,m} = \alpha_0^{DiD} + \alpha_1^{DiD}\widehat{YE}_{i,c,m} + \alpha_2^{DiD} C + \alpha_3^{DiD} M + \alpha_4^{DiD} trend_m + v_{i,c,m}; \quad (5.5)$$

$$H_{i,c,m} = \alpha_0^{RD} + \alpha_1^{RD}\widehat{YE}_{i,c,m} + f(T_{i,m}) + u_{i,c,m}; \quad (5.6)$$

where α_1^{DiD} and α_1^{RD} are the coefficients on years of education and are our coefficients of interest. Both α_1^{DiD} and α_1^{RD} are identified by the variation in years of education that comes from the variation generated by the school reforms.

To identify our 2SLS coefficients we need to assume that the reforms affected our health outcomes only via their effects on years of education (the exclusion restriction) and that reform exposure is as good as random given

[15] For estimation to be feasible we have to limit the municipality trends estimation to municipalities that roll out the reform in the same year rather than model trends for each individual municipality.

our control strategies. The exclusion restriction would be violated if the reforms had other impacts on students over and above their impact on years of education that then impacted on health. For the 8 year reform the education system remained the same and so did the curriculum. However, for the 9 year reform, in addition to the increase in years of compulsory schooling, both the curriculum and the school system were changed. Prior to the 9 year reform, students were selected into different schools based on their academic ability. The 9 year reform abolished this and instead, students were kept in the same school and classes until the ninth grade.[16] Tracking has been found to impact educational achievement and later life outcomes (Betts et al., 2011) suggesting that its removal may have had an impact on educational quality. The removal of tracking will also have changed the peer group mix that students were exposed to, potentially impacting learning, health related behaviours and even assortive mating. Peer effects have been found for health outcomes and health related behaviours such as drinking, smoking and drug use (for an overview on peer effects see e.g. Sacerdote et al., 2011). In addition to the change in tracking, the 9 year reform coincided with the introduction of a new national curriculum. Although we have no evidence of the impact on quality this change made, it seems reasonable to assume that it had some impact on the variation in quality of schooling across the municipalities.

In order to use the 9 year reform as an IV we have to assume that both the changes to the tracking system and the introduction of a national curriculum had no impact on schooling quality or peer effects that could in turn impact our health outcomes. A number of articles have made this claim (Spasojevic, 2010; Lundborg, Nilsson and Rooth, 2014) whilst others view this as controversial and focus on estimating the reduced form effect of the reforms (Meghir and Palme, 2005; Meghir, Palme and Simeonova, 2017). In this paper we take the latter view but present IV estimates based on the 9 year reform as a way of quantitative comparison to the 8 year reform, a reform which we argue more convincingly meets the exclusion restriction requirements.

5.4.2 First stage results and diagnostic tests

Both of our identification strategies build upon our method of treatment status assignment performing well. In addition to this and the exclusion restriction, our 2SLS estimates require our education reform impacts on schooling to be as good as random given our control strategies. For our DiD estimates our control strategy hinges on the assumption that conditional on birth cohort and municipality fixed effects, exposure to treatment is as good as random. For our RD estimates our control strategy hinges on the assumption that conditional on our modelling of age relative to the first cohort within the municipality impacted by the reform, there are no jumps in the error term at the cut-off. In this case, any jumps we do find in years of schooling at the cut-off can then be assumed to be as good as random. In this section we establish

[16]There were some exceptions, where tracking was used for some subjects but overall students were much more mixed.

the existence of a first stage and provide some diagnostic tests to assess the plausibility of our identification assumptions.

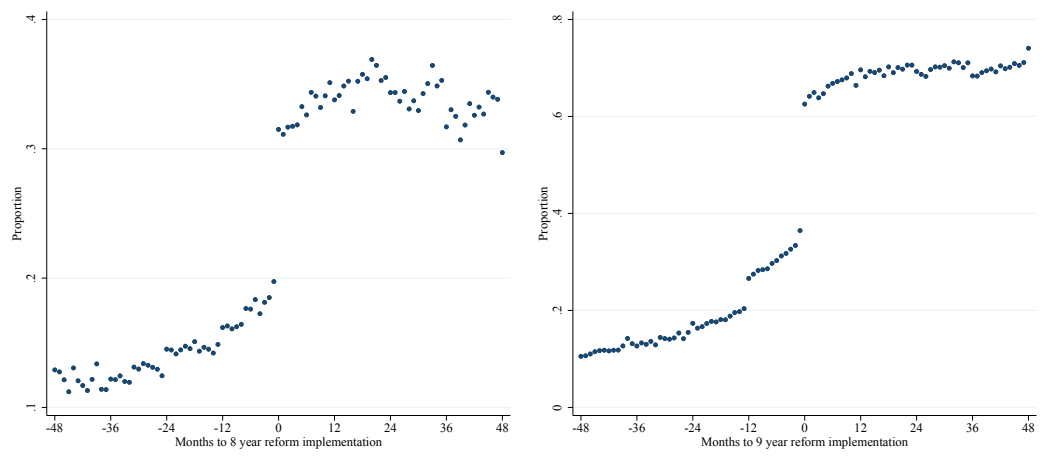


FIGURE 5.2: Effect of the reforms on the proportion with the new minimum years of schooling

Notes: Scatter plots of the proportion with the new minimum years of schooling by age in months measured as months to reform implementation in their municipality. Left panel is for the 8 year reform, right panel the 9 year reform. Reform implementation is at time zero.
Source: SIP. Own calculations.

In figure 5.2 we present the raw data of the probability of having achieved 8 years of old primary schooling (left hand side panel) and 9 years of schooling (right hand side panel) against birth cohort measured in months relative to the first cohort impacted by each reform respectively. For each monthly bin the proportion with 8 years/9 years of schooling is plotted. We see that there is an increasing trend with time until exposure and that at the cut-off there are clear jumps in the proportion with the new minimum years of schooling. Note that it is entirely expected that a proportion of students have the new compulsory schooling before reform implementation. Students who repeat a grade would naturally receive an extra year of schooling. For both reforms it is also documented that there was partial roll out that was non-mandatory prior to the reform becoming mandatory. We also see a jump in the proportion with the new compulsory years of schooling in period t-1 and this is much clearer for the 9 year reform. Hjalmarsson, Holmlund and Lindquist (2015) suggest that the pre-reform increase in schooling is due to either measurement error in the exposure variable or due to pupils being in the wrong grade based on their age due to choosing to repeat a grade. Hjalmarsson, Holmlund and Lindquist (2015) cite evidence that grade repetition was not a common occurrence for those in the old 7 year primary school system but grade repetition and dropping out was for those who were tracked into the junior secondary school. Those at junior secondary school who were born a year too early but had dropped out would have normally gone back to old primary school, but because of the reform they would have instead been caught by the 9 year school reform and would as a consequence be a year older than their peers in the same class. This last explanation fits with what

we see in the data. There is possibly a small jump in t-1 for the 8 year reform and this fits with the reported observation that grade repetition was not very common in the old primary school system. There is however a clear jump in t-1 for the 9 year reform, and this is quite likely in part driven by dropouts from junior secondary school.[17]

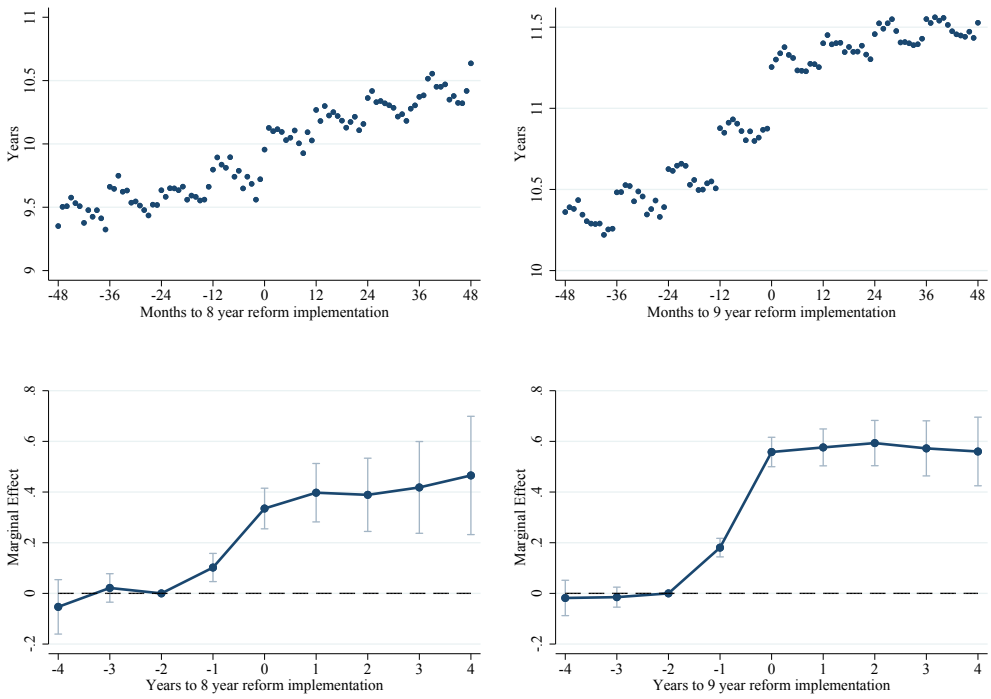


FIGURE 5.3: Effect of the reforms on years of education

Notes: Top two panels: Scatter plots of mean years of schooling by age in months measured as months to reform implementation in their municipality where the first cohort impacted is at zero. The bottom two panels: plot regression coefficients of an individual's birth year relative to the first reform cohort in their municipality on years of education (spikes represent the 95% confidence interval for each coefficient estimate). Municipality and birth year fixed effects and municipality level time trends are controlled for, a bandwidth of 10 years is used and clustered standard errors are estimated at the municipality level. Category 4 is four or more years after the first reform cohort. The reference category is *two years before the first reform cohort* (t-2).
Source: SIP. Own calculations.

[17]In the appendix we also show figures equivalent to figure 5.3 but for the proportion with just 7 years of schooling, just 8 years of schooling and just 9 years of schooling. These all confirm the jump at the cut-off. They also confirm the pre-reform jump in t-1. The jump in t-1 for the 9 year reform coincides with a clear drop in 8 years of old primary school in t-1 suggesting it is driven by individuals dropping out of junior secondary school after one year in municipalities who had introduced the 8 year reform. We can also see that there is measurement error in the exposure variable as after the 8 year reform there are still some with 7 years schooling. Similarly for the 9 year reform there is still a proportion with old primary school after reform exposure. This is partly explained by partial implementation in the municipality where exposure is given as 1 if just part of the municipality enacted the reform.

In figure 5.3 the top two panels present the raw data in a similar fashion to that of figure 5.2, this time with years of education on the y-axis. The bottom two panels of figure 5.3 are event study graphs from our DiD regressions and show the conditional marginal effect and the corresponding 95% confidence interval for each year cohort relative to the first cohort impacted by the reform (t=0) on years of education. The reference cohort is the cohort born two years before the first treated birth cohort. The estimates are from a regression controlling for municipality and birth cohort fixed effects and standard errors are clustered at the municipality level. From figure 5.3 we can see a jump in the average years of schooling due to both reforms and that this jump is larger for the 9 year reform.

Table 5.3 presents the regression results of the impact of the reforms on years of education for all individuals, and also split by gender. Column (1) in table 5.3 presents the results for the 8 year reform on years of education for all individuals and we find an increase of 0.23 years of schooling using RD and 0.27 years of schooling using DiD. Column (4) presents the results for the 8 year reform for all individuals and we find an impact of 0.39 years using RD and 0.53 years using DiD. Note that the 9 year reform estimates presented here are much larger than previously documented (see e.g. Holmlund 2008; Lundborg, Nilsson and Rooth 2014; Meghir, Palme and Simeonova 2017) and the reason is because we use a different measure of years of education which better captures the impact of increased compulsory schooling on years of education. Indeed, using a schooling measure calculated in the same way as Holmlund (2008) we find that the impact of the 9 year reform on years of schooling is 0.3 years using DiD and 0.25 years using RD, both much smaller than our preferred estimates presented in table 5.3 (see model (7) of table D.3 in the appendix for the administrative data based education variable results).

TABLE 5.3: Compulsory schooling reforms' impact on education

	(1)	(2)	(3)	(4)	(5)	(6)
	8 YEAR REFORM			9 YEAR REFORM		
	ALL	FEMALES	MALES	ALL	FEMALES	MALES
Difference in Difference	0.272***	0.227***	0.316***	0.523***	0.493***	0.551***
	(0.023)	(0.029)	(0.026)	(0.019)	(0.019)	(0.023)
F-stat	140.64	59.44	147.39	779.52	688.97	576.09
N	534,403	264,237	270,166	1,247,808	613,317	634,491
Regression Discontinuity	0.230***	0.208***	0.251***	0.392***	0.349***	0.433***
	(0.023)	(0.031)	(0.030)	(0.023)	(0.027)	(0.024)
F-stat	101.29	44.25	72.36	299.31	172.71	317.89
N	534,403	264,237	270,166	1,247,808	613,317	634,491

Notes: This table shows the impact of the 8 year and 9 year school reforms on years of education. Each coefficient is from a separate regression by reform, method and group. DiD specification includes birth cohort and municipality fixed effects and municipality level time trends. Regression discontinuity estimates have separate second polynomials in the running variable either side of the cut-off and a full set of dummy variables for month of birth and gender. Bandwidth of up to 10 years is used for both DiD and RD. Robust standard errors clustered by municipality level (for DiD) and by the running variable (for RD) are in parentheses. Testing the null of the coefficient: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$
Source: SIP. Own calculations.

The 9 year reform had a larger impact on years of education compared to the 8 year reform. This makes sense because the 9 year reform increased years of schooling by two years for students who were in municipalities only offering 7 years of primary school and one year for those offering 8 years, whereas the 8 year reform was just a single year increase for all municipalities affected. The 8 year reform was also predominantly rolled out in urban areas that had less potential compliers as more students in urban areas went to junior secondary school. The results split by gender show that the impact for males is slightly larger across modelling strategies and reforms. The F-statistic results suggest across the board that we have a strong first stage, based on the the rule of thumb for weak instruments of an F-statistic above 10 (Stock, Wright and Yogo, 2002). In table D.3 in the appendix we investigate whether the results in table 5.3 are sensitive to our choice of bandwidth and inclusion of municipality specific linear trends (DiD only). Using bandwidths with a range of 2 years to up to 12 years we find that the point estimates change only slightly (see columns (1-6) in table D.3). Inclusion of trends in our DiD estimates makes little difference to the point estimates.

The event study graphs in figure 5.3 show increases in years of education for the birth cohort born a year before the first treated birth cohort (t-1) although this is much smaller for the 8 year reform. Hjalmarsson, Holmlund and Lindquist (2015) find the same pattern when including lags of the treatment in their assessment of the 9 year reform. We also find that our RD estimates are consistently smaller than our DiD estimates. The increase we observe for the t-1 cohort in the figure explains most of the difference between our RD and DiD estimates. The RD estimates only capture the impact

of the reforms in the reform period whereas the DiD estimates capture some of the pre-reform treatment differences. We illustrate this by dropping the t-1 cohort as a sensitivity test (see model (8) table D.3 in the appendix) and find that the gap between the RD and DiD estimates largely disappears.

We conclude that both the 8 year and 9 year reforms lead to increased years of education, that the increases were slightly larger for males and that the 9 year reform actually had much more bite than previous research has suggested. Our RD estimates are smaller than our DiD estimates and this is likely due to DiD capturing more compliers to the reform. This is a consequence of them capturing different sub-populations.[18]

We have shown that the reforms coincide with substantial increases in years of education using both DiD and RD and therefore that our method of reform assignment is working well. In addition to a strong first stage regression, our 2SLS estimates require exposure to reform to be as good as random, conditional on our control strategies. This may be violated if selective migration to and from reform municipalities occurred, either to escape or gain access to the reform. In previous work assessing the 9 year reform, both Meghir and Palme (2005) and Holmlund (2008) have tested for selective migration and have found that it was not a problem. We are not able to test it for the 8 year reform but make the assumption that the results of both Meghir and Palme (2005) and Holmlund (2008) apply to the earlier reform as well. We view this as a plausible assumption given that the 8 year reform was just a pure years of schooling change and would have provided much less of a reason to move compared to the comprehensive 9 year school reform - a reform that itself led to very limited selective migration.

Our estimates are robust to the inclusion of lags and leads and various forms of model specification but there may still be concern that our error term remains correlated with our explanatory variables, in particular reform assignment. In figure 5.4 we perform an RD diagnostic test of manipulation of the forcing variable in the spirit of McCrary (2008) by plotting the population density by age relative to the first cohort in the municipality impacted by the reform (top two panels). We observe no obvious jump at the cut-off point and therefore no clear changes in the fertility timing decisions around the reform. We also present scatter plots of father's education by age relative to the first reform birth cohort and again see no clear jump in father's years of schooling at the cut-off. In addition we perform a batch of balancing tests of predetermined characteristics and reform assignment in order to assess our exclusion restriction in table 5.4. The results show that when we control only for birth cohort fixed effects (columns 1 and 4) our predetermined characteristics are predicted by reform status. The correlations also go the way

[18]We have also tested whether seasonal variation in years of schooling changes after the reforms. We find no impact for the 8 year reform and a small negative impact for the 9 year reform on years of schooling but not on later health outcomes. This is consistent with work looking at the impact of school starting age on longer term labour market outcomes, that also finds both an impact on years of schooling but also no impact on later life outcomes (Fredriksson and Öckert, 2014). Including separate monthly dummies each side of the threshold however comes at a severe loss of efficiency, we therefore choose to model month effects without a reform interaction.

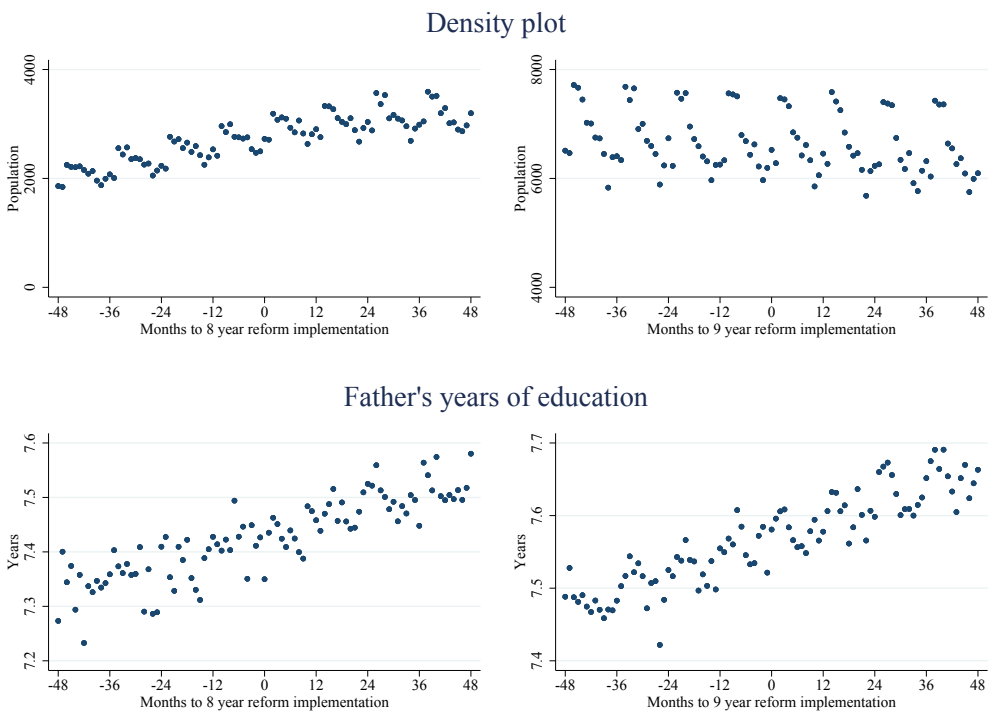


FIGURE 5.4: Diagnostic tests

Notes: Top two panels: Density plots of age measured as the distance in months to reform in their municipality (the first cohorts to be impacted are at zero). The bottom two panels: Placebo tests of reform status on father's years of education - plotted as mean father's years of schooling in monthly bins of months to reform.
Source: SIP. Own calculations.

we might expect: the reforms were introduced earlier in areas where parents were more educated and had better jobs. The inclusion of municipality fixed effects and municipality specific time trends and hence our DiD strategy (see columns 2 and 5) reduces the size of the coefficients bringing them down to zero. Similarly in columns (3) and (6) using RD to identify the impact of the reforms we find the size of the coefficients tends towards zero and they are insignificant. Whilst this evidence is just indicative that our reforms are not correlated with our error terms, they provide certain credibility to our strategies. In sum, we have shown that our assignment method works well and that with the application of our DiD and RD strategies we have provided support to our claim that reform exposure is as good as random.

TABLE 5.4: Diagnostics: Balancing test for differences in predetermined characteristics by reform status

	(1)	(2)	(3)	(4)	(5)	(6)
	8 YEAR REFORM			9 YEAR REFORM		
	OLS	DiD	RDD	OLS	DiD	RDD
PANEL A: MOTHER						
Years of Education	0.095*	-0.009	-0.009	0.074**	-0.002	0.005
	(0.039)	(0.007)	(0.009)	(0.026)	(0.004)	(0.006)
Blue collar worker	0.033***	0.002	0.001	0.002	-0.001	-0.000
	(0.007)	(0.002)	(0.003)	(0.005)	(0.001)	(0.002)
White collar worker	0.032***	0.004	-0.002	0.019**	0.000	-0.000
	(0.008)	(0.002)	(0.002)	(0.006)	(0.001)	(0.001)
No occupation	-0.068***	-0.006*	0.002	-0.021*	0.000	-0.001
	(0.012)	(0.002)	(0.003)	(0.009)	(0.002)	(0.002)
PANEL B: FATHER						
Years of Education	0.118	-0.005	-0.011	0.128***	0.005	0.005
	(0.065)	(0.011)	(0.014)	(0.038)	(0.007)	(0.009)
Blue collar worker	0.043***	-0.001	0.004	0.010	-0.000	-0.004
	(0.012)	(0.003)	(0.004)	(0.008)	(0.002)	(0.002)
White collar worker	0.060***	-0.001	0.000	0.032**	0.002	0.005*
	(0.017)	(0.003)	(0.003)	(0.012)	(0.002)	(0.002)
No occupation	-0.004*	0.001	0.002	0.001	0.000	-0.000
	(0.002)	(0.001)	(0.002)	(0.002)	(0.001)	(0.001)

Notes: This table shows impact of reform status on various predetermined characteristics. Columns (1) and (4) are simple associations controlling for year of birth. Columns (2) and (5) are estimates from a DiD regression. Columns (3) and (6) are estimates from a RD design regression using a 2nd polynomial in age from reform estimated either side of the cut-off, and dummies for month of birth and gender. All estimates use a bandwidth of up to 10 years and robust standard errors clustered at the municipality level (age on months level for RD) and these are shown in parentheses. Testing the null of the coefficient: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$
Source: SIP. Own calculations.

5.5 Results

5.5.1 Mortality

All cause mortality by 2013

In this section we analyse the impact of the school reforms on mortality risk by 2013 (dying by age 75). In figure 5.5 we show the risk of dying by 2013 and we observe no impact of the reforms on overall mortality in the raw data or in the event study figures.

In table 5.5 we present the LPM regression results for mortality. In column (1) we present the linear association of years of education with mortality probability controlling for year of birth only. This is estimated only for those not treated and with pre or post reform minimum years of schooling. This is to give an idea as to the strength of the education gradient in health for the sub-populations impacted by the reforms. The estimates confirm the

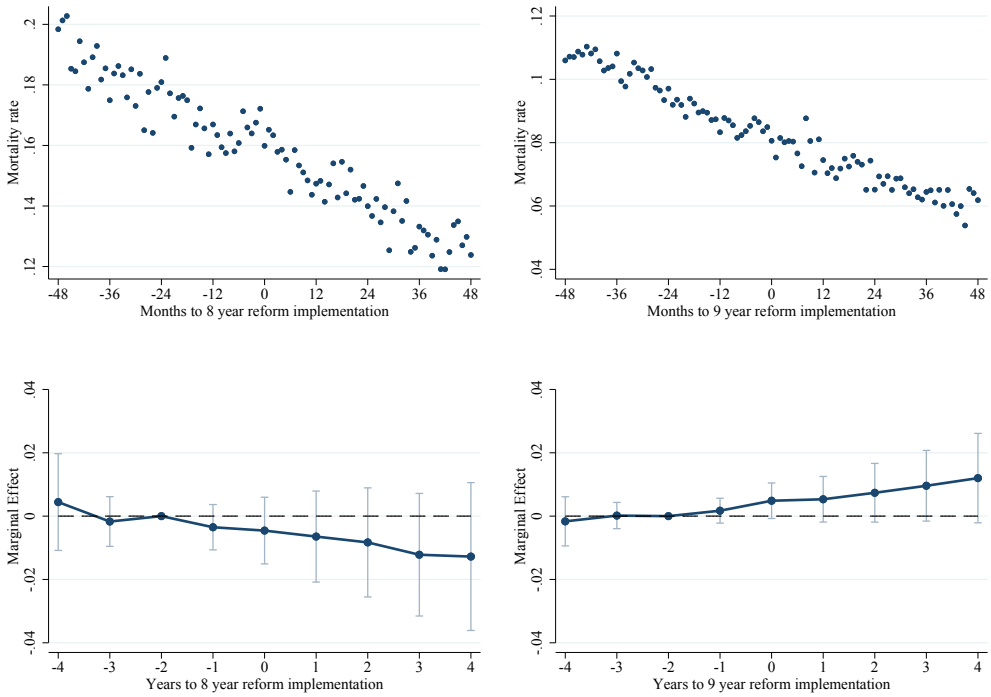


FIGURE 5.5: Impact of the reforms on mortality by 2013

Notes: These figures plot the relationship between time to reform and mortality. The top two figures are raw data scatter plots. The bottom two figures are event study figures of coefficients from DiD regression. See notes for figure 5.3.
Source: SIP. Own calculations.

finding in the wider literature that education is positively (negatively) associated with health (mortality). Columns (2) and (3) present our Reduced Form (RF) and 2SLS (IV) results respectively using our DiD identification strategy. Columns (4) and (5) present our RF and IV RD based results respectively.

TABLE 5.5: Regression results: OLS estimates and reform effects on overall mortality.

	OLS (1)	RF-DiD (2)	IV-DiD (3)	RF-RD (4)	IV-RD (5)
PANEL A: FEMALES AND MALES					
8 Year Reform Impact	-0.0167***	-0.0003	-0.0011	0.0027	0.0119
	(0.0013)	(0.0022)	(0.0082)	(0.0025)	(0.0112)
9 Year Reform Impact	-0.0100***	-0.0003	-0.0005	-0.0016	-0.0041
	(0.0006)	(0.0011)	(0.0021)	(0.0014)	(0.0034)
PANEL B: FEMALES					
8 Year Reform Impact	-0.0163***	0.0024	0.0106	0.0029	0.0139
	(0.0017)	(0.0029)	(0.0131)	(0.0033)	(0.0161)
9 Year Reform Impact	-0.0099***	0.0001	0.0002	-0.0022	-0.0064
	(0.0007)	(0.0014)	(0.0028)	(0.0017)	(0.0048)
PANEL C: MALES					
8 Year Reform Impact	-0.0096***	-0.0028	-0.0088	0.0026	0.0104
	(0.0019)	(0.0031)	(0.0097)	(0.0039)	(0.0159)
9 Year Reform Impact	-0.0063***	-0.0009	-0.0016	-0.0010	-0.0022
	(0.0008)	(0.0017)	(0.0031)	(0.0018)	(0.0042)

Notes: This table presents the OLS, reduced form and 2SLS regression estimates on mortality. Mortality is modelled using an LPM of death by 2013. Sample sizes/No. of deaths: Panel A, 8 year reform (534,403/71,640); Panel A, 9 year reform (1,247,808/120,382); Panel B 8 year (264,237/28,613); Panel B 9 year (613,317/47,531); Panel C 8 year (270,166/43,723); Panel C 9 year (634,491/72,851). Each coefficient estimate is from a separate regression. Column (1) is the association of years of education with mortality controlling for year of birth and the sample is limited to those not treated and with pre or post reform minimum years of schooling. Columns (2) and (3) are reduced form and 2SLS regression estimates using our DiD specification which includes birth cohort and municipality fixed effects, municipality level time trends and a bandwidth of up to 10 years. Columns (4) and (5) are reduced form and 2SLS regression estimates using regression discontinuity and have separate second polynomials in the running variable either side of the cut-off and a full set of dummy variables for month of birth, control for gender and bandwidth of up to 10 years. Robust standard errors clustered by municipality level (for DiD) and by the running variable (for RD) in parentheses. Testing the null of the coefficient: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$
Source: SIP. Own calculations.

In Panel A of table 5.5 the results are for all individuals. The reduced form estimates for the 8 year reform are -0.03 percentage points using DiD and 0.27 percentage points using RD and are much smaller than the OLS correlations. The standard errors rule out even moderately large sized effects. The IV estimates for the 8 year reform are -0.11 percentage points using DiD and 1.2 percentage points using RD (relative impacts of -0.8% and 8.9% respectively) with corresponding 95 percent confidence interval for DiD of -1.5 to 1.4 percentage points and using RD of -1 to 3.3 percentage points. The OLS estimate of column (1) is both larger and more negative than both IV point estimates and lies outside both the DiD and RD 95% confidence intervals. Testing for endogeneity of years of schooling in the OLS estimates, we reject the OLS

estimates based on the RD IV results but not using the DiD results.[19] The 9 year reform reduced form impacts are similar in magnitude to those found for the 8 year reform but the standard errors are half the size compared to those from the 8 year reform regressions. As noted in section 5.4.1 there are strong arguments for not using the 9 year reform as an IV, but if we do we reject the OLS estimates using DiD but not RD.

In panels B and C of table 5.5 we split the results by gender as there are both biological and social differences between the genders that could potentially lead to different health responses to the reforms. The OLS correlations are stronger for females than for males but in general, we find no clear gender specific differences in our reduced form estimates or our causal IV estimates.

All cause mortality sensitivity analysis

In table 5.6 we present alternative estimates to the LPM results of table 5.5, this time based on Cox regression, modelling the proportional hazard function of the probability of dying in the next period. In column (1) of table 5.7 we confirm the LPM findings of table 5.5, that there is a significant positive association between education and health and that this is stronger for females. The reduced form estimates using Cox proportional hazard regression for all cause mortality (columns 2 to 5) mirror the LPM findings; we find no significant impact of either reform on mortality, that the impacts are very close to zero and there are no discernible differences in response to the reforms between the genders.

In the appendix, tables D.4 and D.5, we present sensitivity analysis that assesses the robustness of the results to choice of bandwidth and removal of trends in the LPM and the Cox DiD analysis and find the conclusions are unaffected by these modelling choices. In our analysis of the impact of the reforms on education in section 5.4.2 we found there to be measurement error in our reform assignment and a positive jump in years of schooling for individuals one year too old and that this was much larger for the 9 year reform. This lead to a large discrepancy between the RD estimates and the DiD estimates, but including a doughnut in our regressions (removing the t-1 cohort) removed the difference between the RD and DiD estimates. Our LPM and Cox DiD and RD estimates are in absolute terms very close to one another. However we test if controlling for the t-1 cohort affects our conclusions in column (8) of tables D.4 and D.5 in the appendix and the conclusions remain the same for the 8 year reform and for the 9 year reform using DiD. Using RD we find that our LPM results show a mortality reducing effect, however not for the Cox regression results suggesting this is not robust to modelling strategy. We conclude that we find no evidence of a causal effect of years of schooling on mortality and that this general result is robust to modelling choices, type of school reform and identification strategy.

[19] The test used is a test based on difference-in-Sargan statistics (C-statistic).

TABLE 5.6: Cox proportional hazard estimates of survival till 2013

		8 YEAR REFORM		9 YEAR REFORM	
	COX-PH (1)	COX-DiD (2)	COX-RD (3)	COX-DiD (4)	COX-RD (5)
FEMALES AND MALES					
Reform Impact	0.9150***	0.9905	1.0136	0.9888	0.9925
	(0.0059)	(0.0169)	(0.0190)	(0.0130)	(0.0160)
N	138,460	534,403	534,403	1,247,808	1,247,808
No. Deaths	30,633	71,640	71,640	120,382	120,382
FEMALES					
Reform Impact	0.9013***	1.0112	1.0285	0.9970	0.9828
	(0.0094)	(0.0296)	(0.0319)	(0.0206)	(0.0254)
N	67,737	264,237	264,237	613,317	613,317
No. Deaths	12,105	28,613	28,613	47,531	47,531
MALES					
Reform Impact	0.9561***	0.9790	1.0039	0.9821	0.9989
	(0.0080)	(0.0196)	(0.0264)	(0.0167)	(0.0176)
N	70,723	270,166	270,166	634,491	634,491
No. Deaths	18,528	43,027	43,027	72,851	72,851

Notes: This table presents the impact of the compulsory school reforms on cause specific mortality. Each coefficient estimate is from a separate regression. Column (1) is from a Cox proportional hazard regression of the impact of years of schooling for those not treated and with pre or post reform minimum years of schooling. Columns (2) and (4) are regression results from out DiD specification which includes birth cohort fixed effects, municipalities that reformed in the same year level time trends stratified by municipality and a bandwidth of up to 10 years. Columns (3) and (5) are RD estimates and have separate second polynomials in the running variable either side of the cut-off and a full set of dummy variables for month of birth and gender and a bandwidth of up to 10 years. Robust standard errors clustered by municipality level (for DiD) and by the running variable (for RD) in parentheses. Testing the null of the coefficient: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$
Source: SIP. Own calculations.

Cause specific mortality by 2013

To test whether the reforms had competing impacts by cause of death that potentially offset each other or whether diseases more amenable to health related behaviours, specifically cancer, circulatory diseases and external causes show a response to the reforms we consider some leading causes of mortality by 2013. Figure 5.6 presents the raw data for the average within municipality relationship between birth cohort and cause specific mortality and there are no discernible jumps at the reform cut-offs.

In table 5.7 we present the Cox independent competing risks regression estimates for leading causes of mortality. The RF Cox estimates presented in columns (2-5) are all smaller than those found in column (1). For both the 8 year and 9 year reforms we find no cause specific impacts. The potential exception is the impact of the 9 year reform on deaths due to other causes. However, we only find a significant positive impact using DiD and a negative but insignificant impact using RD. It is therefore not robust to identification strategy.

TABLE 5.7: Cox proportional hazard independent competing risk results: Impact of the reforms on causes of mortality

		8 YEAR REFORM		9 YEAR REFORM	
	COX-PH (1)	COX-DiD (2)	COX-RD (3)	COX-DiD (4)	COX-RD (5)
CANCER					
Reform Impact	0.9526***	1.0221	1.0167	1.0232	0.9851
	(0.0103)	(0.0291)	(0.0280)	(0.0215)	(0.0289)
No. Deaths	11,831	28,255	28,255	45,079	45,079
CIRCULATORY DISEASE					
Reform Impact	0.8659***	0.9603	0.9776	1.0077	0.9747
	(0.0109)	(0.0334)	(0.0395)	(0.0298)	(0.0403)
No. Deaths	8,091	16,792	16,792	26,086	26,086
EXTERNAL CAUSES					
Reform Impact	0.9199***	0.9578	0.9403	0.9483	0.9917
	(0.0258)	(0.0535)	(0.0690)	(0.0336)	(0.0464)
No. Deaths	1,527	5,650	5,650	13,834	13,834
ALL OTHER CAUSES					
Reform Impact	0.9101***	0.9772	1.0449	0.9399***	1.0270
	(0.0108)	(0.0291)	(0.0354)	(0.0218)	(0.0307)
No. Deaths	9,184	20,943	20,943	35,383	35,383
N	138,460	534,403	534,403	1,247,808	1,247,808

Notes: See notes for table 5.6
Source: SIP. Own calculations.

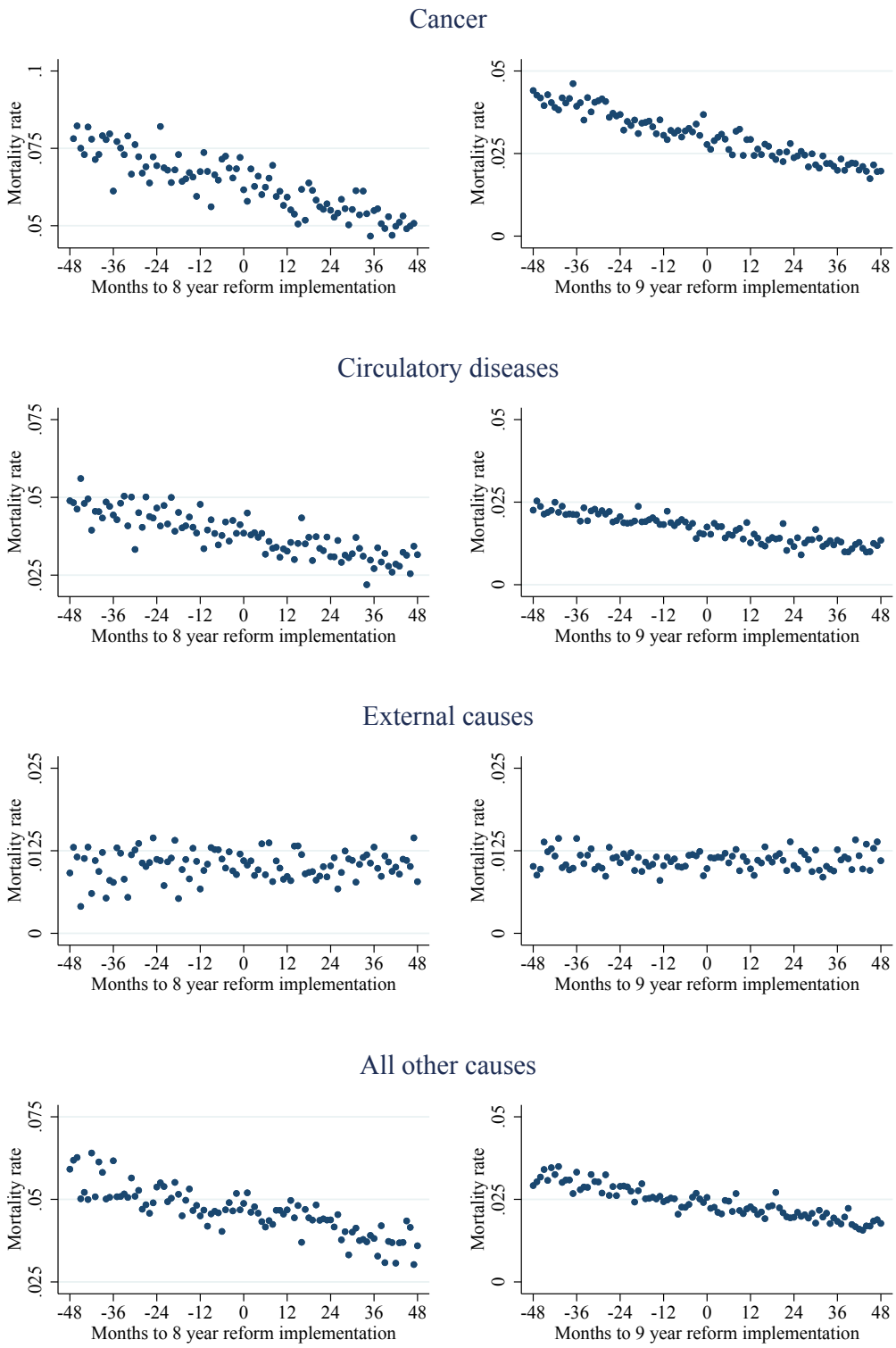


FIGURE 5.6: Impact of the reforms on cause specific mortality by 2013

Notes: Scatter plots of cause specific mortality rate by age in months measured as the age difference of each individual from the first birth cohort in their municipality to be impacted by the reform (the first cohorts to be impacted are at zero).
Source: SIP. Own calculations.

5.5.2 Hospital admissions

All cause days admitted to hospital

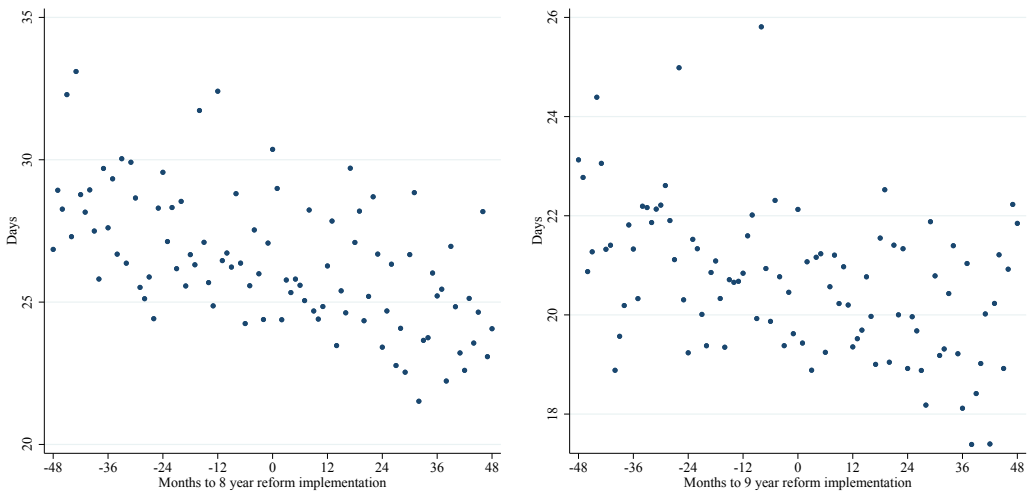


FIGURE 5.7: Impact of the reforms on days admitted to hospital by 2012

Notes: This figure plots the relationship between birth cohort and first cohort impacted by the reform and days admitted to inpatient care (a stay over-night). See notes for figure 5.5.
Source: SIP. Own calculations.

In this section we assess the impact of the school reforms on a potentially more sensitive measure: inpatient hospital admissions. Specifically, days admitted to inpatient hospital care and cause specific inpatient hospital admissions are considered. In the previous section we found no impact on overall mortality over the observation period. This means we can assess other health measures without concern for mortality impacts affecting our results. Figure 5.7 presents the relationship between age relative to the first reform cohort and days admitted to hospital. We find no visually discernible impact of the reforms on days admitted to hospital at the reform cut-offs (0 represents the first cohorts in the municipality impacted by the reforms).

In table 5.8 we present the regression results for days in hospital for both reforms. Column (1) of table 5.8 shows the simple association of years of education for those not treated and with pre or post reform minimum years of schooling. There is a substantial and highly significant negative relationship between years of schooling and days admitted to hospital. The OLS results suggest that for an additional year of schooling, individuals will have about 1.5 fewer days at hospital, or a 6 to 7% reduction. The 8 year reform reduced form point estimates using both DiD and RD are found in columns(2) and (4) and are equal to 0.27 and 0.85 days for DiD and RD respectively (relative impacts of 1.1% and 3.4% respectively) and are insignificant, small and positive. The 8 year reform IV point estimates are 1 and 3.7 days for DiD and RD respectively and also insignificant (increases of 4.1% and 14.9% respectively). The reduced form point estimates for the 9 year reform are -0.07 and

0.2 days for DiD and RD respectively and insignificant. The corresponding IV estimates report increases of -0.6% and 2.4% and are substantially smaller than the OLS correlations. Although both our 8 year and 9 year reform based IV estimates are quite different to the OLS estimates, we are unable to reject the OLS estimates.

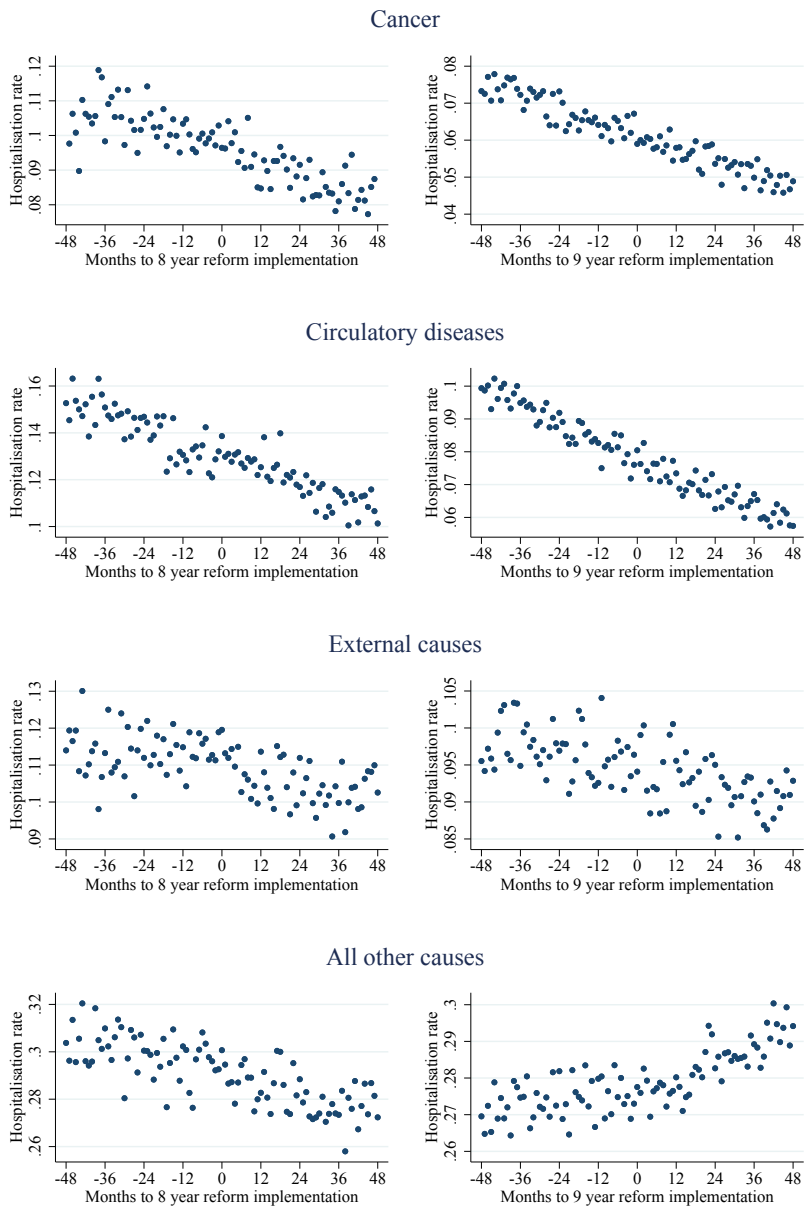


FIGURE 5.8: Impact of the reforms on probability of hospital admission by cause by 2012

Notes: Scatter plots of the probability of hospital admission by age in months measured as the age difference of each individual from the first birth cohort in their municipality to be impacted by the reform (the first cohorts to be impacted are at zero).
Source: SIP. Own calculations.

Cause specific hospital admissions

Hospital admissions can occur for a variety of reasons and differences in education levels may push the quantity of medical care use in different directions. For example particular admissions due to health shocks are potentially more likely amongst those who have invested less in their health which may be a function of having received less education. On the other hand admissions that are in themselves health investments such as screening and examinations or preventative care or early detection may be a function of having received more education. To assess if there are counteracting effects of education we consider three leading reasons for hospital admission: cancer, circulatory diseases and external causes. In figure 5.8 we present the raw data as scatter plots of the probability of admission due to specific causes by age relative to the first cohort impacted by the reform in each municipality. Eyeballing the data, there are no obvious jumps at the reform implementation cut-offs in the hospitalisation rates by cause.

In table 5.8 column (1) we see that the association of years of education is negative and significant for circulatory diseases, external causes and all other causes but not for cancer. We test for jumps using RD in table 5.8 alongside DiD regression and our reduced form estimates (columns (2) and (4)). The results using the 8 year reform find no significant impact of the reform on the probability of inpatient care due to the specific causes we consider except for external causes, where using RD our IV estimate of -1.6 percentage points is nearly four times as large as our OLS estimate and twice as large as our DiD IV estimate. The results using the 9 year reform find evidence of an impact of the reform on the probability of inpatient care, through a reduction in other causes related admissions of -1.5 percentage points using our IV estimate. We should note that we make no adjustment for multiple hypothesis testing here and any attempt to do so would remove any significance we have found here. In general we do not find convincing evidence of an impact of education on cause specific inpatient care that is robust to identification strategy.

Sensitivity analysis of impact on hospital admissions

In the appendix table D.6, we test the sensitivity of these results to bandwidth choice and inclusion and removal of linear trends in the DiD regression specification. In table D.6 we estimate the reduced form results for days of inpatient care of table 5.8 varying the bandwidth between 2 to up to 12 years (see columns (1) - (7) in table D.6). Compared to the results for mortality and years of education, the inpatient days results are more sensitive to bandwidth choice but the choice of a 10 year bandwidth does not impact the conclusions. The exclusion of municipality specific linear trends in our DiD analysis makes little difference to the estimates. Inclusion of a dummy for the t-1 cohort increases the coefficient size across all model types and both reforms, but also the standard errors. In summary we conclude that there is no evidence of an impact of either reform on hospital admissions.

TABLE 5.8: Linear regression results: Impact of the reforms on inpatient hospital admissions

	Mean (1)	OLS (2)	RF-DiD (3)	IV-DiD (4)	RF-RD (5)	IV-RD (6)
PANEL A: 8 YEAR REFORM						
Days at hospital	24.71	-1.4768***	0.2745	1.0083	0.8469	3.6832
		(0.3283)	(0.4855)	(1.7969)	(0.7659)	(3.3420)
PROBABILITY OF HOSPITAL ADMISSION DUE TO:						
Cancer	0.08	-0.0008	0.0004	0.0014	0.0003	0.0014
		(0.0010)	(0.0017)	(0.0064)	(0.0017)	(0.0074)
Circulatory diseases	0.11	-0.0101***	0.0022	0.0082	0.0024	0.0106
		(0.0012)	(0.0017)	(0.0064)	(0.0024)	(0.0107)
External causes	0.10	-0.0045***	-0.0023	-0.0085	-0.0036**	-0.0158**
		(0.0010)	(0.0019)	(0.0071)	(0.0018)	(0.0078)
Any other cause	0.29	-0.0128***	-0.0034	-0.0125	-0.0028	-0.0121
		(0.0015)	(0.0026)	(0.0095)	(0.0035)	(0.0151)
N		138,460	534,403	534,403	534,403	534,403
PANEL B: 9 YEAR REFORM						
Days at hospital	22.17	-1.6405***	-0.0707	-0.1354	0.2070	0.5287
		(0.1684)	(0.4180)	(0.7986)	(0.4980)	(1.2650)
PROBABILITY OF HOSPITAL ADMISSION DUE TO:						
Cancer	0.07	0.0005	-0.0004	-0.0007	-0.0009	-0.0024
		(0.0004)	(0.0009)	(0.0018)	(0.0011)	(0.0029)
Circulatory diseases	0.09	-0.0089***	0.0017*	0.0033*	0.0006	0.0015
		(0.0005)	(0.0010)	(0.0020)	(0.0014)	(0.0035)
External causes	0.10	-0.0051***	0.0005	0.0009	-0.0001	-0.0002
		(0.0005)	(0.0012)	(0.0023)	(0.0016)	(0.0040)
Any other cause	0.29	-0.0110***	-0.0010	-0.0019	-0.0061**	-0.0156***
		(0.0007)	(0.0017)	(0.0033)	(0.0024)	(0.0060)
N		397,961	1,247,808	1,247,808	1,247,808	1,247,808

Notes: This table presents the impact of the compulsory school reforms on inpatient hospital admissions. All coefficients are from separate regressions. See notes for table 5.5.

5.5.3 Health and health related behaviours

In this section we consider the impact of schooling on health outcomes and health related behaviours. These health measures are arguably more sensitive to the potential mechanisms in which education may influence health compared to mortality and hospital visits. So whilst mortality and hospital visits are outcomes that are objectively measured and available for the whole population, they require major health events to occur making them relatively insensitive measures of the impact of education on health. Even though we have found no impact on mortality or hospitalisations up to the age of 75, it is still possible that we may see an impact in more sensitive measures such as health behaviours or in self-reported measures of current health.

In table 5.9 we present the regression estimates of the school reforms on various self-reported health outcomes and health related behaviours using

our survey data. Column (1) shows the simple OLS correlation estimates of years of education on health for those untreated and with years of education equal to or one more year than the legal minimum. Education is found to be associated with lower probability of having fair or bad health and a lower probability of being obese for our population. The cohorts we use in this analysis therefore exhibit the same positive education gradient in self-reported health and health related behaviours that is observed for mortality and hospital admissions in this paper and that has also been observed more widely in the literature.

The reduced form estimates are found in columns (2) and (4) of table 5.9. These are modelled in the same way as for mortality and hospital admissions with the addition of dummies for survey year but the removal of municipality specific trends. Even though we observe a strong and significant effect of both reforms on years of education we find significant impacts of neither the 8 year reform nor the 9 year reform on health or health behaviours.

Focussing on the health variables where there is a significant correlation observed in column (1) between education and health (fair or bad health and obesity), we find that the 8 year reform based IV results show quite large relative drops and in the same direction as the OLS estimates. Our DiD and RD based IV results for fair and bad health report relative drops of -30% and -13% respectively and are large in comparison to the OLS results of a relative effect of -6%. For obesity our DiD and RD based IV results find relative impacts of -47% and -6% respectively. However, our estimates are not precisely estimated and this is with over 30 years of survey data.[20]

The reduced form estimates for the 9 year reform using both DiD and RD on *fair or bad health* imply small positive effects of the reform which is in contrast to the OLS regression estimates. Incidentally, our results indicate that the findings of Spasojevic (2010) who finds a causal impact of the 9 year reform on self-reported health are not robust to model specification. Whilst we are using a different dataset, we mirror her analysis on our dataset. In her paper she only controls for cohort fixed effects (our column 1 results) and the results in column (2) show that controlling for differences across municipalities which are also correlated to background characteristics (see table 5.4) explain away her findings.

We test the sensitivity of these results to using a smaller bandwidth, found in the appendix, table D.7, and the results show our conclusions remain the same. We find no clear impact of the reforms on measures of self-reported health or health behaviours.

[20]The 8 year reform is a strong instrument using RD but not quite so strong using DiD. See table D.2 in the appendix.

TABLE 5.9: Education effects on self-reported health and health behaviours

	OLS (1)	RF-DiD (2)	IV-DiD (3)	RF-RD (4)	IV-RD (5)
PANEL A: 8 YEAR REFORM					
Fair or bad health	-0.019*	-0.021	-0.063	-0.017	-0.028
	(0.010)	(0.018)	(0.056)	(0.027)	(0.043)
N	2,691	8,181	8,181	8,181	8,181
Mean	0.31	0.21	0.21	0.21	0.21
Smoke daily	-0.008	0.001	0.004	-0.004	-0.006
	(0.009)	(0.021)	(0.061)	(0.026)	(0.041)
N	2,661	8,138	8,138	8,138	8,138
Mean	0.24	0.26	0.26	0.26	0.26
Obese	-0.029***	-0.022	-0.052	-0.005	-0.007
	(0.009)	(0.019)	(0.048)	(0.030)	(0.035)
N	1,683	4,996	4,993	4,996	4,996
Mean	0.15	0.11	0.11	0.11	0.11
Anxiety, concern etc.	-0.004	-0.007	-0.018	-0.029	-0.033
	(0.011)	(0.022)	(0.055)	(0.032)	(0.037)
N	1,776	5,388	5,385	5,388	5,388
Mean	0.17	0.15	0.15	0.15	0.15
PANEL B: 9 YEAR REFORM					
Fair or bad health	-0.018***	0.005	0.010	0.004	0.012
	(0.004)	(0.011)	(0.021)	(0.016)	(0.045)
N	12,866	19,124	19,122	19,124	19,124
Mean	0.28	0.19	0.19	0.19	0.19
Smoke daily	-0.007*	-0.011	-0.021	-0.012	-0.032
	(0.004)	(0.014)	(0.025)	(0.016)	(0.045)
N	12,741	19,033	19,030	19,033	19,033
Mean	0.27	0.26	0.26	0.26	0.26
Obese	-0.010***	0.001	0.002	0.013	0.036
	(0.003)	(0.011)	(0.020)	(0.012)	(0.033)
N	7,935	11,514	11,496	11,514	11,514
Mean	0.13	0.09	0.09	0.09	0.09
Anxiety, concern etc	-0.001	-0.015	-0.026	0.009	0.026
	(0.004)	(0.014)	(0.023)	(0.018)	(0.054)
N	8,468	12,487	12,472	12,487	12,487
Mean	0.16	0.15	0.15	0.15	0.15

Notes: This table presents the impact of compulsory school reforms on self-reported health and health behaviours. See notes for table 5.5. Note that a full set of dummy variables for survey year are included in all regressions and DiD regressions are modelled without municipality trends.
Source: ULF-Survey. Own calculations.

5.6 Discussion

Our findings show that across two major school reforms that led to clear and substantial increases in years of education we observe only small and generally insignificant changes in mortality and other measures of health. Our IV point estimates for the 8 year reform find that an additional year of education yields a -0.01 and 1.2 percentage point change in mortality using DiD and RD respectively and the lower bound of our confidence intervals allows for a 1.7 percentage point reduction in mortality.

The research that most closely aligns to that of ours is that of Lleras-Muney (2005), Mazumder (2008) and Clark and Royer (2013). Our IV estimates are much smaller than those of Lleras-Muney (2005) and are more in line with the findings of Clark and Royer (2013) for Britain, and Mazumder (2008) for the USA. The findings of Clark and Royer (2013) are potentially the most convincing evidence gathered so far. However, there are concerns that the results of Clark and Royer (2013) are based upon two reforms in Britain that impacted very different cohorts (1947 and 1972) and both reforms were implemented overnight nationwide. The cohorts are likely to have been very different, both in terms of their own characteristics but also in terms of the health and labour market structures they were exposed to and there may have been large general equilibrium effects of a nationwide roll out of increased compulsory schooling that potentially reduce the earnings effect of these reforms. Our results from Sweden are based on two reforms, different in design, that were rolled out over time and with overlapping cohorts and evaluated using two different identification strategies and we still find only small or zero impacts on health. Our findings together with those of Clark and Royer (2013) suggest that the timing of the reforms and the nature of their roll out has little bearing on the results. Our own findings are also consistent across the two reforms and suggest it is not specific features of the Swedish compulsory school setting that explain the Swedish results because the two reforms were in fact quite different.

Whilst our findings for the 9 year reform are similar to those of Meghir, Palme and Simeonova (2017), who also study the impact of the 9 year reform on mortality in Sweden, our results add a large sense of robustness to their findings. We have shown that previous estimates of the impact of schooling have been downward biased because of the measure of years of education used and in fact the 9 year comprehensive school reform had a much greater effect on years of education than previous estimates suggest. We have also extended the analysis of the 9 year reform to include self-reported measures of health and health related behaviours. The final and leading contribution of our paper is that we introduce another school reform that allows us to instrument the effect of education on health. The 9 year reform has been argued to not be a pure years of schooling reform (Meghir and Palme, 2005) and therefore analysis using the 9 year reform has to be kept to considering the reduced form impacts. This is not to say that analysis of the 9 year reform is not of interest, it is just that the theories that we are testing often relate to years of education (Grossman, 2015).

The results of this paper and of Meghir, Palme and Simeonova (2017) stand in contrast with previous Swedish research of Lager and Torssander (2012) and Spasojevic (2010) who find small health improving impacts of the 9 year reform. In the appendix D.2 we have attempted to replicate the results from Lager and Torssander (2012) who found a small but significant reduction in mortality due to the 9 year reform. Unfortunately we are unable to replicate their results exactly, but the analysis highlights how our conclusion is robust to their reform assignment and a different observation sample. We have also shown that we can repeat the significant finding of a causal impact of the 9 year reform on self-reported health outcomes of Spasojevic (2010) using a different and larger dataset. However, this result is not robust to a DiD or RD identification strategy.

The nature of our study is that we have been able to pin down quite a few variables that potentially explain the differences in impacts of compulsory school reforms found in the literature. We start with two reforms that both have a substantial impact on years of schooling. The reforms themselves were also different to each other and were rolled out in a way that concerns about resource shocks such as lack of teachers and schools and even general equilibrium effects that may be the case with the reforms in Britain, for example, do not apply in the same way to the Swedish reforms. That both reforms were rolled out during similar time periods further implies that the cohorts exposed to the two reforms later acted under the same welfare and labour market institutions. The clean comparisons of the two reforms further strengthen the internal validity of our findings. We have used a large dataset that includes a whole range of health outcomes. Different econometric methods have been used to estimate the impact on mortality. Two identification strategies have been used to assess the sensitivity of estimating across different sub-groups and under different identifying assumptions. Our finding of no or small mortality impacts of education are robust to modelling strategy, both LPM and Cox proportional hazard regression, identification strategy, either DiD or RD design, and school reform type.

The results presented here have strong internal validity. But how relevant are the findings outside of Sweden? Like the National Health Service (NHS) in Britain, the Swedish health care system has universal coverage and is publicly provided with access at the point of need paid for through taxation. Whilst universal provision removes the direct role of financial resources in determining health care quality, a channel which improved education could influence, there is still plenty of scope for the more educated to achieve better health. Any publicly provided health care system has to prioritise resources and more educated individuals are potentially more likely to be able to manipulate the system to their advantage, either through knowing how the system works, being more aware of the services that are available or through the ability to convince doctors of the need for treatment. Medical services also only in part determine health outcomes. Health behaviours and investments are also very important in determining health outcomes and these are also

potentially impacted by education through better understanding and knowledge. Financial resources may also play a role in determining our health behaviours and health investments. To understand how education may impact health we need to understand which of these potential channels are channels that matter and this means the results of this paper, due to their high internal validity, are of importance.

The reform cohorts we have considered in this paper were born across the span 1938-1954. Like Clark and Royer (2013) who considered the school reform of 1947, some of these cohorts were impacted by the Second World War (WWII). Children in Britain during the war were moved out of the big cities and lived with members of the extended family or even volunteers in the countryside. Sweden was neutral during WWII and was much less affected in general and there were no specific policies to move children out of the large cities.[21] So whilst WWII was an unusual time, it is unlikely to have impacted the external validity of the results drawn here because life for children in Sweden during the war was largely unaffected.

We argue therefore that our results are not specific to the cohorts we consider. The results also help us understand which economic channels, if any, education has an impact on health. Our result, that we are not able to identify an effect of education on mortality, hospital admissions or self-reported health, results that are internally very robust, is therefore an important finding and of relevance beyond Sweden.

5.7 Conclusion

The literature documenting the education gradient in health is vast yet the causal effect literature using compulsory school reforms as instruments for education has produced results that have been difficult to summarise. In this paper we have been able to pin down many of the potential explanations for differences in results across studies and have shown using a large dataset with a long follow-up period that the causal impact of education on mortality is small. This is also true for hospital admissions and other health measures. For mortality we can rule out impacts of years of education larger than -1.7 percentage points. These results hold across econometric technique and identification strategy. We argue that the conclusions we draw from the results of this paper are not specific to the Swedish context, rather they have a more general relevance.

Compulsory school reforms have provided a powerful way to assess the causal impact of education on health. They often impact a large population and can provide exogenous variation in years of schooling under certain parametric assumptions. They also impact a sub-population that is often a public health policy focus: the lowly educated who live short lives and have poorer health outcomes generally. However, the mounting evidence suggests

[21] During WWII it was possible for schools to cancel classes in case of a threat. However, any time lost had to be caught up later on. Also if a teacher was called for military service a substitute teacher had to be called in. Historical sources suggest no educational disruptions in Sweden during the period of WWII (see Bhalotra, Karlsson and Nilsson, 2016).

compulsory schooling laws as policy levers for public health improvements may not be very effective. It is important to note that we do not conclude that education has no impact on health outcomes. Evidence has been found that compulsory schooling reforms can have an impact on health across generations (Lundborg, Nilsson and Rooth, 2014). There is also hope found further up the education ladder, where evidence using the Vietnam draft as an instrument to incentivise college attendance in the USA (Buckles et al., 2016; Grimard and Parent, 2007; De Walque, 2007) has found a mortality reducing impact and an improvement in health related behaviours. Experience from the compulsory schooling literature suggests the results from the Vietnam draft induced college attendance need to be replicated for other populations, health outcomes and identification strategies before any firm conclusions can be drawn however.

Chapter 6

Does Early School Starting Age affect Cognitive Health in the Old Age? Quasi-Experimental Evidence from Germany

6.1 Introduction

Within an ageing population, cognitive disorders in older people become a major challenge for health care systems and societies in general. Alzheimer's disease (AD) and other forms of dementia are associated with a progressive loss of cognitive functioning accompanied with the necessity of support in daily life.[1] In 2007, Ziegler and Doblhammer (2009) estimate a lower bound of 1.07 million adults aged 60^+ suffering from dementia only for Germany which corresponds to 7% of the relevant age group.[2] Due to the high demand for intensive long term care, dementia comes with high costs for patients, carers and the health care system (Schwarzkopf et al., 2012). Estimates of annual net costs for dementia patients in Germany range from €8,000–€45,000, depending on the severity of cognitive impairment which leads to accumulated net costs from dementia of at least €10 billion, amounting approximately in 3% of total health care expenditures for only one specific disease. Incorporating the full psychological and physical costs for carers and patients, overall societal costs could likely be even larger.

Without a cure to the disease available, research has also focussed on non-medical treatments which potentially delay the onset or slow down the progression of dementia. This includes the evaluation of circumstances early in in life. In this line of reasoning, a wide range of observational studies has documented a strong negative association between the level of education and the risk of developing dementia (Fratiglioni, Paillard-Borg and Winblad, 2004; Caamaño-Isorna et al., 2006; Valenzuela and Sachdev, 2006). The protective

[1] The loss of independence in daily routines is important for a dementia diagnosis. WHO defines dementia as *a syndrome in which there is deterioration in memory, thinking, behaviour and the ability to perform everyday activities.*

[2] Estimates are conservative an might underestimate the actual cases as mild form of dementia are not detected.

effects is often explained by more educated individuals building up a *cognitive reserve* against developing dementia by acquiring more education (Meng and D'Arcy, 2012; Stern, 2012). However, there is little evidence whether the observed patterns actually represent a causal relationship. The negative association between education and dementia could also be induced by biological or socio-economic predetermined characteristics such as parental background.

In order to identify causal effects of education, social scientists routinely apply quasi-experimental designs generating exogenous variation in education. This approach is especially challenging in the case of cognitive disease risk in the older population as suitable *natural* experiments additionally need a sufficient length of follow-up time in order to identify effects on dementia risk. Currently, the empirical literature on the causal effect of education on dementia is limited. Based on an instrumental variable approach using US compulsory schooling laws from the early 20th century, Nguyen et al. (2016) find that one additional year of education has a substantial protective effect on dementia risk based on the Health and Retirement Study cohort.[3] Seblova et al. (2017) use an extension of compulsory education during the 1930s and 1940s and full population Swedish administrative data to estimate the causal effects of education on dementia. The compulsory schooling extension increased education for 60%–70% of the population by one year, had moderate positive effects on labor earnings and potentially increased longevity (Fischer, Karlsson and Nilsson, 2013; Fischer et al., 2017). Results in Seblova et al. (2017) suggest that the reform also had a causal protective effect on dementia, but only for men aged 65–74.[4]

In order to study the causal effects of education on cognitive health in old individuals I exploit exogenous variation from school entry age (SSA) regulations, which create persistent educational differences by different school entry age. The policy is broader in its interpretation compared to previous studies using compulsory schooling extensions as a source for exogenous variation. SSA might also have had direct effects on cognitive health and not purely operating through accumulated human capital.

[3]The study uses a constructed genetic risk score. IV estimates based on the compulsory schooling reform for the reeducation in dementia risk are up to 10 times larger than the OLS association. The results are however based on a small survey population and a dementia measure constructed on the basis of telephone interview. For the relevant compulsory schooling extensions see Acemoglu and Angrist (2000); Lleras-Muney (2005).

[4]A closely related literature focusses on the effects from education on cognitive ability of the older people instead of directly looking at dementia. Schneeweis, Skirbekk and Winter-Ebmer (2014); Banks and Mazzonna (2012), and Glymour et al. (2008) find supporting evidence that extensions of compulsory education in Europe and the US increased cognitive abilities for older generations based on test-scores in surveys. Given that we do not fully understand the mechanism underlying dementia and education, it should be noted that the two outcomes to not necessarily lead identical results. E.g. could education raise cognitive abilities in test-scores and at the same time not substantially lowering dementia risks. This results would contradict the cognitive reserve hypothesis and suggest no protective effect of education. Therefore, test-scores in the older people and dementia diagnosis are important complements in order to understand underlying mechanisms.

Several studies have investigated the effects of the school entry age on the performance in school, educational attainment and labor market outcomes. The results typically show that children entering later do better in school (see e.g. Elder and Lubotsky, 2009; Bedard and Dhuey, 2006; Puhani and Weber, 2007). Those studies usually cannot differentiate whether effects are mainly driven by the age at entrance or the relative age gap when performance is measured. With respect to completion of educational qualifications and labor market outcomes, the more recent literature suggests that effects of an earlier school start vanish over the life cycle (Black, Devereux and Salvanes, 2011; Fredriksson and Öckert, 2014; Dustmann, Puhani and Schönberg, 2017). This is in line with the idea that the relative age gap in school rather than the early entrance itself is the main underlying factor. But the results also provide evidence that sufficient flexibility of the school system is a prerequisite for the absence of long-run effects. Fredriksson and Öckert (2014) show that the delay of academic tracking in Sweden significantly reduced the persistence of relative age effects at school entry. Dustmann, Puhani and Schönberg (2017) explain the absence of long-lasting effects by the ability of the contemporary German school system to adjust for initial misallocation. While children are tracked early at age 10, there exist possibilities of up and downgrading between academic tracks later in the educational system. Both results imply that in the absence of sufficient flexibility within the school system, initial misallocation likely persists.

With respect to health, there exist a growing literature investigating the relationship between an earlier school starting age and health earlier in life. This literature is mainly focussed on mental health or smoking behavior during schooling age. Black, Devereux and Salvanes (2011) find small negative effects from an earlier school start on mental health during military medical inspection for 18-year-old males conscripts. Several further studies show an increase in attention-deficit/hyperactivity disorder (ADHD) diagnosis resulting from an earlier SSA (Elder, 2010; Evans, Morrill and Parente, 2010; Dee and Sievertsen, 2015; Schwandt and Wuppermann, 2016). Ballatore, Paccagnella and Tonello (2017) finds evidence of increased bullying for the youngest in class. Bahrs and Schumann (2016) analyse the effects of school starting age on smoking behaviour and health for adults in their mid thirties. Their results suggests that a delayed school starting age reduces the likelihood to smoke and leads to higher self-evaluated health status. No study investigates the long run health effects of an earlier school start on health in the older population.

In this paper I exploit school entry age (SSA) regulations from the early 20th century in Germany. From 1922 onwards, Germany's formerly largest province Prussia introduced a unified cut-off date for school entry. Children born before the 1st of July had to enter school the year they turned six. Children born after the 1st of July entered school the year they turned seven, leading to an age-discontinuity of one year at school entry around the cut-off date. Before the legislation no explicit entry regulation existed. Common practise was to let children enter school if they had turned six before the school year started which typically coincided with Easter. The southern

states of Germany adapted the same entry regulation 16 years later in 1938, lending to a suitable control group. Occurring differences in educational attainment were purely driven by a transmission of an initial relative age gap.[5]

My results show that the Prussian school entry regulation lead to significant decrease in educational attainment and earnings for children who started one year earlier in treated areas. The persistence of negative effects till adulthood is in contrast to more recent findings for Germany (Dustmann, Puhani and Schönberg, 2017). There are several potential explanations for the differences in results: The school system during the 1920s and 30s was much less flexible and more selective in terms of academic tracking. The enforcement of the entry regulation was much stricter and elaborated school readiness examinations were missing. Finally, the cohorts experienced major disruptions in educational career paths due to World War II. The difference in results compared to more recent cohorts in Germany suggests that the German school system has evolved. Early inequalities can be addressed within the education system and disappear over the life cycle.

In contrast to significant differences in educational attainment, I cannot detect a significant decrease in the risk of getting diagnosed with dementia based on administrative health insurance data in the affected population when reached age 75^{+}. This finding is in line with evidence from Sweden suggesting only protective effects from schooling for younger age groups Seblova et al. (2017). In contrast a later school start is associated with reduced overall mortality. As mortality constitutes a competing risk to dementia, increased survival might also have lead to increased dementia cases.

The following section provides a brief overview of the institutional background. Section 6.3 describes the data and sample selection. Section 6.4 describes the empirical strategy. Section 6.5 presents the empirical results. Section 6.6 gives a discussion of the findings and 6.7 concludes.

6.2 Historical and Institutional Background

The Constitution of the Weimar Republic from 1919 and the Primary School Act from 1920 (*Grundschulgesetz*) were the first pieces of legislation setting nationwide standards for primary *schooling* in Germany. Both legislation explicitly emphasized an obligatory school attendance. This implied that home-schooling and special primary preparation schools for higher education were replaced by a common local primary school which all children had to visit until the 5th grade.[6] After finishing 4 years of primary education children were streamed into three different school tracks based on their

[5]In contrast to countries with minimum school drop out age, the length of schooling was not directly affected by the age at entry. All children had to undergo at least 8-years of compulsory schooling.

[6]The German education system since then differentiates between compulsory schooling (*Schulpflicht*) and compulsory education (*Bildungspflicht*). The latter only refers to obligatory education of children but is not necessarily connected institutionalized schooling facilities, compulsory schooling implies mandatory attendance at school.

prior school performance. The middle track prepared for more skilled workers and the higher track for academic education. Only the basic lower track was tuition free. With tuition and performance-based streaming, only a small fraction around 15% of students attained more than basic compulsory schooling. The mandatory length of compulsory schooling was generally 8 years, with the exception of rural areas in southern Germany only offering 7 years of compulsory schooling and the federal state of Schleswig-Holstein already having introduced a 9th grade for boys (Kluger, 2000). In case children left school before turning 18 they had to continue to visit a vocational school. This form of continuation school was part-time and combined with practical vocational training.[7] Usually vocational school meant 3 years of part-time instruction, with the exception of farming school only having a duration of two years. Figure 6.1 gives a stylized version of the school system which is essentially still in place today.

Both pieces of legislations had no specific guidelines about the school entry age. Without a certain cut-off date for school entry, the usual practise was to let children enter school if they had turned 6 before the beginning of the school year. In most parts of Germany this coincided with end of Easter (Rüdiger, Kormann and Peez, 1976). In 1922, the largest German region, Prussia, was the first to set a fixed cut-off rule for school entry based on the date of birth. Children born before the 1st of July had to enter school the year they turned six. Children born after the 1st of July entered school the year they turned seven. The school year continued to start at Easter. Therefore, the youngest children, born just before the cut-off date, started school at the age of 5 years and 9 months while the oldest in the first grade were 6 years and 9 months. The southern provinces continued to let children enter school if they turned 6 before the school year started at Easter. In 1938, the Primary School Act (*Reichschulpflichtgesetz*) harmonized the school entry date across Germany. The 1st of July as cut-off then became common practise (Reichsgesetzblatt, 1938) also in the southern states. Additionally, the new legislation specifically allowed an earlier school entry for children born between July and September if parents supported this and the physical and psychological condition of the child was considered sufficient to allow an earlier school start (Rüdiger, Kormann and Peez, 1976). Before this new piece of legislation an earlier start was a rare exception. It became very common to allow children to start before reaching their school starting age after World War II in Germany (Rüdiger, Kormann and Peez, 1976).

Major modification in the years following the *Reichschulpflichtgesetz* from 1938 were shifts in the cut-off date and an extension of the the period allowing an earlier school entry up to half a year. During World War II, the cut-off date was moved to the end of the year (Reichsgesetzblatt, 1941). After World War II the German federal states regained their sovereignty in terms of educational policy and the school entry age deviated between federal states. In 1964 the *Hamburger Abkommen* harmonized the school entry across federal states and set the cut-off date again back to the 30th of June.

[7] Today this rather Germany specific type of education is often referred to as as the *dual system*.

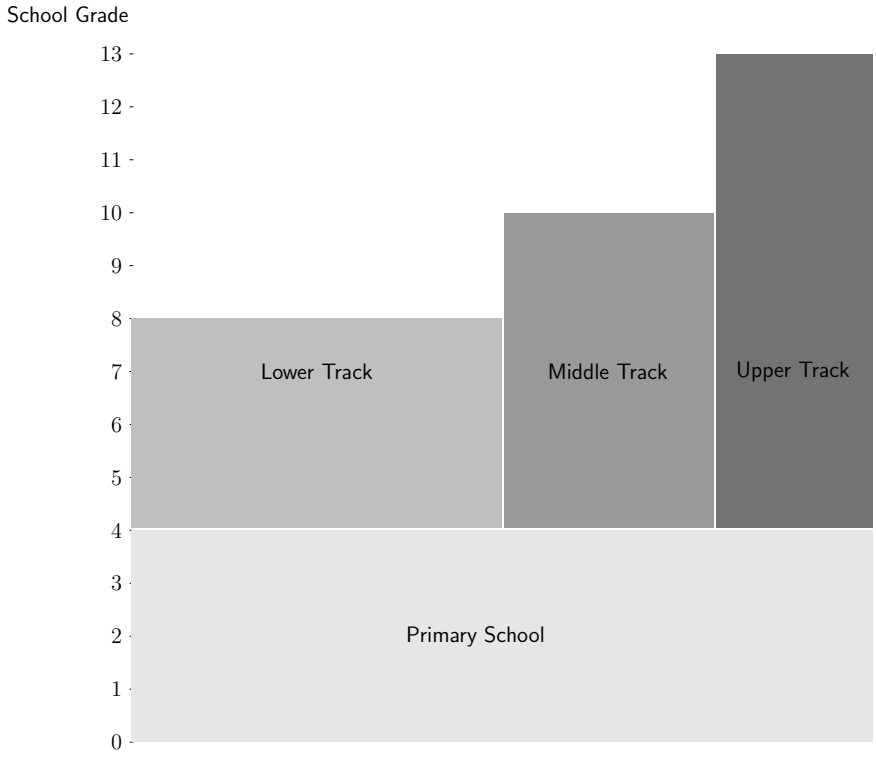


FIGURE 6.1: German School System, 1919

Notes: The figure provides a stylized version of the German education system during the Weimarer Republic and Nazi Germany which is generally still in place today. Notable differences are the absence of a 9th grade in the lowest track (introduced during the 1950s and 60s) and the absence of specialized upper track schools (*Fachgymnasium*, introduced 17th March, 1967). After finishing primary school in grade 4 students were sorted based on their academic performance into three different tracks. Students attended the lower track (*Hauptschule*) or middle school (*Realschule*) typically started working directly or pursuing a vocational training after graduating from school. Students who have attended the upper track (*Gymnasium*) were prepared to continue to university.

Many of the cohorts in this study are exceptional in the sense that they experienced massive changes and turbulence during their youth and schooling age. First, the years of the Weimarer Republic were characterized by several economic and political crises. The rise to power by the Nazis in 1933 then lead to several massive disruptions in the lower and higher education levels. Following the *Law for the Restoration of the Professional Civil Service*[8], the first period from 1933–1936 was characterized by an expulsion of teachers and university professors, most due to their Jewish heritage or social-democratic and socialist political orientation. Up to 10% of all teachers were removed from duty. The massive expulsion of large amounts of teachers very likely lead to a substantial decline in the quality of education. Waldinger (2010, 2011) shows the diametral effects from the policy on higher education and

[8]Gesetz zur Wiederherstellung des Berufsbeamtentums (7th April, 1933).

science, findings in Akbulut-Yuksel and Yuksel (2015) suggest negative effects on human capital in general. From 1937 onwards, the Nazis also actively tried to affect the content of education more directly in order to indoctrinate their ideology. This included e.g. the replacement of religion by physical exercise, putting more emphasis on certain periods in history (or what was understood as history) and shift content in biology towards teaching eugenics. To what extent the Nazi government was actually successful in effectively changing curricular is not fully clear. Overall the period was rather short and the expulsion of teacher likely had greater impact on the learning environment. Furthermore, the World War itself disrupted the educational attainment of children and young adults. Ichino and Winter-Ebmer (2004) show that especially cohorts born during the 1930s experienced a great loss in human capital. Akbulut-Yuksel (2014, 2017) quantifies the adverse effects from WWII destruction on educational attainment and health. Also males born 1927 or earlier fought in the World War II. These are important and special contextual aspects which need to be taken into consideration when the long-run effects from the school starting regulation are interpreted.

6.3 Data and Sample Selection

The following empirical analysis is based on several administrative data sources. The main analysis is restricted to cohorts affected by the Prussian School Act in 1922, i.e. those born 1916–1931.

6.3.1 West German Census (1970)

In order to study effects from SSA on educational and income, I use a representative 10% sample of the 1970 German Census.[9] The Census is household based and contains information on educational qualifications and monthly net-income. I measure educational attainment separately by primary, secondary and tertiary levels of education as well as approximated years of education based on the typical length associated with the levels of education.[10] Monthly net-income is reported for each adult household member on 8 relatively broad categories. I approximate net-income by interpolating the data and assign the mean value of each category as net-income.[11]

[9] Date of Census Enumeration: 27th May, 1970.

[10] I follow the transformation used in the GSOEP based on Helberger (1988) with the exception that I assign 8 years of education for the basic lower secondary track. A 9th grade was only introduced during the 1950s and 60s.

[11] In appendix E.4.2 I present the OLS returns to education based on the approximated net-income. While the income measure is admittedly a rather crude, estimates from Mincer regressions for West Germany are surprisingly similar and comparable with earlier studies, suggesting returns to education around 8% (Krueger and Pischke, 1995; Ammermüller and Weber, 2005). As my main interest is to evaluate effects on socio-economic status rather than estimating *returns to education*, I use income of the household head instead of own income. For males the income measures are identical in 98% of all cases. In contrast, the majority of females ($>$ 60%) in 1970 had zero earnings and their own income was likely a bad proxy for their socio-economic status. Zero earnings could reflect high earnings of the spouse as well

The Census contains geographical information on the federal state (*Bundesland*) of residence in 1970 and the place of residence at the 1st September, 1939 prior to World War II. The latter contains information whether individuals lived within 1970 West-German borders, borders of the former GDR, former Eastern Prussian Provinces, former Czechoslovakia or other Eastern Europe territory prior to the War. The date of birth is captured on a monthly basis. Descriptive statistics for the main variables of the analysis are given in the upper panel of table 6.3.

As the whole Germany was not exposed to the Prussian SSA Act, I need to determine individual exposure. As a usual caveat with German administrative or survey data, the West-German Census does not contain information on the place of birth or place of residence during childhood. Therefore, I assign individual exposure based on the federal state of residence in 1970.[12] I code federal states as former Prussian if the majority of their current territory was part of Western Prussian Provinces[13] before World War II. This is rather innocuous given that many of the small areas not part of Prussia adopted the school entry rule of their larger neighbor before the overall unification of the SSA by the Education Act of 1938.[14] Figure 6.2 shows that my coding leads to a north south division of West Germany with the two southern federal states of Bavaria and Baden-Württemberg defying from the new school entry rule.[15]

The approximation by the place of residence could pose a threat to identification. Migration in general and especially World War II related refugee migration raise potential concerns for the analysis. For several reasons, I will argue that the approximation by place of residence is nevertheless a reasonable choice for determining individual exposure to the entry regulation during childhood. First, given the north-south division only long-distance migration would lead to severe misclassification while migration within a small area would usually not affect individuals' SSA regulation. Based on the GSOEP I can demonstrate in appendix E.3 that such long distance migration was typically low in West Germany. This is also in line with prior research showing relatively low levels of geographical mobility in Germany

as female employment could reflect low earnings of the household head. The household head income for females reflects a combination of assortative mating and own productivity.

[12]The place of residence in 1939 is unfortunately not sufficient to identify exposure to the Prussian SSA Act as no information on federal state level for West Germany is given. It only states that an individual lived within West German borders.

[13]The 6 *classical* Western Prussian Provinces included Saxony, Westphalia, Rhineland, Schleswig-Holstein, Hannover, Hesse-Nassau. With the exception of Saxony located in the former GDR they cover most of the northern part of West Germany. They were located west of the river Elbe.

[14]In appendix table E.3 I calculated transition the ratio of early entries across provinces. The results suggest that by 1937 also most inhabitants of non-prussian provinces in the northern part of Germany also followed the same entry rule with the cutoff at the 1st of July.

[15]I leave out the city states Bremen and Hamburg from the analysis as they have not been part of prussia and the SSA regulations are unclear. However, inclusion of the city states does not affect the results. Furthermore, I also had to exclude West-Berlin due to missing information on the month of birth.

if compared to e.g. Anglo-Saxon countries (Akbulut-Yuksel, 2017; Hochstadt, 1999; Rainer and Siedler, 2009).

World War II related migration raises further concerns as for those individuals the place of school attendance is obviously different to their current place of residence in 1970. Table 6.3 shows that with approximately 25% a sizable fraction of the inhabitants in West Germany in 1970 had a refugee migration background. Without knowledge of the former place of residence, the large number of refugees could disturb the treatment assignment by place of residence if refugees had different entry rules prior to their re-settlement. Based on the place of residence in 1939, I can show that the intake of World War II related refugees only leads to a moderate misclassification with respect to the Prussian School Act. Table 6.1 shows that refugees mainly settled in closest proximity to their origin which coincided with having the same school starting entry rules: refugees from Eastern Prussian Provinces[16] had mostly the same entry regulation. They resettled mainly to northern federal states of West Germany. Southern Eastern Europe regions, mostly the former Czechoslovakia and Romania, had different entry regulation. But here the majority resettled to the southern federal states. In the following analysis, I will use refugees from Eastern Prussian Provinces as an alternative treatment group identification in the Census. With the majority of individuals in this group being subject the to Prussian school entry regulation, they provide a robustness check for the effectiveness of the policy. In the following, I will refer to Western Prussian Provinces located in West Germany as Western Provinces and the remaining Prussian Provinces as Eastern Provinces.[17]

TABLE 6.1: Refugee Origin and Settlement

	Refugee Origin					
Residence in 1970	South-Eastern Europe		Eastern Provinces		Total	
	Obs.	%	Obs.	%	Obs.	%
Western Provinces (Treatment Group)	76,726	36.4	417,806	72.8	494,532	63.0
Southern States (Control Group)	131,082	62.2	125,359	21.8	256,441	32.7
Bremen & Hamburg	3,044	1.4	30,877	5.4	33,921	4.3
Total	210,852	100.0	574,042	100.0	784,894	100.0

Notes: Statistics refer to cohorts 1916 to 1931. Refugees from Eastern Prussian Provinces including former GDR followed in majority the Prussian school regulation from 1922. Refugees from South-Eastern Europe followed different school entry regulations.
Source: German 1970 Census. Own calculations.

Finally, foreign immigration from southern Europe to Germany increased over the 1960s. But this group only constituted a small fraction ($\approx 4\%$) of the cohorts used in this study. Also, due to restrictive naturalization laws they are identifiable in the majority of cases simply by nationality in the Census and can be excluded from the analysis.

[16]The 6 *classical* Eastern Prussian Provinces included Brandenburg, Pomerania, Silesia, Posen, West Prussia, East Prussia. They were located east of the river Elbe.

[17]This is accurate except for the province Saxony: Formerly a Western Prussian Provinces it was located in the former GDR after World War II.

Overall I expect the assignment by place of residence leading only to small misclassification in exposure. Following the above exposition I will split my analysis into three groups: Individuals reside in northern federal states (1st treatment group, mainly former Western Prussian provinces or same cutoff regulation), southern federal states (control group, not part of Prussia, different entry regulation) and refugees from Eastern Provinces or the former GDR (2nd treatment group, mainly former eastern Prussian provinces or same cutoff regulation).

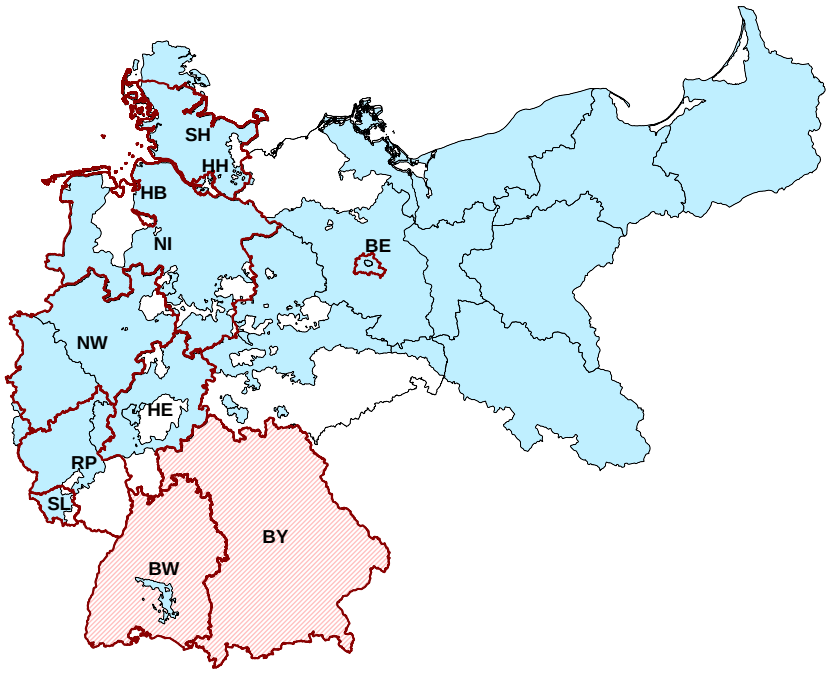


FIGURE 6.2: West German Federal States (1970) and Prussian Provinces (1914)

Notes: The colored area shows the Prussian territory in 1914 within the German Empire. The red-rimmed areas present the West-German federal states in 1970.
Abbreviations: SH Schleswig-Holstein, HH Hamburg, HB Bremen, NI Niedersachsen, BE Berlin, NW Nordrhein-Westfalen, HE Hessen, RP Rheinland-Pfalz, SL Saarland, BW Baden Württemberg, BY Bayern.
Treatment group: Majority of territory was Prussian territory before World War II (SW, NI, NW, SL, HE, RP), **Control group:** Southern Germany with only minor part / no part was Prussian territory before World War II (BW and BY).

6.3.2 Health Insurance Data

The analysis of later life health outcomes is based on administrative health insurance data from one group of statutory health insurer (SHI), covering the years 2006–2011 and a fraction of 10% of the total statutory insured population in Germany. For the cohorts 1916–1931 exposed to the Prussian school starting age regulation, individuals are aged 75–90 years at entry and have a follow up period of 6 years. The data covers individual information on all-cause mortality, frequency of hospitalization and primary and secondary inpatient diagnoses based on ICD-10 Codes. I exploit geographic information on the place of residence on municipality level. The date of birth is available on the exact daily basis. In line with the above reasoning from the 1970 West-German Census, I assign exposure to the reform when the current municipality of residence in majority was located in former Prussian territory.[18]

To identify dementia incidence, I rely on the specific inpatient diagnoses. All-cause and cause specific hospitalization is measured as the accumulated incidences over the sample period. In order to construct dementia cases based on inpatient data from insurance claims I follow the approach of Schwarzkopf et al. (2012) and use all hospitalizations associated with a dementia diagnosis as primary and secondary diagnosis ICD-10 Codes. All relevant ICD-10 codes are listed in table 6.2. I denote a dementia diagnoses for individual i at date t with ICD_{it}^{Dem}. Whenever at least one of these ICD-10 codes was reported within the sample period from 2006–2011, the corresponding person was identified as a suffering from dementia. As misclassification between types of dementia is common in inpatient data (Rizzuto et al., 2018), I do not differentiate between alzheimer disease and other forms of dementia. Thus, my indicator for dementia d_i is defined as

$$d_i = \mathbb{1}\left(\sum_{t=1.1.2006}^{31.12.2011} \mathbb{1}\left(ICD_{it} = ICD_{it}^{Dem}\right) > 0\right).$$

In the majority of cases dementia is a secondary condition.[19]

Additionally, I also investigate all-cause mortality. In construct a binary indicator m_i for the risk of dying over the sample period. If T_i^{Death} is the date

[18] Figure 6.3 maps former Prussian territory to municipalities in 2006. I include also East-German municipalities in the main analysis if they were located in Prussia. Their exclusion does not substantially affect the results.

[19] Validation studies using linked Swedish administrative data suggest that inpatient data exhibits about 60% detection rate for actual dementia cases (*sensitivity*) and an almost perfect *specificity* (Jin et al., 2004; Rizzuto et al., 2018). The results are likely to be transferable to my application as my dementia indicator also relies on the inpatient diagnoses and similar birth cohorts. Also, both countries exhibit a comparable skill level of health care professionals in hospitals. Overall, diagnostic errors should not be fundamentally different between the countries and I expect very similar detection rates of dementia cases also from German inpatient data. The results from the validation study suggests that I will probably underestimate the prevalence of dementia but can exclude the possibility of having a high rate of false positive cases in the hospitalization data. A more problematic result in Rizzuto et al. (2018) is that education potentially shortens the time from the onset of dementia measured by clinical manifestation to the detection in inpatient data. Given right-censoring of my data, an earlier detection of dementia could bias effects from higher education in the direction of an

of death i measure all cause mortality by

$$m_i = \mathbb{1}\left(T_i^{Death} \leq 31.12.2011 | T_i^{Death} \geq 01.01.2006\right).$$

As a potential caveat, my data only contains statutory health insured individuals. With approximately 90%, most of the population are covered by public insurance. But the German health care system allows for employees with very high annual income[20], civil servants and self-employed to opt out of the statutory health insurance and choose private health insurance (*sickness funds*). Higher educated individuals are typically more likely to be privately insured given they have higher income as well as are more likely to work as civil servants. As the decision to opt out of the statutory health insurance is also related to bad health risks (Bünnings and Tauchmann, 2015), an increase in education leading to higher income and different occupations might lead to an increase of bad risks in the pool of statutory health insured individuals. This fact would also introduce a negative bias in the effects of education on dementia risk as well as on mortality. Although I cannot test this directly based on the health insurance data alone, I will provide indirect evidence in the appendix E.2 that the school starting age regulation did not increase the group allowed to opt out. I do not detect an increase in the number of public servants/self-employed or in the highest income group from an early SSA. Thus, I argue the insurance status was likely also not affected.

TABLE 6.2: ICD codes used to identify dementia hospitalisations

DIAGNOSIS	ICD 10 CODE
Mental disorders due to known physiological conditions	
F00	Dementia in Alzheimer disease
F01	Vascular dementia
F02	Dementia in other diseases classified elsewhere
F03	Unspecified dementia
Other degenerative diseases of the nervous system	
G30	Alzheimer disease

Notes: International Statistical Classification of Diseases and Related Health Problems 10th Revision, 2016.

Descriptive statistics are given in the lower panel of table 6.3.

increase in dementia risk. This bias is probably small given that correlation studies suffering from the same problem nevertheless suggest a large protective effect of education when using inpatient data.

[20]The annual income threshold (*Versicherungspflichtgrenze*) is adjusted for inflation but lies by roughly 90th percentile of the annual income distribution.

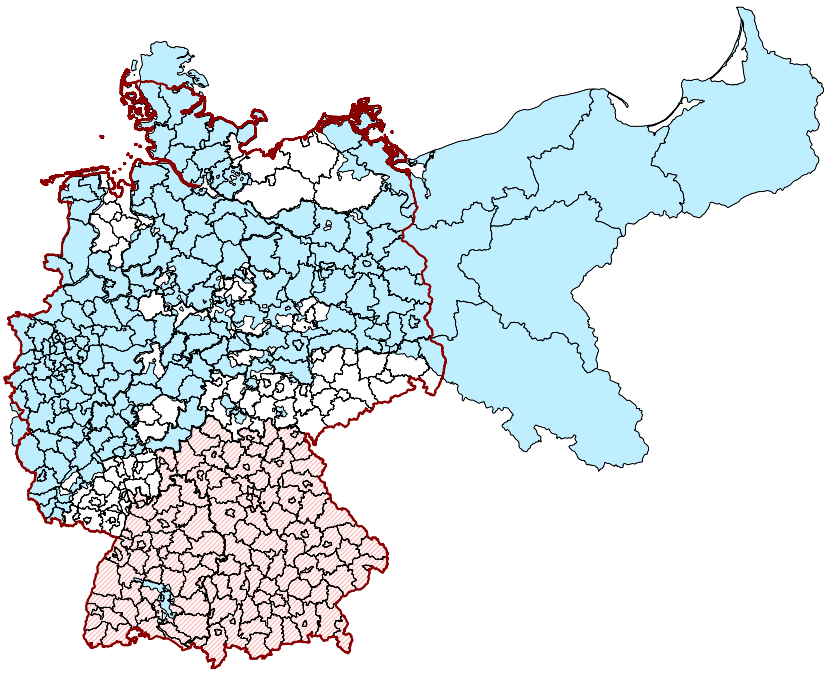


FIGURE 6.3: German Municipalities (2006) and Prussian Provinces (1914)

Notes: The colored area shows the Prussian territory in 1914 within the German Empire. The red-rimmed areas present current boarders of Germany. Thin lines represent current German districts.
Treatment group: Current district was Prussian territory before World War II. **Control group:** Current district in southern Germany and not Prussian territory before World War II.

TABLE 6.3: Descriptive Statistics

	Treatment						Placebo		
	WESTERN PROVINCES			EASTERN PROVINCES			BAVARIA & BADEN-WÜRTTEMBERG		
	Mean	Std.Dev.	Obs	Mean	Std.Dev.	Obs	Mean	Std.Dev.	Obs
GERMAN CENSUS 1970									
More than Basic Track	0.16	0.37	667,086	0.16	0.37	138,782	0.17	0.37	341,825
Highest Secondary Track	0.06	0.23	667,086	0.05	0.22	138,782	0.06	0.24	341,825
University Degree	0.04	0.19	667,144	0.03	0.18	138,792	0.04	0.20	341,866
Years of Education	9.44	2.05	667,086	9.35	1.98	138,782	9.42	2.16	341,825
WWII Refugee	0.26	0.44	667,144	1.00	0.00	138,792	0.25	0.43	341,866
Male	0.45	0.50	667,144	0.47	0.50	138,792	0.45	0.50	341,866
Houshold Income 1970	1,124.35	762.55	667,144	1,093.15	679.53	138,792	1,118.97	794.95	341,866
Married	0.93	0.25	667,144	0.93	0.26	138,792	0.91	0.29	341,866
INSURANCE CLAIMS DATA									
Crude Mortality Rate	0.314	0.464	387,573	-	-	-	0.304	0.460	153,587
Dementia Rate	0.146	0.353	387,573	-	-	-	0.138	0.345	153,587
# Hospitalizations	2.022	3.041	387,573	-	-	-	1.969	2.956	153,587
Male	0.391	0.488	387,573	-	-	-	0.399	0.490	153,587
Age 2006	79.745	3.954	387,573	-	-	-	79.812	3.940	153,587

Notes: Descriptive statistics refer to cohorts 1916 to 1931.
Source: West German 1970 Census, Health Insurance Claims Data 2006–2011. Own calculations.

6.3.3 Military Conscription Records

In order to study direct effects related to World War II I rely on military conscription records from armed forces in Nazi Germany. A full description of the data is given in Rass and Rohrkamp (2009). The data base covers 18,536 almost complete military records for the male population liable to military service, born 1878–1930. The data includes information on the exact date of birth as well as detailed information on World War II related events such as death in combat, missing and begin of duty. Additionally, a wide range of socio-economic background variables is including such as information on parental occupation, family composition and the place of birth. Therefore, the conscription data can also be used in order to study effects of seasonality in births and assess the randomness of the entry cutoff date. Individuals come from more than 4,000 rural and urban places with the vast majority located in Prussia. I drop individuals with incomplete information on date of birth as well as those born outside of Prussian provinces. This leaves me with a final sample of 14,801 observations.

6.4 Empirical Strategy

6.4.1 Reduced Form Estimation

To identify the reduced-form effects of *school starting age* (SSA) on long run outcomes I employ a *regression discontinuity design* (RDD) (Imbens and Lemieux, 2008). As children had to start school based on their date of birth, the day of birth gives the running R_{ic} variable for the RD design. I normalize the day of birth to zero around the school entry date at the 1st of July.

For long term outcome variables Y_i the regression equation of interest is given by

$$Y_i = \beta_0 + \beta_1 D_i + \beta_2' x_i + f(R_i) + \eta_i, \tag{6.1}$$

where $f(R_i)$ a flexible polynomial in the day of birth, D_i is an indicator such that $D = 1$ for individuals born after the 1st of July and 0 otherwise. x_i controls for gender and birth cohort effects. Following the standard recommendation in the literature, I cluster standard errors at the level of the running variable (Lee and Card, 2008). If the exact date of birth is not available I restrict the sample to births in the months June and July. This is equivalent to a nonparametric RD design with a local average and a one month window around the threshold in regression equation 6.1 and an uniform kernel. As in this case cluster-based inference with only two clusters as running variable is not feasible, I present robust standard errors.

The coefficient β_1 measures a discontinuity in the outcome variables for individuals born on either side of the school entry date. In case the school entry law would have been fully binding, β_1 identifies the effect of starting school one year later. In the case of non-compliance to the rule, β_1 captures the effect of the school entry regulation and gives an intent-to-treat parameter. It measures the effect of the relative age gap at entry.

In order to allow a better interpretation of the reduced form estimates, knowledge on compliance with the school starting age rule is clearly beneficial. I am not able to directly evaluate effects on school starting age from the school entry rule due to the lack of individual level data for the cohorts born before World War II in Germany. However, based on aggregated historical records on enrollment into primary education, I can show in appendix E.1 that compliance was likely to be very high. Only about 8% of the students did not enter school according to the cut-off rule. The relative high compliance is in stark contrast to more recent cohorts in Germany. During the 1970s more than 25% of each school cohort did not enter school according to the cut-off (Rüdiger, Kormann and Peez, 1976).[21]

When conceptualizing how an early SSA potentially affects long-run cognitive health, effects could be either direct or mediated through education and further later-life outcomes themselves, which may be affected by an early school start. A direct influence could stem from long-lasting effects on the psychological well-being of students due to stress as a result of the relative age gap in school. For instance, earlier school entry was found to be associated with higher risk of a ADHD diagnosis[22] while, at the same time, there exists evidence that ADHD is also positively associated with dementia risk (Golimstok et al., 2011). Indirect effects could arise if initial differences in school achievements due to the relative age gap transmit throughout the life cycle. As the German school system tracks students early at grade 4 based on achievements, relative age is likely to have effects on the tracking decision. Depending on the ability of the school system to correct misallocation at later stages, this initial relative age effect might transmit to later career stages in life. Figure 6.4 depicts a simple illustration on the causal chain between SSA and health later in life.

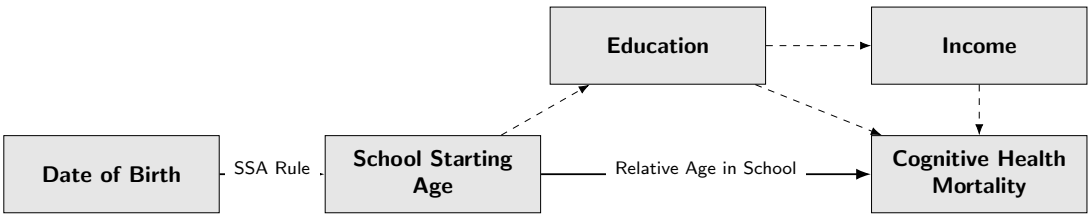


FIGURE 6.4: Graphical Causal Path Diagramm

[21]This was partly due to postponed entry due to the lack of readiness. The majority however entered school earlier due to a transition period from July to December allowing to enter the current year even if born after the cut-off date. Apparently many parents send their children to school earlier. In May 1970 60% of the students born in July of 1963 had already entered school.

[22]The increase in ADHD diagnoses could also represent an over-diagnosis of ADHD cases rather than actual ADHD cases (Elder, 2010; Evans, Morrill and Parente, 2010). Non-conform behavior due to the relative age gap compared to the rest of the class leads to misclassification with ADHD. Note, that this does not rule out negative psychological effects from stress induced by the relative age disadvantage in school. The misclassification is more a result of stress related behavior. In the historical context of this paper, ADHD was also not a known diagnosis. Alternative treatments for children with disciplinary problems were likely harsh and could cause long-lasting adverse effects on mental health.

6.5 Results

6.5.1 Validity of the Research Design

In order to allow a causal interpretation of the reduced form estimates based on the school entry rule, births around the cutoff need to be randomly distributed. Especially, I want to exclude that results a driven by seasonality in births. There exists an extensive literature showing that season of birth captures important socio-economic background information (see e.g. Buckles and Hungerman, 2013). I present empirical evidence that my results are unlikely to be driven by seasonality. First, I compare individuals' socio-economic characteristics prior to school entry around the cut-off date based on the West-German Census and conscription data for the exposed cohorts 1916–1931. I estimate my regression discontinuity specifications for outcome variables which should not be affected by the school starting regulation. Results from such balancing regressions are given in table 6.4 for Western Provinces. For all background variables I estimate differences between individuals born in June vs. July. The conscription data also exhibits the exact date of birth allowing for more precise estimation around the cutoff. Independently of specification and sample choice I do not find statistically significant difference between individuals born before and after 1st July, with the exception for the probability of fathers being deceased at conscription age. This significant result likely arises due to multiple testing given many background variables and specifications.

As the 1970 Census does not cover a wide range on background variables and is especially missing information on the parents for the cohorts affected by the Prussian School Act, I additionally estimated the effects of the cutoff date for cohorts born between 1958 and 1969. Those cohorts were subject to the same cut-off date and reside together with their parents during the Census in 1970 which allows to identify parental characteristics.[23] Again, I cannot detect any indication of systematic differences around the cutoff.

[23]While external effects from season of birth such as infectious diseases are less likely to affect those cohorts due to better medical treatment, I would expect manipulation of births being more pronounced given that consequences of school starting age were much better understood and improved methods of birth control were available.

TABLE 6.4: Balancing Regression (Western Provinces)

	(1) **Obs.**	(2) **Mean** *Outcome*	(3) **Month of Birth** *June/July no Poly.*	(4) **Day of Birth** *Full Sample* 2nd order Poly.	(5) *2-Months*
PANEL A: CENSUS 1970 (COHORTS 1916 – 1931)					
Male	1,132,492	0.514	0.003	–	–
			(0.003)		
WWII Refugee	1,132,492	0.155	0.002	–	–
			(0.003)		
Catholic	1,132,492	0.491	0.000	–	–
			(0.003)		
Non-Christian	1,132,492	0.031	-0.000	–	–
			(0.001)		
PANEL B: INSCRIPTION DATA (COHORTS 1916 – 1930)					
Father Died (Age 18)	8,045	0.132	-0.029	-0.020*	-0.033*
			(0.019)	(0.011)	(0.020)
Mother Died (Age 18)	8,045	0.070	-0.010	-0.007	-0.013
			(0.013)	(0.008)	(0.014)
Father High Skilled	7,406	0.263	-0.021	-0.012	-0.009
			(0.023)	(0.014)	(0.025)
Father Farmer	7,406	0.021	-0.005	-0.003	-0.007
			(0.008)	(0.005)	(0.008)
Number of Siblings	8,657	2.770	0.089	0.054	0.096
			(0.130)	(0.082)	(0.142)
Catholic	8,657	0.526	-0.042	-0.028	-0.039
			(0.029)	(0.017)	(0.030)
Born in Urban Area	8,657	0.474	0.017	-0.006	0.032
			(0.025)	(0.015)	(0.027)
PANEL C: CENSUS 1970 (COHORTS 1958 – 1969)					
Years of Education (Father)	1,115,229	8.498	0.003	–	–
			(0.006)		
Years of Education (Mother)	1,103,409	8.315	-0.001	–	–
			(0.005)		

Notes: Table shows estimated effects of cut-off date (1st July) on pre-schooling characteristics. Robust standard errors are clustered on day of birth. If exact day of birth was not available robust standard error were calculated. All regressions control for year of birth fixed effects. Significance levels: $*$ 0.10 $**$ 0.05 $***$ 0.01.
Columns: (1) Number of Observations (full sample) (2) Mean of Outcome (4) Regressions are restricted to a 1-month window before and after the cut-off date (June vs. July). (5) Full sample with polynomial of 5th degree in day of birth (6) Regressions are restricted to a 2-month window before and after the cut-off date. Separate linear regression in day of birth around cut-off date.
Source: West German Census, 1970. Conscription Data, Armed Forces, 1935-1945. Own calculations.

Secondly, I compare socio-economic outcomes for slightly younger cohorts which were also born around the 1st of July but subject to a different entry regulation. Individuals born between 1935–1939 constitute a excellent placebo group as they are of comparable age in 1970 but their cutoff date was shifted to the end of the year across Nazi Germany.[24] If effects from the school starting regulation were spuriously driven by seasonality, they should also be detectable for those cohorts. Reassuringly, results in table 6.5

[24] After WWII federal states deviated from the common entry rule. Thus for cohorts born 1940 and later entry rules began to differ again across regions.

indicate no significant differences between individuals born in July and June with respect to their educational attainment or income in 1970 for the placebo cohorts.

TABLE 6.5: Placebo Cohorts

	Treatment		Control
	WESTERN PROVINCES	EASTERN PROVINCES	BAVARIA & BADEN-WÜRTTEMBERG
YEARS OF SCHOOLING			
1st July (Early SSA)	-0.000	0.006	0.038
	(0.020)	(0.040)	(0.029)
Observations	45,534	13,994	24,484
Mean Dep. Var.	9.700	9.919	9.670
MORE THAN COMPULSORY			
1st July (Early SSA)	0.001	-0.001	0.002
	(0.004)	(0.007)	(0.005)
Observations	45,534	13,994	24,484
Mean Dep. Var.	0.177	0.206	0.179
HOUSEHOLD LOG-INCOME			
1st July (Early SSA)	-0.001	0.006	0.004
	(0.004)	(0.007)	(0.006)
Observations	42,626	13,445	22,702
Mean Dep. Var.	6.946	6.949	6.913

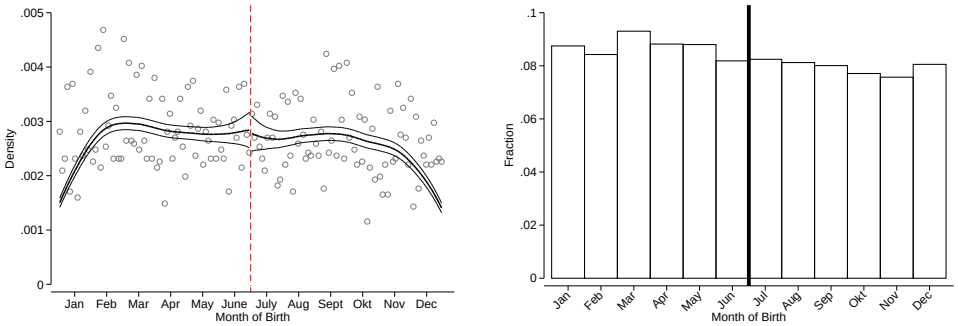
Notes: Regressions are restricted to a 1-month window before and after the cut-off date (June vs. July). All regressions control for cohort and gender fixed effects.
Results refer to cohorts 1935–1939. Robust standard errors are given in parenthesis.
Treatment (Western Provinces) refers to federal states which in 1970 consist mainly out of former prussian regions. **Treatment** (Eastern Provinces) refers to individuals which lived in Eastern Prussia before World War II (refugee migrants). **Control** units are federal states without former prussian regions.
Source: West German Census, 1970. Own calculations

Finally, I want to exclude the possibility of systematic manipulation of births by parents. Hypothetically, parents could try to plan their fertility in order to take advantage of a later or earlier school starting age of their child. In the current setting manipulation of the date of birth, however, is extremely unlikely for several reasons. First, note that the discussion about potential disadvantage from an earlier school starting age mainly started after WWII as a *consequence* of the policies investigated in this study. Parents would have had predicted effects before those became common knowledge. Furthermore, during the 1920s possibilities of birth control and timing of birth were very limited.

In order to empirically assess the above reasoning, I test for any discontinuities in the distribution of births around the cut-off date based on McCrary (2008). In the absence of historical birth records, I turn to the distribution of birth dates in the conscription data and the 1970 West-German Census. The

figures 6.5(a) and 6.5(b) suggest the absence of systematic differences in the number of births around the 1st July. Additionally, formal tests of differences around the cut-off date given in table 6.6 do not reject the null hypothesis of no discontinuity.[25]

Overall, I do not find supporting evidence against the validity of the research design. It is unlikely that results are mainly driven by seasonality in births.



(a) (b)

FIGURE 6.5: Distribution of Births: (**a**) Density Day of Birth (Males, at conscription) ; (**b**) Histogram Month of Birth 1970

Notes: Figure 6.5(a) gives graphical representation of distribution of day of birth at conscription age. Figure 6.5(b) shows the distribution of individuals by birth month in 1970. Results refer to cohorts 1916 to 1931 born/residing in Western Provinces.
Source: West German Census, 1970. Conscription Data, Armed Forces, 1935-1945. Own calculations.

TABLE 6.6: McCrary Density Test

	DAY OF BIRTH (MALES, AT CONSCRIPTION)		Month of Birth (1970)	
	Difference	$p-$value	Difference	$p-$value
1st July (Early SSA)	-0.024	0.609	0.004	0.901
	(0.087)		(0.014)	

Notes: Table shows estimated difference in log density of day of birth around the cut-off date (1st July). $p-$value for McCrary-Test with H_0: No Discontinuity of the distribution around the cut-off. Results refer to cohorts 1916 to 1931. Estimated standard errors in parenthesis.
Source: West German Census, 1970. Conscription Data, Armed Forces, 1935-1945. Own calculations.

[25]Following Frandsen (2017), for results based on the distribution of month of birth in the 1970 Census I take the discrete nature of the running variable into account. This allows for small departures from exact uniform distribution of the discrete running variable. I chose the linearity parameter allowing for seasonality according to the federal states which did not implement the school starting age cuf-off date.

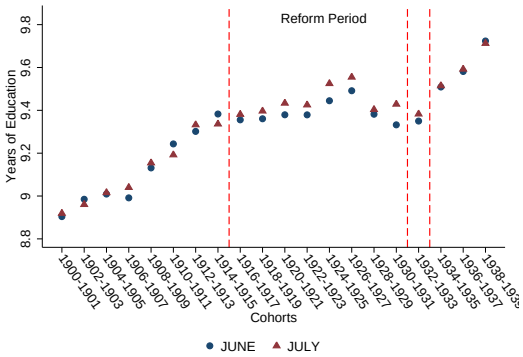
6.5.2 SSA Regulation and Educational Attainment

Figures 6.6(a) and 6.6(b) plot the evolution of years of education and from residuals based on a regression of years of education regressed on a cubic cohort trend separately by month of birth for residence in Western Provinces in 1970. In line with earlier research, the overall picture shows that cohorts born in the end of the 1920s and early 1930s enrolled in school experience the greatest decline in human capital accumulation during World War II (Ichino and Winter-Ebmer, 2004). Additionally the figures reveal that for individuals born between 1916–1931 educational attainment is systematically larger for individuals born in July compared to those born in June. Those individuals were subject to the Prussian school entering legislation setting the 1st July as cutoff date. Interestingly, the cohorts 1932 and 1933 show only a small difference between June and July born, even though the School Act from 1938 set the same cut-off date. But the School Act also introduced the possibility of an earlier school start, potentially reducing the bite of the school entry regulation for those cohorts. Different entry rules applied for cohorts born before 1916 or after 1933. Reassuring, the figure does not reveal systematic differences between the two birth months for those cohorts.

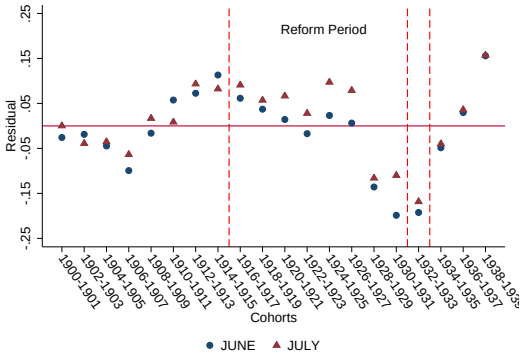
6.5.3 Regression Results

Educational Attainment and Earnings

Based on the 1970 Census, I had defined three geographical regions: individuals reside in northern federal states (exposed to the Prussian SSA), southern federal states (control/placebo group) and refugees from Eastern Provinces or the former GDR (exposed to the Prussian SSA). Table E.4 shows the main results for socio-economic outcomes in 1970 for each group. In the treatment groups being born in July instead of June is associated with a significant increase of 0.05 years of education and an increase by 1 percentage point in completing more than basic secondary education. Given the overall low level of education, with 80–85% only attending the basic compulsory secondary school track, the effects are sizable in relative terms. Given a baseline of only 15–20% having more than compulsory secondary school, being born in July raises the odds of gaining at least a middle school degree by 5% or more. Table E.4 also shows a small but statistically significant increase in household head net-income from being born on the right side of the cutoff. The size of the income effects is compatible with general OLS returns to education on earnings close to 10% and the estimated increase in years of education of 0.05 years. For the southern control states all estimates are statistically insignificant and basically zero. Figure 6.7 gives a graphical presentation of month of birth effects for years of education. The figures shows a clear discontinuity for the treatment groups as well as the absence of such in the control states.



(a)



(b)

FIGURE 6.6: Difference July vs. June over Time in Western Provinces

Notes: Figure 6.6(a) plots years of education over cohorts. Figure 6.6(b) plots residuals from a regression of years of education regressed on a cubic cohort trend. Sample is restricted to northern federal states mainly consisting of former Western Prussian Provinces.
Source: West German Census, 1970. Own calculations.

Dementia and Mortality

Table 6.8 shows the main results on dementia risk, all-cause mortality and all-cause hospitalization, separated by my treatment and control assignment. In order to balance the tradeoff between efficiency and bias, I provide regressions for the full population as well as a more narrow 2-month bandwidth around the cutoff date. The running variable is included as a second order polynomial, separately before and after the cutoff. I also add the simple month of birth comparison used for the Census data. Appendix E.4.1 provides further evidence on the robustness of the results with respect to the specification choice for the running variable.

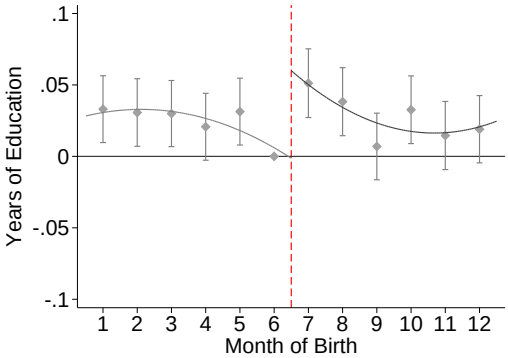
Overall, I do not find supporting evidence on dementia risk in both groups, except for using the largest possible bandwidth in the treatment group. This result is, however, not robust to alternative specifications of the running variable. In contrast, I find a statistically significant and robust reduction in all-cause mortality in the treatment group. The size of the effect is large with a 0.5–0.8 percentage point decrease given a baseline mortality of 30 percent.

TABLE 6.7: Main Results: Education and Income

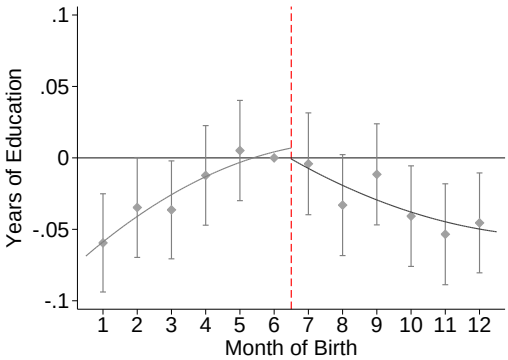
	Treatment		Control
	WESTERN PROVINCES	EASTERN PROVINCES	BAVARIA & BADEN-WÜRTTEMBERG
YEARS OF EDUCATION			
1st July (Early SSA)	0.051***	0.068***	-0.004
	(0.012)	(0.024)	(0.018)
Observations	107,682	32,984	55,305
Mean Dep. Var.	9.448	9.608	9.453
MORE THAN COMPULSORY			
1st July (Early SSA)	0.009***	0.011**	0.001
	(0.002)	(0.004)	(0.003)
Observations	107,682	32,984	55,305
Mean Dep. Var.	0.164	0.202	0.172
NET INCOME			
1st July (Early SSA)	0.007**	0.004	0.001
	(0.003)	(0.005)	(0.005)
Observations	94,994	30,216	47,958
Mean Dep. Var.	6.939	6.918	6.903

Notes: Regressions are restricted to a 1-month window before and after the cut-off date (June vs. July). All regressions control for cohort and gender fixed effects.
Results refer to cohorts 1916 to 1931. Robust standard errors are given in parenthesis.
Significance levels: $*$ 0.10 $**$ 0.05 $***$ 0.01.
Treatment (Western Provinces) refers to federal states which in 1970 consist mainly out of former prussian regions. **Treatment** (Eastern Provincesn) refers to individuals which lived in Eastern Prussia and former GDR before World War II (refugee migrants). **Control** units are federal states without former prussian regions.
Source: West German Census, 1970. Own calculations.

Figure 6.8 gives a graphical illustration of the effects on all-cause mortality. There is no effect detectable on all-cause hospitalization, although the sign is also negative for the treatment group. For the control group I cannot reject the null hypothesis across all specification and outcomes.



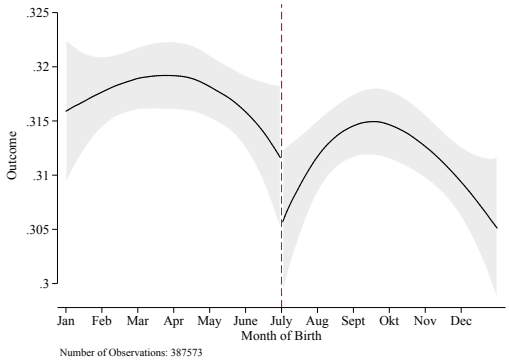
(a)



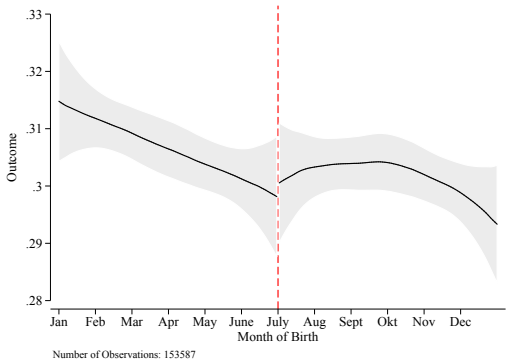
(b)

FIGURE 6.7: Years of Education: (**a**) Western Provincesn Territory (Treated) (**b**) South Germany (Control)

Notes: The figure plots the effects from a regression of years of education on month of birth indicators. CIs are based on robust standard errors. A quadratic fit in month of birth is added. Results refer to cohorts 1916 to 1931. Controls for gender and birth cohort are included.
Treated (Western Provinces) refers to individuals living in federal states in 1970 which consist mainly out of former prussian regions. **Control** units are individuals living in federal states without former prussian regions.
Source: West German Census, 1970. Own calculations.



(a)



(b)

FIGURE 6.8: All-Cause Mortality: (**a**) Western Provinces ; (**b**) Bavaria & Baden-Württemberg

Notes: Figure shows separate polynomial regression around cut-off date (1st July) on all-cause mortality. Robust standard errors are clustered on day of birth. Estimates are bases on full sample with separate polynomial of 2nd degree in day of birth before and after cutoff. All regressions control for cohort and gender fixed effects.
Source: Health Insurance Claims Data, 2006-2011. Own calculations.

TABLE 6.8: Main Results: Dementia/Mortality

	(1)	(2)	(3)	(1)	(2)	(3)
	WESTERN PROVINCES			BAVARIA & BADEN-WÜRTTEMBERG		
	Month of Birth	**Day of Birth**		**Month of Birth**	**Day of Birth**	
	June/July	*Full Sample*	*2-Months*	*June/July*	*Full Sample*	*2-Months*
	no Poly.	2nd order Poly.		*no Poly.*	2nd order Poly.	
DEMENTIA RISK						
1st July (Early SSA)	-0.0011	-0.0036**	-0.0004	0.0034	0.0023	0.0047
	(0.0032)	(0.0018)	(0.0032)	(0.0037)	(0.0050)	(0.0041)
Observations	63,040	387,573	128,047	24,890	153,587	50,657
Mean Dep. Var.	0.147	0.146	0.147	0.142	0.138	0.140
CRUDE MORTALITY RISK						
1st July (Early SSA)	-0.0084**	-0.0089***	-0.0074*	0.0023	0.0007	0.0014
	(0.0036)	(0.0022)	(0.0038)	(0.0058)	(0.0079)	(0.0061)
Observations	63,040	387,573	128,047	24,890	153,587	50,657
Mean Dep. Var.	0.310	0.314	0.313	0.302	0.304	0.303
# HOSPITALIZATIONS						
1st July (Early SSA)	-0.0213	-0.0192	-0.0299	0.0067	-0.0257	0.0081
	(0.0206)	(0.0147)	(0.0216)	(0.0370)	(0.0513)	(0.0396)
Observations	63,040	387,573	128,047	24,890	153,587	50,657
Mean Dep. Var.	2.051	2.022	2.039	1.961	1.969	1.976

Notes: Table shows estimated effects of cut-off date (1st July) on later life health outcomes. Robust standard errors are clustered on day of birth. If exact day of birth was not available robust standard error were calculated. All regressions control for cohort and gender fixed effects. Significance levels: $*$ 0.10 $**$ 0.05 $***$ 0.01.

Columns: (1) Regressions are restricted to a 1-month window before and after the cut-off date (June vs. July). (2) Full sample with separate polynomial of 2nd degree in day of birth before and after cutoff. (3) Regressions are restricted to a 2-month window before and after the cut-off date. Separate polynomial of 2nd degree in day of birth before and after cutoff.

Source: Health Insurance Claims Data, 2006-2011. Own calculations.

6.5.4 World War II and Gender Heterogeneity

A priori, it is not obvious how World War II interacts with early life circumstances and how it affects inequalities. Theoretically, it is possible that the massive negative shock especially hurt the already disadvantaged and catalyses the initial negative effects. On the other hand, it could be difficult for privileged individuals to capitalize their advantages and the war could act as a *great equalizer to the bottom*. For health outcomes, recent evidence for Germany supports the first hypothesis. Akbulut-Yuksel and Yuksel (2017); Blum and Strebel (2016) show that for both World Wars children from lower socio-economic status suffered the most in terms of long-term health outcomes such as height.

With respect to initial disadvantages in education from an earlier school start and World War II, my analysis suggests that the war increased initial inequalities. Figure 6.6(b) shows that the adverse effects from entering school earlier were largest for cohorts in school during World War II. Previous research has shown that their educational attainment was most negatively affected by the events (Akbulut-Yuksel, 2014, 2017). Similarly to the increased health inequalities, the war led to a greater inequality in educational attainment by an earlier school entry. When I estimate the school starting effects for cohorts separately, figure 6.6 reveals the same cohorts born in the mid and late 1920s are most affected by the school starting regulation. Those cohorts were in the crucial transition period to middle school and higher track during World War II.

Regarding gender heterogeneity, the results are rather similar between females and males. The smaller effects on years of education for females is probably due to the absence of tertiary education for the majority of females. The effects of having more than compulsory education are of similar size. The health effects are also similar, with some loss in precision. For males, I find individuals born after the cut-off served slightly less in World War II without finding evidence of an increased survival.

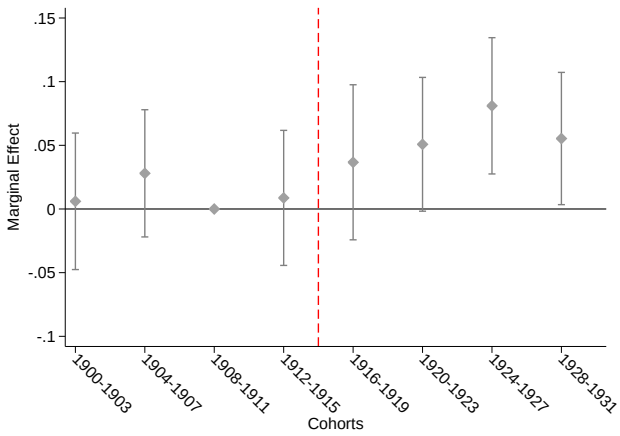


FIGURE 6.9: Event-Study Graph (RDD, Years of Education)

Notes: The figure plots regression coefficients from cut-off date (1st July) interacted with cohorts on years of education for Western Provinces. Regression controls for gender fixed effects and a cubic trend in age.
Source: West German Census, 1970. Own calculations

TABLE 6.9: Heterogeneity by Gender for Education and Income

	MALES			FEMALES		
	Treatment		**Control**	**Treatment**		**Control**
	WESTERN PROVINCES	EASTERN PROVINCES	BAVARIA & BADEN-WÜRTTEMBERG	WESTERN PROVINCES	EASTERN PROVINCES	BAVARIA & BADEN-WÜRTTEMBERG
YEARS OF EDUCATION						
1st July (Early SSA)	0.071***	0.113***	0.012	0.035**	0.026	-0.018
	(0.020)	(0.038)	(0.031)	(0.015)	(0.030)	(0.021)
Observations	49,295	15,610	24,988	58,387	17,374	30,317
Mean Dep. Var.	9.908	10.023	9.982	9.060	9.234	9.018
MORE THAN COMPULSORY						
1st July (Early SSA)	0.010***	0.014**	0.002	0.008***	0.008	0.001
	(0.003)	(0.006)	(0.005)	(0.003)	(0.006)	(0.004)
Observations	49,295	15,610	24,988	58,387	17,374	30,317
Mean Dep. Var.	0.182	0.209	0.193	0.150	0.195	0.155
HOUSEHOLD INCOME						
1st July (Early SSA)	0.005	0.014**	-0.003	0.009*	-0.005	0.004
	(0.004)	(0.007)	(0.006)	(0.005)	(0.008)	(0.007)
Observations	45,742	14,984	22,625	49,252	15,232	25,333
Mean Dep. Var.	6.984	6.964	6.970	6.898	6.873	6.844

Notes: Regressions are restricted to a 1-month window before and after the cut-off date (June vs. July). All regressions control for cohort and gender fixed effects.
Results refer to cohorts 1916 to 1931. Robust standard errors are clustered on year-month of birth cells. Significance levels: $*$ 0.10 $**$ 0.05 $***$ 0.01.
Treatment (Western Provinces) refers to federal states which in 1970 consist mainly out of former prussian regions. **Treatment** (Eastern Provincesn) refers to individuals which lived in Eastern Prussia before World War II (refugee migrants). **Control** units are federal states without former prussian regions.
Source: West German Census, 1970. Own calculations

TABLE 6.10: Heterogeneity by Gender for Health Outcomes

	MALES			FEMALES		
	Month of Birth	**Day of Birth**		**Month of Birth**	**Day of Birth**	
	June/July	*Full Sample*	*2-Months*	*June/July*	*Full Sample*	*2-Months*
	no Poly.	2nd order Poly.		*no Poly.*	2nd order Poly.	
Dementia Risk	0.0015	-0.0004	0.0029	-0.0027	-0.0010	-0.0026
	(0.0042)	(0.0025)	(0.0044)	(0.0038)	(0.0049)	(0.0039)
Observations	24,724	151,387	50,021	38,316	236,186	78,026
Mean Dep. Var.	0.129	0.128	0.128	0.159	0.158	0.159
Crude Mortality Risk	-0.0085	-0.0100***	-0.0067	-0.0085*	-0.0077	-0.0080
	(0.0056)	(0.0035)	(0.0058)	(0.0046)	(0.0066)	(0.0050)
Observations	24,724	151,387	50,021	38,316	236,186	78,026
Mean Dep. Var.	0.343	0.349	0.346	0.289	0.292	0.292
# Hospitalizations	-0.0098	-0.0196	-0.0190	-0.0275	-0.0576	-0.0369
	(0.0325)	(0.0240)	(0.0351)	(0.0274)	(0.0361)	(0.0282)
Observations	24,724	151,387	50,021	38,316	236,186	78,026
Mean Dep. Var.	2.188	2.163	2.184	1.963	1.931	1.946

Notes: Table shows estimated effects of cut-off date (1st July) on later life health outcomes separated by gender. Robust standard errors are clustered on day of birth. If exact day of birth was not available robust standard error were calculated. All regressions control for cohort fixed effects. Significance levels: $*$ 0.10 $**$ 0.05 $***$ 0.01.
Columns: (1) Regressions are restricted to a 1-month window before and after the cut-off date (June vs. July). (2) Full sample with separate polynomial of 2nd degree in day of birth before and after cutoff. (3) Regressions are restricted to a 2-month window before and after the cut-off date. Separate polynomial of 2nd degree in day of birth before and after cutoff.
Source: Health Insurance Claims Data, 2006-2011. Own calculations.

TABLE 6.11: Service and Survival World War II

	COHORTS	
	1900–1915	1916–1930
DIED OR MISSING DURING WORLD WAR II		
1st July (Early SSA)	-0.038	0.014
	(0.027)	(0.024)
Observations	7,236	8,672
Mean Dep. Var.	0.130	0.145
DAYS OF ACTIVE SERVICE		
1st July (Early SSA)	-40.476	-50.277*
	(60.052)	(28.860)
Observations	6,422	8,221
Mean Dep. Var.	1,986	1,336

Notes: Table shows estimated effects of cut-off date (1st July) on World War II outcomes. Results refer to cohorts 1916–1930. Robust standard errors are clustered on day of birth. All regressions control for cohort fixed effects. Significance levels: $*$ 0.10 $**$ 0.05 $***$ 0.01.
Source: Inscription Data, German Military, 1935-1945. Own calculations.

6.6 Discussion

First, my empirical results show that for cohorts born before World War II, early school entrance in Germany caused lower educational attainment and had negative effects on economic well-being. The effects on education and earnings persist until individuals reach their 40s and 50s. This contrasts Dustmann, Puhani and Schönberg (2017) most recent findings on long-run effects from an earlier school starting age for Germany. While they find that early track choice is affected by the relative age gap, later up and downgrading between school tracks correct for initial missallocation. Effects on educational attainment or earnings do not persist. There are several possible reasons why results differ. Most importantly my cohorts are born on average 40 years earlier. The German school system was much less flexible for cohorts born during the 1920s than for individuals born in the 1960s. In fact, much of the flexibility in the current school system was introduced to counteract inefficient initial sorting, e.g. the introduction of *specialised high track* in 1967. The differences in the results across cohorts might be interpreted as evidence for an improvement of the German school system.

Furthermore, I find that World War II probably exaggerated the persistence of adverse effects. Even in the absence of direct involvement in combat, the war disrupted human capital accumulation for many individuals and initial differences are even more likely to persist. Earlier research has shown that especially those cohorts born around 1930 experienced the largest loss in human capital (Akbulut-Yuksel, 2014, 2017). In line with this research, my results show that the disadvantage from an earlier school start is also largest

for cohorts being in secondary school during World War II. Given the uniqueness of the event, external validity using those cohorts in the German WWII context is questionable. On the other hand, it is interesting on its own right to investigate how initial inequalities are affected by further adverse events later in life. In order to gain a better overall understanding, cross-country comparison with countries having similar, selective school systems but were unaffected by the war would clearly be appreciable.

The reduced form effects on education an earnings are large in relative terms, implying an increase in years of education between 0.06–0.07 years from a delayed school start. What effect size is to be expected on dementia risk? OLS associations from several studies suggest a reduction of approximately 2–3 percentage points per additional year of education (Tognoni et al., 2005; Meng and D'Arcy, 2012). Given the increase in education this would imply a decrease in dementia risk by 0.1–0.2 percentage points. My estimates do not reject an OLS association of this size. But at the same time, I cannot reject the null hypothesis of no effects on dementia risk from school starting age.

In contrast, my analysis suggest a large reduction of raw mortality by 0.5–0.8 percentage points given an overall mortality risk of about 30 percent across cohorts. Given that estimates are robust across various specifications, and the absence of mortality effects in the southern control federal states, it is unlikely that the results are driven mainly by seasonality. The most similar study in design comes from Van Kippersluis, O'Donnell and van Doorslaer (2011) analyzing mortality effects from a Dutch compulsory schooling extension on individuals aged 81–89. The study suggests a decrease in overall mortality of 2–3 percentage points given a mortality risk of about 50 percent. While the relative risk of dying is 3 times larger, the Dutch compulsory schooling reform had 10 a times larger effect on educational attainment. The relative larger effects suggests that the school starting did not purely operate through educational attainment or that very different margins of individuals were affected. The school starting age regulation mostly prevented individuals from attaining higher levels of education. My data unfortunately does not provide cause of death preventing further insights on the channel causing the decrease in mortality.

6.7 Conclusion

The literature has documented a substantial gradient between education and cognitive disease risk for the older population. According to the *cognitive reserve* hypothesis, more education can be protective against the onset and progression of dementia. In this paper, I have investigated whether variation in school entry age affects the risk of dementia, either directly or by affecting education and other socio-economic outcomes during the life-cycle.

My results suggest that early school starting age regulation in Germany had sizable effects on educational attainment and income. With high compliance, limited correction possibilities of initial missallocation and WWII as additional adverse event, effects from an relative age disadvantage persisted.

My results therefore complement research showing that long-term outcomes of more recent cohorts in Germany were much less adversely affected from school starting regulations. However, my results do not show significant decrease in dementia risk. Potentially, this could be due to competing risks as I find sizable reductions in overall mortality.

Chapter 7

Conclusion

The thesis investigates whether formal education can improve health and socio-economic outcomes in the very long-run. In order to address the impact of education on health and income in the older population the thesis concentrates on changes in historical school policies affecting cohorts born in the first half of the 20th century in Sweden and Germany, respectively. All studies rely on changes in the primary and lower secondary school leading to variation in human capital accumulation.

This thesis delivers knowledge to under which circumstances certain educational policies work and when they might fail. The multiple different quasi-experiments under similar but not identical contextual circumstances increase the understanding and trace out some of the mechanisms underlying various educational policies. The results based on different policies are indeed not unambiguous. But many results are informative, support theoretical predictions or resolve earlier puzzling findings.

Extensions of instructional time from term length, affecting individuals early in their school career, increase earnings more than comparable increases at the end of their schooling career. This is of relevance in the light of the literature on early childhood interventions and dynamic complementarities. Interestingly, the results from various compulsory schooling extensions across multiple studies in this thesis on earnings are remarkably stable for Sweden. The local average treatment effects for the returns to education from various reforms are in the range of 2–3 percent.

The results on longevity in this thesis fit into the broader literature on the causal effects of education on mortality suggesting that interventions earlier in time seem to be more effective. Our results also point into the direction of occupational sorting, potentially into less physically demanding jobs outside of the agricultural sector, as an important mediator for results from these earlier periods.

The thesis also delivers complementary insights into the existing empirical literature. It resolves a rather puzzling finding for the well-studied Swedish comprehensive school reform. Previous studies reported that the reform substantially increased earnings for individuals with low-educated parents and substantially lowered earnings for individuals with high-educated parents. The earlier results on a socio-economic gradient in the returns to earnings were initially explained by a decrease in efficiency of the school system due to the abolishment of academic tracking and potential peer effects. At the

same time the reform had no long-term health effects on any group. If earnings and more generally labor market outcomes are important mediators for a causal impact of education on health, this results is difficult to explain. A potential explanation could be the relatively equal access to health services in Sweden. The results presented in this thesis however point into a different direction. The gradient in the effects of earnings did not exist and the comprehensive school reform had overall only modest effects on earnings. School quality was unlikely a relevant issue. Small and homogenous increases in earnings are then also more compatible with small or zero causal effects of education on health from the comprehensive school reform.

What are the policy recommendations which can be drawn on the basis of the results of this thesis? First, it should be noted that while the returns to mandatory education are moderate, they are also uniformly positive. It is also unlikely that the various policies were harmful for individuals in the long-run. This is important given the mentioned finding of potential negative effects from the introduction of a comprehensive school system on individuals from higher socio-economic background. The results on the first German school starting age regulation also stress out the importance of a flexible school system which is able to mitigate initial inequalities.

Several aspects of the thesis call for further research. First, related to the question of external validity of the results, the thesis also mainly concentrates on interventions affecting primary and lower secondary education. It does not consider higher education. There are theoretical mechanism such as e.g. peer effects which suggest that education might operate very differently at different levels. Findings on such heterogeneity would be of great additional value. Given that substantial expansions in the tertiary educational sector happened only more recently in developing countries, finding suitable quasi-experimental designs for the evaluation of very long-term effects of higher education on health is a challenging future research topic.

The thesis has a limited regional focus on Sweden and Germany. This was chosen deliberately and has the advantage of a comparable institutional context within the thesis. Whether my results transfer to other institutional contexts outside of Sweden or Germany and other time periods needs careful assessment. As mentioned earlier, the comparison with prior research on similar institutional changes delivered some important key insights on *why and when* educational policies impact long-term outcomes.

The full potential of an evaluation of the long-term effects especially from the school reforms in the former Swedish primary school is clearly not reached. The investigation of long-run health outcomes has been exercised partly in this thesis (with respect to all-cause mortality) and elsewhere (with respect to dementia, see Seblova et al., 2017) but there is no doubt that this analysis can be easily extended. Especially, the finding of differential impacts from term length extensions and the compulsory schooling extensions on earnings and occupational sorting suggest that further research could be fruitful.

Finally, a more in depth comparative study between the two countries could reveal interesting suggestive evidence on how education operates. Previous research e.g. has documented substantial negative effects on human

capital accumulation for cohorts attending school during the war in Germany. Essential for the conclusion was a comparison of trends in education with neutral countries such as Sweden, being much less affected by World War II. Similar analysis based on longevity and cognitive disease risks between the two countries could give interesting insights into the interaction of adverse events during childhood, human capital and long-term health.

Appendix A

Additional Material: Swedish School System and Reforms

A.1 Compulsory Schooling in Sweden (1919-1948)

Sweden has a long-lasting tradition of compulsory education. Already in 1842, church parishes[1] were obliged to offer schooling by an approved teacher and children in *school age* had to attend the local primary schools (*Folkskola*) (Fredriksson, 1971).[2] School age was defined as the years during which children had to fulfil their compulsory curriculum. After several changes during the 19th century, the school age was finally set to the year children turned seven and lasted until the year they turned fourteen in 1897.

Still the school age neither explicitly determined the compulsory years of schooling, nor the total amount instructional time within a given school year in the late 19th century. Different local regulations with respect to term length and various options to fulfil the compulsory curriculum[3] 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.[4] 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

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

[2] 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)

[3] 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.

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

level. Primary schooling was free of charge and attendance was compulsory until a student had completed the highest grade of *Folkskola* in the school district he/she was registered as a resident.[5] Parents were responsible for that their children fulfilled the compulsory school attendance and had to report to the school district board that they had a child in school age. §51 of the "Royal Decree of the Folkskola" of 1930 states that parents were legally obliged to send their children to school and that parents that did not send their children to school could get penalty payments and even lose custody.[6] The legal, administrative and pedagogical control of the school district was handled by regional school inspectors appointed by the Ministry of Ecclesiastical affairs.[7] The county governments were the highest legal instance responsible for the enforcement of regulations related to compulsory school attendance.

Following the new guidelines the 1920s were characterized by an intensive reform debate about the need for further extensions of compulsory education and the need for greater equality of opportunity within the Swedish schooling system. In 1920 a clause was introduced in the primary school code[8] that a seventh school year *could* be made compulsory in a school district (Fredriksson, 1950). However, only very few districts introduced a mandatory seventh year and still *Folkskola* generally covered six years.[9]

With respect to equality of opportunity, the Swedish school system in the early 20th century was very selective. Children were tracked into separate schools based on their academic achievements after spending the first four years in *Folkskola*. High achievers could follow the academic track at the 5-Year *Realskola* (junior secondary education) which could lead to *Gymnasium* (upper secondary education) and University. The remaining students stayed in *Folkskola* for at least two more years, when there was another option to enter lower secondary education. Students not enrolling to secondary education took low intensity courses six weeks per year in continuation schools for two more years after compulsory schooling. Figure 2.1 in the main text gives a stylized presentation of the various school types and continuation options following the basic primary education.[10]

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

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

[7]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).

[8]Paragraph 47 mom. 4.

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

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

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

A.1.1 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,[11] the national government decided that seven-year schooling should be made compulsory in 1936.[12] 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*.[13]

twentieth century, this reform is the subject of a number studies on the impacts of education and very well documented, see e.g. Meghir and Palme (2005); Lundborg, Nilsson and Rooth (2014); Hjalmarsson, Holmlund and Lindquist (2014); Lager and Torssander (2012). Interestingly it often went unnoticed that the comprehensive school reform only constituted the second part of an ongoing reform process of the Swedish primary and lower secondary school system which eventually culminated in the establishment of the new type of school. In fact, Sweden exhibited a continuous roll-out of extending the mandatory amount of schooling from 6 to 9 years over a period of 40 years.

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

[12] 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).

[13] *The strengthening of teaching of the most important topics of Folkskolan, that according to the experts are utterly necessarily, can likley not be achieved in any other way than through an extension of the length of the novitiate.* Own translation of: *Den förstärkning av undervisningen i folkskolans viktigaste färdighetsämnen, som enligt de sakkunnigas mening är behövlig, torde icke kunna vinnas annorlunda än genom en utsträckning av lärotidens längd.* (Ecklesiastikdepartementet,

In line with this motive, the reform did not come with any fundamental changes with respect to learning goals or curricula, but instead emphasized the goal of achieving more long-lasting results of schooling. The recommendation from the central administration of the Ministry for Ecclesiastical Affairs school districts should distribute the pre-reform compulsory school curricula over seven years instead of six (Ecklesiastikdepartementet, 1935*a*).[14]

Due to the soft transition rules, the compulsory schooling reform did not cause any major difficulties in the school districts. The implementation was also facilitated by the fact that the responsibility for funding of school buildings, teaching materials and teachers' salaries was the responsibility of the central government and not the school districts (Larsson, 2011*b*). 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.[15] The reform could also be implemented without any organizational difficulties since there were an oversupply of teachers in the 1930's and 1940's (Fredriksson, 1971).[16]

With the bill of 1936, school districts were also allowed to introduce an eight year of compulsory schooling, but for this they needed to send in a formal application and to be given the king's consent

A.1.2 Extensions of the Term Length

With the 1919 Education plan there was also a harmonization with respect to the length of a school year across school districts (Paulsson, 1946). The yearly reading time was divided into an autumn and a spring semester, with the academic year starting in autumn. More or less parallel with the compulsory schooling extension, the government decided to extend the term length in

1935*a*)(p.49) and *Apparently the time in school has been too short for the children that leave Folkskolan at age 12–13 after only six years of education to get the amount of training and repetition that is needed to gain lasting skills.* Own translation of: *För de barn, som vid 12–13 års ålder lämna folkskolan efter endast sex års undervisning, har skoltiden uppenbarligen varit för kort för att lämna tillräckligt utrymme åt den övning, den innötning, förutan vilken bestående färdighet i berörda ämnen icke kan vinnas.* (Ecklesiastikdepartementet, 1935*a*) (p.54)

[14]*For the seventh year to fulfill its aim to generate a more thorough and deeper knowledge and understanding there is at this time no need for any extra curriculum in addition to the ones that are provided by the 1919 education plan.* Own translation of: *För vinnande av syftet med det sjunde skolåret — ett grundligare och mera fördjupat inhämtande av folkskolans lärokurs — torde några kursplaner för den sjuåriga folkskolan utöver dem, som äro upptagna i 1919 års undervisningsplan, tillsvidare icke vara erforderliga.* (p.130)

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

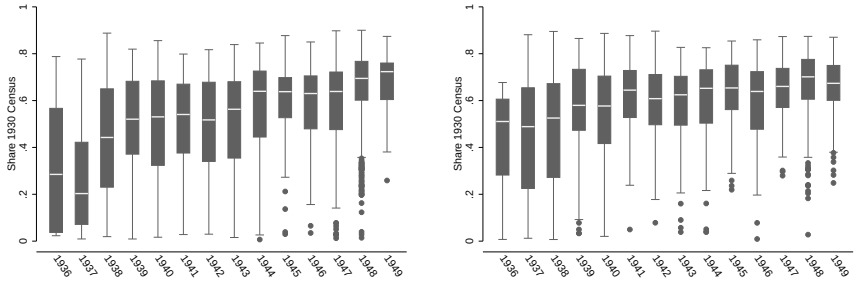
[16]Following the oversupply of teachers the Ministry of Ecclasticial Affairs cut the intake to the teachers colleges in the early 1940's. The situation changed in the 1950's, but it was not until the early 1960's and the implementation of the comprehensive school system that that there was a real teacher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

Folkskolan. In 1937 the regular term length was approximately eight months, or minimum 34.5 weeks. The number of weekly teaching hours was recommended to 30 and could not exceed 36, and the school day could not exceed six hours (Paulsson, 1946). In conjunction with a wage reform for teachers in the *Folkskola*, the parliament of 1937 decided that the school year should be 34.5 (207 days), 36.5 (219 days) *or* 39 weeks (234 days) long, and that different wages would be paid for the different term lengths, all covered by the national government. The main motivation for the term extension was once again that Sweden was lagging behind other Western European countries and that the school year was to short to accomplish learning objectives. Following this policy, several school districts prolonged their school year. In 1938, about 400 districts switched to 39 weeks, and about 200 to 36.5 weeks (Örtendal, 1938). Similar to the compulsory schooling reform, the term extension did not come with any curricula changes.

In 1939, the Swedish Parliament decided to further raise the minimum school year duration to 36.5 weeks, thus removing the shortest option of 34.5 weeks. Because of the Second World War and the national savings programme that followed, a transition time was given for the implementation of this reform. The school districts providing 34 4/7 weeks were given the opportunity to wait until 1941/42 to choose either one of the longer school year durations (Weijne, 1942). Eventually, in 1953, 39 weeks was implemented as the standard term length across Sweden (Statistics Sweden, 1974).[17]

The box plots in Figures A.1(a) and A.1(b) show that both reforms were implemented later in school districts with high shares of employment in agriculture. This pattern is especially pronounced for the compulsory school extension. On small farms children at the age of 13 were generally a valuable source of labor which can explain a greater reluctance to implement the compulsory school extension in school districts in rural regions.

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



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

FIGURE A.1: Local Share Working in Agriculture in 1930 by Implementation Year

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

A.2 The Introduction of a Comprehensive School (1949-1969)

A.2.1 The 8 Year Reform

With the start of the World War II, the Swedish political debate came to place a large focus on how to best foster democratic members of society. More education was seen as one of the main components for fulfilling this goal (Edgren, 2011). Thus, despite the on-going national implementation of the seventh year of compulsory schooling, a reform work was initiated and assigned to a new expert commission (*Skolutredningen* later replaced by *Skolkommissionen*) in 1940. This was the first governmental commission with a real mission to investigate primary and secondary education together, and with an aim to replace the tracking system with a unified comprehensive school system tying compulsory schooling closer to secondary education.[18]

Between 1940-1948 the commission continuously released reports evaluating the current school system and developing proposals and guiding principles for the future compulsory school (Marklund, 1982). Although the main focus of the commission's work was to postpone tracking decisions to higher grades, and by that improve equality of opportunity, and despite the on-going implementation of the seven-year compulsory schooling, there were also continued efforts to further extend compulsory schooling within the old

[18]Since the 1890's there had been a quite heated debate about the rational of the the so-called parallel system where student took different tracks. The main argument in the debate for that all students should have to complete the very same basic education before continuing to secondary schooling, was that it created inequalities (Morawski, 2010).

system. The commission's work discussed an eight year extension of *Folkskola*, and in 1945 the Minister of Ecclesiastical Affairs proposed a bill introducing compulsory eight year schooling (without changing tracking options), but no action was taken by the Government (Fredriksson, 1971).

As stated above one of the main arguments for extending compulsory education was *democratic fostering*. This motive was not new. The 7 year reform was also motivated by fostering democracy including universal suffrage which was argued to place great demands on members of society, wherefore a solid education is necessary (Ecklesiastikdepartementet, 1935*a*). The on-going war made this argument even more important in the debate. Specifically an eight-year extension was believed to improve student performance with respect to elementary skills in reading, writing and math, but also other subjects. An extension would also allow for the introduction of foreign languages (English) as a subject. In addition to theoretical arguments an extension was further justified by social and ethical arguments and that an eight-year could fill a supportive and nurturing role for young people that have not established on the labour market (SOU, 1945).

A second argument for extending compulsory education was induced by international *benchmarking* – that Sweden was lagging behind (Waldow, 2013). Compared to other countries few students matriculated to higher levels of education, and the time spent in compulsory education was still quite modest. For example, compulsory education in the US endured at least until age 16, in Germany there were Volkschule or Hapuptschule until the age of 15 and in the UK students generally had nine years of compulsory schooling in the late 1930s.

A third argument for extending compulsory schooling was the *increasing specialization of the labour market* and the *increased complexity* of society and societal life, implying a need to significantly increase educational goals of Folkskolan. Finally, the economic and societal *duality* that existed between urban and rural areas was brought forward to motivate a general 8 year reform or a compulsory school reform. Specifically with respect to education, the rural areas of the country was falling behind, e.g. smaller shares of students matriculating to junior secondary education in rural compared to urban areas (Centralbyrån, 1977). With a general implementation of the 8 year reform such differences could decrease.

The main arguments of the proponents of the 8 year reform to why the realization of an eight-year extension was seen as preferred compared to a comprehensive school reform was that there was (i) no large demand from students nor from the parties of the labour market for a 9 year comprehensive reform, and that (ii) the supply of teachers was too limited for a comprehensive reform, but also that the teachers generally had too limited education (SOU, 1945).

Likely spurred by the political debate some municipalities applied and

got consent from the king and took the opportunity to implement a mandatory eight-year of Folkskola (Folkskollärarförbund, 1943).[19] The first two municipalities to implement an eight mandatory year were Kävlinge and Mariestad in the school year of 1941/42. The number of municipalities offering an eighth year gradually increases in the next-coming decade: In 1946/47 there were 33 and in 1958/59 207 municipalities, respectively. A characteristic of the municipalities introducing a mandatory eight-year in this time period is that they were urban and most of the larger cities of Sweden were early birds in this development. Consequently a quite large share of all students in the country had eight years of compulsory schooling: in the school year 1948/49 this was 16 per cent and in the school year 1951/52 this was 25 per cent (Folkskollärarförbund, 1952)

All municipalities introducing the eighth year followed the *main form* curriculum requiring full time reading and a teacher with an appropriate teacher degree.[20] Normative and binding curricula regarding the eight-year were missing in the early period, but the curriculum and hourly plan presented in the proposal of Skolutredningen in 1946 generally became the norm for the school districts that implemented an eight-year of Folkskola. The mandatory subjects in the eight grade were the same as in seventh grade, but local preferences could to some extent be met (Fredriksson, 1971).

A.2.2 The 9 Year Reform

In 1948, the expert commission proposed to replace the compulsory primary and the junior secondary school with a nine-year compulsory comprehensive school. The expert commission however wanted to evaluate the new school form before introducing it to all schools across the country. The reform was therefore introduced during an assessment period where the 9 year comprehensive school was introduced in different locations at different points in time. Starting from 1949/1950 the 9 year reform was rolled out at the municipality level.[21] For the first year of the roll-out of the reform 14 municipalities are selected to participate in the assessment. [22] The evaluation period was

[19]Only in a few cases a municipality did not get the permission to implement the extension. The reason was that the district asked to do an isolated change and only introduce the change in a separate school in a municipality (Fredriksson, 1971).

[20]The alternative to the main form were *exception forms*, characterised by half time reading or that the teacher did not have an appropriate teachers degree. In the early 1940's more than 90 percent of all pupils in Sweden went to a school that were assigned to the main forms (Fredriksson, 1950).

[21]The comprehensive school system is introduced throughout the whole municipality, or in certain schools within a municipality. At the time there were 1037 municipalities in Sweden.

[22]Municipalities had to show interest in the reform and also report on various issues, such as e.g. population growth, local demand for education, tax revenues and school situation, and all municipalities that took part in the first year of assessment were required to have eight year comprehensive schooling. The 14 first-movers were selected out of 144 municipalities.

not run as a random experiment, but the National School Board chose the areas from a group of applicants to form a representative set based on observable municipality characteristics. Municipalities participating in the early assessment period were compensated with earmarked money from the central government for the increased costs following the expansion of mandatory education (Holmlund, 2008). After the assessment period, the national parliament decides to permanently introduce the 9 year reform to all schools the country in 1962. Seven years later, by 1969, all municipalities were obliged to have the new comprehensive school running (Marklund, 1982) and *Folkskolan* was fully discontinued.

The reform reshaped the entire school system and compared to the old tracking system students were kept in the same school type for nine years. Besides extending compulsory education from seven or eight to nine years and postponing tracking, the educational reform also came with a change in the national curriculum implying English and civics became a compulsory subject, but there were no major changes to the total number of hours or the distribution of hours taught in different subjects (Richardson, 1992).

The educational reform was also pedagogical. The commission proposal of of 1948 was very clear on that the traditional school and its working methods were obsolete. Specifically whole-group teaching and questions-response methods should be replaced by more individualized and activating elements, pandering students drive and independence (Marklund, 1982).[23]

Based on the principles of the final report of *Skolkommissionen* a new educational plan for schools to follow is released 1962 (Lgr 62). The pedagogical key concepts of the plan are individualization and activity learning (Larsson, 2011*a*). The pedagogical fundament on the special position of the individual and that the school should foster independent individuals did not meet any major objections (Marklund, 1989). However the first reform municipalities experienced difficulties in getting accurate work material and text books (Marklund, 1982).

A.2.3 Comparing the Two reforms

Based on the above it is evident that Sweden experienced a continuous rollout of extending the compulsory amount of schooling from 6 to 9 years over a period of 40 years, and that the 8 year reform and the 9 year reform were implemented across overlapping cohorts. On average the 8 year and the 9 year reform was 7 years apart in a municipality.

Both reforms introduced change in the extent of compulsory schooling. As regards the exact definition of treatment it however seems that the two

[23]The emphasis on the importance of the need of new working methods can also be assigned to the aim that education should foster democratic societal members. As discussed by Richardson (1978) there is also a change regarding the view of the individual in the late 1940's. The development of the individual now matters more than the societal development. An essential feature of the report by the commission is that that the school should be more pupil centred and less subject-matter oriented. Another novel perspective is the view that parents, not the school, are responsible for the pupil.

reforms differ somewhat. Treated students of the 8 year reform faced no significant school system changes, nor any changes in working methods in class. Thus, any effects from the 8 year reform should mainly be driven by changes in the amount of time spent in education.[24] With the abolishment of the tracking system the 9 year reform implied a fundamental change of the complete school system and the reform also came with a new curriculum program and methods. Any effects from the 9 year reform can thus be driven by changes in the amount of time spent in education, by that the new system kept students together in the same school until the ninth grade, and/or changes in curricula, working methods and pedagogics.

As discussed above schools and teachers initially faced some problems in that they lacked appropriate teaching materials corresponding to the new curricula and teaching methods of the comprehensive school. According to Marklund (1982) teachers degrees of freedom with respect to novel and open-ended activities were also limited by that students and parents that wanted *Realskola* but instead had to undergo compulsory schooling in the comprehensive system, translated their ambitions and goals for the former school type to the latter. Together this suggest that the first part of the 9 year reform likely was more similar to the 8 year reform. Also the first period of the 9 year reform was more similar to the previous school system in the sense that most schools still streamed students into different classes according to their choices regarding languages or vocational training and harder and easier courses in some subjects (Marklund, 1982).

The two reforms were gradually implemented across municipalities. The timing of implementation in individual municipality was based on a mixture of local and national decisions. As regards the wider institutional context, we are unaware of any reforms that might have coincided with the 8 year or the 9 year school reform at the local level. During the assessment period of the 9 year reform it was only municipalities that showed interest in the reform that could be selected implying reform implementation was not random. Previous studies suggest that 9 year reform was implemented earlier in municipalities with higher incomes and with higher average education, see e.g. Lundborg, Nilsson and Rooth (2014). Regarding the 8 year reform the early-birds tended to be more urban and most of the larger cities implemented a mandatory eight year. Smaller municipalities followed and in the end more than half of all municipalities had introduced a mandatory 8th grade before implementing the comprehensive 9 year school reform.

A.2.4 Reform data and Validation

The reform data for the 9 year reform was generously shared by Helena Holmlund and we rely on a dataset as used in Hjalmarsson, Holmlund and Lindquist (2015), of which an earlier version is described in detail in Holmlund (2008). The dataset encompasses information on the year a specific school district introduced the new comprehensive school.

[24]See e.g. discussion by Orring, Read et al. (1962) on that all earlier reforms than the 9 year reform more or less left the fundamental work of schools unaffected.

While the 9 year reform has previously been used in several economic applications, this paper is the first to use the 8 year reform and the reform data on the timing of the year of introduction of the eight year in each municipality was purposively collected from archives and digitized by the authors. Various official sources provide aggregate information on the development of the implementation on the 8 year reform. To check the accuracy of the gathered reform data we perform checks to confirm that the collected information conform with aggregate official statistics. For example, information on the share of school districts in the country that had eight years of compulsory schooling in certain years from Skolöverstyrelsen (1955) and from Centralbyrån (1977), respectively, suggest our data conform with aggregate statistics.

The decision to introduce eight years of compulsory schooling was made on the municipal level, and the assumption is that schools within the same district implemented the reform in the same year. Theoretically there could however be discrepancies between schools within municipalities. We believe the assumption is valid since official sources state that the change generally applied to a whole district (see e.g. Fredriksson (1971) and Skolöverstyrelsen (1955)). Moreover, aggregate figures on the share of all students taking on the extra year of compulsory education in certain years (Centralbyrån, 1977) suggest that there should be no major deviations from this rule.

Appendix B

Additional Material: Chapter 2

B.1 The Swedish Labor Market

B.1.1 Measuring Returns to Education

To estimate the returns to education of the term extension and the compulsory schooling extension, respectively, we use annual labor earnings in 1970 and annual pensions as dependent variables. In order to assess the reliability of these income measures we compare our estimates to standard Mincer wage regressions based on information on hourly wages from the LNU 1968. Survey data clearly reduce the sample size, but wages capture productivity differentials better than annual earnings as they are less prone to differences in labor supply. Table B.1 compares OLS estimates for the returns to education with hourly wage regressions as the benchmark.

TABLE B.1: Mincer Wage Regression

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

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

Table B.1 suggests our earnings measure leads to very reasonable estimates regarding the returns to education in a Mincerian framework for males. This is not very surprising as it has been documented that labor earnings when men are in their 30s constitute an excellent proxy for their life-cycle

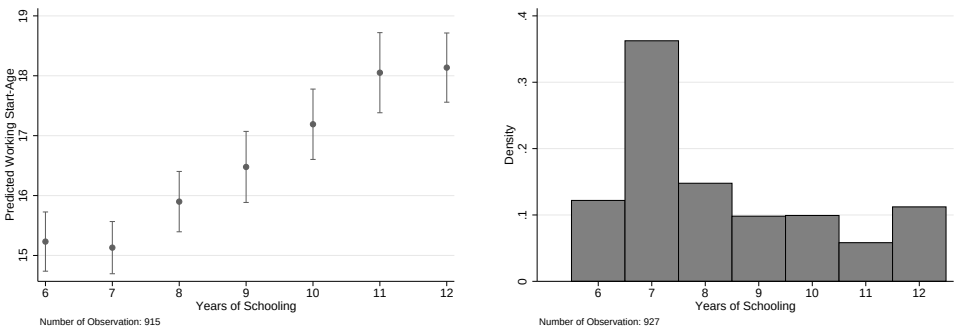
earnings. For women estimates for the earnings in 1970 are overestimated due to effects on labor supply. As shown by Böhlmark and Lindquist (2006) the life-cycle bias in earnings estimates is largest for women around their late 30s. For men full-time employment in 1970 was close to 100%, while only 66 % of all women worked full-time. Assuringly the pension estimates for both men and women are extremely close to the estimates based on the LNU survey. Given that pensions were based on the *best* 15 income years it is reasonable that labor supply frictions are minimized.

B.1.2 Labor Market Entrance

In addition to Figure 2.4 we also informally test whether an additional school year from 6 to 7 shifts labor market entrance by running a regression of working starting age, wsa_i, on years of schooling:

$$wsa_i = \beta_0 + \sum_{s=6}^{12} \gamma_s I(Schooling = s) + \beta_X X + u_i. \tag{B.1}$$

In the regression we also control for any education beyond any schooling. Figure B.1(a) plots the predicted values from this regression. There is no significant difference at labor market entrance between having 6 or 7 years of schooling. This suggests that the opportunities for adolescents between the ages 12 to 14 to enter the formal labor market were low in the 1930s and 1940s.



(a) (b)

FIGURE B.1: Regression Years in School on Working Start Age: (**a**) Predicted WSA; (**b**) Distribution Years of Schooling.

Notes: Figure B.1(a) shows predicted values from a regression of Working Start Age (WSA) on years of schooling, birth cohort and years of post-schooling. Post-schooling (e.g. vocational training, university...) is set to zero for predicted values. Figure B.1(b) shows the distribution of years of schooling.
Results refer to cohorts 1930-1940.
Source: LNU Survey, 1981. Own calculations.

B.1.3 Skills, Tasks and Occupation

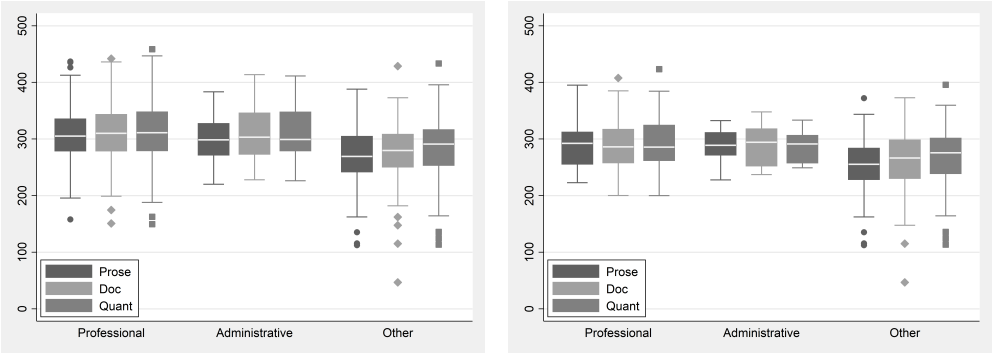
Table B.2 provides the information of Table 2.1 in section 2.2.3 but for males and females separately.

TABLE B.2: Tasks for Occupational Groups

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

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

The skill profile of occupational groups in terms of primary school GPA which is apparent in Table 2.1 and Table B.2 is also found for adult literacy scores. Figure B.2 plots the distribution of scores in the three dimensions *prose literacy*, *document literacy* and *quantitative literacy* from the IALS survey. Due to the low number of observations, we group respondents into three main groups: the two occupations with the highest GPAs in Table B.2 ("Professional and Managerial" and "Accounting, Administration" respectively) and a third group consisting of all other occupations. The first two groups have consistently higher literacy scores in all three dimensions, and this difference remains after excluding individuals with secondary education.



(a) All Working Individuals

(b) Working Individuals, No Secondary Education

FIGURE B.2: Adult Literacy Scores by Occupation and Education.

The graphs show the distribution of literacy scores of working individuals older than 50 in three dimensions according to the IALS 1994 study. Each score dimension ranges from 0 to 500. The 'Professional' and 'Administrative' groups correspond to the two top groups of Table B.2. The third group ('Other') includes all other working individuals.

B.2 Migration, Hospital Births and Reform Assignment

This section evaluates migration patterns and reform assignment based on the place of birth. For cohorts born in the 1920s and early 1930s the dominant mode of delivery was home births, while the norm from the mid 1930s was to give birth in an institution. As mentioned in Section 2.3.2 the location of the hospital, and not the place of residence of the parents, was recorded as the place of birth in Swedish register data until 1947. Figure B.3 illustrates the effects arising from a hospital opening on the recorded births for the city of Motala as an example. After the opening of the hospital in 1927 the recorded births in the city grew rapidly while the parishes around the city recorded fewer newborns. Children from parents living around the city, but who were born in the newly established hospital were now recorded as being born in Motala. From 1947 onwards the place of residence of the parents is registered as place of birth and the number of births in the city sharply decreased.

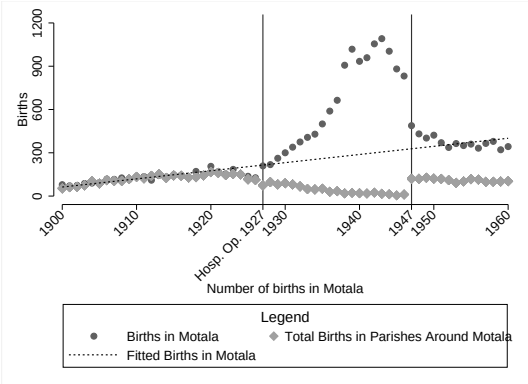


FIGURE B.3: Recorded Births: Motala

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

Whether the place of birth (PoB) is a sufficiently good proxy for the place of residence during adolescence depends on the actual information recorded as place of birth but also on migration patterns. Figure B.4 shows the share of individuals in the 1950 Census for each year of birth where the *place of birth* coincides with the *place of residence in 1950*. In order to capture the effect from the hospital coding we add information from openings of birth centres across Sweden in the period of interest and split the population by the presence of a birth center in the parish of birth. For parishes without a hospital, deviations stem mostly from migration. These occur mainly during the first years of life and in the age 15-30. For children during schooling age and for older adults migration appears to be a seldom phenomena as their parents unlikely move in this period of life.

The gap between parishes with and without a hospital at the year of birth is a direct result of the recording of the place of birth prior to 1947. Figure B.4 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.

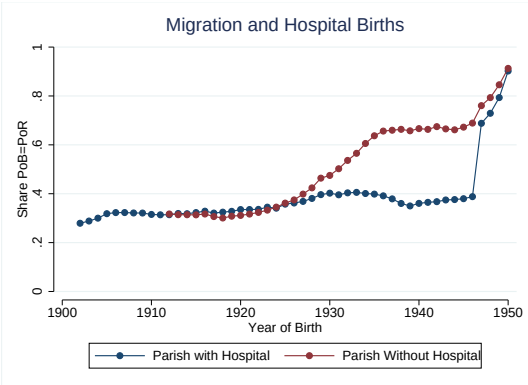
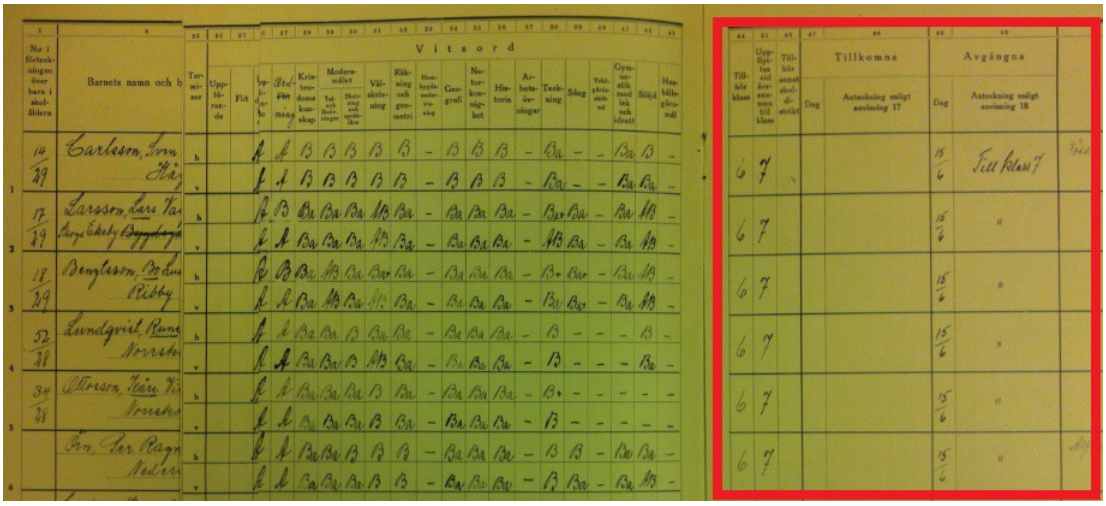


FIGURE B.4: Hospital Births vs. Migration

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.

B.3 Data Source

FIGURE B.5: Exam catalogue from Archive



Appendix C

Additional Material: Chapter 3

C.1 Additional Results

C.1.1 Descriptive Statistics

TABLE C.1: Descriptive Statistics

	(1)	(2)	(3)	(4)
	1948 COHORT		1953 COHORT	
	Reform	Non-reform	Reform	Non-reform
Reform assignment	0.344		0.802	
Female	0.492	0.491	0.487	0.488
Fathers Education more than compulsory	0.182	0.119	0.176	0.099
Years of Education (Palme)	12.169	11.477	12.200	11.572
Years of education	11.431	10.492	10.575	9.998
Less than 9 Years of Schooling	0.031	0.186	0.011	0.135
9 Years of Schooling	0.182	0.084	0.180	0.111
More than 9 Years of Schooling	0.787	0.729	0.809	0.754
Log Labor Earnings	7.190	7.124	7.072	7.012

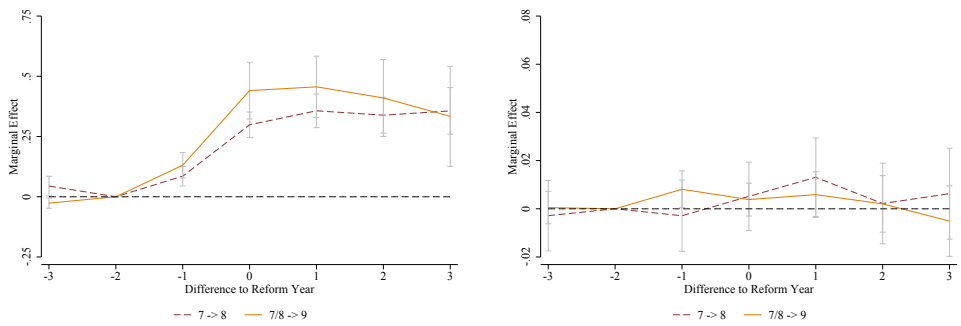
Notes: Results refer to cohorts 1948 and 1953 (sample chosen accordingly to Meghir and Palme (2005)). Statistics refer to full population.

C.1.2 Exogeneity of Reform

TABLE C.2: The Effect of the Reforms on Years of Schooling (*First Stage*)

	$\hat{S}^{NEW}$			$\geq 8/9$ YEARS	$\hat{S}^{TRAD}$ MEASURE
	(1)	(2)	(3)	(4)	(5)
MALES					
Reform (7 → 8)	0.237***	0.230***	0.230***	0.206***	0.023
	(0.029)	(0.028)	(0.028)	(0.018)	(0.032)
Fstatistic	66.883	67.997	67.999	137.428	0.497
Reform (7/8 → 9)	0.426***	0.448***	0.448***	0.240***	0.286***
	(0.024)	(0.030)	(0.030)	(0.011)	(0.026)
Fstatistic	324.154	219.349	219.104	450.607	120.606
FEMALES					
Reform (7 → 8)	0.181***	0.180***	0.180***	0.178***	-0.008
	(0.041)	(0.042)	(0.042)	(0.017)	(0.032)
Fstatistic	19.665	18.282	18.281	113.111	0.060
Reform (7/8 → 9)	0.380***	0.412***	0.412***	0.204***	0.147***
	(0.021)	(0.023)	(0.023)	(0.010)	(0.019)
Fstatistic	326.730	308.902	308.572	426.140	57.934
BIRTH COHORT FE	✓	✓	✓	✓	✓
MUNICIPALITY FE	✓	✓	✓	✓	✓
LINEAR TRENDS		✓			
QUADRATIC TRENDS			✓		

Notes: Table shows the reform effects on educational attainment. Robust standard errors clustered at municipality level are reported in parenthesis. Significance levels: $*$ 0.10 $**$ 0.05 $***$ 0.01.
Results refer to cohorts 1938 and 1952. All regressions include birth cohort FE and a 5-year window around the pivotal cohort.
Dependent Variables: Alternative measure S^{NEW} based 1970 Census and Educational Registers (allows to capture complete compliance to the reforms). $\geq$ *8/9 Years* refers to compliers for each compulsory schooling reform ($\equiv$ more than old compulsory years of schooling). S^{TRAD} gives effects for the standard Swedish register years of education measure based on highest level of education (SUN2000).
Source: SIP. Own calculations.



(a) Years of Education (First Stage) (b) Log Labor Earnings (Reduced Form)

FIGURE C.1: Event Study Graphs

Notes: Figures plot lags and leads of the reform dummy on years of education. 95% CI based on robust standard errors clustered at municipality level. The earnings variable is defined as average log-labor earnings age 30-35.
Baseline specification with sample restricted individuals born within a 5-Year window around the pivotal reform cohort. Results refer to cohorts from 1938 to 1952. All regressions control for birth cohort FE and municipality FE.
Source: SIP. Own calculations.

TABLE C.3: Balancing Regression (Cohorts 1948 and 1953)

	(1)	(2)	(1)	(2)
	FATHERS		MOTHERS	
REFORM: COMPREHENSIVE SCHOOL $7/8 \rightarrow 9$				
Years of Education	0.265**	0.023	0.233**	0.040
	(0.118)	(0.025)	(0.113)	(0.042)
High Education	0.060**	0.007	0.057**	0.007
	(0.028)	(0.009)	(0.027)	(0.010)
Blue Collar	-0.002	-0.003	-0.004	0.002
	(0.018)	(0.008)	(0.018)	(0.007)
White Collar	0.086***	0.011	0.093***	0.004
	(0.029)	(0.010)	(0.030)	(0.005)
Age at Birth	-0.368***	0.089	-0.239*	0.114
	(0.115)	(0.175)	(0.124)	(0.077)
Missing	0.005	0.000	0.004	0.002
	(0.004)	(0.002)	(0.004)	(0.003)
MUNICIPALITY FE		✓		✓

Notes: Results refer to cohorts 1948 and 1953 (sample chosen accordingly to Meghir and Palme (2005)). Robust standard errors are clustered on municipality level. All regressions control for Cohort FE and gender of the child. Significance levels: $*$ 0.10 $**$ 0.05 $***$ 0.01. Parental **Years of Education** is calculated based on schooling variables from the Swedish Census 1970. **Blue and White Collar** are defined based on the SEI classification in the Swedish Census 1960.
Source: SIP. Own calculations

TABLE C.4: Balancing Regression (Cohorts 1938 – 1954)

	(1)	(2)	(1)	(2)
	FATHERS		MOTHERS	
REFORM: COMPULSORY SCHOOLING REFORM 7 → 8				
Years of Education	0.107**	-0.021*	0.081**	-0.012*
	(0.054)	(0.011)	(0.031)	(0.007)
High Education	0.022*	-0.003	0.020**	-0.003
	(0.012)	(0.003)	(0.009)	(0.002)
Blue Collar	0.028***	0.006*	0.025***	0.006*
	(0.008)	(0.003)	(0.008)	(0.003)
White Collar	0.037***	-0.004	0.051***	-0.005
	(0.010)	(0.003)	(0.014)	(0.003)
Age at Birth	-0.440***	0.094*	-0.164***	0.021
	(0.109)	(0.049)	(0.056)	(0.043)
Missing	0.001	-0.003*	-0.000	-0.001
	(0.002)	(0.002)	(0.001)	(0.001)
REFORM: COMPREHENSIVE SCHOOL 7/8 → 9				
Years of Education	0.260***	-0.008	0.160***	-0.004
	(0.079)	(0.009)	(0.053)	(0.007)
High Education	0.054***	-0.003	0.040***	-0.003
	(0.015)	(0.002)	(0.013)	(0.002)
Blue Collar	0.006	0.002	-0.003	0.001
	(0.008)	(0.003)	(0.009)	(0.003)
White Collar	0.059***	0.001	0.076***	0.000
	(0.013)	(0.002)	(0.019)	(0.002)
Age at Birth	-0.438***	-0.009	-0.159***	-0.009
	(0.081)	(0.040)	(0.039)	(0.034)
Missing	0.004**	-0.000	0.001*	-0.001
	(0.002)	(0.001)	(0.001)	(0.000)
MUNICIPALITY FE		✓		✓

Notes: Results refer to cohorts 1938 to 1954 (full reform period). Robust standard errors are clustered on municipality level. All regressions control for Cohort FE and gender of the child. Significance levels: $*$ 0.10 $**$ 0.05 $***$ 0.01.
Parental **Years of Education** is calculated based on schooling variables from the Swedish Census 1970. **Blue and White Collar** are defined based on the SEI classification in the Swedish Census 1960.
Source: SIP. Own calculations

C.2 Regression Results

TABLE C.5: Replication Meghir and Palme (2005)

	Males and Females			Males			Females		
	All	Father LE	Father HE	All	Father LE	Father HE	All	Father LE	Father HE
Panel A: Original estimates in MP, with further control variables									
Dependent Variable:	Years of Education ($\hat{S}^{TRAD}$)								
Reform (10% Sample, AER)	0.298***	0.405***	-0.130	0.252***	0.300***	0.092	0.339***	0.512***	-0.415**
	(0.075)	(0.070)	(0.124)	(0.081)	(0.093)	(0.174)	(0.105)	(0.087)	(0.193)
N	19,311	15,985	3,326	9,756	8,081	1,675	9,555	7,904	1,651
Municipalities	929	929	575	924	919	417	917	909	413
Dependent Variable:	Log Labor Earnings								
Reform (10% Sample, AER)	0.014	0.034***	-0.056***	0.009	0.031**	-0.077**	0.021*	0.038***	-0.042
	(0.009)	(0.009)	(0.019)	(0.014)	(0.014)	(0.031)	(0.012)	(0.013)	(0.027)
N	209,683	173,435	36,248	107,858	89,341	18,517	101,825	84,094	17,731
Municipalities	925	925	573	920	915	419	912	905	411
Panel B: No further control variables									
Dependent Variable:	Years of Education ($\hat{S}^{TRAD}$)								
Reform (10% Sample, AER)	0.184**	0.364***	-0.300**	0.212**	0.261***	0.304	0.090	0.433***	-0.993***
	(0.086)	(0.072)	(0.145)	(0.096)	(0.100)	(0.197)	(0.160)	(0.109)	(0.196)
N	19,311	15,985	3,326	9,756	8,081	1,675	9,555	7,904	1,651
Municipalities	929	929	575	924	919	417	917	909	413
Dependent Variable:	Log Labor Earnings								
Reform (10% Sample, AER)	0.009	0.033***	-0.074***	0.006	0.030**	-0.071**	0.008	0.036***	-0.077***
	(0.009)	(0.009)	(0.018)	(0.014)	(0.014)	(0.033)	(0.014)	(0.014)	(0.029)
N	209,683	173,435	36,248	107,858	89,341	18,517	101,825	84,094	17,731
Municipalities	925	925	573	920	915	419	912	905	411

Notes: Results refer to cohorts 1948 and 1953. Robust standard errors clustered at municipality level are reported in parenthesis. Significance levels: $*$ 0.10 $**$ 0.05 $***$ 0.01. All regressions control for municipality FE, birth cohort FE and gender.
Panel A shows the original estimates with additional control variables added to the regression. Control variables include test scores and school grades obtained when the pupils were in sixth grade and indicators for the county of residence. Panel B shows a standard difference-in-differences specification without further control variables.
Estimates are stratified by father's having only compulsory education (LE) or more than compulsory education (HE).
Source: Original data from Meghir and Palme (2005), AER (10% sample of total population). Own calculations.

TABLE C.6: Full Population (Cohorts 1948 and 1953)

	MALES AND FEMALES			MALES			FEMALES		
	All	Father LE	Father HE	All	Father LE	Father HE	All	Father LE	Father HE
DEPENDENT VARIABLE:	Years of Education ($\hat{S}^{TRAD}$)								
Reform	0.260***	0.292***	0.116	0.308***	0.327***	0.127	0.207***	0.252***	0.084
	(0.057)	(0.052)	(0.075)	(0.072)	(0.064)	(0.098)	(0.054)	(0.056)	(0.107)
N	215,888	141,118	25,197	110,250	72,139	13,049	105,638	68,979	12,148
Municipalities	1,026	1,026	958	1,026	1,026	873	1,026	1,026	840
DEPENDENT VARIABLE:	Years of Education $\hat{S}^{NEW}$								
Reform	0.458***	0.496***	0.160**	0.479***	0.508***	0.117	0.432***	0.479***	0.213**
	(0.111)	(0.076)	(0.073)	(0.135)	(0.095)	(0.098)	(0.091)	(0.064)	(0.092)
N	208,486	136,461	24,354	106,122	69,549	12,578	102,364	66,912	11,776
Municipalities	1,026	1,026	949	1,026	1,026	861	1,026	1,026	832
DEPENDENT VARIABLE:	Log Labor-Earnings								
Reform	0.014***	0.013**	0.025**	0.013*	0.010	0.026	0.016**	0.017**	0.020
	(0.005)	(0.006)	(0.013)	(0.007)	(0.008)	(0.018)	(0.007)	(0.008)	(0.020)
N	2,464,540	1,613,809	288,859	1,265,826	830,106	150,142	1,198,714	783,703	138,717
Municipalities	1,026	1,026	958	1,026	1,026	872	1,026	1,026	840

Notes: Results refer to cohorts 1948 and 1953 (sample chosen accordingly to Meghir and Palme (2005)). Robust standard errors clustered at municipality level are reported in parenthesis. Significance levels: $*$ 0.10 $**$ 0.05 $***$ 0.01. All regressions control for municipality FE, birth cohort FE and gender. Estimates are stratified by father's having only compulsory education (LE) or more than compulsory education (HE).
Source: SIP. Own calculations.

TABLE C.7: Full Population (Cohorts 1938 – 1954, 9-Year Reform)

	MALES AND FEMALES			MALES			FEMALES		
	All	Father LE	Father HE	All	Father LE	Father HE	All	Father LE	Father HE
DEPENDENT VARIABLE:	Years of Education ($\hat{S}^{TRAD}$)								
Reform	0.255***	0.282***	0.019	0.312***	0.360***	-0.014	0.187***	0.191***	0.046
	(0.022)	(0.019)	(0.035)	(0.029)	(0.025)	(0.051)	(0.024)	(0.026)	(0.053)
N	745,564	441,162	80,059	400,299	238,030	42,047	345,265	203,132	38,012
Municipalities	1,016	1,016	1,000	1,016	1,016	965	1,016	1,016	958
DEPENDENT VARIABLE:	Years of Education $\hat{S}^{NEW}$								
Reform	0.404***	0.429***	0.121***	0.426***	0.472***	0.046	0.380***	0.384***	0.202***
	(0.019)	(0.016)	(0.041)	(0.024)	(0.021)	(0.050)	(0.021)	(0.019)	(0.059)
N	775,877	463,959	78,567	394,433	235,817	40,448	381,444	228,142	38,119
Municipalities	1,016	1,016	998	1,016	1,016	957	1,016	1,016	962
DEPENDENT VARIABLE:	Log Labor Earnings (Age 30-35)								
Reform	0.002	0.002	0.014	0.009**	0.012***	0.014	-0.005	-0.009	0.014
	(0.003)	(0.004)	(0.011)	(0.004)	(0.004)	(0.013)	(0.006)	(0.008)	(0.020)
N	801,801	476,929	81,071	414,366	245,768	42,287	387,435	231,161	38,784
Municipalities	1,016	1,016	1,001	1,016	1,016	965	1,016	1,016	961

Notes: Results refer to cohorts 1938 to 1954 (full reform period). Robust standard errors clustered at municipality level are reported in parenthesis. Significance levels: $*$ 0.10 $**$ 0.05 $***$ 0.01. All regressions control for birth cohort FE and fathers level of education interacted with gender. All regressions include municipality FE (sample chosen accordingly to Meghir and Palme (2005)).
Estimates are stratified by father's having only compulsory education (LE) or more than compulsory education (HE).

TABLE C.8: Full Population (Cohorts 1938 – 1954, 8-Year Reform)

	MALES AND FEMALES			MALES			FEMALES		
	All	Father LE	Father HE	All	Father LE	Father HE	All	Father LE	Father HE
DEPENDENT VARIABLE:	Years of Education ($\hat{S}^{TRAD}$)								
Reform	0.013	0.016	-0.057	0.035	0.045	-0.034	-0.012	-0.014	-0.141
	(0.023)	(0.034)	(0.072)	(0.034)	(0.041)	(0.111)	(0.038)	(0.054)	(0.124)
N	297,105	130,897	16,995	159,836	70,893	9,107	137,269	60,004	7,888
Municipalities	414	414	389	414	414	367	414	412	362
DEPENDENT VARIABLE:	Years of Education $\hat{S}^{NEW}$								
Reform	0.208***	0.247***	0.063	0.237***	0.272***	0.142	0.181***	0.224***	-0.066
	(0.027)	(0.038)	(0.087)	(0.029)	(0.043)	(0.110)	(0.041)	(0.047)	(0.143)
N	311,955	138,677	16,725	157,765	70,306	8,697	154,190	68,371	8,028
Municipalities	414	414	387	414	414	365	414	413	362
DEPENDENT VARIABLE:	Log Labor Earnings (Age 30-35)								
Reform	0.005	0.002	0.022	-0.000	-0.011*	0.017	0.011	0.019	0.026
	(0.006)	(0.009)	(0.022)	(0.005)	(0.007)	(0.024)	(0.011)	(0.018)	(0.046)
N	319,579	141,343	17,476	166,661	73,404	9,304	152,918	67,939	8,172
Municipalities	414	414	387	414	414	368	414	413	360

Notes: Results refer to cohorts 1938 to 1954 (full reform period). Robust standard errors clustered at municipality level are reported in parenthesis. Significance levels: $*$ 0.10 $**$ 0.05 $***$ 0.01. All regressions control for birth cohort FE and fathers level of education interacted with gender. All regressions include municipality FE (sample chosen accordingly to Meghir and Palme (2005)).
Estimates are stratified by father's having only compulsory education (LE) or more than compulsory education (HE).

TABLE C.9: 2SLS Estimates based on Alternative Measure $\hat{S}^{NEW}$

	MALES AND FEMALES			MALES			FEMALES		
	All	Father LE	Father HE	All	Father LE	Father HE	All	Father LE	Father HE
9-YEAR REFORM:	Cohorts 1948 and 1953, Earnings 1985-1996 (MP)								
Years of Education	0.031***	0.029**	0.170	0.027**	0.022	0.195	0.039**	0.040**	0.101
	(0.010)	(0.012)	(0.104)	(0.013)	(0.016)	(0.198)	(0.015)	(0.017)	(0.113)
N	2,381,434	1,561,806	279,154	1,218,690	800,692	144,657	1,162,744	761,114	134,497
Municipalities	1,026	1,026	949	1,026	1,026	859	1,026	1,026	831
9-YEAR REFORM:	Cohorts 1938 and 1954, Earnings Age 30 – 35								
Years of Education	0.045***	0.040***	0.049***	0.032***	0.026***	0.039***	0.077***	0.049***	0.074***
	(0.001)	(0.002)	(0.004)	(0.001)	(0.002)	(0.004)	(0.002)	(0.008)	(0.007)
N	762,142	456,948	76,739	390,509	233,918	39,806	371,633	223,030	36,933
Municipalities	1,016	1,016	998	1,016	1,016	956	1,016	1,016	957
8-YEAR REFORM:	Cohorts 1938 and 1954, Earnings Age 30 – 35)								
Years of Education	0.053***	-0.005	0.059***	0.037***	0.030***	0.046***	0.057***	0.099***	0.020**
	(0.001)	(0.028)	(0.005)	(0.001)	(0.002)	(0.005)	(0.007)	(0.005)	(0.009)
N	304,629	136,067	16,431	156,881	69,968	8,627	147,748	66,099	7,804
Municipalities	414	414	384	414	414	365	414	413	358

Notes: Table shows 2SLS estimates with reforms as instrument for years of education. Years of education is measured by the alternative measure S^{NEW}. Robust standard errors clustered at municipality level are reported in parenthesis. Significance levels: $*$ 0.10 $**$ 0.05 $***$ 0.01. All regressions control for birth cohort FE and fathers level of education interacted with gender. All regressions include municipality FE (sample chosen accordingly to Meghir and Palme (2005)).
Estimates are stratified by father's having only compulsory education (LE) or more than compulsory education (HE).

TABLE C.10: 2SLS Estimates based on Traditional Measure $\hat{S}^{TRAD}$

	MALES AND FEMALES			MALES			FEMALES		
	All	Father LE	Father HE	All	Father LE	Father HE	All	Father LE	Father HE
9-YEAR REFORM:	Cohorts 1948 and 1953, Earnings 1985-1996 (MP)								
Years of Education	0.053***	0.044**	0.296	0.040**	0.031	0.221	0.081**	0.068**	0.555
	(0.018)	(0.021)	(0.255)	(0.020)	(0.024)	(0.203)	(0.033)	(0.034)	(1.900)
N	2,461,495	1,612,423	288,629	1,263,609	829,026	149,965	1,197,886	783,397	138,664
Municipalities	1,026	1,026	958	1,026	1,026	871	1,026	1,026	839
9-YEAR REFORM:	Cohorts 1938 and 1954, Earnings Age 30 – 35								
Years of Education	0.144***	0.163***	-0.130	0.069*	0.062	-0.313	0.200***	0.226***	-0.023
	(0.028)	(0.031)	(0.159)	(0.037)	(0.049)	(0.344)	(0.037)	(0.041)	(0.179)
N	703,779	418,244	74,533	379,069	226,279	39,275	324,710	191,965	35,258
Municipalities	1,016	1,016	996	1,016	1,016	956	1,016	1,016	949
8-YEAR REFORM:	Cohorts 1938 and 1954, Earnings Age 30 – 35								
Years of Education	0.057	0.085	-0.185	0.106***	0.178***	-0.118	0.004	-0.022	-0.466
	(0.041)	(0.061)	(0.209)	(0.034)	(0.061)	(0.187)	(0.088)	(0.124)	(0.798)
N	281,660	124,804	15,936	153,108	68,077	8,516	128,552	56,727	7,420
Municipalities	414	414	384	414	414	365	414	412	355

Notes: Table shows 2SLS estimates with reforms as instrument for years of education. Years of education is measured by the traditional measure S^{TRAD} based on the highest level of education. Robust standard errors clustered at municipality level are reported in parenthesis. Significance levels: $*$ 0.10 $**$ 0.05 $***$ 0.01. All regressions control for birth cohort FE and fathers level of education interacted with gender. All regressions include municipality FE. Estimates are stratified by father's having only compulsory education (LE) or more than compulsory education (HE).

C.3 Measuring Years of Education using Swedish Administrative Data

In the below sections we present two approaches – the traditional and a novel approach, respectively – to approximate years of education when using Swedish administrative data. The traditional approach adopted in the empirical literature is to assign years of education based on the highest attained educational level of education. We compare this to an alternative approach using information from different administrative registers on levels of education. We show that the latter has advantages when evaluating compulsory schooling reforms with administrative data.

C.3.1 Implementing the Traditional Approach

TABLE C.11: Sources for Traditional Measure $\hat{S}^{TRAD}$

EDUCATIONAL LEVEL	SUN2000 LEVEL	$\hat{S}^{TRAD}$ *(MP, 2005)*	$\hat{S}^{TRAD}$ *(Holmlund, 2008)*
Folkskola	1	7.33	7
Grundskola	2	9.62	9
Realskola	2	9.62	9.5
Upper Secondary $<$ 2 Years	3	10.39	11
Upper Secondary $\geq$ 2 Years	4	12.19	12
Post Secondary $<$ 2 Years	5	13.87	14
Post Secondary $\geq$ 2 Years	6	16.77	15.5
Post Graduate $\geq$ 2 Years	7	19.57	19

Notes: The table shows the assigned years of education for the highest level of education based on the SUN2000 Levels. The assigned length are taken from Meghir and Palme (2005) and Holmlund (2008).

Previous empirical work on education based on Swedish administrative data generally builds upon the Swedish Educational Registers which capture information on the highest level of education achieved (*level and type of education*).[1] To identify the total years of education the literature typically relies on a transformation of the highest level of education into an average length in years of education:

$$\text{YEARS OF EDUCATION} = \text{AVERAGE YEARS TO COMPLETE HIGHEST LEVEL OF EDUCATION.}$$

Some authors have implemented 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

[1]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.*

of total education (see e.g. Meghir and Palme, 1999) or Isacsson (1999).[2] 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 level of education classification and apply these averages as years of education. From this perspective, the standard transformation is empirically well-grounded and generally deliver a reasonable approximation of the actual length of education (see e.g. Antelius and Björklund (2000)). Table C.11 shows examples of the assigned years of education for the highest level of education based on the SUN2000 levels from Meghir and Palme (2005) and Holmlund (2008).

In the following we argue that the standard approach to approximate total years of education can miss important variation in the actual amount of education and that these differences can matter quantitatively. By construction, years of education based on the highest level of education are insensitive towards the amount of education below the highest achieved level of education, except cases where primary and lower secondary education coincides with the overall highest level of education.

For example, following the standard transformation all individuals with an upper secondary vocational degree (as indicated by their highest level of education in the Educational Registers) receive an equivalent number of years of education – corresponding to the average length of primary/lower secondary education – irrespective of their actual amount of education prior to their vocational training. This concerns a substantial part of the population for cohorts born prior to 1960. Before the unification of the primary and lower secondary educational system in the 1950s and 1960s, the diversity of various tracks lead to substantial variation in the amount of education before acquiring the highest level of education. Figure C.2 exemplifies this variation for the levels of education according to the SUN2000 classification in the LNU Survey of 2000.

[2]LNU questionnaire 2000: *Hur många år har Din sammanlagda skol- och yrkesutbildning på heltid varat? (Från första klass och uppåt.)*

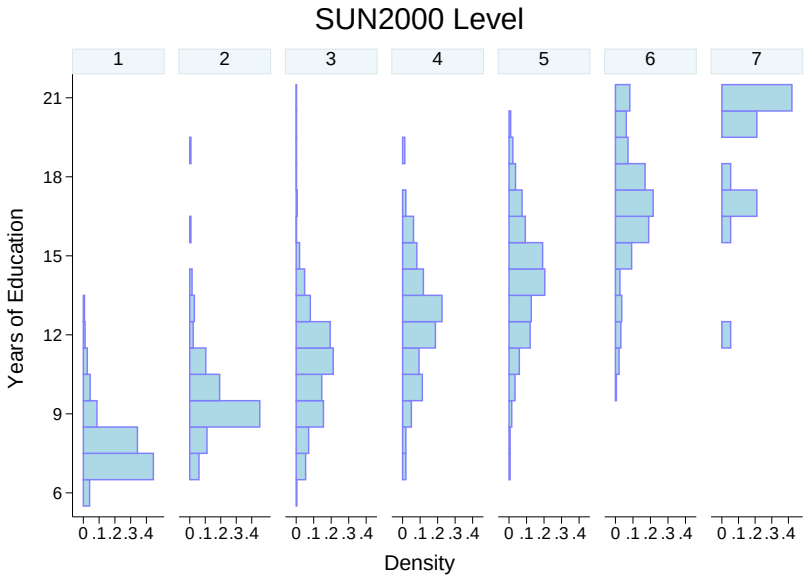


FIGURE C.2: Variation in Years of Education Within Highest Level of Education

The figure shows the distribution of years of education for each highest educational level according to the SUN2000/ISCED97 classification. Years of education are self-reported. In the figure years of education are top censored at 21 years.
Results refer to cohorts born 1938 – 1954.
Source: LNU survey, 2000. Own calculations

Figure C.2 clearly indicates large *within-group* variation in years of education for each educational level. This variation can be attributed to two sources:[3]

1. Variation in the length of the highest level of education.

2. Variation in the length of education *before* the highest level.

The first point arises from aggregating various programmes into broader categories and can be easily addressed by using a finer SUN2000 classification. 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 the highest level can be derived directly from the Educational Registers.

Regarding the second point, differences in the length of primary/lower secondary education before the highest level cannot be extracted from the information in the Educational Registers. These differences are hidden below the highest level of education.

C.3.2 An Alternative Approach

To overcome shortcomings of the standard approach we suggest an alternative approximation which combines information from the Swedish Census

[3]Additional possible sources of variation are grade retention, multiple vocational or academic degrees and misreporting in the self-evaluated variable.

from 1970 and the highest level of education from the Educational Registers. In the 1970 Census individuals born between 1911-1954 were asked to report their highest attended primary/lower secondary school type. Panel A of Table C.12 gives a classification of the categories from the Census 1970 and the years of education assigned to these school types.[4]

Regarding years of upper secondary education and post-secondary education 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. Panel B and C of Table C.12 provides the associated years of education for different SUN2000 codes. In order to avoid double counting we exclude highest level of educations 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 any further education. As the Census accounts already for *Gymnasium* we do not add additional years of education for the same level.

[4]The associated years of education are identical to the standard approach in case this degree matches the highest level of education. The years of education nicely coincide with a self-evaluated variable in the Swedish Living Conditions Survey (ULF) and are therefore also 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 level of education the SUN2000 Classifications indicate the length of associated type of *Gymnasium*. We used the most-frequent combination to classify different types of *Gymnasium* in the Census in short and long *Gymnasium*.

TABLE C.12: Sources for New Education Measure $\hat{S}^{NEW}$

EDUCATIONAL LEVEL	SOURCE CODE	*Education Programme*	*Length L*	*SUN2000 Level D*
PANEL A: PRIMARY AND LOWER SECONDARY EDUCATION		*G*		
Folkskola (7 Years)	11 (FoB1970, Utb)	1	7	1
Folkskola (8 Years) / Dropouts Realsk.	21 (FoB1970, Utb)	1	8	1
Grundskola	31 (FoB1970, Utb)	2	9	2
Realskola	41 (FoB1970, Utb)	2	9.5	2
PANEL B: UPPER SECONDARY EDUCATION		*U*		
Vocational Oriented Training (1 Year)	310/313/317 (SUN2000)	3	1	3
Vocational Oriented Training (2 Year)	320/323/327 (SUN2000)	4	2	4
Vocational Oriented Training (3 Year)	330/333/337(SUN2000)	5	3	4
Short Gymnasium	61 – 71 (FoB1970, Utb)	6	2	4
Gymnasium	51 – 59,72,81 – 89 (FoB1970, Utb)	7	3	4
PANEL C: POST SECONDARY EDUCATION		*C*		
Post-Secondary Non-Tertiary (1 Year)	400-430 (SUN2000)	8	1	5
Tertiary Education (2 Years)	520-529 (SUN2000)	9	2	6
Tertiary Education (3 Years)	530-539 (SUN2000)	10	3	6
Tertiary Education (4 Years)	540-549 (SUN2000)	11	4	6
Tertiary Education (5 Years)	550-559 (SUN2000)	12	5	6
Licentate	600-620 (SUN2000)	13	7	7
Doctoral Degree	640 (SUN2000)	14	9	7

Notes: The table shows the assigned years of education for the highest primary/lower secondary, upper secondary and post-secondary degree seperately.

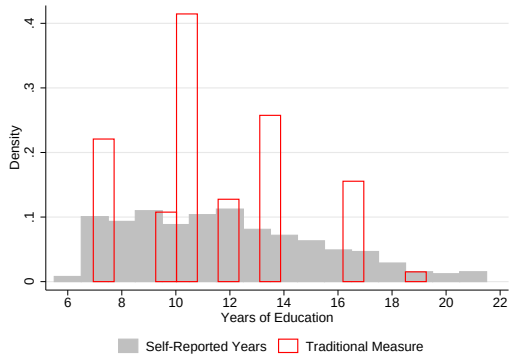
The combination of the information derived from the Census 1970 and the length of the highest level of education from the Education Registers gives an alternative measure for years of education:

$$\begin{aligned}\text{YEARS OF EDUCATION} &= \text{YEARS OF PRIMARY/LOWER SECONDARY} \\ &\quad + \text{YEARS OF UPPER SECONDARY} \\ &\quad + \text{YEARS OF TERTIARY.}\end{aligned}$$

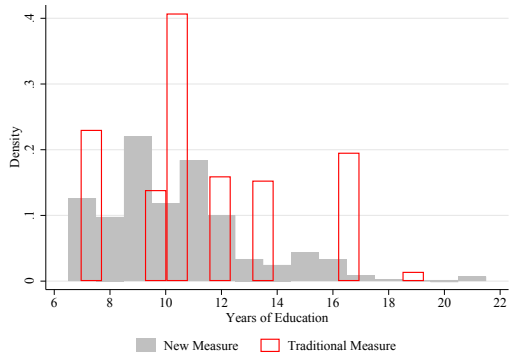
This measure is sensitive to changes within the primary/lower secondary school system – e.g. changes in compulsory education – and is able to capture the variation in education within highest level of education groups in the registers.

Figures C.3(a) and C.3(b) compare the distributional features of the new alternative measure compared to the self-evaluated variable from the LNU survey and the standard transformation based on the highest level of educations. The new 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.[5] Therefore, the total amount of education is slightly underestimated. In stark contrast, the distributional features of the standard transformation based solely 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).

[5]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*).



(a)



(b)

FIGURE C.3: Comparison Distribution Years of Education: (**a**) LNU Survey; (**b**) SIP.

The left figure shows the distribution of self-reported years of education compared to the traditional years of education measure $\hat{S}^{TRAD}$. The The right figure shows the distribution of the alternative measure S^{NEW} compared to the traditional years of education measure $\hat{S}^{TRAD}$. The traditional measure $\hat{S}^{TRAD}$ is constructed as in Meghir and Palme (2005). Density estimates refer to cohorts born 1938 – 1954.
Source: LNU survey, 2000 & SIP. Own calculations

C.3.3 Bias in Evaluating Compulsory Schooling Reforms

The traditional years of education measure $\hat{S}_i^{TRAD}$ leads to an underestimation of effects on years of education from compulsory schooling extensions. The bias is systematic and extends with varying degree to all measures of years of education based on overall highest educational level / ISCED classification. We illustrate this point by analytically deriving the bias from using $\hat{S}_i^{TRAD}$ when evaluating a compulsory schooling reform.

We denoted the highest overall educational degree for individual i by D_i, the highest primary/lower secondary educational level by G_i, the highest upper secondary programme by U_i and the highest tertiary programme by C_i. The traditional measure $\hat{S}_i^{TRAD}$ is given by

$$\hat{S}_i^{TRAD} = E(S|D_i) = f(D_i).$$

where $E(S|D_i)$ gives average years of education to achieve D_i as highest level of education.

Our alternative measure for years of education S_i^{NEW} assigns years of education separately for highest primary and lower secondary level of education, U_i the highest upper secondary level and C_i the highest post-secondary and tertiary level. It is defined as

$$\begin{aligned}\hat{S}_i^{NEW} &= \underbrace{E(L|G_i)}_{\text{a) Primary/Lower Sec.}} + \underbrace{E(L|U_i)}_{\text{b) Upper Secondary}} + \underbrace{E(L|C_i)}_{\text{b) Tertiary}} \\ &= l(G_i) + l(U_i) + l(C_i)\end{aligned}$$

where L denotes the average length of study needed for completion.

In order to derive the bias analytically we adopt a potential outcome framework. We implicitly assume that the reform is independent of potential outcomes after conditioning on further controls X such that $S^1, S^0 \perp Z|X$. For convenience, we omit further control variables from the notation. To simplify derivations and isolate the main source of the bias in the traditional measure we make three additional assumptions. First, we make the generally accurate assumption that the reform did not change the length of educational levels other than the compulsory levels, i.e. $l(U_i)^1 = l(U_i)^0 = l(U_i)$ and $l(C_i)^1 = l(C_i)^0 = l(C_i)$. Second, we assume that the reform had no spill-over effects to higher educational levels beyond primary and lower secondary education $U_i^1 = U_i^0$ and $C_i^1 = C_i^0$. This is a strong assumption as it excludes any substitution effects between types of education and the positive spill-over effects to higher education. Note that the alternative measure captures education at later stages separately and therefore directly incorporates such effects (see figure 3.2). We omit spill-over effects in order to focus on the main source of the bias from using the traditional measure. We also assume that the compulsory schooling reform was enforced, i.e. we do not exhibit *never takers*. This leads us the expected effect of a compulsory schooling extension defined as follows:

Definition Let Δ denote the change in the length of compulsory schooling. Under above assumptions the effect of the compulsory schooling reform is defined by

$$\mathbb{E}\left(\hat{\alpha}_1^{S_i}\right) = \mathbb{E}\left(S_i^1 - S_i^0\right) = \mathbb{P}\left(G_i^0 = 1\right) \cdot \Delta. \tag{C.1}$$

This represents the share of students with basic primary education before the reform $G_i^0 = 1$ which were forced to stay Δ years longer in school. We denote by $\hat{\alpha}_1^{S_i^{NEW}}$ the first-stage regression of the instrument Z on years of education based on the alternative measure and $\hat{\alpha}_1^{S_i^{TRAD}}$ based on the traditional measure.

Swedish Comprehensive School Reform

The Swedish comprehensive school reform made lower secondary education ($G = 2$) compulsory by introducing a universal 9-year comprehensive school. As secondary schooling was generally also 9 year prior to the reform, the length of educational levels was unaffected.[6]

Proposition C.3.1. *The alternative measure $\hat{\alpha}_1^{\hat{S}_i^{NEW}}$ is unbiased for estimating effects from the comprehensive school reform on years of education:*

$$\mathbb{E}\left(\hat{\alpha}_1^{\hat{S}_i^{NEW}}\right) = \mathbb{E}\left(\hat{S}_i^{1,NEW} - \hat{S}_i^{0,NEW}\right) = \mathbb{P}\left(G_i^0 = 1\right) \cdot \Delta. \tag{C.2}$$

Proof. Under the assumption that education is only affected at the compulsory level of education and the length of the compulsory degrees unaffected it follows that

$$\begin{aligned}
\hat{\alpha}_1^{\hat{S}_i^{NEW}} &= \hat{S}_i^{1,NEW} - \hat{S}_i^{0,NEW} \\
&\overset{\textit{no spill over}}{=} \sum_{g=1}^{2} 1\left(G_i^1 = j\right) \mathbb{E}\left[L|G_i^1 = j\right] - \sum_{g=1}^{2} 1\left(G_i^0 = j\right) \mathbb{E}\left[L|G_i^0 = j\right] \\
&\overset{\textit{no never taker}}{=} \underbrace{\mathbb{E}\left[L|G_i^1 = 2\right]}_{l^1(2)} - \sum_{g=1}^{2} 1\left(G_i^0 = j\right) \mathbb{E}\left[L|G_i^0 = j\right] \\
&\overset{l^1(2)=l^0(2)}{=} \left(1\left(G_i^0 = 1\right) + 1\left(G_i^0 = 2\right)\right) l(2) - 1\left(G_i^0 = 2\right) l(2) - 1\left(G_i^0 = 1\right) l(1) \\
&= (l(2) - l(1)) \cdot 1\left(G_i^0 = 1\right) \\
&= \Delta \cdot 1\left(G_i^0 = 1\right).
\end{aligned}$$

Taking expectations we get

$$\begin{aligned}
\mathbb{E}\left[\hat{\alpha}_1^{\hat{S}_i^{NEW}}\right] &= \mathbb{E}\left[1\left(G_i^0 = 1\right)\right] \cdot \Delta. \\
&= \mathbb{P}\left(G_i^0 = 1\right) \cdot \Delta.
\end{aligned}$$

□

In order to derive the bias for estimating the reform effect on education by using the traditional measure $\hat{S}_i^{TRAD}$ we denote the set of highest upper secondary and tertiary educational programmes with $\mathfrak{H}_i = \{U_i, C_i\}$

[6]For a small fraction of individuals with lower secondary education prior to the reform the length of lower secondary schooling was reduced from 10 to 9 years by the reform. Neither measure can exactly identify this difference. We follow the common practise to adjust the years of education for lower secondary education prior to the reform.

Proposition C.3.2. *The downward bias using the traditional education measure $\hat{S}_i^{TRAD}$ is given by*

$$bias\left(\hat{\alpha}_1^{\hat{S}_i^{TRAD}}\right) = (-1) \cdot \mathbb{P}\left(G_i^0 = 1 \wedge \mathfrak{H}_i \neq \emptyset\right) \cdot \Delta. \tag{C.3}$$

The bias reflects the compulsory schooling effect on all individuals with only basic compulsory education before the reform $G_i^0 = 1$, but having any sort of secondary (vocational) or tertiary education after graduating from school.

Proof. First, note that in the case of the overall highest educational level being lower secondary education ($D_i \leq 2$), the difference between primary and lower secondary education is again the compulsory schooling extension

$$\Delta = E(S|D_i = 2) - E(S|D_i = 1)$$

Following the same arguments as for the alternative measure above, it follows for $\hat{\alpha}_1^{S_i^{TRAD}}$

$$\begin{aligned}
\hat{\alpha}_1^{\hat{S}_i^{TRAD}} &= \hat{S}_i^{1,TRAD} - \hat{S}_i^{0,TRAD} \\
&\overset{\text{no spill over}}{=} \sum_{d=1}^{2} 1\left(D_i^1 = j\right) \mathbb{E}\left[S|D_i^1 = j\right] - \sum_{d=1}^{2} 1\left(D_i^0 = j\right) \mathbb{E}\left[S|D_i^0 = j\right] \\
&\vdots \\
&= \Delta \cdot 1\left(D_i^0 = 1\right)
\end{aligned}$$

and

$$\mathbb{E}\left[\hat{\alpha}_1^{\hat{S}_i^{TRAD}}\right] = \mathbb{P}\left(D_i^0 = 1\right) \cdot \Delta.$$

A first stage based on $\hat{\alpha}_1^{\hat{S}_i^{TRAD}}$ only identifies the reform effect on individuals with primary education as overall highest educational level prior to the reform $D_i^0 = 1$. The bias follows immediately from splitting the total effect on all compliers ($D_i^0 = 1$) into those with and without any sort of secondary (vocational) or tertiary education:

$$\begin{aligned}
\hat{\alpha}_1^{\hat{S}_i^{NEW}} &= \Delta \cdot 1\left(G_i^0 = 1\right) \\
&= \Delta \cdot 1\left(G_i^0 = 1 \wedge \mathfrak{H}_i \neq \emptyset\right) + \Delta \cdot 1\underbrace{\left(G_i^0 = 1 \wedge \mathfrak{H}_i = \emptyset\right)}_{=D_i^0=1}
\end{aligned}$$

Only the latter part is identified by applying $\hat{S}_i^{TRAD}$. □

Extension in the Old Primary School

We also estimate effects for a one-year compulsory schooling extensions in the old primary school system. This reform left the highest compulsory schooling level unaffected, but extended the length of the lowest level $l^1(1) = 8 > l^0(1) = 7$. As the traditional measure $\hat{S}_i^{TRAD}$ does not differentiate between different length at the old primary schools level, it cannot identify these changes. For the alternative measure it follows from above notation that the effects for this extension are captured in the alternative measure $\hat{S}_i^{NEW}$.

Appendix D

Additional Material: Chapter 5

D.1 Online tables and figures

TABLE D.1: ICD codes used to define causes of death and hospitalisation

DIAGNOSIS	ICD 10 CODE	ICD 9 CODE	ICD 8 CODE	ICD 7 CODE
Cancer	C00-D48	140-239	140-239	140-239
Circulatory disease	I	390-459	390-458	400-468
External causes	S,T,V,W,X,Y	800-999,E800-999	800-999,E800-999	800-999,E800-999

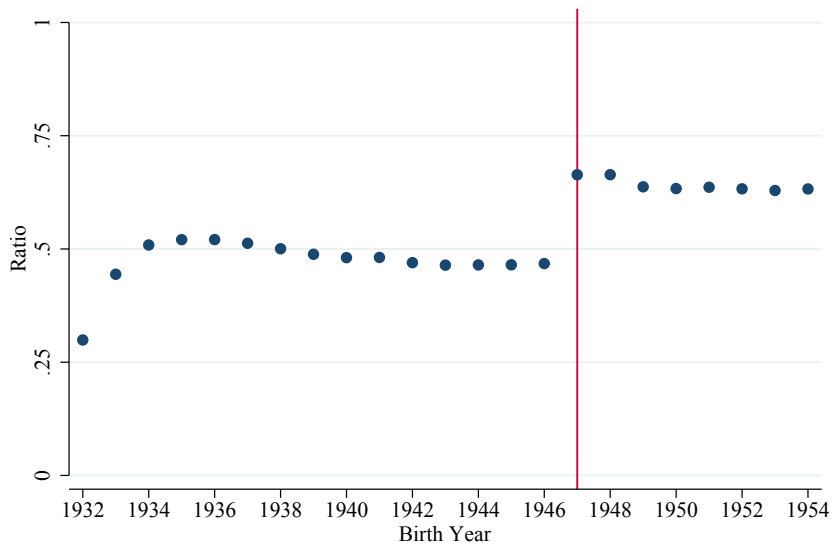


FIGURE D.1: Correlation between place of birth and place of residence over time

Notes: Scatter plots of correlation between place of birth and place of residence as recorded in the 1960 census over time. The vertical line at 1947 indicates when place of birth was changed from being recorded as municipality of the hospital to being recorded as place of residence of the mother.
Source: SIP. Own calculations.

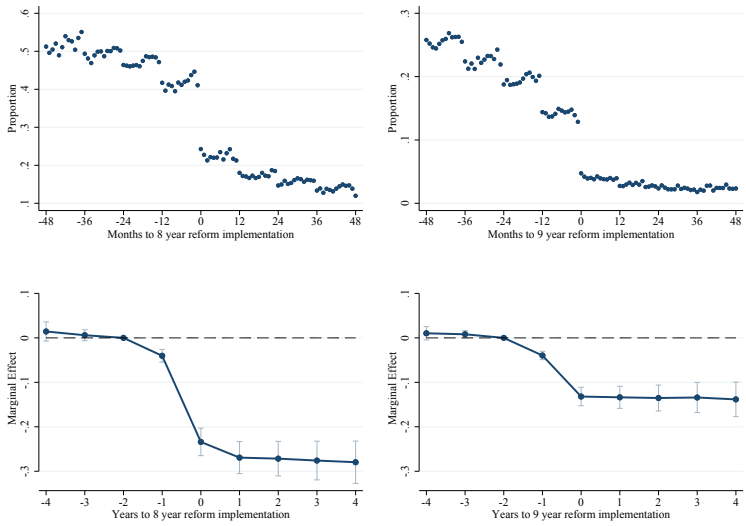


FIGURE D.2: Impact of the reforms on leaving school with 7 years of old primary school

Notes: Top two panels: Scatter plots of proportion with 7 years of old primary schooling by age in months measured as months to reform implementation in their municipality where the first cohort impacted is at zero. The bottom two panels: plot regression coefficients of an individual's birth year relative to the first reform cohort in their municipality on proportion with 7 years of old primary schooling (spikes represent the 95% confidence interval for each coefficient estimate). Municipality and birth year fixed effects and municipality level time trends are controlled for, a bandwidth of 10 years is used and clustered standard errors are estimated at the municipality level. Category 4 is four or more years after the first reform cohort. The reference category is "two years before the first reform cohort" (t-2).
Source: SIP. Own calculations.

TABLE D.2: Compulsory schooling reforms' impact on education (ULF survey)

	(1)	(2)
	8 YEAR REFORM	9 YEAR REFORM
Difference in Difference	0.333***	0.530***
	(0.124)	(0.074)
F-stat	7.20	50.90
N	8,200	19,153
Regression Discontinuity	0.627***	0.355***
	(0.160)	(0.105)
F-stat	15.45	11.56
N	8,200	19,153

Notes: This table shows the impact of the 8 year and 9 year school reforms on years of education. Each coefficient is from a separate regression by reform, method and group. The DiD specification includes birth cohort, survey year and municipality fixed effects and an observation window of up to 10 years before and after the first cohort impacted by the reform. Regression discontinuity estimates have separate second polynomials in the running variable either side of the cut-off and a full set of dummy variables for month of birth, survey year and gender. Robust standard errors clustered by municipality level (for DiD) and by the running variable (for RD) are in parentheses. Testing the null of the coefficient: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$
Source: ULF-Survey. Own calculations.

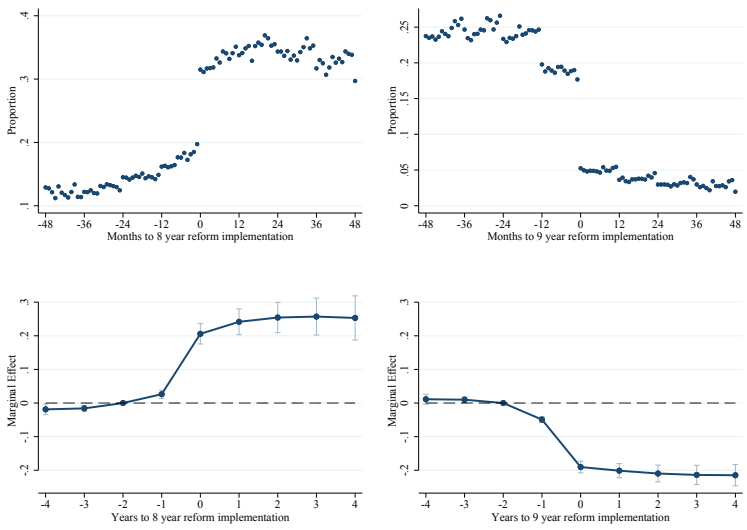


FIGURE D.3: Impact of the reforms on leaving school with 8 years of old primary school

Notes: Top two panels: Scatter plots of proportion with 8 years of old primary schooling by age in months measured as months to reform implementation in their municipality where the first cohort impacted is at zero. The bottom two panels: plot regression coefficients of an individual's birth year relative to the first reform cohort in their municipality proportion with 8 years of old primary schooling. See notes for table D.2
Source: SIP. Own calculations.

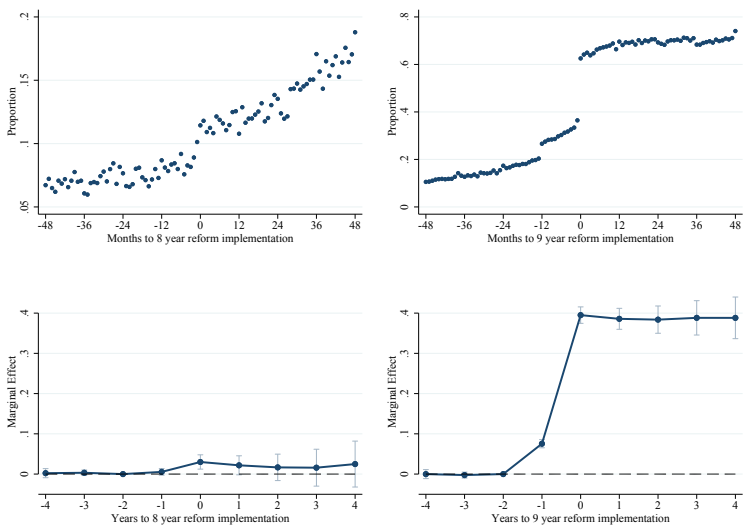


FIGURE D.4: Impact of the reforms on leaving school with 9 years of old primary school or 9 years of comprehensive school

Notes: Top two panels: Scatter plots of proportion with 9 years of old primary schooling or new comprehensive schooling by age in months measured as months to reform implementation in their municipality where the first cohort impacted is at zero. The bottom two panels: plot regression coefficients of an individual's birth year relative to the first reform cohort in their municipality on 9 years of old primary schooling or new comprehensive schooling. See notes for table D.2
Source: SIP. Own calculations.

TABLE D.3: Sensitivity analysis: Reforms' impact on education

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
PANEL A: 8 YEAR REFORM								
DiD	0.223***	0.221***	0.245***	0.271***	0.281***	0.292***	0.033	0.310***
	(0.031)	(0.027)	(0.023)	(0.024)	(0.024)	(0.025)	(0.021)	(0.027)
DiD with municipality trends	0.159***	0.218***	0.242***	0.264***	0.272***	0.281***	0.031	0.294***
	(0.033)	(0.025)	(0.022)	(0.023)	(0.023)	(0.024)	(0.020)	(0.025)
RD	0.264***	0.241***	0.240***	0.225***	0.230***	0.204***	0.017	0.285***
	(0.047)	(0.036)	(0.028)	(0.025)	(0.023)	(0.022)	(0.025)	(0.030)
N	143553	251958	352728	447649	534403	608642	534403	506676
PANEL B: 9 YEAR REFORM								
DiD	0.353***	0.438***	0.485***	0.511***	0.533***	0.546***	0.306***	0.630***
	(0.022)	(0.018)	(0.018)	(0.017)	(0.018)	(0.019)	(0.014)	(0.020)
DiD with municipality trends	0.259***	0.402***	0.460***	0.500***	0.523***	0.538***	0.299***	0.615***
	(0.024)	(0.018)	(0.019)	(0.018)	(0.019)	(0.019)	(0.014)	(0.022)
RD	0.194***	0.231***	0.298***	0.357***	0.392***	0.435***	0.249***	0.529***
	(0.031)	(0.020)	(0.021)	(0.022)	(0.023)	(0.024)	(0.015)	(0.021)
N	332517	599339	856337	1070486	1247808	1384702	1247808	1180536
Bandwidth (years)	2	4	6	8	10	12	10	10
Admin. data based educ. measure							✓	
Drop $t-1$ cohorts								✓

Notes: This table presents sensitivity analysis of the impact of the reforms on years of education. Model (5) is as in table 2.6 and Models 1 to 6 are as model (5) but vary the bandwidth. Model (7) is as (5) but uses the administrative data based measure of years of schooling as used in prior research. Model (8) is as model (5) but is a sandwich/donut estimator, removing individuals up to one year too old to be eligible for the reform. Testing the null of the coefficient: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$
Source: SIP. Own calculations.

TABLE D.4: Sensitivity analysis: LPM estimates of impact of the reforms on mortality by 2013

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
PANEL A: 8 YEAR REFORM								
DiD	0.0016	0.0017	0.0018	0.0012	0.0005	-0.0001	-0.0010	0.0001
	(0.0035)	(0.0031)	(0.0027)	(0.0024)	(0.0021)	(0.0021)	(0.0020)	(0.0024)
DiD with municipality trends	0.0024	0.0004	0.0012	0.0002	-0.0000	-0.0003	-0.0015	-0.0001
	(0.0043)	(0.0034)	(0.0028)	(0.0025)	(0.0022)	(0.0022)	(0.0022)	(0.0027)
RD	-0.0078*	-0.0005	0.0000	0.0018	0.0012	0.0027	0.0067***	0.0042
	(0.0040)	(0.0040)	(0.0034)	(0.0032)	(0.0028)	(0.0025)	(0.0026)	(0.0037)
N	143553	198598	251958	352728	447649	534403	608642	534403
PANEL B: 9 YEAR REFORM								
DiD	0.0020	0.0022	0.0013	0.0001	-0.0006	-0.0002	0.0001	-0.0002
	(0.0019)	(0.0016)	(0.0015)	(0.0013)	(0.0012)	(0.0011)	(0.0010)	(0.0012)
DiD with municipality trends	0.0021	0.0024	0.0013	-0.0004	-0.0010	-0.0003	0.0001	-0.0005
	(0.0023)	(0.0018)	(0.0016)	(0.0014)	(0.0012)	(0.0011)	(0.0011)	(0.0012)
RD	-0.0014	-0.0007	0.0016	0.0031*	0.0018	-0.0016	-0.0041***	-0.0057***
	(0.0020)	(0.0019)	(0.0019)	(0.0016)	(0.0015)	(0.0014)	(0.0014)	(0.0020)
N	332517	466287	599339	856337	1070486	1247808	1384702	1247808
Bandwidth (years)	2	3	4	6	8	10	12	10
Dummy for t-1 cohorts								✓

Notes: This table presents sensitivity analysis of the LPM estimates of the reforms on death by 2013. Model (5) is as in table 5.5. See notes for table D.3. Testing the null of the coefficient: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$
Source: SIP. Own calculations.

TABLE D.5: Sensitivity analysis: Cox proportional hazard regression results of the reforms' impact on mortality

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
PANEL A: 8 YEAR REFORM								
DiD	1.0155	1.0104	1.0114	1.0020	0.9942	0.9918	0.9856	0.9882
	(0.0296)	(0.0256)	(0.0225)	(0.0196)	(0.0172)	(0.0169)	(0.0163)	(0.0194)
DiD with municipality trends	1.0077	0.9995	1.0064	1.0003	0.9937	0.9905	0.9851	0.9894
	(0.0338)	(0.0264)	(0.0222)	(0.0197)	(0.0171)	(0.0169)	(0.0163)	(0.0194)
RD	0.9497*	1.0012	1.0074	1.0159	1.0119	1.0136	1.0210	1.0093
	(0.0297)	(0.0305)	(0.0261)	(0.0245)	(0.0210)	(0.0190)	(0.0185)	(0.0253)
N	143553	198598	251958	352728	447649	534403	608642	534403
PANEL B: 9 YEAR REFORM								
DiD	1.0266	1.0195	1.0061	0.9890	0.9839	0.9922	0.9982	0.9864
	(0.0245)	(0.0207)	(0.0182)	(0.0160)	(0.0143)	(0.0130)	(0.0126)	(0.0139)
DiD with municipality trends	1.0130	1.0148	1.0030	0.9872	0.9827	0.9888	0.9887	0.9844
	(0.0279)	(0.0216)	(0.0187)	(0.0160)	(0.0142)	(0.0130)	(0.0127)	(0.0138)
RD	0.9769	0.9906	1.0138	1.0269	1.0043	0.9925	0.9905	0.9754
	(0.0244)	(0.0234)	(0.0229)	(0.0199)	(0.0182)	(0.0160)	(0.0154)	(0.0192)
N	332517	466287	599339	856337	1070486	1247808	1384702	1247808
Bandwidth (years)	2	3	4	6	8	10	12	10
Dummy for t-1 cohorts								✓

Notes: This table presents sensitivity analysis of the Cox PH estimates of the reforms on survival till 2013. Model (5) is as in table 5.7. See notes for table D.3. Testing the null of the coefficient: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$
Source: SIP. Own calculations.

TABLE D.6: Sensitivity analysis: Reforms' impact on inpatient days admitted to hospital

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
PANEL A: 8 YEAR REFORM								
DiD	-1.0262	-0.5604	0.4691	0.7164	0.3372	0.2160	0.5121	0.4649
	(1.2257)	(0.9418)	(0.6379)	(0.5575)	(0.5086)	(0.4776)	(0.4835)	(0.6359)
DiD with municipality trends	-1.0365	-0.9562	-0.0454	0.4792	0.3770	0.2745	0.5651	0.6246
	(1.5446)	(0.9809)	(0.6723)	(0.5860)	(0.5329)	(0.4855)	(0.4839)	(0.6596)
RD	1.0865	0.2052	-0.4837	-0.2628	0.4911	0.8469	0.2609	1.9432*
	(1.1899)	(0.9675)	(0.9021)	(0.8472)	(0.8229)	(0.7659)	(0.7066)	(0.9906)
N	143553	198598	251958	352728	447649	534403	608642	534403
PANEL B: 9 YEAR REFORM								
DiD	0.4357	0.6149	0.0387	-0.0503	0.0622	-0.0561	-0.0273	0.0784
	(0.7895)	(0.7215)	(0.6081)	(0.5374)	(0.4671)	(0.4192)	(0.3890)	(0.4658)
DiD with municipality trends	-0.3990	0.6067	0.1252	0.0280	0.0887	-0.0707	-0.0244	0.0958
	(0.8890)	(0.7354)	(0.6252)	(0.5397)	(0.4661)	(0.4180)	(0.3877)	(0.4594)
RD	1.4492*	0.4467	0.1299	0.1559	0.1706	0.2070	-0.0244	1.1018*
	(0.7926)	(0.9088)	(0.7857)	(0.6385)	(0.5507)	(0.4980)	(0.4647)	(0.6199)
N	332517	466287	599339	856337	1070486	1247808	1384702	1247808
Bandwidth (years)	2	3	4	6	8	10	12	10
Dummy for t-1 cohorts								x

Notes: This table presents sensitivity analysis of the regressions estimates of the reforms on hospital admissions by 2013. Model (5) is as in table 5.8. See notes for table D.3. Testing the null of the coefficient: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$
Source: SIP. Own calculations.

TABLE D.7: Sensitivity analysis: Education effects on self-reported health and health behaviours for different bandwidth choice (5 year bandwidth)

	OLS (1)	RF-DiD (2)	IV-DiD (3)	RF-RD (4)	IV-RD (5)
PANEL A: 8 YEAR REFORM					
Fair or bad health	-0.028**	-0.017	-0.049	0.002	0.003
	(0.013)	(0.022)	(0.063)	(0.039)	(0.047)
N	2,857	5,047	5,045	5,047	5,047
Mean	0.31	0.22	0.22	0.22	0.22
Smoke daily	-0.012	0.006	0.017	-0.051	-0.060
	(0.010)	(0.023)	(0.061)	(0.031)	(0.037)
N	2,827	5,024	5,022	5,024	5,024
Mean	0.25	0.26	0.26	0.26	0.26
Obese	-0.025***	-0.011	-0.027	0.013	0.012
	(0.009)	(0.023)	(0.058)	(0.038)	(0.034)
N	1,799	3,092	3,070	3,092	3,092
Mean	0.14	0.11	0.11	0.11	0.11
Anxiety, concern etc	-0.004	-0.008	-0.021	-0.029	-0.025
	(0.010)	(0.028)	(0.067)	(0.036)	(0.033)
N	1,894	3,322	3,307	3,322	3,322
Mean	0.17	0.15	0.15	0.15	0.15
PANEL B: 9 YEAR REFORM					
Fair or bad health	-0.020***	0.006	0.011	-0.021	-0.079
	(0.004)	(0.014)	(0.027)	(0.016)	(0.074)
N	13,868	13,049	13,036	13,049	13,049
Mean	0.27	0.18	0.18	0.18	0.18
Smoke daily	-0.005	-0.025	-0.047	0.003	0.012
	(0.004)	(0.018)	(0.034)	(0.022)	(0.079)
N	13,731	12,988	12,975	12,988	12,988
Mean	0.27	0.28	0.28	0.28	0.28
Obese	-0.010***	0.006	0.014	0.014	0.071
	(0.003)	(0.015)	(0.033)	(0.017)	(0.090)
N	8,553	7,697	7,639	7,697	7,697
Mean	0.13	0.09	0.09	0.09	0.09
Anxiety, concern etc	-0.001	-0.006	-0.013	0.010	0.044
	(0.003)	(0.017)	(0.036)	(0.021)	(0.095)
N	9,126	8,417	8,363	8,417	8,417
Mean	0.16	0.15	0.15	0.15	0.15

Notes: This table presents the impact of compulsory school reforms on self-reported health and health behaviours. A 5 year bandwidth is used in DiD and RD regressions instead of 10 years as used in 5.9 to assess sensitivity to bandwidth choice. See notes for table 5.5.
Source: ULF-Survey. Own calculations.

D.2 Reconciliation with Lager and Torssander (2012)

Our results differ to those of Lager and Torssander (2012) who also look at the impact of the 9 year reform on mortality in Sweden. They found a small and statistically significant mortality reducing effect of the 9 year comprehensive school reform for the population aged 40 plus. There are two obvious differences between our paper and theirs. In their paper they make some adjustments to reform assignment based on the observation that a large proportion of individuals in certain municipalities had the minimum years of schooling one or two years before the reform was officially implemented and adjust the reform assignment accordingly. The other major difference is that they have data only up to 2007 and make some slightly different sample restrictions. In table D.8 rows 1 to 4 we move slowly between our main results towards their specification, reform assignment and sample selection. Column (1) is for the sample aged 40 plus and column (2) is for all ages. In row 2 we, like them, model the hazard without trends. This has next to no impact on the coefficients or the standard errors. In row 3 we use their reform assignment code that they kindly shared with us. This increases the impact slightly. In row 4 we attempt to replicate the sample restrictions of Lager and Torssander (2012) (cohorts born between 1943-1955, observed in 1960 and 1965 censuses, followed up to 2007, not emigrated). The results are essentially the same as for row 3 but the standard errors have increased. Column 5 are the actual results from Lager and Torssander (2012) where they find a small but significant reduction in mortality due to the school reform. Their result is not substantially different from our row 4 replication, but we are unable to repeat their significant finding exactly or their sample size. What we can conclude is that the results of this current paper are robust to reform assignment methodology and sample selection and suggest that the impact of education on health is really quite small.

TABLE D.8: Replication of Lager and Torssander (2012): Cox proportional hazard estimates

	9 YEAR REFORM	
	FROM AGED 40	ALL AGES
	(1)	(2)
ROW 1*: own sample, own reform assignment, trends*		
9 year reform	0.9826	0.9888
	(0.0132)	(0.0130)
ROW 2*: As per Row 1, no trends*		
9 year reform	0.9862	0.9922
	(0.0134)	(0.0130)
ROW 3*: As per Row 2, LT (2012) reform assignment*		
9 year reform	0.9794	0.9859
	(0.0136)	(0.0133)
N	1,242,843	1,247,808
No. Deaths	115,417	120,382
ROW 4*: As per Row 3, LT (2012) sample restrictions*		
9 year reform	0.9769	0.9896
	(0.0194)	(0.0160)
N	1,280,550	1,304,807
No. Deaths	73,909	98,166
ROW 5*: Results from LT (2012)*		
9 year reform	0.96	0.98
	[0.93-0.99]	[0.95-1.01]
N	1,200,519	1,247,867
No. Deaths	65,329	92,351

Notes: This table presents the impact of the 9 year compulsory school reform on mortality. See text for details. Robust standard errors clustered by municipality level in parentheses. Testing the null of the coefficient: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. 95% confidence intervals in brackets.

Appendix E

Additional Material: Chapter 6

E.1 Enrollment Statistics and Compliance to the SSA rule

I will provide indirect evidence on compliance with the school starting rule based on historical records on enrollment into primary education. The school statistics list students enrolled in primary school and the lowest secondary track according to their school cohorts for the years 1935, 1937 and 1938.[1] School cohorts were based on the Prussian entrance rule, running from July to June rather than by year of birth.

Recall that children born before the 1st of July had to enter school the year they turned six. If a student was enrolled but only 5 years old at the 1st of July, the student enrolled earlier. A student not enrolled but 6 years old at the 30th of June enrolls late.[2] Based on school cohort transitions for the three available school years, I can identify the share of early and late entries. I denote with N_a^t the number of students in school year t being a years at the 30th of June and estimate the following shares from the school records:

$$\begin{aligned} \text{Early Entrance} &: \frac{N_5^{1937}}{N_6^{1938}}, \frac{N_5^{1935}}{N_7^{1937}}. \\ \text{Late Entrance} &: \frac{N_7^{1938} - N_6^{1937}}{N_7^{1938}}, \frac{N_7^{1937} - N_5^{1935}}{N_7^{1937}}. \end{aligned}$$

Table E.1 provides enrolled students by their school cohort and the estimates for early and late entering in Prussia. The estimates based on transition suggest that compliance with the legislation was likely to be high. Later as well as earlier school entrance played a minor role and the majority of children followed the school entrance regulations. Column 6 shows that only 5% of the students enter school too early.[3] Column 5 shows that 2–3% of all students enter one year to late. Taken together only about 8% of the students did not enter school according to the cut-off rule. The relative high compliance is

[1] Enrollment statistics are given for three years in Statistisches Reichsamt (1936, 1938, 1939)

[2] Almost no student entered primary school younger than 5 years or older than 7 years.

[3] This is in line with the legal regulation which until 1938 did not include a transition periods allowing for an earlier entrance.

TABLE E.1: Student Primary School / Lower Track (Prussia)

	(1) Age 30th of June 1937	(2) Students N^{1937}	(3) Age 30th of June 1938	(4) Students N^{1938}	(5) Rel. Change $\frac{N^{1938}-N^{1937}}{N^{1937}}$	(6) Ratio $\frac{N^{1937}}{N^{1938}}$
School Cohort						
1.7.1931–30.6.1932	5	27,018	6	533,429	1874.35%	0.05
1.7.1930–30.6.1931	6	569,432	7	587,733	3.21%	0.97
1.7.1929–30.6.1930	7	615,583	8	612,722	-0.46%	1.00
	1935	N^{1935}	1937	N^{1937}	$\frac{N^{1937}-N^{1935}}{N^{1937}}$	$\frac{N^{1935}}{N^{1937}}$
1.7.1930–30.6.1931	4		6	569,432		
1.7.1929–30.6.1930	5	34,967	7	615,583	1660.47%	0.06
1.7.1928–30.6.1929	6	609,084	8	620,552	1.88%	0.98

Notes: Table shows enrollment of students in the school years 1937/38 and 1938/39.
Source: Statistik des Deutschen Reiches (1938,1939). Own calculations.

in stark contrast to more recent cohorts in Germany. During the 1970s more than 25% of each school cohort did not enter school according to the cut-off (Rüdiger, Kormann and Peez, 1976).[4]

Additionally, the aggregated statistics provide evidence on the different cut-off rule in the southern parts of Germany. In the school statistics, a school cohorts was also defined in accordance to the Prussian school legislation for Bavaria, Baden and Württemberg. As children in the southern regions had to enter school only when they had turned 6 at eastern instead before the 1st July, the increase in the statistics for grade 2 should be considerably larger as students born in May and June were not enrolled yet. In line with this expectations, appendix table E.2 shows a 10% larger increase for the southern states between grade 1 and grade 2. Table E.3 shows the ratio of early entries across provinces on a lower geographical level.

TABLE E.2: Student Primary School / Lower Track (Bavaria & Baden Würtemberg)

	(1) Age 30th of June 1937	(2) Students N^{1937}	(3) Age 30th of June 1938	(4) Students N^{1938}	(5) Rel. Change $\frac{N^{1938}-N^{1937}}{N^{1937}}$	(6) Ratio $\frac{N^{1937}}{N^{1938}}$
School Cohort						
1.7.1931–30.6.1932	5	4,808	6	168,254	3399.46%	0.03
1.7.1930–30.6.1931	6	178,249	7	204,836	14.92%	0.87
1.7.1929–30.6.1930	7	210,711	8	210,141	-0.27%	1.00

Notes: Table shows enrollment of students in the school years 1937/38 and 1938/39.
Source: Statistik des Deutschen Reiches (1938,1939). Own calculations.

[4]This was partly due to postponed entry due to the lack of readiness. The majority however entered school earlier due to a transition period from July to December allowing to enter the current year even if born after the cut-off date. Apparently many parents send their children to school earlier. In May 1970 60% of the students born in July of 1963 had already entered school.

TABLE E.3: Student Enrollment for School Cohort 1.7.1930–30.6.1931 at *Regierungsbezirk*

REGION GERMANY 1919-1945	FEDERAL STATE 1970 / DDR	STUDENTS		REL. CHANGE
		N^{1937}	N^{1938}	$\frac{N^{1938}-N^{1937}}{N^{1937}}$
PRUSSIA				
Total		569,432	587,733	3.21%
Brandenburg	DDR	38,063	39,188	2.96%
Stettin	DDR	18,905	19,305	2.12%
Province Saxiona	DDR	48,469	50,151	3.47%
Hannover	NI	10,884	11,406	4.80%
Hildesheim	NI	8,687	8,973	3.29%
Lüneburg	NI	7,519	7,942	5.63%
Stade	NI	7,756	8,137	4.91%
Osnabrück	NI	8,462	8,948	5.74%
Wiesbaden	HE/RP	17,228	17,704	2.76%
Koblenz	RP	13,599	13,754	1.14%
Trier	RP	8,069	8,144	0.93%
BAVARIA & BADEN WÜRTEMBERG				
Total		178,249	204,836	14.92%
Pfalz	RP	14,368	16,254	13.13%
REST OF GERMANY				
Total		166,507	181,994	9.30%
Saxiona	DDR	61,175	63,327	3.52%
Thüringen	DDR	20,070	23,856	18.86%
Hesse	HE	20,070	20,817	3.72%
Hamburg	HH	14,537	17,330	19.21%
Mecklenburg	DDR	10,308	12,140	17.77%
Oldenburg	NI	7,684	9,240	20.25%
Braunschweig	NI	6,817	7,040	3.27%
Bremen	BR	4,118	4,590	11.46%
Anhalt	DDR	4,591	5,496	19.71%
Schaumburg-Lippe	NI	3,191	3,364	5.42%
Saarland	SA	13,946	14,244	2.14%

Notes: Table shows enrollment of students in the school years 1937/38 and 1938/39. *Source:* Statistik des Deutschen Reiches (1938,1939). Own calculations.

E.2 Eligibility for Private Insurance

TABLE E.4: Eligibility for Private Sickness Fund (1970)

	Treatment		Control
	WESTERN PROVINCES	EASTERN PROVINCES	BAVARIA & BADEN-WÜRTTEMBERG
PUBLIC OR SELF EMPLOYED			
1st July (Early SSA)	0.001	-0.001	-0.006
	(0.003)	(0.005)	(0.004)
Observations	70,882	22,973	40,922
Mean Dep. Var.	0.196	0.148	0.211
HIGH INCOME			
1st July (Early SSA)	0.004	-0.002	-0.006
	(0.003)	(0.005)	(0.004)
Observations	52,870	18,940	28,450
Mean Dep. Var.	0.155	0.144	0.143

Notes: Regressions are restricted to a 1-month window before and after the cut-off date (June vs. July). All regressions control for cohort and gender fixed effects.
Results refer to cohorts 1916 to 1931. Robust standard errors are clustered on year-month of birth cells. Significance levels: $*$ 0.10 $**$ 0.05 $***$ 0.01.
Treatment (Western Provinces) refers to federal states which in 1970 consist mainly out of former prussian regions. **Treatment** (Eastern Provincesn) refers to individuals which lived in Eastern Prussia and former GDR before World War II (refugee migrants). **Control** units are federal states without former prussian regions.
Dependent Variables: Public or Self Employed are identified by the type of employment at Census week. High income refers to employees with monthly net-income larger than 1,200DM. The income threshold (*Versicherungspflichtgrenze*) in 1970 was euqal to a gross monthly earnings of 1,350DM. The Census reports only monthly net-income measured by 8 income categories. I assign eligibility if reported monthly net-income was larger than 1,200DM.
Source: West German Census, 1970. Own calculations

E.3 Migration

I classify the state of residence during school as well as the latest state of residence in the GSOEP into north and south. The survey only reports the state of school attendance if the last secondary school was finished after 1948. Therefore, I can only reasonably analyse migration patterns for individuals slightly younger than the study cohorts. However, I expect that migration patterns were relatively stable across cohorts and present a good proxy for the non-refugee population in West-Germany. Table E.5 gives the transition matrix, suggesting that with approximately 90% the clear majority of individuals did not encounter in north-south migration. (Hochstadt, 1999; Rainer and Siedler, 2009)

TABLE E.5: Migration Rates

	Latest State of Residence							
State of Residence	Southern States		Northern States		Bremen & Hamburg		Total	
during School	Obs.	%	Obs.	%	Obs.	%	Obs.	%
Southern States	1395	86.6	101	3.4	2	1.2	1498	31.4
Northern States	200	12.4	2822	94.5	50	29.2	3072	64.5
Bremen & Hamburg	15	0.9	62	2.1	119	69.6	196	4.1
Total	1610	100.0	2985	100.0	171	100.0	4766	100.0

Notes: Table shows migration rates from place of residence at last school attendance and last place of residence in the survey. Results refer to cohorts 1935–1960. Migration is net migration as individuals could potentially have migrated back and forth. State of Residence during school is based on the federal state of last secondary school attendance. State of residence is based on the either the latest state of residence I observe in the survey. This is either the year 2009 or the dropout year.
Northern States: Schleswig-Holstein, Lower Saxony, North Rhine-Westphalia, Hesse, Rhineland-Palestine, Saarland. **Southern States:** Bavaria & Baden Württemberg.
Source: GSOEP 1984–2009. Own calculations.

E.4 Additional Results

E.4.1 Robustness Specification

TABLE E.6: Balancing Regression (Southern States)

	(1) **Obs.**	(2) **Mean** *Outcome*	(3) **Month of Birth** *June/July*
PANEL A: CENSUS 1970 (COHORTS 1916 – 1931)			
Male	1,132,492	0.514	0.003
			(0.004)
WWII Refugee	361,358	0.237	0.008**
			(0.004)
Catholic	361,358	0.581	0.005
			(0.004)
Non-Christian	361,358	0.043	0.001
PANEL C: CENSUS 1970 (COHORTS 1958 – 1969)			
Years of Education (Father)	1,115,229	8.498	0.003
			(0.006)
Years of Education (Mother)	1,103,409	8.315	-0.001
			(0.005)

Notes: Table shows estimated effects of cut-off date (1st July) on pre-schooling characteristics for Prussian regions. Robust standard errors are given in parenthesis. If exact day of birth was not available robust standard error were calculated. All regressions control for year of birth dummies. Significance levels: $*$ 0.10 $**$ 0.05 $***$ 0.01.
Columns: (1) Number of Observations (full sample) (2) Mean of Outcome (4) Regressions are restricted to a 1-month window before and after the cut-off date (June vs. July). (5) Full sample with polynomial of 5th degree in day of birth (6) Regressions are restricted to a 2-month window before and after the cut-off date. Separate linear regression in day of birth around cut-off date.
Source: West German Census, 1970. Conscription Data, Armed Forces, 1935-1945. Own calculations.

TABLE E.7: Specification Robustness: WESTERN PROVINCES

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Treatment Group: WESTERN PROVINCES						
Dementia Risk	-0.0011	0.0038	-0.0010	-0.0010	0.0004	-0.0034*	0.0019
	(0.0032)	(0.0039)	(0.0020)	(0.0020)	(0.0032)	(0.0018)	(0.0039)
Crude Mortality Risk	-0.0084**	-0.0085	-0.0059**	-0.0059**	-0.0070*	-0.0088***	-0.0059
	(0.0036)	(0.0056)	(0.0024)	(0.0024)	(0.0038)	(0.0022)	(0.0050)
# Hospitalizations	-0.0213	-0.0323	-0.0014	-0.0016	-0.0254	-0.0176	-0.0345
	(0.0206)	(0.0302)	(0.0161)	(0.0161)	(0.0216)	(0.0147)	(0.0288)
Observations	63,040	30,794	126,805	126,805	126,805	386,331	386,331

Notes: Table shows estimated effects of cut-off date (1st July) on later life health outcomes. Robust standard errors are clustered on day of birth. All regressions control for cohort and gender fixed effects. Significance levels: $*$ 0.10 $**$ 0.05 $***$ 0.01.
Columns: (1) Regressions are restricted to a 1-month window before and after the cut-off date (June vs. July).
(2) Regressions are restricted to a 15-days window before and after the cut-off date (June vs. July).
(3) Regressions are restricted to a 2-month window before and after the cut-off date with a linear trend in day of birth.
(4) Regressions are restricted to a 2-month window before and after the cut-off date with separate linear regression in day of birth before and after cutoff.
(5) Regressions are restricted to a 2-month window before and after the cut-off date with separate polynomial of 2nd degree in day of birth before and after cutoff.
(7) Full sample with separate polynomial of 2nd degree in day of birth before and after cutoff.
(7) Full sample with separate polynomial of 5th degree in day of birth before and after cutoff.
Treatment (Western Provinces) refers to municipalities in 2006 consist mainly out of former prussian regions. **Control** units located in southern federal states without former prussian regions.
Source: Health Insurance Claims Data, 2006-2011. Own calculations.

TABLE E.8: Specification Robustness: SOUTHERN STATES

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Control Group: BAVARIA & BADEN-WÜRTTEMBERG						
Dementia Risk	0.0034	0.0034	0.0041	0.0040	0.0053	0.0020	0.0032
	(0.0037)	(0.0052)	(0.0028)	(0.0028)	(0.0041)	(0.0026)	(0.0051)
Crude Mortality Risk	0.0023	-0.0036	-0.0016	-0.0016	0.0017	-0.0040	0.0010
	(0.0058)	(0.0085)	(0.0042)	(0.0042)	(0.0062)	(0.0039)	(0.0081)
# Hospitalizations	0.0067	-0.0155	0.0151	0.0151	0.0227	-0.0226	-0.0038
	(0.0370)	(0.0539)	(0.0244)	(0.0243)	(0.0382)	(0.0217)	(0.0488)
Observations	24,890	12,140	50,171	50,171	50,171	153,101	153,101

Notes: Table shows estimated effects of cut-off date (1st July) on later life health outcomes. Robust standard errors are clustered on day of birth. All regressions control for cohort and gender fixed effects. Significance levels: $*$ 0.10 $**$ 0.05 $***$ 0.01.
Columns: (1) Regressions are restricted to a 1-month window before and after the cut-off date (June vs. July).
(2) Regressions are restricted to a 15-days window before and after the cut-off date (June vs. July).
(3) Regressions are restricted to a 2-month window before and after the cut-off date with a linear trend in day of birth.
(4) Regressions are restricted to a 2-month window before and after the cut-off date with separate linear regression in day of birth before and after cutoff.
(5) Regressions are restricted to a 2-month window before and after the cut-off date with separate polynomial of 2nd degree in day of birth before and after cutoff.
(7) Full sample with separate polynomial of 2nd degree in day of birth before and after cutoff.
(7) Full sample with separate polynomial of 5th degree in day of birth before and after cutoff.
Treatment (Western Provinces) refers to municipalities in 2006 consist mainly out of former prussian regions. **Control** units located in southern federal states without former prussian regions.
Source: Health Insurance Claims Data, 2006-2011. Own calculations.

E.4.2 Returns to Education

TABLE E.9: Mincer Wage Regression

	ALL	MALES	FEMALES
Years of Education	0.090***	0.084***	0.107***
	(0.000)	(0.000)	(0.001)
Observations	648,849	434,076	214,773

Notes: Table shows the OLS returns to years of education. Robust standard errors are reported in parenthesis. Significance levels: $*$ 0.10 $**$ 0.05 $***$ 0.01.
Results refer to cohorts 1916 to 1931. All regressions include birth cohort FE. The pooled estimates also includes a gender dummy.
Dependent Variable: *Monthly Log Net-Income*.
Source: West-German 1970 Census. Own calculations.

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