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A Researcher's Guide to the Swedish Compulsory School Reform

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Executive summary

A recent development in the economics literature makes use of natural experiments such as educational reforms in order to causally identify returns to education (both pecuniary and non-pecuniary). One of the reforms used in the literature is the Swedish compulsory school reform, that in the 1950s and 60s extended compulsory education from seven to nine years. The reform was rolled out gradually across the country's municipalities, and this design has inspired researchers to use the variation over time and across regions in a differences-in-differences approach in order to estimate the effect of education on different outcomes. This paper provides a guide to researchers who are interested in the Swedish compulsory school reform: it describes the institutional context and the introduction of the reform, discusses the data sources available for quantitative analysis of reform effects, performs some baseline empirical analysis and tests as to whether the reform is a valid instrument for education.

Several different data sources are available to researchers who wish to perform a micro-level empirical analysis of the effects of the reform. In this paper, the focus is on administrative data originating from Statistics Sweden, which do not contain information on type of school system, but in which we can assign individuals to the reform based on year of birth and municipality of residence. This way of assigning reform status to individuals is validated by matching it to an alternative data source, containing information on reform participation collected on individual level. With two independent measures of the reform coding at hand, the degree of attenuation bias due to measurement error can be approximated: estimates of the lower bound of attenuation bias are in the range of 0.66-0.91.

The empirical analysis shows a baseline estimate of the effect of the reform on years of education of around 0.13 for women and 0.21 for men. These estimates are likely downward biased due to measurement error, but provide a lower bound of the effect. Estimates of the upper bound of the reform effect are considerably higher: 0.44 for women

and 0.61 for men. The main focus of the empirical analysis, however, is on the reform as a potential instrument for education. Since years of education is likely correlated with many unobserved factors also determining an individual's labour market success, it is hard to find a causal estimate of the returns to education. Educational reforms provide researchers with a "natural experiment" and allow for a research design where individuals in the new system are compared to a "control group" of individuals in the old system. The underlying assumption is that the implementation of the educational reform is uncorrelated with unobserved individual characteristics. The paper therefore performs a series of tests in order to better understand whether the Swedish educational reform is a valid instrument for education. In order to be a valid instrument, the reform should be a strong predictor for years of education, and be uncorrelated with unobserved factors determining the individual's educational success (the reform should be exogenous).

First, the findings show that the reform passes the tests for "weak instruments" and is a strong predictor for years of education. And second, when it comes to reform exogeneity, tests introducing parental education variables and region-specific trends, indicate that estimates are rather sensitive to the inclusion of trends. Therefore, empirical analysis of the Swedish compulsory school reform in an instrumental variables setting should carefully examine the role of region-specific trends.

A final remark on the use of the Swedish compulsory school reform as an instrument for education is that it affected the lower end of the educational distribution, and results based on this source of variation can therefore not be generalized to the full range of educational outcomes.

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Acknowledgments

1.   Introduction

In recent years the economics literature has been enriched by a growing number of studies focusing on educational reforms when assessing the effect of education on different socio-economic outcomes. In the 25 years following the Second World War, many western European countries undertook major educational reforms with the main purpose of extending compulsory education. Countries in Scandinavia, continental Europe and the United Kingdom, with different traditions of educational policy, were all part of this widespread expansion (Viarengo 2007). The strong economic growth in the post war era created a demand for a higher skilled workforce, and in some countries, for example Sweden, there was also a strong push for reforming the education system in order to increase equality of opportunity. The European experience was also a reflection of an earlier development in the United States, where compulsory school attendance and child labour laws were enacted throughout the states in the early decades of the 20th century.

Sweden, one of the reform countries in Europe, extended compulsory education gradually across the country, starting in the late 1940s. The reform was implemented in different municipalities at different points in time, meaning that for a given birth cohort some individuals went through the old seven-year school and others went through the new school, comprising of two more years. Similar gradual expansion paths were also adopted in the other Scandinavian countries, and the design of these reforms has inspired to several recent studies that exploit the variation across regions and over time as a potential source of exogenous variation in education (Meghir and Palme 2005, Black et al. 2005, Pekkarinen et al. 2006). Closely related to these papers is the U.S. literature, which with a similar methodological approach studies the compulsory school leaving ages across U.S. states (see for example Lochner and Moretti 2004 and Llleras-Muney 2005). Thus, compulsory school reforms open

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up for many different applications following this recent literature using the reforms as instruments for education.[1]

This paper aims at describing and exploring the Swedish educational reform; it provides a background to existing papers based on the reform, but also to future studies where it might be used. A main contribution is to document the data sources used for constructing a coding of reform municipalities, and to perform a reliability analysis of the reform coding using alternative data sources. Moreover, I describe the Swedish reform implementation and its political background, and discuss and evaluate whether the reform is a valid instrument for education. In particular, there will be a strong focus on whether the reform is to be considered as exogenous.

The remainder of the paper unfolds as follows: section 2 describes the Swedish compulsory school reform, section 3 presents an introduction to how a coding of the reform has been obtained and section 4 briefly introduces the data. Section 5 studies the effect of the reform on educational outcomes, discusses the validity of the reform as an instrument and clarifies how IV estimates of returns to education should be interpreted. Section 6 offers conclusions. For more detail, the appendix contains a careful description of the coding of reform, and a reliability analysis of this coding.

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[1] There is a long tradition in empirical labour economics of studying wage returns to education, and a more recent focus also on non-pecuniary returns. These studies all face the same challenge, that is, how to control for unobserved ability or other unobserved factors that are correlated with education and also with the outcome of interest. One remedy to this problem has been to make use of some exogenous source of variation in education, typically in the form of a natural experiment. There are a number of well-known examples, for example Acemoglu and Angrist (2000), Angrist and Krueger (1991), Black et al. (2005), Chevalier (2004), Currie and Moretti (2003), Lleras-Muney (2005), Locher and Moretti (2004), Maurin and McNally (2005), Meghir and Palme (2005) and Oreopoulos et al. (2006). The outcomes in the studies mentioned above all range from the pecuniary return to education, to effects on mortality, crime, birth outcomes and the education of the children (the intergenerational effect of education).

2.   The Swedish Reform

The Swedish educational reform, both the initiating debate and its aftermath, is carefully described in the work by Marklund (1980, 1981). Detailed information can also be found in a report by the National Board of Education (1960). The following brief description builds on these sources, which are recommended for further details on the topic.

In 1946, a parliamentary committee (*1946 års skolkommission*) was given the task to analyse the Swedish school system and to develop proposals and guiding principles for the future compulsory school. At this time, in general pupils went through grades 1 to 4 or 1 to 6 in a common school (*folkskolan*). In either fourth or sixth grade, more able students were selected (based on past performance) for the five or three/four year long junior-secondary school (*realskolan*). Remaining students stayed in the common school until compulsory education was completed. In most cases, compulsory education comprised of seven years, but in some municipalities, mainly the big cities, the minimum was eight years. Two years after the appointment of the committee, in 1948, the committee released its proposals. The main suggestion was to introduce a nine-year compulsory school, where pupils were kept together in common classes longer than in the earlier school system. As a compromise between the opponents of early tracking and its advocates, the committee proposed tracking in 9th grade; pupils would follow either a vocational track, a general track, or a theoretical track preparing for upper-secondary school. The 9th grade streaming was later abandoned in favour of a completely comprehensive system.

The purposes underlying the proposal were among others to postpone the tracking decision to higher grades, in an effort to increase equality of opportunity, and to meet the demand for junior secondary education among the baby boom cohorts of the mid 1940s. To evaluate the appropriateness and whether the proposed nine-year comprehensive school would serve its purpose, the committee suggested that an "experiment" would take place,

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where during an assessment period some municipalities and schools would implement the new school system such that the results could be scrutinized before further decisions were made.

The assessment programme came to start in 1949/1950. The new comprehensive school was to be introduced throughout a whole municipality, or in certain schools within a municipality. Following the 1948 proposal of the parliamentary committee, a number of municipalities had declared interest in reforming their comprehensive schools. For this reason, 264 municipalities (out of around 1000) were asked if they were willing to introduce the nine year school immediately or within a few years. The municipalities that were approached had either shown interest in the reform or expanded their junior secondary school to four years. 144 municipalities showed interest in the reform. 14 municipalities were selected for the first year of the assessment (1949/50), all of those were required to have an eight year comprehensive school already.

The following years, the National Board of Education continued with the implementation of the reform. Year by year, more municipalities joined the reform assessment programme. Municipalities that wanted to take part in the reform were asked to report on their population growth, on the local demand for education, tax revenues and local school situation. For example, the availability of teachers, the number of required teachers for the nine year comprehensive school, and the available school premises were explored. The National Board of Education took these municipality characteristics into account when deciding on their participation. In general, implementation of the reform started in grades 1 and 5, the following year covering grades 1, 2, 5 and 6 and so on. From 1958 the reform was introduced in grades 1-5 already from the starting year.

Apart from extending compulsory education from seven years to nine years, and to postpone tracking, the educational reform was also pedagogical and affected the curriculum

somewhat. The main change of the curriculum was that English was introduced in 5th grade in the new comprehensive school, while this was not necessarily a compulsory subject in the old school system. The school starting age was set at the year the child turned seven in both the old and new comprehensive school.

The assessment period was also accompanied by financial support to families and to municipalities that implemented the reform. A universal child allowance was introduced in 1948 and implied support for children until the age of 16. In reform municipalities, a means-tested scholarship compensated families for foregone earnings from keeping their children longer in school, and municipalities were compensated with ear-marked money from the central government for the increased costs following the expansion of mandatory education.

In 1962, the parliament came to a final decision to permanently introduce the nine-year school throughout the country. At this point, the implementation came to be a matter for each municipality; by 1969 they were obliged to have the new comprehensive school running. Since the timing was much in the hands of each municipality, the implementation was far from a randomized experiment, but nevertheless provides a source of variation in schooling laws that may be fruitfully explored by the empirical researcher.

3.   For Quantitative Analysis: Which Municipalities Implemented, and When?

Since the educational reform provides a potential source of exogenous variation in education, I take a closer look at the available data. There are, to the best of my knowledge, three different micro data sets available to study reform effects on individual outcomes:

The IS data

It is possible to use the IS (individual statistics) data, from the Institute of Education at Gothenburg University (Härnqvist 2000). The data stem from surveys, conducted in 6th grade, of around 10 percent of the cohorts born in 1948 and 1953. When these data were collected,

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information on type of school (the old comprehensive or the new nine-year compulsory school) that each individual attended was recorded, based on information provided by the local school. This is the data set explored in Meghir and Palme's (2005) work on the Swedish compulsory school reform.

The Swedish Level of Living Survey

The Swedish Level of Living Surveys, based on random samples of the Swedish adult population, have been conducted in 1968, 1974, 1981, 1991 and 2000 (Erikson and Åberg 1984). The surveys ask specifically whether an individual went through the old comprehensive school or the new nine year school. These data have been used by Jasmina Spasojevic (2003) in her work on the effects of education on health. Unfortunately, the sample size of LLS is not big enough to allow for precisely estimated differences-in-differences.

Register data from Statistics Sweden

The Swedish administrative registers do *not* contain information on whether individuals in the affected cohorts went through the old or the new school system. With help from other sources it is however possible to deduct when and for which grades each municipality introduced the new comprehensive school, and based on this information one can assign a reform dummy to the individuals in a data set extracted from registers. In the registers, birth year is known, and through the censuses it is feasible to track in which municipality an individual lived at the time of compulsory education. With this information it is possible to attach a reform indicator to each individual based on year of birth and municipality of residence, maintaining the assumption that individuals are in the right grade according to their age. In some cases it is also necessary with more detailed information on in which parish or school district the individual went to school, since the reform was sometimes introduced in parts of a municipality in different years. Any given dataset with information on birth year and

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municipality/parish of residence can assign reform participation to the cohorts that were subject to the education reform.

There are two possible ways to construct a reform coding that can be matched to individual-level register data. In the remainder of the paper I will label them *CODING 1* (based on documentation) and *CODING 2* (deduced from register data).

The sources of information necessary to construct *CODING 1* of the reform implementation are the following:

i) Marklund (1981) and the National Board of Education (1953-1962). These sources document the assessment programme when the reform was gradually introduced across the country, and they include lists of which municipalities implemented the reform each year. In the latter publication it is also possible to see which grades that were affected in a particular municipality. These sources cover the assessment period and only allow coding of the cohorts born 1938-1949.

ii) The Educational Bureau (Undervisningsbyrån) (1960-1964) and Statistics Sweden (1968-1969). From municipality-level tables of the number of pupils in each grade in the old and new school system, it is possible to deduct when the reform was implemented. With these sources it is possible to code the remaining cohorts that were affected by the gradual change of the educational system.

Register data sets with large sample sizes, also allow for another procedure to create a reform coding. *CODING 2* is obtained as follows: for each municipality/birth year cell, it is possible to deduce the minimum level of education, and if the minimum level jumps up from *folkskola* (the old compulsory minimum) to *grundskola* (the new minimum), it tells us when the reform was implemented.

Appendix A explains in detail how the different sources have been used to create *CODING 1* and *CODING 2*, and also highlights some of the difficulties relating to the coding

of some municipalities, where the reform was not implemented universally at one point in time. This gradual implementation within a municipality, combined with the fact that I assume that pupils were in the right grade according to their age when assigning them the reform dummy, naturally introduces measurement error in the reform indicator. As shown in Appendix B, measurement error in a binary variable is non-classical, which just as in the classical case implies an estimate attenuated towards zero (Aigner 1973, Kane et al. 1999). With two independent measures at hand (*CODING 1* and *CODING 2* - both measured with error), I assess the degree of attenuation bias with the reliability ratio, that is, the fraction of variation in one variable that can be explained by the variation in the true variable, measured without error. In the case of non-classical measurement error, however, the reliability ratio will represent a lower bound of the attenuation bias. My estimates of the reliability ratio – see Appendix B – show that *CODING 1* fares relatively well, with an estimated lower bound of the attenuation bias in the range 0.66-0.91.[2]

In the remainder of this paper I discuss and demonstrate the reform, using a data set compiled from Swedish registers. *CODING 1* is available from the author at time of publication, and for any remaining questions about the coding, and the tables underlying it, please contact the author.

4.   Data

In this paper I use data from Swedish administrative registers, based on a 35 percent random sample of cohorts born in Sweden in 1943 to 1955.[3] These data are available for researchers through the registers held by Statistics Sweden. By means of population registers, parents of the sampled individuals have been identified and merged to the data. Moreover, information

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[2] *CODING 2* comes out with estimates in the range 0.53-0.70. The estimates reported here are for reliability estimates of the differenced reform indicators, that is, taking into account the cohort and municipality effects of the differences-in-differences specification.
[3] The data actually spans cohorts born 1932-1967, but the cohorts affected by the reform, for which the data allows me to assign reform participation are the 1943-1955 cohorts.

from the bi-decennial censuses in the years 1960 to 1990 has been matched to the data. In the censuses there is information on municipality of residence, and they also contain some background characteristics of the parents of the sampled individuals. The Swedish education register from 2003 provides information on highest completed degree.

In studies of the educational reform, on how it affected education and other outcomes, there are a few key issues related to the data. One is on what basis to assign reform status to individuals. This can be done either by using municipality of birth or the municipality in which the individual lived at the time he/she went to school (school municipality). In practice, the data at my disposal have a few limitations that complicate the assignment based on municipality of birth/residence. The data contain information on municipality of residence in the bi-decennial censuses starting in 1960. For the early cohorts affected by the reform, born around 1940, the municipality of residence in 1960 is however not a reliable indicator of their school municipality. These individuals are around 20 years old and may have moved out of their family home, either for studies or work. The data also include information on parish of birth. Then, why not use the information on where they were born? The problem is that, for cohorts born until 1946, the parish of birth that was reported refers to the location of the hospital in which they were born (Skatteverket 2007). At this time, most births did take place out of the home, and a majority of all municipalities did not have their own maternity ward. Thus, the information on parish of birth can unfortunately not be used for the early cohorts either. Therefore, I use the information on municipality of residence in the censuses of 1960 and 1965 to assign the reform to individuals. I also limit the sample to cohorts born in 1943 to 1955. The lower cut-off is motivated by the fact that older cohorts may have moved out of their home municipality by 1960.[4][5]

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[4] Figure C1 in Appendix C shows the share of each cohort that is living in the same household as their mother in 1960, and it is clear that for pre-1943 cohorts, this share drops significantly. The 1943 cohort is 17 years old in 1960 and at this point and I observe them in their home municipality.

The second crucial data issue for this type of study is the information compiled from the education register. I use the education register in 2003 (or earlier if 2003 is missing) that reports on highest achieved level of education, which I translate into years of schooling according to the formal length required to receive a degree.[6]

In parts of the analysis of the paper, I will include controls for parental education. This information comes from the 1970 census, but the schooling records are not complete, either because of non-response or simply because the parent was deceased at this time. In order to make the most out of this information, I take the mean of both parents education, including also observations where only one parent's schooling is observed. Remaining missing observations are included in the analysis, and controlled for with a dummy variable.

I arrive at a data set of 496,773 observations, after excluding individuals with missing observations on either education or municipality of residence in 1960/1965.[7] For these individuals, reform participation is identified for 450,885 individuals (using *CODING 1* based on documentation).

5. An Analysis of the Swedish Compulsory School Reform

5.1 Descriptive statistics

Table 1 reports on the descriptive statistics of the sample. The first column shows that for the 1943-1955 cohorts, average length of education is 11.48 years. Around 40 percent of individuals belonging to these cohorts were subject to the reform, using both coding schemes. The table also shows that there is a large share of missing in the parental education variables.

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[5] Municipality in the 1960 census is used for the 1943-1949 cohorts, and for the remaining cohorts the 1965 census is used.

[6] The information in the education register has been translated into years of education in the following way: 7 for (old) primary school, 9 for (new) compulsory school, 9.5 for (old) pos-primary school (realskola), 10 for less than two years of high school (or incomplete high school), 11 for short high school, 12 for long high school, 13 for less than two years of post-secondary education, 14 for short university, 15 for three years of university, 16 for four years of university, 17 for five or more years of undergraduate university studies (including magister), 18 for a lower graduate degree (licentiate), and 20 for a PhD.

[7] The number of dropped observations due to missing amounts to: 27,985.

The second column lays out the descriptives for the sample for which the reform is identified, and there are no deviations from the overall picture in column 1.

Figure 1 shows the trends in years of schooling for the birth cohorts in the sample, for men and women separately. The increasing trend is strong and clear, and women are more highly educated than men. Figure 2 shows the share of individuals in the reform, by birth cohort, from three independent sources. The origins of the two series are described carefully in Appendix A, whereas the data points referring to Meghir and Palme (2005) are taken from the data used in their 2005 paper (available on http://www.aeaweb.org). The different coding schemes all follow each other closely in terms of the share of individuals in each birth cohort that is affected. For the first cohorts affected by the reform, implementation was rather slow. It is not until the cohorts born in the mid 1940s that the gradual implementation reaches larger groups. For the 1950s cohorts, no longer affected by the experimental period, but by the parliamentary decision to introduce a nine-year comprehensive school throughout the country, the increase continues and comes close to 1.

5.2 Baseline estimates

In evaluating the reform, a central estimation result is the reform effect on educational outcomes. I estimate a baseline differences-in-differences specification according to the following equation:

$$S_{icm} = \alpha_0 + \alpha_1 REFORM_{cm} + \alpha_2 X_{icm} + \eta_c + \mu_m + v_{icm} \tag{1}$$

S_{icm} is years of schooling for individual i, belonging to cohort c, going to school in municipality m. *REFORM* is an indicator that takes the value 1 if the individual belongs to a birth cohort that was subject to the reform in the particular municipality, and that is 0 otherwise. X is a vector of observable characteristics, η_c represents birth cohort effects and μ_m municipality fixed effects. The results are reported in Table 2. In the first two columns, I

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exclude the municipality-specific effects, in order to understand better how they affect the estimates. Without including municipality controls, I find an average effect of the reform on schooling of 0.59 years without controls for parental background, and 0.41 years with controls for parental background. In columns 3 and 4, when the municipality-specific controls are added, these effects are remarkably lower, indicating the importance of these controls. Now the effects are in the range 0.17-0.19 when pooling women and men together. Clearly, among the affected cohorts, many individuals obtained levels of education higher than the new compulsory minimum regardless of the reform, and therefore the average effect is much lower than the one or two years that the reform implied. Moving on to columns 5-12 in Table 2, results separated by gender show that the reform had a stronger effect on increasing men's educational attainment, the point estimate for men is 0.23 whereas for women it is much lower: 0.15. This result should not come as a surprise given the pattern of gender differences in Figure 1; for these cohorts, women's educational attainment is higher than men's, and therefore the reform had less of an impact on women.

Having established that the reform implied an increase in years of education, it is worth taking a closer look at the dynamics of this effect. Did the reform solely add two years of education for those at the bottom of the distribution, or did it actually also induce a shift beyond the new compulsory minimum? Such a spill-over effect could occur for example if the pre-reform early selection was unfavourable to talented children from disadvantaged backgrounds, which in the new school system possibly got the chance to move on further in the education system. Table 3 shows estimates from a regression of a dummy indicating completion of two-year secondary school or more on the reform.[8] The results of this table indicate that a small spill-over effect is present: both women and men affected by the reform have a 0.01 higher probability to complete two years of upper-secondary education or more

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[8] Two-year secondary school is the lowest post-compulsory degree and refers to vocational post-compulsory education.

(although the effect is only marginally statistically significant for men). These effects are slightly lower and imprecisely estimated once including controls for parental education. As a point of comparison, the overall probability to attend two-year secondary school or any education beyond that, is 0.67 for men and 0.70 for women. When looking for spill-over effects at the next level of education: three-year of upper-secondary school, corresponding to the academic track of post-compulsory education, I find no dynamic effects. I therefore conclude that a small spill-over effect is present, pushing education beyond the new compulsory minimum, but only up to two-year secondary school.[9]

As discussed in the section describing the reform coding, and as described in detail in Appendix B, measurement error in the coding is likely to bias the reform estimates towards zero. With two independent measures of the reform, one common approach is to instrument one measure with the other, in order to obtain consistent estimates. In the case of non-classical measurement error, however, this strategy will not produce consistent estimates, but rather upward biased estimates (Kane et al. 1999). Thus, I am not able to pin down the true effect of the reform, but the downward biased OLS and the upward biased IV estimates constitute bounds for the true parameter. Taking the (downward biased) estimates presented in Table 2 as a benchmark, Table 4 presents the corresponding IV estimates, where *CODING 2* has been used as an instrument for *CODING 1*. The differences between the OLS and the IV estimates are large; the latter are in the order of magnitude three or four times as large as the OLS estimates, as they are estimated to be around 0.4 for women and 0.6 for men. Therefore, my preliminary conclusion is that the true parameter estimate of the effect of the reform on years of schooling lays roughly in the range 0.13-0.44 for women, and 0.21-0.61 for men.

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[9] Regressions of spill-over effects on university level education also show that there are no such effects present. Results can be obtained from the author upon request.

5.3 The reform as an instrument for education

Up to date, there are several examples of educational reforms that have been used in an instrumental variables framework; the Swedish reform is one of them. In the following, I discuss and evaluate the validity of the compulsory school reform as an instrument for education. Typically, we have a model specified in the following way:

$$y_{icm} = \beta_0 + \beta_1 S_{icm} + \beta_2 X_{icm} + \varpi_c + \theta_m + \varepsilon_{icm} \tag{2}$$

$$S_{icm} = \alpha_0 + \alpha_1 REFORM_{cm} + \alpha_2 X_{icm} + \eta_c + \mu_m + v_{icm} \tag{3}$$

We are interested in estimating the effect of years of schooling, S_{icm}, on an outcome y_{icm}, as expressed in equation 2. As before, ϖ_c, η_c and θ_m, μ_m stand for cohort and municipality effects, respectively. X_{icm} is a vector of observed individual controls. $\varepsilon_{icm}, v_{icm}$, the error terms, capture all unobserved factors that affect the outcomes y_{icm} and schooling S_{icm}, respectively. The common problem when attempting to identify the causal effect of schooling on the outcome y_{icm}, is that $Corr(S_{icm}, \varepsilon_{icm} \mid X_{icm}, \varpi_c, \theta_m) \neq 0$. For example, unobserved ability will influence both schooling and the outcome y_{icm} directly, and thus the error term in the outcome equation will be correlated with schooling, and this leads to a bias in the coefficient β_1.

The instrumental variables literature attempts to circumvent this problem by finding a source of variation in schooling that is uncorrelated with the unobserved characteristics that enter the error terms. In the case of educational reforms, if the gradual implementation of the reform is uncorrelated with unobserved characteristics, we have found a good instrument. That is, if $Corr(S_{icm}, \varepsilon_{icm} \mid X_{icm}, \varpi_c, \theta_m) \neq 0$, it might still be true that $Corr(REFORM_{cm}, \varepsilon_{icm} \mid X_{icm}, \eta_c, \mu_m) = 0$ (the reform is exogenous), and we can use *only* the variation introduced by the reform to estimate the schooling effect on the outcome y_{icm}.

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Naturally it is thus important that the instrument actually also is a good predictor of the schooling variable.

Practically, the instrumental variables approach is commonly estimated using two-stages-least-squares, where in the first stage schooling is regressed on the instrument (and the other controls as specified in equation 3), and in the second stage the outcome y_{icm} is regressed on the fitted values from stage one (substituting in for education) and the other controls. I now turn to analyse whether the educational reform constitutes a valid instrument for years of schooling.

5.3.1 Is the reform a strong predictor for education?

An instrument of high quality should be highly correlated with the variable it is meant to instrument for. What "highly correlated" means might be dubious, but the literature on weak instruments (Bound et al. 1995, Staiger and Stock 1997) following the Angrist and Krueger (1991) paper using quarter of birth as an instrument for education, has led to greater awareness among economists of what constitutes a strong instrument, and provides us with some guidelines of how to assess the quality of our instruments. Staiger and Stock's (1997) rule of thumb states that when the F-statistic on the instrument is below 10, the instrument is to be considered weak.

Table 2, presenting the reform effects on education, also reports the F-statistic of the reform. The F-statistics presented are convincingly high, ranging from 10 to 28 for the preferred specifications (including municipality controls). Comparing those with the ones presented by Llleras-Muney (2002) on the US compulsory schooling laws (in the range 0.4 to 14), the F-statistics obtained for the Swedish compulsory school reform are comfortably high.[10]

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[10] Another statistic that has been proposed in the literature is the partial R-squared of the instrument, which is a measure of the variation in the endogenous variable (in this case education) that is explained by the instrument, holding other explanatory variables constant (Shea 1997). The partial R-squared is also reported in Table 2,

5.3.2 Exogeneity of the reform

As mentioned above, it is crucial that the reform is uncorrelated with unobserved characteristics (determining the outcome of interest) in order for it to bee a valid instrument. As explained in the section describing the Swedish policy reform, implementation was not random across the population or across the municipalities in the country. On the contrary, in some respects municipalities were chosen to participate in the "experiment" based on certain characteristics. Moreover, after the assessment period had come to an end, municipalities themselves decided when to join. This indeed makes it likely that the reform is correlated with characteristics specific to the municipalities. Worrisome as this may sound, it is taken care of by the differences-in-differences specification: the municipality dummies control for all municipality-specific characteristics that are constant over time. Nevertheless, it is still possible that the reform correlates with municipality-specific trends or other unobserved factors not taken care of in the analysis so far.

A first test as to whether the reform is exogenous is already provided in Table 2. The differential results when excluding/including municipality effects clearly show that the reform is correlated with municipality-specific factors. Given the differences-in-differences specification however, if the reform is uncorrelated with unobserved characteristics, the point estimate of the effect of the reform should remain constant once we include controls for further background characteristics (see for example columns 3 and 4 in Table 2). When parental education is added to the specification, the point estimates are somewhat reduced, which signifies that the reform is not entirely exogenous, and that it likely is positively correlated with other factors that positively determine children's education (as parental education). The result is that the parameter estimate is slightly upward biased. The reductions

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showing that the reform instrument explains about 1-2 percent of the variation in women's schooling, whereas it explains 2-2.5 percent of the variation in men's schooling, holding other factors constant.

of the coefficients are moderate, however, which leads to the conclusion that the exclusion of parental background controls does not bias the estimates largely.

Another route to eliminating bias, commonly used when estimating differences-in-differences, is to include controls for municipality-specific trends in the specification. This has the potential of capturing trends (specific to each municipality) in education that correlate with the reform. Table 5 shows results introducing both linear and quadratic municipality-specific trends.[11] I present results both with and without controls for parental education, and the first two columns repeat the coefficients from the individual-level regression, previously presented in Table 2. The next two columns show the corresponding specification based on data aggregated on municipality-birth year level, and moving further along the table linear and quadratic municipality-specific trends are included in the regressions.

The baseline estimates change slightly depending on whether linear or quadratic trends are introduced. For women, the linear trend brings up the schooling estimates, whereas the inclusion of quadratic trends lowers the estimates, which are now in line with the coefficients from a regression without municipality-specific trends. The estimates are in the range of 0.13-0.20 for women. For men, the results are insensitive to whether the municipality-specific trends are linear or quadratic, but the coefficients are higher than the baseline estimates excluding trends. The coefficients for men range from 0.26-0.28.

A more direct strategy to assess whether the reform is correlated with parental background is to check whether parental background (in this case parental education) can predict reform participation, controlling for cohort and municipality fixed effects. Given the chosen specification, the preferred result in order to demonstrate instrument exogeneity is to find that parental education does not predict reform participation. Table 6 presents the results from such an exercise. The first column excludes municipality effects, and we see that one

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[11] Note here that I move from individual level data to a format aggregated on municipality/cohort level, for computational reasons.

more year of parental schooling increases the probability that an individual is affected by the reform by 0.10. But in the following specifications, where municipality effects are included, in no case does parental education predict reform participation of the children. Missing information in the parental schooling variable seems to be correlated with the reform, which could potentially indicate that there is a correlation between parental schooling and reform participation present. It is difficult, however, to ascertain in what direction this would bias the estimates of the reform, without making assumptions about how actual parental schooling is correlated with non-reported parental schooling. The last column, including quadratic trends, shows no association at all between parental education and reform participation, and hence this is my preferred specification.

Finally, as a last investigation of instrument exogeneity, I take on a different approach and estimate the effect of the reform on schooling for cohorts who are too old to have been affected by the reform. If the reform is exogenous, it should not have had an effect on cohorts that had already passed through the educational system once the reform was introduced. And if there indeed is such an effect, it is a signal that the policy is actually correlated with some unobserved factors that are not captured by the differences-in-differences approach. In Table 7 I present results for the effect of the reform on cohorts that were 1, 2, 3 or 4 years too old to have been subject to any changes. The first panel corresponds to the baseline specification, and the lower panels introduce linear and quadratic municipality-specific trends. The first two panels show that future reform implementation indeed is positively associated with years of schooling. The coefficients are in general not as high as those for the "true" reform, but are statistically significant and non-negligible.

Further down the table, in panel C, the effect of a future policy change is present only for individuals who are one year older than the actual affected cohort. In all other cases the effect is zero. It is convincing that in this specification, with quadratic trends, the reform

passes the test. The fact that there is a significant effect for the cohort one year ahead is not unexpected, for two reasons. First, there is some measurement error in the reform coding, since in some cases it is not possible to assign a clear-cut starting date of the reform. This introduces coding errors, but only for one cohort ahead or behind what is coded as the starting year. Second, one underlying assumption of the analysis is that individuals are in the expected grade according to their age. Some of those who repeat a grade might actually have gone to the new school, although in the data they are coded as non-participants. This should also give us a positive effect of the reform for those in the cohort one year ahead. Therefore, an effect for only the cohort one year ahead, as apparent in panel C of Table 7, is not problematic.

Taken together, what have we learned from the different strategies above, that in different ways investigate the validity of the compulsory school reform as an instrument? There are a couple of lessons to learn. First, the estimates are quite insensitive to the inclusion of a parental background variable such as parental education. Second, the estimates are sensitive to the inclusion of municipality-specific trends; estimates change slightly depending on the specification, and the piece of evidence in Table 7 shows that the most credible specification should allow for non-linear municipality-specific trends.

5.3.3 Endogenous mobility

A warranted objection when analysing policy changes on regional level is related to selective mobility: individuals might respond to the new policy, moving from it in order to avoid it, or to it order to benefit from it. The National Board of Education (1960) finds that municipalities that had introduced the new comprehensive school lost pupils to neighbouring municipalities that still practiced the old system. The old junior-secondary school was considered to be of higher quality, and some parents chose to move their children to avoid the comprehensive school. The National Board reports that in the years 1954-1958, 5.3 percent of all pupils left the comprehensive school by at the end of 4th grade, and 10.3 percent left by the end of 6th

grade. The majority of these drop-outs transferred to the old junior-secondary school (*realskola*).

For a sub sample of my data (born 1947-1955), I identify both municipality of birth and home municipality at school age, and among these cohorts, 34 percent lived in a different municipality when going to school than at birth. With this information I can assess the role of selective mobility, to the extent that mobility actually implied a change of home municipality. Pupils who went to school in a neighbouring municipality will not be classified as movers, and therefore I am not able to fully assess the implications of selective mobility. Comparing municipality of birth with later home municipality, I find that 3.74 percent moved away from a reform municipality to one that had not yet implemented the reform, and an equally large proportion moved from a non-reform municipality to one introducing the new school. These numbers are somewhat lower than those in Meghir and Palme (2003), who find that 4.3 percent of their sample move from a non-reform municipality to a reform one, and that the stream in the opposite direction was of the same size.

Regressing a dummy indicating "moving from reform municipality" on parental education and year effects, gives a positive and significant coefficient of father's schooling, but the size of the effect is very small: 0.003. Thus, this indicates that there was little systematic mobility with respect to parental background, for this particular sample. The analysis in Meghir and Palme (2003) also supports that there was little selective mobility, and that it can not be explained by observed characteristics. Nevertheless, mobility based on unobserved individual traits might still be a problem, and we should bare this in mind when interpreting the results.

5.3.4 Pre-reform trends

In the above section I show that the results can be sensitive to the inclusion of municipality-specific trends. This topic is the focus in a recent paper by Wolfers (2006), who studies the

sensitivity of differences-in-differences estimates to the inclusion of region-specific trends. He argues that adding region-specific trends to the regression may capture actual responses to a policy change, and not just control for pre-policy trends. If a policy implies both a level and a trend shift, municipality-specific trends will actually partly control for the policy responses that we want to estimate. This can lead to biased estimates, and the problem is aggravated when there are few observations before the policy change is in effect.[12] Therefore, in Table 8, I present reform effects on years of education, including a municipality-specific predicted pre-reform trend in education as a control.[13] The trend is predicted using only observations prior to the reform introduction, and therefore they should not capture trend shifts induced by the policy. Table 8 shows that the results are stable to the inclusion of pre-reform trends: the coefficients are close to those reported in Table 5, around 0.15 for women and 0.23 for men. To sum up, in the case of the Swedish education reform, the estimates are robust to the inclusion of municipality-specific trends, regardless of the specification. Whether this is true for reform estimates measuring the effect on other outcomes remains to be investigated in each specific case, but for the "first stage" regression of education on the reform, I conclude that the inclusion of region-specific trends is important, and that the results are stable across different specifications of trends.

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[12] As an alternative estimation strategy, Wolfers suggests to estimate dynamic responses to the reform, letting the policy have different effects in the years following the introduction of the policy. The empirical application in his paper differs from those involving policy changes in education, however. Wolfers studies the introduction of unilateral divorce across U.S. states, and before the new policy is in effect, there is a growing stock of married individuals who would like to file for divorce. With the new policy in place one can therefore expect divorce rates to go up for several years since it takes some time for the stock of unhappily married people to go through with their divorce. The education example is different since there is no stock of people waiting for more compulsory education, and therefore I take on a different approach to tackle this issue.

[13] I predict a pre-reform trend in education using cohorts born in 1932-1955. For individuals born 1932-1942, I use their mothers' municipality in 1960 as an indicator of where they went to school (the assumption being that mothers are less mobile than young people leaving the parental home). In a first stage, education is regressed on municipality and cohort dummies, and municipality-specific trends, using only the years prior to the reform. In a second stage, the predicted values from the first stage are included as a control in a regression of education on the reform dummy and municipality and cohort controls, using the 1943-1955 cohorts.

5.3.5 Interpreting IV estimates of returns to education

A standard assumption when interpreting instrumental variables estimates of returns to schooling is that the returns are constant across the education distribution. If this assumption holds, it does not make any difference if the identification originates from a compulsory school reform (a shift in the lower end of the distribution) or by policy changes affecting higher education. What if the assumption on constant returns is invalid? One more year of schooling at compulsory level compared to university level may have very different pay-offs. Angrist and Imbens (1995) show that when relaxing this assumption, the IV estimate is to be interpreted as an Average Causal Response (ACR), which is a weighted average of Local Average Treatment Effects at each (discrete) point in the education distribution. The underlying assumption necessary regards monotonicity, which in the case of the Swedish reform translates into the following: with probability 1, $S_{R=1} - S_{R=0} \geq 0$ for all individuals. The ACR is interpreted as the average effect of a 1-year increase in schooling, for people whose schooling was influenced by the reform. Who was influenced determines the weights in the weighted average of LATEs at each point in the distribution.

The method of Angrist and Imbens (1995) allows me to identify the weights of the ACR, and at the same time verify the underlying monotonicity assumption. For monotonicity, the cumulative distribution function (CDF) of schooling for reform- and non-reform participants should not cross: $\Pr(S \geq j \mid R = 1) \geq \Pr(S \geq j \mid R = 0)$ implies that $F_S(j \mid R = 0) \geq F_S(j \mid R = 1)$ where F_S is the CDF of schooling. Taking the difference between the CDF of the non-reform and the reform groups, it should always remain positive or equal to zero. Moreover, the difference in the CDFs, at each point j of the schooling distribution, captures the weight of that particular point to the ACR estimate. The difference in the CDFs at each j shows the fraction of people who achieved at least one more year of schooling due to the reform.

Figure 3 shows the difference between the CDFs.[14] Not unexpected, the people affected by the instrument are all in the lower tail of the distribution, and individuals with 11 or more years of education do not contribute at all to the ACR estimate. The figure also shows that the monotonicity assumption is violated: the difference between the CDFs is negative for $j=9$, which implies that the fraction of people with 9 or fewer years is higher for those participating in the reform than the corresponding fraction for those not affected (a 95 percent confidence interval is shown in Figure 3). This result is driven by the translation of levels of education into years of schooling: the old junior-secondary school (which was still in place in non-reform municipalities, and which the new 9-year comprehensive school was to replace) implied 9 or 10 years of schooling, whereby I have coded it as 9.5 years. Individuals who wished to stop at this level would in the data have 9 years if in a reform municipality, and 9.5 years if in a non-reform municipality. That is, for some individuals, taking part of the reform might actually have reduced years of schooling. To verify that this result is a consequence of assigning junior-secondary school 0.5 more years than the new comprehensive school, I assign 9 years to both of them, and find that the CDFs do not cross and that the monotoniticy requirement is satisfied (results not shown). The choice of how to assign years to levels of education might seem arbitrary, but the result in Figure 3 highlights an important point: for some individuals living in a municipality with a four-year upper-secondary school, the introduction of the 9-year comprehensive school might actually have reduced their schooling, if they did not wish to continue to higher levels. Unfortunately, the education register does not distinguish between junior-secondary school (*realskola*) of different lengths, so I cannot verify whether the assumption to code it as 9.5 is reasonable. Since the violation of the monotonicity assumption relies partly on my choice of coding, and since it concerns only a

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[14] Each point in the CDF difference figure has been obtained by regressing a binary indicator of $S \leq j$ (j=years of schooling) on a non-reform indicator, a constant, and municipality and cohort effects.

very small fraction of people, I conclude that it is not a major concern for the interpretation of the IV estimate.

6. Conclusions and Recommendations

The Swedish compulsory school reform was introduced gradually across the country's municipalities, starting in the late 1940s and continuing through the 1960s. The reform implied two more years of compulsory education, and that pupils were kept together in a comprehensive system throughout nine years. This paper describes this reform and provides an assessment of its role in the economics literature, providing a reliability analysis of the reform coding, and a sequence of tests as to whether the reform is a valid instrument for education.

First, the reliability analysis shows that the lower bound of the reform coding reliability estimate (in differences), lies somewhere in the range 0.66-0.91. These numbers provide the researcher with an idea of the degree of attenuation bias in the reform estimates, and given that they represent lower bounds, I consider them to be on an acceptable level.

Second, the analysis shows that the Swedish compulsory school reform increased years of schooling by at least 0.13-0.20 years for women, and by at least 0.23-0.28 years for men. One might have expected the effect of the reform to be higher, since two more years of education were mandated. The moderate effects thus confirm that the reform was introduced in a period when demand for education beyond the old seven-year minimum was high, and when a high fraction of each cohort would attend junior-secondary school in the absence of the reform.

Third, this paper investigates the potential for the reform to be used as an instrument for education in an instrumental variables setting. The reform passes the test of weak instruments, with F-statistics well above 10. When it comes to exogeneity of the reform with respect to

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education, a battery of tests show that given municipality and cohort fixed effects, and the inclusion of municipality-specific trends, the reform can be used as an instrument for education.

Finally, I would like to highlight that the reform affected the lower end of the educational distribution, whereby any results based on this source of variation in education can not be generalized to the full range of educational outcomes. This has the further implication that we might not necessarily be able to identify the effects of education on a range of possible outcomes. Some outcomes might be mostly affected by higher education, and those effects can not be identified with the compulsory-school instrument.

For those who wish to use the reform in an IV framework, I wish to share another two insights. First, the effect of extended compulsory education on secondary outcomes might work through different channels. One channel is that the human capital for the treated individual increases, but another likely mechanism is an "incarceration effect", which changes or postpones adolescent behaviour until after compulsory school is completed. This could be the case for outcomes such as crime and teenage fertility. And second, we should not forget that the Swedish compulsory school reform did not only impose more years of schooling, but also postponed tracking and introduced a comprehensive school system for all pupils throughout 9th grade. The effects of the reform are likely a combination of extending the number of years and of postponing tracking, and unfortunately it is not possible to separate the two.

To conclude, I reflect over the progressive environment in which this reform was implemented. The "assessment period" was introduced in order to undertake proper evaluation of the new school system, and has proven a useful tool also today, 50 years later. Hopefully also policy makers of future reforms will understand the importance of designing policies as to facilitate evaluation and research.

7. Checklist for Using Educational Reforms as an Instrumental Variable

Check that the reform is a strong predictor of education. F-statistics above 10.	
Perform tests to assess whether the reform is exogenous.	
Identify which part of the education distribution is affected by the reform.	
Think about whether your chosen outcome is likely to be affected by education in the specific part or the distribution where the instrument has an impact.	
Include region-specific trends (linear and quadratic) to test the robustness of estimates.	
Assess the role of measurement error (in case the nature of the data requires the assignment of reform participation on the basis of cohort and region information).	
Investigate whether the reform was binding, and the role of selective mobility in response to the reform.	
Identify other possible policy changes occurring simultaneously.	

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Figure 1: Years of schooling

Figure 2: Share in the reform

Figure 3: CDF difference
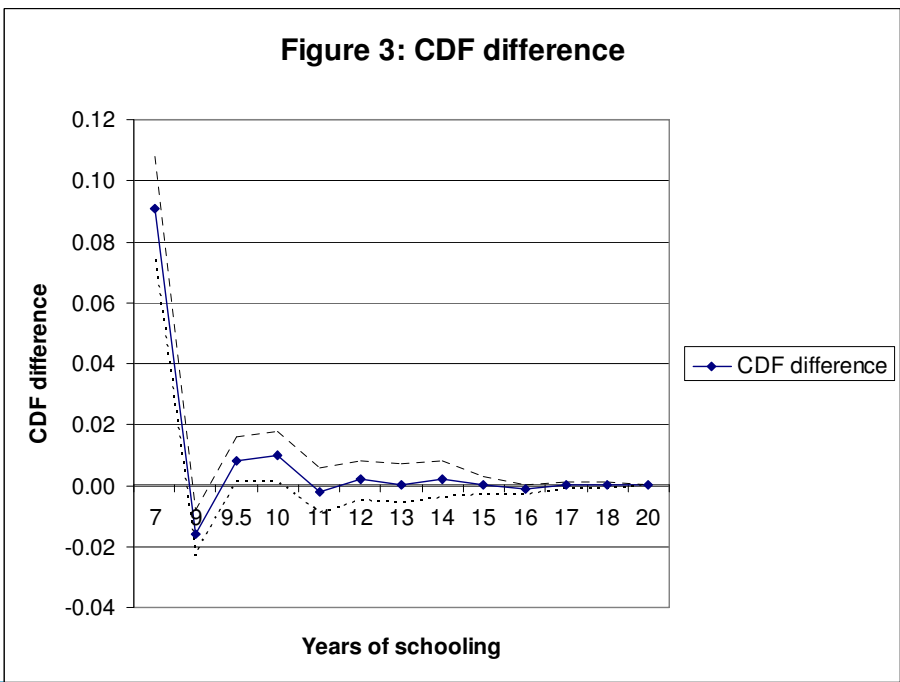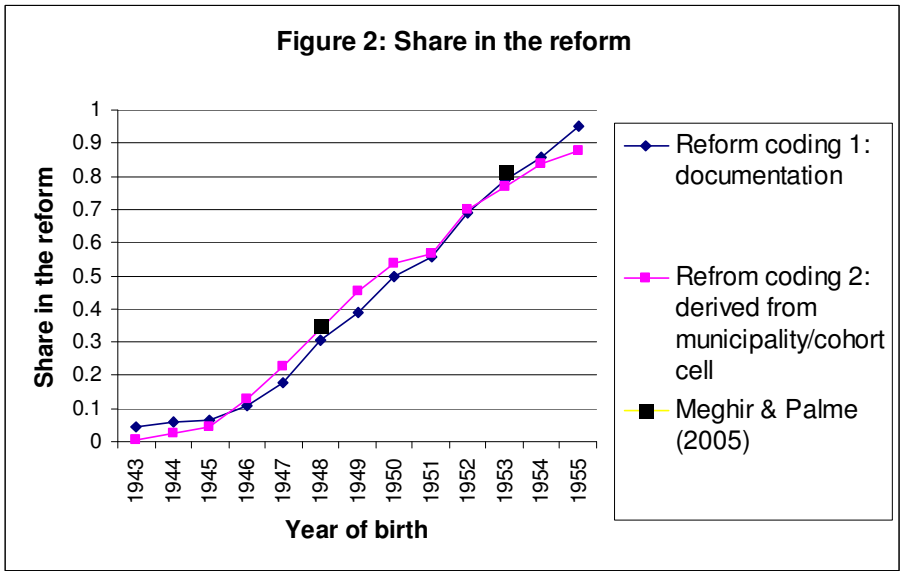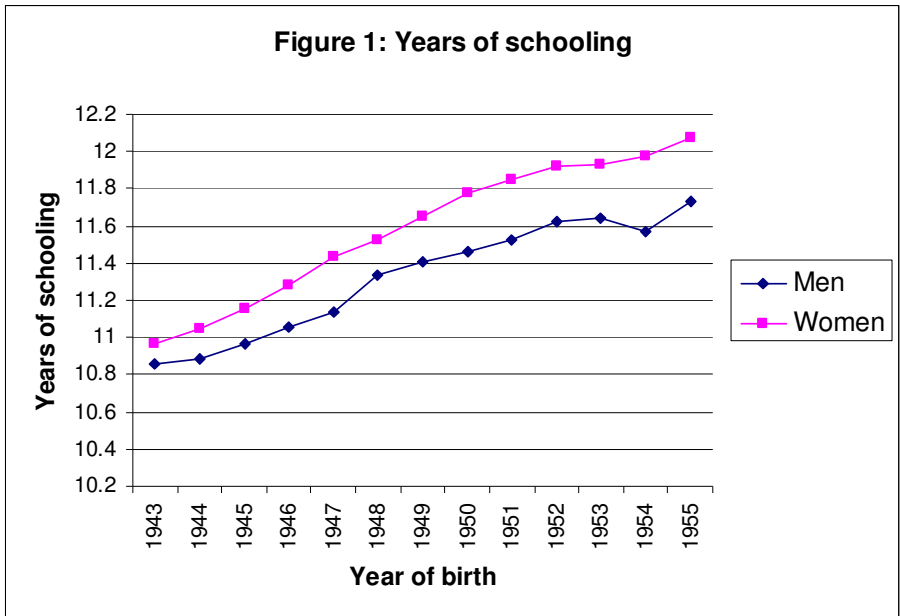
Table 1

Descriptive statistcs of a random sample of cohorts born in 1943-1955

Variable	All individuals		Reform status is defined		Reform status defined, excluding big cities (overlap MP with Marklund coding)	
	(1) Obs	(2) Mean (Std. Dev)	(3) Obs	(4) Mean (Std. Dev)	(5) Obs	(6) Mean (Std. Dev)
Years of schooling	496773	11.48 (2.72)	450885	11.44 (2.72)	394972	11.35 (2.72)
Year of birth	496773	1948.76 (3.72)	450885	1948.82 (3.74)	394972	1948.78 (3.72)
Reform [CODING 1]	450885	0.41 (0.49)	450885	0.41 (0.49)	394972	0.38 (0.49)
Reform [CODING 2]	496773	0.41 (0.49)	450885	0.41 (0.49)	394972	0.40 (0.49)
Father's schooling	337724	8.83 (2.67)	305967	8.76 (2.64)	267902	8.59 (2.56)
Mother's schooling	412758	8.18 (1.98)	374590	8.14 (1.96)	328381	8.02 (1.90)
Female	496773	0.49 (0.50)	450885	0.49 (0.50)	394972	0.49 (0.50)

Table 2

The effect of the reform on years of schooling

Dependent variable: Years of schooling

	All				Women				Men			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Reform	0.587	0.413	0.190	0.170	0.476	0.316	0.149	0.127	0.693	0.506	0.229	0.211
	(0.047)**	(0.029)**	(0.036)**	(0.042)**	(0.042)**	(0.027)**	(0.032)**	(0.037)**	(0.056)**	(0.035)**	(0.046)**	(0.051)**
Year effects	x	x	x	x	x	x	x	x	x	x	x	x
Municipality effects			x	x			x	x			x	x
Parental background		x		x		x		x		x		x
F-stat on reform	153.07	204.95	27.96	16.68	127.30	135.28	21.78	10.2	154.91	204.73	24.89	17.29
Partial R-squared of reform	0.0824	0.0612	0.1269	0.0181	0.0696	0.0484	0.0162	0.0137	0.0939	0.0728	0.0235	0.0221
Observations	450885	450885	450885	450885	221178	221178	221178	221178	229707	229707	229707	229707
R-squared	0.02	0.13	0.06	0.15	0.02	0.12	0.06	0.13	0.02	0.14	0.08	0.17

Notes: Robust standard errors in parentheses are clustered on municipality. * significant at 5%; ** significant at 1%

Table 3

Spill-over effects: The effect of the reform on post-compulsory education

Dependent variable: Dummy indicating two years of upper-secondary school or more

	All		Women		Men	
	(1)	(2)	(3)	(4)	(5)	(6)
Reform	0.010	0.007	0.009	0.006	0.010	0.008
	(0.004)*	(0.005)	(0.004)*	(0.005)	(0.006)+	(0.006)
Year effects	x	x	x	x	x	x
Municipality effects	x	x	x	x	x	x
Parental background		x		x		x
Observations	450885	450885	221178	221178	229707	229707
R-squared	0.04	0.07	0.04	0.07	0.05	0.09
F-stat on reform	5.24	2.51	4.11	1.81	3.39	1.99

Notes: Robust standard errors in parentheses are clustered on municipality.

+ significant at 10%; * significant at 5%; ** significant at 1%

Estimates from a linear probability model.

Table 4

The effect of the reform on years of schooling - reform instrumented with alternative reform indicator

Dependent variable: Years of schooling

REFORM CODING 1 instrumented with *REFORM CODING 2*

	All		Women		Men	
	(1)	(2)	(3)	(4)	(5)	(6)
Reform	0.528	0.500	0.443	0.403	0.610	0.595
	(0.105)**	(0.087)**	(0.094)**	(0.076)**	(0.118)**	(0.103)**
Year effects	x	x	x	x	x	x
Municipality effects	x	x	x	x	x	x
Parental background		x		x		x
Observations	450885	450885	221178	221178	229707	229707
R-squared	0.06	0.15	0.05	0.13	0.07	0.16
1st stage coefficient	0.534	0.534	0.533	0.533	0.535	0.535
	(0.087)**	(0.087)**	(0.088)**	(0.088)**	(0.087)**	(0.087)**
F-stat on instrument	37.61	37.60	37.03	37.02	37.87	37.87

Notes: Robust standard errors in parentheses are clustered on municipality. * significant at 5%; ** significant at 1%

Table 5

The effect of the reform on years of schooling

Linear and quadratic trends

Dependent variable: Years of schooling

	No trends				Linear trends		Quadratic trends	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
A. All								
Reform	0.190	0.170	0.191	0.190	0.243	0.237	0.201	0.198
	(0.036)**	(0.042)**	(0.036)**	(0.035)**	(0.020)**	(0.021)**	(0.026)**	(0.026)**
F-stat on reform	27.96	16.68	28.39	28.71	151.88	122.33	61.15	55.61
Observations	450885	450885	13085	13085	13085	13085	13085	13085
R-squared	0.06	0.15	0.69	0.71	0.74	0.76	0.77	0.79
B. Women								
Reform	0.149	0.127	0.149	0.143	0.199	0.189	0.140	0.127
	(0.032)**	(0.037)**	(0.032)**	(0.032)**	(0.025)**	(0.025)**	(0.032)**	(0.031)**
F-stat on reform	21.78	10.2	21.76	20.54	64.34	58.07	19.34	16.42
Observations	221178	221178	13048	13048	13048	13048	13048	13048
R-squared	0.06	0.13	0.50	0.54	0.57	0.60	0.61	0.64
C. Men								
Reform	0.229	0.211	0.229	0.231	0.282	0.278	0.259	0.265
	(0.046)**	(0.051)**	(0.046)**	(0.044)**	(0.027)**	(0.028)**	(0.035)**	(0.035)**
F-stat on reform	24.89	17.29	24.87	27.74	106.80	97.95	54.88	58.13
Observations	229707	229707	13054	13054	13054	13054	13054	13054
R-squared	0.08	0.17	0.60	0.63	0.66	0.68	0.69	0.71
Year effects	x	x	x	x	x	x	X	x
Muni effects	x	x	x	x	x	x	x	x
Parental education		x		x		x		x
Linear muni trends					x	x		
Quadratic muni trends							x	x
Data	Ind	Ind	Aggr	Aggr	Aggr	Aggr	Aggr	Aggr

Notes: Robust standard errors in parentheses are clustered on municipality. * significant at 5%; ** significant at 1%
The first two columns report estimates based on micro data on individual level. Estimates in the remaining columns have
been obtained using a data set aggregated on year of birth/municipality level, where each observation is weighted by 1/n.

Table 6

The effect of parental schooling on reform participation

Dependent variable: reform

	(1)	(2)	(3)	(4)
Parental schooling	0.095	0.004	0.011	0.005
	(0.013)**	(0.01)	(0.012)	(0.008)
Schooling missing (individual level)	-0.095	0.104	0.02	0.016
	(0.080)	(0.057)+	(0.042)	(0.039)
Schooling missing (municipality/cohort level)	0.740	-0.044	0.264	-0.048
	(0.136)**	(0.103)	(0.134)*	(0.067)
Year effects	x	x	x	x
Municipality effects		x	x	x
Linear municipality trends			x	
Quadratic municipality trends				x
Observations	13085	13085	13085	13085
R-squared	0.43	0.68	0.76	0.84

Robust standard errors in parentheses are clustered on municipality.

+ significant at 10%; * significant at 5%; ** significant at 1%

Estimates have been obtained using a data set aggregated on year of birth/municipality level, where each observation is weighted by 1/n.

Estimates from a linear probability model.

Table 7

Test for exogeneity of reform

The effect of the reform on cohorts t years too old to have been affected

Dependent variable: Years of schooling

	(1) t=1	(2) t=2	(3) t=3	(4) t=4
A. Baseline results				
Reform for cohort t years too old	0.129 (0.024)**	0.072 (0.023)**	0.09 (0.029)**	0.107 (0.039)**
Reform for actual affected cohort	0.111 (0.041)**	0.17 (0.039)**	0.189 (0.036)**	0.209 (0.031)**
R-squared	0.69	0.69	0.69	0.69
B. Including municipality-specific linear trends				
Reform for cohort t years too old	0.120 (0.020)**	0.052 (0.018)**	0.057 (0.021)**	0.046 (0.022)*
Reform for actual affected cohort	0.178 (0.024)**	0.233 (0.021)**	0.246 (0.019)**	0.252 (0.019)**
R-squared	0.74	0.74	0.74	0.74
C. Indluding municipality-specific quadratic trends				
Reform for cohort t years too old	0.104 (0.026)**	0.010 (0.025)	0.013 (0.029)	0.000 (0.026)
Reform for actual affected cohort	0.174 (0.027)**	0.206 (0.025)**	0.209 (0.025)**	0.205 (0.026)**
R-squared	0.77	0.77	0.77	0.77
Observations	13085	13085	13085	13085

Robust standard errors in parentheses are clustered on municipality.

+ significant at 10%; * significant at 5%; ** significant at 1%

Estimates have been obtained using a data set aggregated on year of
birth/municipality level, where each observation is weighted by 1/n.

Table 8

The effect of the reform on years of education

Regressions including pre-reform municipality-specific trends in education

Dependent variable: Years of schooling

	All		**Women**		**Men**	
	(1)	(2)	(3)	(4)	(5)	(6)
	Linear trend	Quadratic trend	Linear trend	Quadratic trend	Linear trend	Quadratic trend
Reform	0.194	0.191	0.150	0.150	0.230	0.228
	(0.032)**	(0.036)**	(0.030)**	(0.032)**	(0.045)**	(0.046)**
Linear pre-ref trends	x		x		x	
Quadratic pre-ref trends		x		x		x
Observations	13085	13085	13048	13048	13054	13054
R-squared	0.69	0.69	0.51	0.50	0.60	0.60

Robust standard errors in parentheses are clustered on municipality

+ significant at 10%; * significant at 5%; ** significant at 1%

Estimates have been obtained using a data set aggregated on year of birth/municipality level, where each observation is weighted by 1/n.

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Appendix A

Reform coding for register data

There are two independent ways to obtain a reform code to attach to register data, one is to use available documentation on when the reform was in place (*CODING 1*), the other is to deduce it from a register-based data set of large sample size (*CODING 2*).

A.1 Reform coding based on documentation (CODING 1)

To obtain a complete code of the implementation of the nine-year comprehensive school, I have, with help from Anders Björklund, Valter Hultén and Mikael Lindahl, used two main sources:

a. Marklund and the National Board of Education

Marklund (1981) provides a list with the quantitative development of the reform from 1949/50 to 1960/61. This documentation states which municipalities (or which parts of a municipality) that entered the assessment programme each year. However, Marklund (1981) does not list which grades that were exposed to the reform, in each municipality and each year. This information is available in the yearly publications that the National Board of Education published during the course of the trial period (Aktuellt från Skolöverstyrelsen 1953-1962). These publications summarized many of the aspects of the ongoing educational development, one of which was the participating municipalities and also which grades that were subject to the reform. These publications together cover the years 1951/52 to 1960/61. It is noteworthy that these two sources, Marklund and the Board of Education reports, in general coincide in terms of municipalities listed. There is one difference in that the yearly publications from the Board of Education list a few more municipalities as participating in the reform than what is mentioned in Marklund.

In the guidelines for the reform assessment, it was stated that only pupils in grades 1 through 5 would be subject to any changes. Therefore the above information from Marklund

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and the Board of Education, that covers almost the whole assessment period with the last year being 1960/61, makes it possible assign whether individuals born in 1938 to 1949 were subject to the reform or not (the 1938 cohort was the first one to be affected, 1949 is the cohort of 5th graders in 1960/61). From 1961/62 these sources do not tell us what is going on, but we know for sure that pupils in 6th grade and above should not be subject to any changes. Therefore we can code the cohort of 6th graders in 1961, and older pupils (the 1949 cohort and older), but for younger cohorts there could be changes from 1961 an onwards that are not captured by these sources.

b. The Educational Bureau (1960-1964) and Statistics Sweden (1968-1969)

When the final decision about the complete introduction of the new school was taken, in 1962, the experimental period also came to an end. Now, municipalities were required to implement the reform, but a transition period allowed them to postpone the implementation, however no longer than until 1969. Thus, also in the early 1960s there is some variation in reform implementation, affecting cohorts born from 1950 an onwards. Marklund (1981) and the publications from the Board of Education were mainly concerned with the assessment programme, and thus they do not document reform implementation in the 1960s. To use the variation in compulsory schooling legislation for the 1950s cohorts, it is possible to trace reform implementation in the early 1960s from municipality tables from Statistics Sweden (1968, 1969) and from the Educational Bureau (*Undervisningsbyrån*) (1960, 1961/62, 1963/64). For each municipality, the tables from the Educational Bureau give the number of pupils in each grade in both the old school (*folkskolan*) and the new nine year comprehensive school (*grundskolan*). From such a table it is possible to see in which grade and year the implementation took place. See the examples following below.

Example 1

Municipality m in year t, cohort size is around 500.

Grade	1	2	3	4	5	6	7	8	9
Type of school	Nr of pupils								
Folkskola (old)	0	500	500	500	0	500	500	0	0
Grundskola (new)	500	0	0	0	500	0	0	0	0

Municipality m in year t+1

Grade	1	2	3	4	5	6	7	8	9
Type of school	Nr of pupils								
Folkskola (old)	0	0	500	500	0	0	500	0	0
Grundskola (new)	500	500	0	0	500	500	0	0	0

Municipality m in year t+2

Grade	1	2	3	4	5	6	7	8	9
Type of school	Nr of pupils								
Folkskola (old)	0	0	0	500	0	0	0	0	0
Grundskola (new)	500	500	500	0	500	500	500	0	0

Municipality m in year t+3

Grade	1	2	3	4	5	6	7	8	9
Type of school	Nr of pupils								
Folkskola (old)	0	0	0	0	0	0	0	0	0
Grundskola (new)	500	500	500	500	500	500	500	500	0

From these tables it is possible to conclude that the cohort of 5th graders in year t, that is, the cohort born in t-11, is the first cohort in municipality m, to be affected by the reform. All younger birth cohorts were also affected (since even if you were in grades 2-4 in year t, you would eventually reach grade 5 and thus be fazed into the new school).

Example 1 is a stylized example, in reality the tables year-by-year might look either as in example 2 or 3 below.

Example 2

Municipality m in year t

Grade	1	2	3	4	5	6	7	8	9
Type of school	Nr of pupils								
Folkskola (old)	0	500	500	500	250	500	500	0	0
Grundskola (new)	500	0	0	0	250	0	0	0	0

Municipality m in year t+1

Grade	1	2	3	4	5	6	7	8	9
Type of school	Nr of pupils								
Folkskola (old)	0	0	500	500	0	250	500	0	0
Grundskola (new)	500	500	0	0	500	250	0	0	0

In this case, it is not clear which cohort that should be assigned as the first reform cohort. Is it the cohort in 5th grade in year t or in t+1? In these cases the reform implementation has been set to start when at least half of a cohort is facing the reform. However, it is clear that the coding here will introduce some measurement error.

Example 3

Municipality m in year t

Grade	1	2	3	4	5	6	7	8	9
Type of school	Nr of pupils								
Folkskola (old)	0	500	500	500	0	500	500	0	0
Grundskola (new)	500	0	0	0	500	0	0	0	0

Municipality m in year t+1

Grade	1	2	3	4	5	6	7	8	9
Type of school	Nr of pupils								
Folkskola (old)	0	0	500	500	0	0	0	0	0
Grundskola (new)	500	500	0	0	500	500	500	0	0

This example shows that the tables are not always coherent between years. In year t, it looks like the first cohort is the fifth graders in t, whereas in year t+1, it seems like the first cohort is the one of 7th graders in t+1. In these cases, the information on which cohort entered first is

taken from the last table that reveals a shift between the old and the new school (in the light of the example above, it would be the 7th graders in year t+1, that is the cohort born in t+1-13).

Note that the first table from the Educational Bureau is from 1960/61, which means that municipalities that introduced the reform very early can not be coded using this second source of information. That is, in the case all pupils in grade 1 through 9 were already in the new school in 1960, it is not possible to see when the shift took place. In those cases, we rely solely on the first source (Marklund). Luckily, there is some overlap between the two sources: for 158 municipalities I have obtained a coding from both Marklund and the Bureau. In 9 out of 158 cases, the coding differs between these sources, and in those cases, I have used Marklund.

In some cases we know from Marklund that a municipality introduced the reform in different parts of the municipality at different points in time. If these were early implementers, the statistical tables do not reveal when the majority of the pupils in a municipality were shifted into the new system. In the case Marklund states which school district, or which schools within a municipality that introduced the new school, it is possible to assign these schools to a sub-region of the municipality (a parish). There are however, a few municipalities where we know that the reform was introduced gradually, but there is no information on which schools or which part of the municipality. These municipalities can not be coded and must be dropped from the sample: Hälsingborg, Jönköping, Linköping, Skellefteå, Sundbyberg and Södertälje.

The three big cities, Stockholm, Göteborg and Malmö, are also problematic to code. They implemented the reform at different points in time in different parts of their municipalities, and the coding has been constructed as follows (note that information on parish is necessary)[1]:

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[1] Assigning the reform based on information of parish of residence is an approximation, where I map a given school (or school district) to the parish/es in which it lies.

- Stockholm. From the statistical tables from the Educational Bureau, it is clear that in 1962, the whole cohort of 8th graders (the 1948 cohort) was shifted into the new comprehensive school. However, reform implementation had started gradually earlier, at first in the southern suburbs of Stockholm. Based on information on parish of residence, the south suburbs can be dropped, and the change that affects (approximately all the rest) of Stockholm can be coded and the first cohort affected is set to 1948.

- Göteborg. The first cohort where all pupils are in the new school is the 1950 cohort. Early implementing parishes are dropped.

- Malmö. The first cohort where all pupils are in the new school is the 1949 cohort. Early implementing parishes are dropped.

The procedure outlined above allows me to find the first cohort affected by the reform in almost all municipalities. Some could not be coded due to ambiguity as to which part of the municipality implemented the reform (mentioned above). Yet another three municipalities could not be coded, simply because they did not show up in the statistical records: Fjälkinge, Svarteborg and Sörbygden.

A.2 Reform coding deducted from large-sample register data (CODING 2)

With a large enough sample, it is possible to adopt the following strategy to find out when a municipality implemented the reform: drop all individuals with education higher than the new compulsory minimum (*grundskola*), using the information on completed education levels in Statistcis Sweden's education register. Now we are left with only observations of the old minimum (*folskola*) and the new minimum (*grundskola*). Assign a dummy equal to one for the new comprehensive school. Collapse this data by birth cohort and municipality, and look at the average of the comprehensive school dummy for each cell. With a clean-cut

implementation, we should observe that within a municipality, the average shifts from 0 to 1 between two specific cohorts, and this is when the reform is implemented. In reality, the cohort-to-cohort changes are not always so clean, and one can assign a reform to cohorts where the cohort/municipality-average of the compulsory school dummy is >=0.5.

With this procedure it might be the case that you assign the reform to cohort t, but in cohort t+1 the dummy average is <0.5 and for cohort t+2 it shifts back to >=0.5. In the empirical part of this paper I have, in the case I observe two shifts, assigned the reform to the shift that is based on the largest number of observations, but this can naturally be treated in various ways.

Note also that some municipalities did not implement the reform uniformly within itself; most notably this was the case in the big cities. Therefore, for Stockholm, Göteborg and Malmö, I derive the reform indicator on parish level.

This method is of course more reliable the larger the sample size. In my case, I sometimes end up with very few observations of individuals with the lowest level of education in each birth year/municipality cell. Larger samples than the 35 percent of each cohort that I use in this study are preferable.

Appendix B

Reliability analysis

The register data that I use as the base for this overview has a few limitations. The data on reform assignment are in some cases an approximation of the starting year of the reform. As described above, some municipalities kept parallel school systems, which means that there is no possibility to find a clear-cut starting point. Hence, the coding of the reform does in some cases represent an average or the majority in a given municipality and birth cohort, which will introduce measurement error in the reform indicator. Another aspect is that even though implementation might have been extensive, there was room for single individuals to apply for an exemption. We also need to assume that pupils are in the expected grade according to their age; if grade repetition or skipping a grade was a prevalent phenomenon among the affected cohorts, this is also one source of measurement error to keep in mind. To understand better the consequences of measurement error in regression analysis based on the reform, I now turn to analysing the quality of my reform indicators.

As a starting point to a reliability analysis of the reform coding, I acknowledge that since reform participation is a binary indicator variable, the measurement error is not classical. That is, the measurement error is correlated with the true underlying variable (Aigner 1973). The formula describing attenuation bias in the case of classical measurement error must now be modified to represent the case of non-classical measurement error.

I want to estimate reform effects on an outcome y in the following way:

$$y = \alpha + \beta r^* + \varepsilon \tag{B1}$$

where r^*_{icm} denotes the true (unobserved) reform status of an individual i, belonging to cohort c, going to school in municipality m. In the data we observe two measures of the reform measured with error (omitting the subscripts for simplicity and following the notation in Kane et al. 1999):

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$$E(r_1 \mid r*, r_2, y) = \pi_{10} + \pi_{11}r^* \qquad \text{for } CODING\ 1 \text{ and} \qquad (B2)$$

$$E(r_2 \mid r*, r_1, y) = \pi_{20} + \pi_{21}r^* \qquad \text{for } CODING\ 2. \qquad (B3)$$

Given the true reform participation $r*$, I assume that the observed variables r_1 and r_2 are independent of each other and of y. In order for the measurement error to be classical, the further assumptions $\pi_{11} = \pi_{21} = 1$, and $\pi_{01} = \pi_{02} = 0$ must be satisfied. With a binary indicator variable these assumptions do not hold and the measurement error is correlated with the true underlying variable. We have that $\pi_{11} < 1$, $\pi_{21} < 1$ and $\pi_{10} > 0$, $\pi_{20} > 0$.

Following Aigner (1973) and Kane et al. (1999) the probability limit of β in the case of measurement error in a binary variable can be derived as follows:

$$p \lim \beta_{OLS} = \beta \left[1 - P(r* = 1 \mid r = 0) - P(r* = 0 \mid r = 1) \right] \qquad (B4)$$

Just as in the case of classical measurement error, the OLS estimate is biased towards zero and the estimated effect is attenuated. In the case of classical measurement error, a standard remedy to inconsistencies in OLS parameters has been to use an instrumental variables strategy. With two independent measures of the variable of interest, two-stages-least squares when one measure is used as an instrument for the other produces consistent coefficients. When measurement error is non-classical, however, an IV strategy is not likely to produce consistent estimates. Nevertheless, an IV estimate can be informative, since with non-classical measurement error it will be upward biased (Kane et al. 1999):

$$p \lim \beta_{2SLS} = \beta \frac{1}{\pi_{11}} \qquad (B5)$$

where r_1 has been instrumented in the first stage using r_2 as an instrument. We see that only in the case of classical measurement error ($\pi_{11} = 1$) 2SLS produces consistent estimates, and with measurement error in the categorical variable ($\pi_{11} < 1$), β_{2SLS} will be upward biased.

Thus, with measurement error in the binary indicator variable for reform participation, it turns out that both the OLS and the IV estimate (using two reform codings and instrumenting one with the other) are inconsistent, one downwards and the other upwards. Therefore, the two estimates provide a lower and an upper bound for the true parameter, and we are able to narrow down the range of possible true effects. The lower and upper bound estimates are discussed in section 5.2 of the paper.

Classical measurement error is often assessed by estimating the reliability ratio, which gives an idea about the degree of attenuation bias. With two independent measures of the variable of interest, the attenuation bias is estimated with the reliability ratio of r_1: that is, the fraction of the variability in r_1 that is due to the true variability in r^* (see for example Ashenfelter and Krueger 1994, Krueger and Lindahl 2001). Empirical estimates of the reliability ratios γ_1 and γ_2 can be obtained by regressing r_2 on r_1 and vice versa. Tthe reliability ratio for a binary variable measured with error does not give us a consistent estimate of the attenuation bias, but provides a lower bound thereof. Expressing the measurement error as $r_1 = r^* + u_1$ and $r_2 = r^* + u_2$, where $u_1 = \pi_{10} + r^*(\pi_{11} + 1)$ and $u_2 = \pi_{20} + r^*(\pi_{21} + 1)$, and estimating the reliability ratio of r_1 in the following regression framework:

$$r_2 = \delta + \gamma r_1 + \varepsilon \qquad (B6)$$

we obtain a reliability estimate $\hat{\gamma}$:

$$p \lim \hat{\gamma} = \gamma \left(1 - \Pr(r^* = 1 \mid r_1 = 0) - \Pr(r^* = 0 \mid r_1 = 1)\right) \qquad (C7)$$

where $\gamma = \dfrac{Cov(r_2, r^*)}{Var(r^*)} = \dfrac{Var(r^*) + Cov(r^*, u_2)}{Var(r^*)}$. Since the measurement error for a binary indicator variable is always negatively correlated with the true underlying variable (Aigner 1973, Savoca 1998), $\gamma < 1$ and $\hat{\gamma}$ represents a lower bound of the attenuation bias in the OLS regression.

The upper panel of Table B1 presents the reliability ratios for *CODING 1* and *CODING 2*, using the sample of cohorts born 1943-1955. For each code, I present results for the full sample, and for a sample excluding the three biggest cities (Stockholm, Gothenburg and Malmö), the reason being that there is potentially more measurement error in the city municipalities (see Appendix A for details). The first two estimates correspond to the OLS reliability ratio, whereas the second two columns for each code presents the reliability ratio once municipality and cohort fixed effects are controlled for. Since my identification relies on a differences-in-differences model, it is adequate to focus on the reliability ratio of the differenced variables. Both *CODING 1* and *CODING 2* indicate a lower bound of the attenuation bias of an OLS estimate of the effect of the reform in the range of 0.72-0.75, but moving on to the differences-in-differences reliability ratios, it is clear that the reliability ratio is lower. For *CODING 1* it is around 0.66 for the full sample including the cities, and for *CODING 2* as low as 0.53.

There is also a third coding available for evaluating the quality of the reform indicator. The IS data, that Meghir and Palme used in their 2005 paper, contain information on the municipality in which an individual went to school. With this information *CODING 1* and *2* are matched to their data, where also a third reform indicator is available. For the cohorts born in 1948 and 1953, we thus have three independent measures of reform participation. However, in order to match coding 1 and 2 for the big cities, information on parish of residence is necessary. This information is not available in the particular data set used by Meghir and Palme (2005), and therefore the reliability ratios reported are based on a sample excluding the big cities.[2] Columns 2 and 4 of panel C of Table B1 show that the reliability ratio of *CODING 1*, with respect to the information available in the IS data, is very high: 0.95 for the OLS and 0.91 for the differenced variable. This is a very encouraging and convincing result; the lower

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[2] Column 3 in Table 1 reports on the descriptive statistics of the sample with the restrictions corresponding to those that have to be put on the Meghir and Palme (2005) data set in order to estimate the reliability ratios in Table 3: only cohorts 1948 and 1953 and excluding the big cities.

bound reliability of *CODING 1* is high and establishes that the reform coding contains little error. Moving along panel C it is clear that the reliability ratio of *CODING 2* with respect to the IS code is much lower, around 0.7. I therefore conclude that *CODING 1*, with a reliability ratio of 0.91, is of high quality, although it should be kept in mind that the high ratio obtained was estimated from a sample of only the 1948 and 1953 cohorts, and excluding the cities. (Note that panel B of Table B1 reports estimates corresponding to those in panel A, but on the sample that is used in panel C). Thus, in this paper I concentrate on estimates based on *CODING 1*, since the reliability analysis demonstrates that it has the highest quality of the measures available.

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Table B1

Reliability ratios

The fraction of the variance in a variable measured with error that can be explained by the true variance.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
A. Administrative data, cohorts 1943-1955								
	Reliability of *CODING 1*				Reliability of *CODING 2*			
	0.721	0.755	0.656	0.699	0.72	0.746	0.534	0.691
	(0.036)**	(0.027)**	(0.037)**	(0.019)**	(0.030)**	(0.022)**	(0.087)**	(0.021)**
B. Administrative data, cohorts 1948, 1953								
	Reliability of *CODING 1*				Reliability of *CODING 2*			
	0.686	0.723	0.722	0.72	0.688	0.728	0.486	0.658
	(0.042)**	(0.032)**	(0.103)**	(0.030)**	(0.044)**	(0.033)**	(0.103)**	(0.054)**
Including cities	Yes	No	Yes	No	Yes	No	Yes	No
	OLS	OLS	D-i-D	D-i-D	OLS	OLS	D-i-D	D-i-D
C. Administrative data with respect to IS data (Meghir and Palme 2005), cohorts 1948, 1953								
	Reliability of *CODING 1* wrt IS data				Reliability of *CODING 2* wrt IS data			
		0.945		0.913		0.707		0.669
		(0.007)**		(0.014)**		(0.027)**		(0.032)**
Including cities		No		No		No		No
		OLS		D-i-D		OLS		D-i-D

Notes: Robust standard errors in parentheses are clustered on municipality. * significant at 5%; ** significant at 1%
The reliability ratio for CODING 1 wrt CODING 2 is obtained by regressing CODING 2 on CODING 1. The reliability ratios controlling for differences also include cohort and municipality fixed effects. All other ratios are obtained correspondingly.

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Appendix C

Figure C1: Share living with mother 1960