The Sooner the Better? Compulsory Schooling Reforms in Sweden

Martin Fischer  Martin Karlsson  Therese Nilsson
University of Duisburg-Essen  University of Duisburg-Essen  Lund University · Research
·
RGS Econ  University of Oslo  Institute of Industrial Economics (IFN)

December 15, 2014

Abstract

This paper analyses the impact of a compulsory schooling reform which extended mandatory education from 6 to 7 years during the 1930s and 1940s in Sweden. Gradually introduced over a time period of 13 years in more than 2,500 school districts, the reform generated large exogenous variation in educational attainment and consequently gives an ideal quasi-experimental setting for analyzing the causal impact of education. The empirical evidence suggests the reform had large impacts on educational attainment and earnings. Relying on survey data we are able to carefully evaluate potential mediators of the effects. The reform increased participation in vocational training and labor force participation which can contribute to explain the increase in earnings. The institutional setting and results of the reform are contrasted to the already extensively investigated follow-up schooling reform during the 1950s and 1960s in Sweden which among other things also extended the amount of schooling. While the first reform kept the school system intact which eases interpretation of the results, the latter included changes in curricula, an abolishment of the tracking system, and a change in pedagogical views and guidelines. Putting the Swedish case in a broader international context the paper adds to the understanding under which circumstances extension of compulsory schooling leads to changes later in life.

Keywords: Returns to schooling; Education Reform; Earnings

JEL Classification: I12, I14, I18, I21

*VERY Preliminary – please do not quote without permission

*The authors would like to thank Johanna Ringkvist for excellent research assistance and Arnaud Chevalier for helpful comments. Martin Fischer gratefully acknowledges financial support by the Ruhr Graduate School in Economics and the German Academic Exchange Service (DAAD).
1 Introduction

Evaluations of compulsory schooling reforms became increasingly popular in empirical economics as a tool to estimate causal impacts of schooling on various socio-economic outcomes in economics and overcome problems arising from schooling being essentially an individual choice: Is it formal education that rises wages or due to something else rising simultaneously both and therefore giving the empirical researcher the (false) impression that schooling is beneficial? To circumvent the endogeneity of schooling empirical researchers often relied on sources of various exogenous changes in the amount of schooling. In the absence of randomized experiments, laws that changed the amount mandatory of schooling are likely to be one of the most commonly employed and promising empirical strategies to establish a causal relationship between formal schooling and socio-economic outcomes in observational studies. Usually variants of instrumental variable strategies are employed to estimate the effects of education.

Many early studies on compulsory schooling reforms presented large causal impacts of schooling on earnings, i.e. the instrumental variables estimates exceeded the pure OLS estimates describing the association between education and outcomes. More recent results however indicate small and negligible impacts on economic outcomes or health in various countries, especially across European countries. Given a probably strong prior believe that schooling should be beneficial economist started to explain the puzzle of zero returns to compulsory schooling (see e.g. Pischke and Von Wachter 2008).

Focusing on the diversity in the estimated health gains from education found in the literature, Gathmann et al. 2014 summarize that even researchers using seemingly identical identification strategies - compulsory schooling reforms - arrive at different conclusions depending on the country, the time period or gender studied. In the following analysis they use aggregate data from 18 European compulsory school reforms implemented during the twentieth century and examine the dispersion in mortality gain across time and contexts. Their result suggests that early twenty century reforms are more effective than later reforms in improving a specific health outcome, mortality. Even more importantly they conclude that conflicting results appear to be a systematic feature of the given reform setting.

The question at hand is whether we can learn something from evaluating and comparing results from compulsory schooling reforms across time and countries. It is not that surprising that changing the (quasi-)experimental design also changes the results of the experiment. However, by carefully evaluating the specific context, institutional background and political discussion during the time of implementation important conclusion can probably still be drawn. It appears to exist an almost

\footnote{In fact it seems to be one of the few areas in empirical economics where also zero results are publishable…}
worldwide consensus that some amount of education should be obligatory for the society as a whole. Given the fact that a certain amount of compulsory education is a widespread global phenomena, conditions for a successful implementation of such should be of high interest for policy makers and researchers alike.

As mentioned above many recent findings for European countries regarding changes of the compulsory level of schooling are suggesting rather small effects on various socio-economic outcomes (e.g. Pischke and Von Wachter 2008 or Devereux and Hart 2010). This paper describes Sweden as a counterexample to this currently emerging empirical literature - no zero returns to education in Sweden. 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. Part of this extension is already extensively investigated in various fields in economics and also extremely well documented otherwise. From 1949 onwards till 1969 municipalities in Sweden introduced a comprehensive school system which also included a change in the years of compulsory schooling from 7 to 9 years. The introduction of the new comprehensive school additionally abolished the tracking system, unified the curriculum and exhibited various pedagogical changes. This however constitutes only the second part of an ongoing reformation of the Swedish school system. Starting in 1936 schooldistricts introduced independently a seventh grade which was compulsory after the introduction and raised the mandatory level of schooling by one year. As stated the later reform has been used quite a lot in various strands of the economics literature (see e.g. Meghir and Palme, Lager and Torssander 2013, Lindqvist et al 2014), the impact of the early reform has never been thoroughly evaluated.

The first contribution of the paper is therefore the description and throughout analysis of a yet unexplored compulsory schooling reform implemented in Sweden during the 1930s and 40s. In terms of regional and spatial variation this reform indeed shares strong similarities of the later Swedish reform which is often referred as an excellent social experiment to investigate the causal relationship between education and socio-economic outcomes. We will argue that the earlier reform shared this advantages. Furthermore, by carefully evaluating specific institutional features of two consecutive compulsory schooling during their implementation this paper aims to work out conditions under which raising the minimum level of education actually improves socio-economic outcomes. An important part is the connection of the reforms to their actual intended aims following the discussion during the time of implementation. We argue that the early reform was indeed a relative pure schooling reform in the sense that it mainly aimed at improving human capital of

\(^2\)The responsible regional unit changed with the administrative reform in 1952.

\(^3\)Some schooldistricts especially in the southern region of Sweden Skåne introduced the seventh grade earlier.

\(^4\)Fischer et al 2013 study the educational effect on mortality using aggregated data for the county level on the share of students affected by the increase in compulsory schooling, but to our knowledge no one ever explored reform information on parish level, allowing for more variation across space and time.
students on a large scale. The second reform had much broader aims and might have had less straightforward implications than a simply raise in human capital for exactly that reason. The Swedish school system therefore faced a reform which mainly extended the amount of schooling and another reform which completely changed the school system and the content taught in schools. Although consecutive in time, the two reforms are rarely mentioned together in the literature. Describing the main characteristics two reforms and how they are related, this paper may also provide some novel perspectives and improve our understanding of the results in the existing literature analyzing the more recent compulsory schooling reform in Sweden. Our results are also in line with those presented recently for other European countries.

Though with a special focus on the institutional framework the paper is essentially an empirical paper. The next section summarizes the two compulsory schooling reforms, provides an institutional comparison and embeds it in a broader context of European compulsory schooling reforms. The following sections describe the data used in the empirical section and lay out the empirical strategy employed in the analysis. Section 5 presents the empirical results on a variety of socio-economic outcomes. A discussion of the results is given in section 6.

2 Background on the Educational System and the Reforms

Starting in 1842, compulsory school attendance was introduced an applied to all resident children in Sweden. The compulsory attendance implied both the right to cost-free schooling and an obligation to take part in the schooling offered (Fredriksson 1971). This was the first step towards a more general and thoroughly organized school system where every parish had to offer schooling by an approved teacher (Slunga 2000).

The implementation of general elementary school system was initially slow, but in the 1920s schooling was compulsory for six years and almost all children underwent at least common elementary schooling of six years (Fredriksson 1971). At the time there was however large variation across school districts regarding format and content of education. The central management of the education system was practiced by the Ecklesiastikdepartementet, the Ministry for Ecclesiastical affairs, but there were more than 2,500 school districts in the country and the local administration of compulsory education was the responsibility of a district school board.

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, which came to restructure the school’s work according to central guidelines. Utbildningsplanen was a governing document and included time-tables and

\footnote{To a large extent the school districts corresponded to the division of Swedish parishes.}
sylabuses for compulsory school (Lindmark, 2009) and remained intact until the 1950s.

Education was generally offered by public schools and schooling started at the age of seven. The vast majority ended school after six years in Folkskolan. Students deciding to follow an academic path continued to lower secondary school after finalizing Folkskolan to pass the realexamen. The educational system however had parallel systems, and students could also choose to continue onto lower secondary school already at the age of 11 after the fourth grade. After realexamen, students either left school or entered the upper secondary level and had to sit in the studentexamen – comparable to the French BAC, German Abitur and the English A-levels – after three years of studies.

The Swedish schooling system was highly selective, with a majority of students only completing compulsory schooling and with very few students continuing to secondary schooling. In 1930, less than two per cent of the adult population had upper secondary education or more (Björklund et al., 2004). In 1940, only 10 per cent of the cohort graduating from Folkskolan continued to secondary school (Fredriksson, 1971). With an increasing demand for higher education this share increased continuously and secondary schools became more widely spread geographically (Murray, 1988), but higher education was still a rather rare thing. In 1952 the share of children continuing to junior secondary schooling still was only 38 per cent (Lindensjö et al., 1986).

Students in Folkskolan were attending full-time schooling approximately eight months per year. In the rural areas where children often helped out in the agrarian sector school districts sometimes offered half-time reading, but in 1933, 93 per cent of all pupils took part in full-time schooling (Fredriksson, 1950). It is difficult to state something about the quality of the education across school districts, but completion rates were high: 90% of all pupils finished Folkskolan with full curriculum (Fredriksson, 1950).

2.1 The introduction of seven years of compulsory schooling

July 1 1936, the national Government pass the bill that seven-year schooling should be compulsory. Already, in 1925, a clause had been introduced in the primary school code that a seventh school year could be mae compulsory in a district (Fredriksson, 1950). Following the 1925 clause several school districts started introducing an extra year of schooling already before 1936. Furthermore,

---

6With the exception of girls’ schools, private schools were never important in Sweden (Ecklesiastikdepartementet, 1935).
7In 1918, it was 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). These were all practically oriented one- or two-year courses in domestic science, craft or manufacturing given at local schools, taught with a very low intensity (Fredriksson, 1950)
8Yet another indication of the selective system was that only 5 per cent of a cohort continued to upper secondary schooling in 1940 (Fredriksson, 1971)
in the southernmost region of Scania many districts had introduced an extra year of schooling in 1936 due to a different interpretation of the compulsory schooling law. Figure 2.1 illustrates the development of school district introducing a 7th year across various years.

Still, in 1936 only 16 per cent of all children in rural areas attended seven years of schooling at this time. The implementation itself took place rather independently on the local level. The decision to extend compulsory schooling by an extra year was taken by the school board of the school district.

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 and teachers and school inspectors were concerned about students’ poor knowledge (see e.g. Persson 1933). 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 one additional year of education was of importance to maintain a democratic society (Ecklesiastikdepartementet 1935). In the debate preceding the introduction of the reform, politicians were also often benchmarking 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. At the time most stakeholders seem to agree on the importance to improve Folkskolan and studying the political debate preceeding the 1936 decision there was no major diverging views on the seven year reform.

In line with the underlying motive for the parliamentary decision, 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 was that the school districts should distribute the pre-reform compulsory school

---

9 Manual checks from exam catalogues from the early 20th century actually show that the seventh grade was mandatory in Scania even in the early 1920s at the moment it was introduced.
curricula over seven years instead of six (Ecklesiastikdepartementet 1935).

The reform was not implemented at the same time in all school districts, but it was stipulated that seven years of schooling 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. Consequently, as school districts could decide rather independently when to introduce the seventh grade, the timing is far from being a random assignment. Figure 1(a) and 1(b) underline this issue by showing that districts with higher share of the population working in agriculture and with lower per capita income where late in introducing the seventh year.

\begin{figure}[h]
\centering
\includegraphics[width=\textwidth]{figure1.png}
\caption{Boxplot: (a) Agricultural Share (Census 1930) in School District by Year of Implementation; (b) Total Annual Income per Capita (Census 1930) in School District by Year of Implementation.}
\end{figure}

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 2(a) shows the trends in implementation in urban and rural areas and reports the proportion of school districts that had at least seven years of compulsory schooling at the end of each school year, while Figure 2(b) shows the number of students affected by the reform by birth cohort.\footnote{One view put forward in the final political debate claimed the importance of individual freedom and argued that the seventh year should be an option and a choice rather than mandatory. The same debater is also concerned that the seventh year only will deepen students knowledge, rather than adding new knowledge since there will be no change in the curricula (Riksdagens Protokoll 1936).}

\footnote{The difference between the figures reported by Fredriksson (1950) and figure 2(a) follows from that Fredriksson focus on schools rather than school districts. This difference makes sense since larger districts, having a greater number of schools, were more commonly early adopters of the reform.}

6
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).

2.2 The introduction of the unified nine year compulsory schooling

Despite an on-going national implementation of a seventh compulsory year of schooling and despite
a reform in 1927 with the aim of tying Folkskolan and Secondary Schooling closer, changes were
not seen as enough to foster future democratic members of society (Edgren, 2011). Compared to
other countries few students matriculated to secondary education and compulsory education was
still quite modest and there was a fear that Sweden would be left behind (Marklund, 1982).

Following the above, a reform work was initiated with a mission to investigate issues relating
to the complete school system – both lower and higher education. The main purposes are to
increase equality of opportunity by postponing the tracking decision to higher grades, and to meet
the demand for junior secondary education among the baby boom cohorts born in the mid-1940s.

The political commission in charge of the investigation is also very clear about the view that
traditional school and the working methods there within are obsolete (Richardson, 1978). The
commission suggests that a nine-year compulsory and comprehensive school should be introduced
and the working methods have to change their mold towards more individualized and activating
elements, pandering students drive and independence (Marklund, 1982). The board however wants

\[12^\text{The reform of 1927 implied that students taking an academic path could continue to Realskolan from Folkskolan either after four years, or after six years.}\]

\[13^\text{Compulsory education in the US endured at least until 16 years of age, in Germany there were Volkschule or Hauptschule until the age of 15 and then mandatory vocational training, and in the UK students generally had nine year of compulsory schooling in the late 1930s.}\]
to evaluate the new school form before introducing it to all schools. The reform is therefore introduced during an assessment period as an experiment where compulsory education is extended in different locations at different points in time.

In practical terms not much else other than the implementation of the seven year reform changes in the Swedish school system from 1936 until 1949. However, from 1949/1950 compulsory education is again gradually extended across municipalities in the country. The first year of assessment 14 municipalities are selected to participate in the trial. The evaluation period was not run as a random experiment, but a board chose municipalities in order to obtain a mix of different municipality types. 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 nine-year school throughout the country in 1962. Seven years later, by 1969, all municipalities were obliged to have the new comprehensive school running (Marklund 1982).

The reform reshaped the entire school system and students were kept in the same school for nine years. Besides extending compulsory education from seven 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 and a new educational plan for schools to follow is released in 1962 (Lgr 62) focusing on individualization and activity learning (Larsson and Westberg 2011).

2.3 Comparing the two reforms

Both the reforms introduced an exogenous change in the extent of compulsory schooling in Sweden. As regards the exact definition of treatment it however seems that the two reforms differ. The above account on the earlier reform makes it clear that treated pupils faced no significant school system changes, nor any increase in the curriculum to be covered or working methods in class. Thus, any effects should mainly be driven by changes in the amount of time spent in education. On the other hand, with the abolishment of the tracking system the second reform implied a fundamental change of the complete school system and the reform also came with a new curriculum program.

---

14 At the time there were 1037 municipalities in Sweden.
15 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.
16 See e.g. discussion by Orring et al. (1962) on that all earlier reforms than the 9-year reform more or less left the fundamental work of schools unaffected.
Any effects from the second 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.

There is evidence on that practicing teachers during the time of the second reform initially lacked relevant teaching material. This suggests that the first part of the second reform likely was more similar to the first reform, mainly affecting the time spent in education rather than changes in curricula, pedagogic methods, teaching equipment and working material. Also the first period of the second 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. As stated by Marklund (1982), p. 180: the reform schools between 1955 and 1960 conformed to a streaming system that in terms of routes was not too much different from the old parallel school with one common school route and one junior secondary school route.

Another issue relates to who was affected by the different reforms. As noted in previous research on the 9-year reform, attending a reformed school increased the probability of having at least 9 years of schooling by about 12 per cent (Holmlund et al., 2011), i.e. there was an increase in the probability of attending at least the following higher level of schooling. With respect to the earlier reform the probability of attending secondary education likely increased, but still most students finished after Folkskolan implying that the two reforms probably affect somewhat different margins. Similarly, the 7-year reform postpone the moving to lower secondary schooling of high performing students by one year, but does not abolish the tracking system, while the later reform fully abolish the tracking system.

The reforms were gradually implemented across school districts and municipalities. The timing of implementation in individual school districts 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 seven or the nine year school reform at the local level. During the assessment period of the second 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 second reform was implemented earlier in municipalities with higher incomes and with higher average education, see e.g. Lundborg et al. (2014).

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 seven year 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 first 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).

2.4 An International Comparison

The current literature on compulsory schooling reforms and their returns to labor market achievements tend to agree that context matters - high or low returns to compulsory schooling reforms depend on a whole set of different institutional parameters such as labor market structure, the educational system in general, wage distributions, etc. In order to enhance the understanding of how extensions of mandatory schooling may affect later-life outcomes it appears fruitful to compare these settings also between countries.

The German educational system and the compulsory schooling reform in the middle of the 20th century extending the basic track by one more year from 8 to 9 years of mandatory education is of special interest for the current setting (Pischke and Von Wachter, 2008, see). First because the Swedish school system was extremely similar in spirit to the German educational system until the 1950s when the comprehensive school was introduced in Sweden. The German school system was (and still is) highly selective with tracking students relatively early after 4 or 6 years into three different types of secondary schools on performance. The reforms were also similar in spirit - as described above the reform of the Swedish Folkskola extended the lowest track also by one more year. Interestingly for the current analysis Pischke and Von Wachter (2008) find robust results of at best very low returns to the compulsory schooling reform in Germany. They address

17 The German reform concerned the West German school system. Interestingly, the introduction of the comprehensive school system in Sweden shared many similarities with the East German school system and there was a frequent exchange between the two countries with respect to educational matters. It also appears not to be a coincidence, that the decision of replace an educational system similar to the German one with a system placing strong emphasis on e.g. democratic values and educating citizens falls exactly into the years after Second World War.

18 Before a name change to the current Hauptschule the German equivalent was called Volksschule which is the literal translation.
this convincingly to the fact that the initial stock of skills where already comparably high even in the lowest schooling track in Germany at the time of the reform implementation. Yet another similarity to the Swedish context is the fact that German students seldom started working after finishing school without additional vocational training.

Another relevant study to place our findings in a more general perspective is the recent study by (Grenet, 2013). By contrasting two compulsory schooling reforms in France (1967) and England and Wales (1972) Grenet finds that only the latter increase wages due to the reform. These differences are assigned to a sharp drop in students leaving school without qualification in England and Wales. This rise in credentials appeared not to have solely a signalling effect on earnings but rather increased the skill levels of school-leavers. For various institutional reasons the comparable French reform did not alter this margin.

3 Data and Sample Selection

3.1 Reform data sources

To evaluate the 7-year reform we create a data set with information on the specific year that a school district implemented an extra compulsory year of schooling in Folkskolan. This information was extracted and digitized from historical archives. Primarily the data on the specific school district reform year comes from standardized exam catalogues that every school had to file. The exam catalogue is an annual documentation that provides individual information on each student, e.g. their attendance and their grades in various topics, but it also provides information on the name of the school, the school type, the weekly length of the semester, and information on the number of years of education provided by the school.  

The data set contains information on the year of reform for more than 95 per cent of all existing school districts at the time of interest. A guide to the Swedish Folkskolereform (2014) provides detailed information on the reform and its connection to later school reforms, as well as information on collection procedure and sources of information used to create the seven year reform dataset. The manual is also provides information on various sensitivity tests and check-ups performed to assess the quality and validity of our reform data set and the findings from these exercises. 

\[\text{At the time of interest there were more than 2,500 school districts in Sweden. On average each school district had two schools, with several districts only including one school. In most but not all cases the school district correspond to the geographical area of Swedish parishes at the time.} \]

\[\text{We have already validated our way of assigning reform status to a district in different ways: E.g. by comparing our data with official statistics on the share of school districts that had implemented the seven year reform in each county of Sweden for the years 1938-1945 or manual check-ups for all schools in certain districts to ensure that all schools implemented the reform at the same time within a district. So far we are confident that the accuracy of the reform data is very high.} \]
3.1 gives an impression of the temporal and spatial variation in the implementation.

![Figure 3: Implementation of 7th Grade](image)

Occasionally we also make use of the second reform which introduced the comprehensive 9-year school in Sweden. To classify individuals exposed or unexposed to the nine year compulsory schooling reform and the introduction of *Enhetsskolan* we use a data set provided by Helena Holmlund, described in detail in Holmlund (2008). To control for potential confounding factors and to examine the observable characteristics of the school districts implementing the seven year compulsory school reform early on, we digitized parish level information on the sectoral shares of agriculture and manufacturing, the proportion of women of fertile age in the population, income, wealth and the degree of urbanisation from the 1930 Census.

---

21 We thank Helena Holmlund for generously sharing the data set and syntax making it possible to match the reform data to affected cohorts.
3.2 ULF - Statistics Sweden’s Survey on Living Conditions

To analyse the effects of the reform on socio-economic outcomes we use the Swedish Survey on Living Conditions (ULF). The survey has been conducted on a continuous basis in Sweden since 1975 and comprises 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 are asked to answer questions regarding living conditions.

The ULF survey covers a wide range of variables on economic and educational outcomes and offers a solid basis for our analysis. Most importantly the survey incorporates detailed questions about the schooling history of individuals including how many years they actually spent in school or whether they attended any kind of vocational training. Those detailed variables are important for a better understanding of our study. From the educational register, usually employed in evaluations of school reforms in Sweden, only the highest educational degree can be obtained. These degrees are then typically translated into years of schooling to estimate returns to education. There are two critically issues with this approach in the current case.

First, the registers do not differentiate between different levels in the lowest category of Folk-skola. It is impossible to see from this source whether an individual visited a Folkskola with 6 or 7 years. This could be solved by assigning the mandatory years by the reform indicator itself ((see e.g. Pischke and Von Wachter 2008)). However, this procedure would introduce non-trivial measurement error problems if we assign the years of schooling based on a reform indicator measured with error and use this later on as an instrument for an IV estimator. Measurement error in a binary indicator introduces a non-classical measurement error which nevertheless attenuates estimates. If an instrument is measured with error it generally attenuates reduced form and first stage proportionally resulting in a consistent instrumental variable estimate. If schooling is assigned on the basis of the misclassified instrument the proportionality is destroyed and IV estimates are downward biased. Since the ULF data do not explicitly identify whether an individual is exposed to a school reform, we assign each individual a treatment status based on birth year and parish of birth. Consequently our instrument is measured with error due to assignment by place of birth if they move in their first years of life.

---

22 The present study was approved by the Lund University Regional Ethics Committee, DNR 2013/288.
23 This problem has been brought up by Kemptner et al. (2011) for the German compulsory schooling reform facing essentially the same data limitation. See also Appendix section ??.
24 We do not have information on individuals location of residence in schooling age. According to previous research on the second reform the proportion of children moving between municipalities affected and not affected by the reform between birth and school age was around 4 per cent in each direction Meghir and Palme 2004. On a currently generated micro-level data set we could check the movements between the first year of life and entering the fifth grade for cohorts 1930-1934. Between 5-10% of the students moved in the first 11 years of life.
25 An additional serious source of measurement error comes from the fact that the parish of birth reported for cohorts born until 1946 refers to the place of the actual birth of an individual, i.e. if an individual was born in a
Additionally, the register data does recover the full impact of the reform. Essentially everyone which underwent any kind of vocational training is classified as having a degree higher than just basic education. This however does not imply that these individuals were not affected by the extension of Folkskola and therefore actual had more years of schooling (though the same highest educational degree). Figure 3.2 demonstrates that the underestimation of potential compliers from the registers can be quite severe. The survey includes questions on the highest school type (and not degree) attended. It also covers the categories from the educational registers. Therefore we can compare those two measures of educational attainment. While the registers assign in 1940 already almost 50% of the individuals having more education as only basic education in Folkskola (which is true as they went on and got e.g. an apprenticeship), the survey reveals that the highest school type was nevertheless for 80% just Folkskola. In terms of compliance to the reform and years of education the latter is the relevant group. Note that this issue does not affect and reduced form evaluation of the reform.

![Figure 3.2: Ratio of Students with more than Folkskola](image)

Figure 4: Ratio of more than Folkskola

The use of the survey naturally comes with costs compared to the registers - due to a small sample size we loose precision of estimates. The data in the present study comes from the four waves of the survey collected in 1980-1981, 1988-1989, 1996-1997, and 2003-2004, including a cross hospital the parish of birth reported refers to the location of the hospital in which someone was born (Skatteverket 2007). The transition to institutional delivery started in the late 1920’s and was initially very smooth and mainly driven by individual-level demand-side decisions. In the mid 1940’s, however, most births took place out of the home and many municipalities sis not have their own maternity ward (Wisselgren 2005). All in all, we do not believe this caveat apply for the the cohorts affected by the first reform as they generally are born when most births took place in the home of a family by the help of a midwife. Examining our data we do not find any structural breaks for the treated cohorts of the first reform. 

Note that this problem essentially translates to all studies employing the later reform as instrumental variables. In a separate analysis we are currently examining the sensitivity of results from the change to the comprehensive system based on the Swedish Registers. 

We are currently examining the reduced form effects of reform also on register data based on the total population and extend the survey waves. As the Swedish registers are also the source for the income information in ULF, results are comparable eventually.
section and a rotating panel where the respondents are interviewed every eight years. The waves are special in the sense that they both include a set of baseline variables and answers to a special questionnaire focusing on individual health. The size of the sample is about 7,500 persons per year. Although the response rates in the ULF surveys have decreased over time, from about 86 per cent in 1980/1981 to about 75 per cent in 2003/2004 (Sweden 2008), the general response rate is quite acceptable.

4 Empirical Strategy

The first part of the empirical analysis focuses on estimating the reduced form estimates for the two reform implementation on socio-economic outcomes denoted by $y_{igcs}$.

$$y_{igcs} = \beta_0 + \beta_1 Z_{igsc} + \beta_2 X_{igcs} + \delta_s + \mu_c + \nu_g + \epsilon_{igcs};$$

where $c$ indicates the cohort, $g$ the regional unit, $Z_{igsc}$ is an indicator of whether an individual has been exposed to the extension of compulsory schooling, $X_{igcs}$ is a vector of potential additional covariates including a sex dummy, $\mu_c$ and $\nu_g$ are fixed cohort and regional effects.

The identification strategy essentially relies on a difference in difference comparison. We therefore take care of the non-randomness of both school reforms with respect to the administrative unit (see figure 1(b) and 1(a) for 7 year reform) but assume a common time trend of outcomes across Sweden. Regional specific time trends are included as a robustness test. Due to the presence of always takers, $\beta$ captures an intention to treat parameter, i.e. the effect of the policy on the population. Due to measurement error in the instrument they present a lower bound for the effects of the reform. To facilitate our interpretations with respect to predictions from economic theory we also estimate unconditional quantile treatment effects of the policy along the earnings distribution (see (Firpo et al., 2009) and (Havnes and Mogstad, 2014) or an application).

We further present instrumental variable estimates for attending more schooling than the old compulsory schooling level of 6 years which rescale the reduced form estimates. Under some additional assumptions such as monotonic response to the instrument with respect to educational attainment the IV-estimates capture an average treatment effect on the untreated ((see e.g. Imbens and Angrist, 1994)).

---

28 One should however keep in mind that this actually is more than a simple robustness check. In fact it changes the identification strategy from a common trend to common growth rates and might pick up part of the treatment effect one intends to measure. This might for example in wage regression with higher returns to experience with increasing level of schooling. A specific cohort trend would pick those up (see e.g. (Wolfers, 2006).)

29 Never takers can be ruled out as only in rare exceptions the reform was not binding.
5 Results and Discussion

5.1 Descriptive Analysis

One third of the school-districts introduced the seven year reform only in 1948, the last year of the reform period. As a first step we present a graphical representation for the effects of the reform on parishes introducing the seventh grade in 1948 based on a regression discontinuity design with cohort as running variable.\textsuperscript{[6]} Figures 5(a) and 5(b) indicate large impacts on educational attainment and sizeable income effects. They also reveal that the reform was binding - there are literally no individuals with less than seven years of schooling after the implementation.\textsuperscript{[31]}

\begin{align*}
\text{Figure 5: Parishes introducing 7th Grade in 1948: (a) RDD on Educational Attainment (>6 Years of Schooling); (b) RDD on Log-Income.}
\end{align*}

It seems to be important to take labor market protections laws and compulsory schooling laws together into consideration as the labor market constitutes a relevant outside option in contrast to gain more education. Restricting work possibilities is an option for governments to force students to gain more education, e.g. in the form of vocational training. Indeed figure 6(a) points to that the raise of the minimum working age in 1931 increased educational attainment in form of vocational training in districts with only six years of mandatory schooling. Instead the labor protection law appeared to have no effect on higher education in school. Interestingly the fraction of vocationally trained individuals is even higher if a seventh grade is introduced. This result remains in the regression analysis if selection into reform is taken into account. We will associate a part of the increase in labor market performance to the increased qualification. This conclusion appears in line with the current literature on other educational reforms in Europe. In any case the Labor

\textsuperscript{[6]} As one third of the school-districts introduced the reform only in the last year of the reform period we have sufficient observations for analyzing the year 1948 separately even with survey data.

\textsuperscript{[31]} A placebo test on a brake in 1948 for reform districts implementing the seventh grade prior to 1948 (i.e. already treated) shows no systematic changes around the reform year (see Appendix C).
Act itself does not confound the labor market analysis in this paper as all our cohorts are born after 1920 and therefore all experienced the same labor market restrictions after the new labor protection law was implemented.

![Graphs showing the ratio of students in vocational training and more basis track education over years.](image)

Figure 6: (a) Having Any Kind of Vocational Training; (b) Having more than Primary School Education (Folkskola).

5.2 Regression Analysis: Education

Estimates for educational attainment in table 1 of the seven year reform show an comparable large share of compliers. This is not surprising given the large fraction of the population only taking the basic education track in the 1930s and 1940s. The inclusion of parish characteristics in the regression leads to an expected decrease in the effects of the introduction of the seventh grade, reflecting the positive self selection of more prosperous schooldistricts in implementing the reform early. Taking this into account and comparing the more comparable districts still leads to an substantial increase in the share of students with more than the old compulsory level of 6 years of schooling.

Furthermore, only controlling for cohort effects suggests that quite a high fraction of students also extended their educational attainment and went to the higher track of Realskola or even more. These effects however disappear after controlling for schooldistricts characteristics. Rather than fostering more education within the schoolsystem the reform seem to have fostered vocational training. These regression results therefore are in line with the graphical representation in figure 6(a).

As discussed in section 3 we use the place of birth to assign our reform indicator. This naturally leads to some measurement error in the estimation for all those which migrated before entering the new compulsory schooling level and therefore changed reform status and in cases a missclassificaiton occurs due to hospital coding mentioned in section 3.2. The estimates likely present a lower bound.
Table 1: Reform Effects on Education

<table>
<thead>
<tr>
<th></th>
<th>(1)</th>
<th>(2)</th>
<th>(3)</th>
<th>(4)</th>
</tr>
</thead>
<tbody>
<tr>
<td>Reform Indicator</td>
<td>0.432</td>
<td>0.365</td>
<td>0.360</td>
<td>0.354</td>
</tr>
<tr>
<td></td>
<td>[0.383,0.481]</td>
<td>[0.321,0.409]</td>
<td>[0.299,0.421]</td>
<td>[0.292,0.415]</td>
</tr>
<tr>
<td>Indicator: 7 Years of more Schooling</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Reform Indicator</td>
<td>0.165</td>
<td>0.029</td>
<td>0.020</td>
<td>0.019</td>
</tr>
<tr>
<td></td>
<td>[0.102,0.229]</td>
<td>[-0.010,0.067]</td>
<td>[-0.033,0.072]</td>
<td>[-0.033,0.072]</td>
</tr>
<tr>
<td>Indicator: Realskola or Higher Education</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Reform Indicator</td>
<td>0.084</td>
<td>0.050</td>
<td>0.089</td>
<td>0.098</td>
</tr>
<tr>
<td></td>
<td>[0.041,0.127]</td>
<td>[-0.001,0.101]</td>
<td>[0.016,0.162]</td>
<td>[0.022,0.175]</td>
</tr>
<tr>
<td>Any Vocational Training</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>N</td>
<td>5769</td>
<td>5263</td>
<td>5769</td>
<td>5769</td>
</tr>
<tr>
<td>Cohort Dummies</td>
<td>✓</td>
<td>✓</td>
<td>✓</td>
<td>✓</td>
</tr>
<tr>
<td>Survey Year Dummies</td>
<td>✓</td>
<td>✓</td>
<td>✓</td>
<td>✓</td>
</tr>
<tr>
<td>1930 Census</td>
<td>✓</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Cluster FE</td>
<td></td>
<td>✓</td>
<td></td>
<td>✓</td>
</tr>
<tr>
<td>County Cohort Trends</td>
<td></td>
<td></td>
<td>✓</td>
<td>✓</td>
</tr>
</tbody>
</table>

Standard Errors for 7 Year reform clustered on the school district level. 95% CI in parenthesis.

for the effect of the reform on educational attainment.

5.3 Regression Analysis: Labor Market Outcomes

Table 2 shows the intention to treat parameter for labour market outcomes of the reform introduction. After taking the differences in parish characteristics into account, the estimates still suggest a relatively large response to the 7-year schooling reform. The introduction raised labor earnings in the population on average by at least 5% given that our instrument is measured with error. The large reduced form effect of the policy change is likely attributable to the large share of compliers of the reform. They are also in line with the political debate during the 1930s. In the background section we laid out that the discussion during the time of implementation centered around in recognition of a lack of basic skills for children leaving folkskola after six years. The extension was precisely intended to increase the stock of human capital.

We complement the above results by estimating quantile treatment effects along the distribution of earnings. We interpret the unobserved common factor underlying the quantile regression as basically consisting of ability and background characteristics. This allows an interpretation to what degree education can substitute initial disadvantages. Meghir and Palme (2005) achieve a similar analysis for the 9-year reform by subgroup analysis conditional on measures of cognitive ability and fathers education. Their results from a quantile perspective are represented in figure 7(b). The later reform had a larger impact on those in the lower end of skill/background mix (according to their analysis high ability and low social background) while for other parts of the distribution the impact even turned out to be negative. Interestingly the quantile estimates of
Table 2: Reduced Form: Effects on Labor Market Outcomes

<table>
<thead>
<tr>
<th>(1)</th>
<th>(2)</th>
<th>(3)</th>
<th>(4)</th>
</tr>
</thead>
<tbody>
<tr>
<td>Log-Earnings</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Reform Indicator</td>
<td>0.143</td>
<td>0.051</td>
<td>0.048</td>
</tr>
<tr>
<td>[0.056,0.231]</td>
<td>[-0.036,0.139]</td>
<td>[-0.083,0.179]</td>
<td>[-0.094,0.192]</td>
</tr>
<tr>
<td>N</td>
<td>4089</td>
<td>3700</td>
<td>4089</td>
</tr>
</tbody>
</table>

Work Fulltime

| Reform Indicator | 0.052 | 0.034 | 0.061 | 0.060 |
| [0.014,0.089] | [-0.011,0.078] | [-0.003,0.124] | [-0.010,0.129] |
| Mean Fulltime | 0.615 | 0.610 | 0.615 | 0.615 |
| N | 4142 | 3749 | 4142 | 4142 |

Cohort Dummies ✓ ✓ ✓ ✓
Survey Year Dummies ✓ ✓ ✓ ✓
1930 Census ✓ ✓ ✓ ✓
Cluster FE ✓ ✓ ✓ ✓
County Cohort Trends ✓ ✓ ✓ ✓

Standard Errors for 7 Year reform clustered on the school district level. 95% CI in parenthesis.

the 9-year reform also indicate very low or even negative estimates of the reform on the lowest part of the ranking variable. This likely mirrors that individuals were kept longer in school by the reform (compliers) without gains with respect to their labor market success. This might indicate the possibility of overeducation\textsuperscript{32}.

Figure 7: Boxplot: (a) QTE 7-Years Reform; (b)QTE 9-Years Reform.

IV estimates aiming at capturing the average treatment effect on the untreated indicate comparably high increases in labor earnings for those affected by the 7-year reform. One should however keep in mind that these estimates are not fully attributable to increases in labor market productivity. Estimations for effects of educational attainments on annual labor earnings represent in

\textsuperscript{32}In a dynamic framework this possibility has been discussed by Belzil et al. (2011). Essentially their model incorporates negative instrumental variable estimates from a compulsory schooling reform by allowing for greater returns to experience on the labor market. If compulsory schooling reforms reduce the amount of labor market experience (which they usually do by construction) the net effect might turn out to be negative (even if all returns are strictly positive).
general a mixture of labor supply effects and productivity gains \[\text{Antelius and Björklund (2000)}\]. The reform had in fact also large positive effects on fulltime employment.

<table>
<thead>
<tr>
<th>Table 3: IV Estimates: Effects on Labor Market Outcomes</th>
</tr>
</thead>
<tbody>
<tr>
<td>(1) (2) (3) (4)</td>
</tr>
<tr>
<td>Log-Earnings</td>
</tr>
<tr>
<td>&gt; 6 Years (Survey)</td>
</tr>
<tr>
<td></td>
</tr>
<tr>
<td>0.340</td>
</tr>
<tr>
<td>[0.144,0.537]</td>
</tr>
<tr>
<td>0.130</td>
</tr>
<tr>
<td>[-0.104,0.364]</td>
</tr>
<tr>
<td>0.130</td>
</tr>
<tr>
<td>[-0.208,0.469]</td>
</tr>
<tr>
<td>0.162</td>
</tr>
<tr>
<td>[-0.191,0.515]</td>
</tr>
<tr>
<td>N</td>
</tr>
<tr>
<td>4089</td>
</tr>
<tr>
<td>3700</td>
</tr>
<tr>
<td>4089</td>
</tr>
<tr>
<td>4089</td>
</tr>
<tr>
<td>Work Fulltime</td>
</tr>
<tr>
<td>&gt; 6 Years (Survey)</td>
</tr>
<tr>
<td></td>
</tr>
<tr>
<td>0.122</td>
</tr>
<tr>
<td>[0.037,0.207]</td>
</tr>
<tr>
<td>0.087</td>
</tr>
<tr>
<td>[-0.030,0.205]</td>
</tr>
<tr>
<td>0.160</td>
</tr>
<tr>
<td>[-0.008,0.329]</td>
</tr>
<tr>
<td>0.166</td>
</tr>
<tr>
<td>[-0.009,0.342]</td>
</tr>
<tr>
<td>N</td>
</tr>
<tr>
<td>4142</td>
</tr>
<tr>
<td>3749</td>
</tr>
<tr>
<td>4142</td>
</tr>
<tr>
<td>4142</td>
</tr>
<tr>
<td>Cohort Dummies                                        ✓  ✓  ✓  ✓</td>
</tr>
<tr>
<td>Survey Year Dummies                                   ✓  ✓  ✓  ✓</td>
</tr>
<tr>
<td>1930 Census                                           ✓  ✓  ✓  ✓</td>
</tr>
<tr>
<td>Cluster FE                                            ✓  ✓  ✓  ✓</td>
</tr>
<tr>
<td>County Cohort Trends                                  ✓  ✓  ✓  ✓</td>
</tr>
</tbody>
</table>

Standard Errors for 7 Year reform clustered on the school district level. 95% CI in parenthesis.

That labor supply decision seems to be an important driving factor of the effects on earnings becomes even more apparent if the reform effects on labor supply are separated by gender. It appears that especially women increased their labor force participation after their schooling level increased (see table 5 in the appendix).

5.4 Robustness Checks

On source of concern could be that even after controlling for trends and district characteristics unobserved factors coinciding with the reform implementation drive the results. One way to address this is to look at pre-treatment characteristics of the treated individuals and see whether the reform had an impact prior to the reform. As an example we here use parental education. As expected table 4 only correcting for cohort trends leaves the reform indicator still highly correlated with higher parental education - capturing e.g. families living within the larger cities. After controlling for schooldistrict characteristics this correlation vanishes which makes us confident that indeed the reform itself is driving our results.

6 Concluding Remarks

We have examined the effects of two consecutive compulsory schooling reforms in Sweden during the 20th century with a special focus on the specific intuitional background and the political debate during the time of implementation. This as essentially fueled by a mixture of different results in different countries with respect to changes in the amount of mandatory schooling. We are essentially
<table>
<thead>
<tr>
<th>Table 4: Reform Predictor of Parental Education</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
</tr>
<tr>
<td>-----------------------------------------------</td>
</tr>
<tr>
<td>Fathers Education Folkskola or Less</td>
</tr>
<tr>
<td>Reform Indicator</td>
</tr>
<tr>
<td></td>
</tr>
<tr>
<td>N</td>
</tr>
<tr>
<td>Mean Low Education</td>
</tr>
<tr>
<td>Mothers Education Folkskola or Less</td>
</tr>
<tr>
<td>Reform Indicator</td>
</tr>
<tr>
<td></td>
</tr>
<tr>
<td>N</td>
</tr>
<tr>
<td>Mean Low Education</td>
</tr>
<tr>
<td>Cohort Dummies</td>
</tr>
<tr>
<td>Survey Year Dummies</td>
</tr>
<tr>
<td>1930 Census</td>
</tr>
<tr>
<td>Cluster FE</td>
</tr>
<tr>
<td>County Cohort Trends</td>
</tr>
</tbody>
</table>

Standard Errors for 7 Year reform clustered on the school district level. 95% CI in parenthesis.

in line with the argumentation of prior studies such as (Gathmann et al., 2014) or (Grenet, 2013) who recently consider contextual differences as one main reason for the differences in results in the empirical literature (in addition to purely technical instrumental variable problems or economic arguments). By examining two partially very different compulsory schooling reforms within one country we draw a similar conclusion about the contextual importance when evaluating specific reforms.

INTERNATIONAL COMPARISION - DEGREE RISE, LOW INITIAL HUMAN CAPITAL STOCK

SWEDISH CONTEXT - LARGER IMPACT AS SECOND REFORM

OUTLOOK - GIVEN THE EXCELLENT QUASI-EXP DESIGN THE REFORM RISES POTENTIAL ESPECIALLY TO STUDY LONG RUN EFFECTS OF EDUCATION
References


ECKLESIASTIKDEPARTEMENTET (1935): Betänkande och förslag angående obligatorisk sjuårig folkskola, SOU 1935:58, Ivar Hagströms Boktryckeri A.B.


Lindensjö, B., P. Lundgren, et al. (1986): “Politisk styrning och utbildningsreformer,”.


A Tables

A.1 Heterogeneity: Labor Market Supply by Gender

<table>
<thead>
<tr>
<th></th>
<th>(1)</th>
<th>(2)</th>
<th>(3)</th>
<th>(4)</th>
</tr>
</thead>
<tbody>
<tr>
<td><strong>Working Fulltime</strong></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Reform</td>
<td>0.069</td>
<td>0.045</td>
<td>0.086</td>
<td>0.089</td>
</tr>
<tr>
<td></td>
<td>[0.009, 0.128]</td>
<td>[-0.016, 0.105]</td>
<td>[0.005, 0.167]</td>
<td>[0.006, 0.171]</td>
</tr>
<tr>
<td>Interaction (Male)</td>
<td>-0.033</td>
<td>-0.024</td>
<td>-0.057</td>
<td>-0.059</td>
</tr>
<tr>
<td></td>
<td>[-0.099, 0.034]</td>
<td>[-0.085, 0.038]</td>
<td>[-0.138, 0.025]</td>
<td>[-0.142, 0.024]</td>
</tr>
<tr>
<td><strong>Employed</strong></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Reform</td>
<td>-0.013</td>
<td>-0.010</td>
<td>-0.020</td>
<td>-0.021</td>
</tr>
<tr>
<td></td>
<td>[-0.027, 0.002]</td>
<td>[-0.026, 0.007]</td>
<td>[-0.043, 0.003]</td>
<td>[-0.045, 0.003]</td>
</tr>
<tr>
<td>Interaction (Male)</td>
<td>0.014</td>
<td>0.013</td>
<td>0.020</td>
<td>0.021</td>
</tr>
<tr>
<td></td>
<td>[-0.003, 0.031]</td>
<td>[-0.004, 0.029]</td>
<td>[-0.002, 0.041]</td>
<td>[-0.001, 0.043]</td>
</tr>
<tr>
<td><strong>N</strong></td>
<td>4185</td>
<td>3792</td>
<td>4185</td>
<td>4185</td>
</tr>
<tr>
<td><strong>Cohort Dummies</strong></td>
<td>✓</td>
<td>✓</td>
<td>✓</td>
<td>✓</td>
</tr>
<tr>
<td><strong>Survey Year Dummies</strong></td>
<td>✓</td>
<td>✓</td>
<td>✓</td>
<td>✓</td>
</tr>
<tr>
<td><strong>1930 Census</strong></td>
<td>✓</td>
<td>✓</td>
<td>✓</td>
<td>✓</td>
</tr>
<tr>
<td><strong>Cluster FE</strong></td>
<td>✓</td>
<td>✓</td>
<td></td>
<td></td>
</tr>
<tr>
<td><strong>County Cohort Trends</strong></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
</tbody>
</table>

Standard Errors for 7 Year reform clustered on the school district level. 95% CI in parenthesis.

B Schooling Variable and IV

B.1 Analytical Results

B.1.1 Schooling correctly measured

Denote the following variables:

- $Z^*_i$ randomly assigned binary instrument (compulsory schooling level increased by one year)
- $Z_i$ observed (misclassified) instrument (e.g. due to location of birth instead of location at age 13)
- $S^*_i$ binary indicator for more than lower compulsory schooling level.

Assume that no never-takers exist with respect to the reform indicator (i.e. the change is binding). $S^*_i$ could e.g. be generated by

$$S^*_i = I(\gamma_0 + \gamma_1 Z^*_i + \epsilon_i > 0)$$

with $I(\cdot)$ the indicator function, $\gamma \to \infty$ and $\epsilon_i \sim N(0, 1)$.

Obviously the measurement error in the binary instrument is negatively correlated with the correct instrument. The misclassification leads to an attenuated reduced form. Consider the
reduced form

\[ Y_i = \alpha_0 + \alpha_1 Z_i^* + u_i. \]  

(2)

Taking expectation conditional on the misclassified instrument gives

\[ E[Y_i|Z_i = 1] = \alpha_0 + \alpha_1 \mathbb{P}(Z_i^*|Z_i = 1) \]  

(3)

\[ E[Y_i|Z_i = 0] = \alpha_0 + \alpha_1 \mathbb{P}(Z_i^*|Z_i = 0). \]  

(4)

Therefore

\[ \text{plim } \hat{\alpha}_1 = E[Y_i|Z_i = 1] - E[Y_i|Z_i = 0] \]  

(5)

\[ = \alpha_1 \{ \mathbb{P}(Z_i^*|Z_i = 1) - \mathbb{P}(Z_i^*|Z_i = 0) \}. \]  

(6)

The first stage (regressing \( S_i^* \) on \( Z_i \)) is attenuated by the same factor \( \delta \) leading to a consistent Wald-Estimator if one is interested in estimating the effect of \( S \) on \( Y \).

### B.1.2 Schooling constructed

Only degrees are available (e.g. lowest track such as *folkskola*). The actually years spend in school are not available. Instead define a constructed schooling variable by the instrument (, see e.g. Kemptner et al. (2011) [1] and Pischke and Von Wachter (2008) [2] using this approximation for the years of schooling for Germany. Kemptner et al. (2011) argue correctly that due to that their IV estimates present a lower bound. One should also mention that their misclassification is probably not very severe).

\[ S_i = \begin{cases} 
1 & \text{if } Z_i = 1 \text{ (by construction)} \\
1 & \text{if } Z_i = 0 \land Z_i^* = 1 \text{ (correct instrument at work)} \\
1 & \text{if } Z_i = 0 \land Z_i^* = 0 \text{ (but always taker)} \\
0 & \text{otherwise}
\end{cases} \]

Taking expectation conditional for schooling on the misclassified instrument gives

\[ E[S_i|Z_i = 1] = 1 \]

\[ E[S_i|Z_i = 0] = \mathbb{E}[S_i|Z_i = 0, Z_i^* = 1] \mathbb{P}(Z_i^* = 1|Z_i = 0) + \mathbb{E}[S_i|Z_i = 0, Z_i^* = 0] \mathbb{P}(Z_i^* = 0|Z_i = 0) = \mathbb{P}(Z_i^* = 1|Z_i = 0) + \mathbb{P}(S_i^* = 1|Z_i = 0) \mathbb{P}(Z_i^* = 0|Z_i = 0). \]
Taking the difference leads to

$$E[S_i|Z_i = 1] - E[S_i|Z_i = 0] = 1 - [P(Z_i^* = 1|Z_i = 0) + P(S_i = 1|Z_i^* = 0)P(Z_i^* = 0|Z_i = 0)]$$

$$= 1 - P(S_i = 1|Z_i^* = 0)$$

First Stage correct

$$-P(Z_i^* = 1|Z_i = 0) + P(S_i = 1|Z_i^* = 0)P(Z_i^* = 1|Z_i = 0).$$

$$\epsilon \sim (0.1)$$

This attenuated first stage is no longer proportional to the reduced form. As a consequence the Wald-IV is biased. The bias is downwards due to a positive correlation introduced between the constructed schooling $S_i$ and $Z_i$.

C Figures

![Graph](image)

**Figure 8:** Placebo Estimate - Parishes introducing 7th Grade in 1936-1945: (a) RDD on Eduactional Attainment (>6 Years of Schooling); (b) RDD on Log-Income.