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Graded Children – Evidence of Longrun Consequenses of School Grades from a Nationwide Reform

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Graded children
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Abstract

Swedish elementary school children stopped receiving written end of year report cards following a grading reform in 1982. Gradual implementation of the reform creates an opportunity to investigate the effects of being graded on adult educational attainments and earnings for children in the cohorts born 1954–1974, using a difference-in-differences strategy. Accounting for municipal time trends and tracing out reform dynamics, there is some evidence that being graded increases girls' years of schooling, but has no significant average effect on boys. Analysis of effects by family background suggests that getting grades increases the probability of high school graduation for boys and girls with compulsory school educated parents. Sons of university graduates, however, earn less and are less likely to get a university degree if they were graded in elementary school.

Keywords: school policy, grades, educational attainment, adult earnings, family background, difference-in-differences.
JEL-codes: I21, I28, J13, J24

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1 Introduction

There is growing evidence that individual teachers and schools make a difference for student performance (Rivkin, Hanushek and Kain, 2005; Rockoff, 2004). We are also starting to learn in what ways schools and teachers matter. Grönqvist and Vlachos (2008) show how the matching of students to teachers matters and also that various dimensions of teacher ability matter differentially for pupils of different aptitude. Grading standards and graduation requirements have also been shown to affect student performance (Figlio and Lucas, 2004; Betts, 1998). Increased testing of children has also spurred an interest in what ways learning and human capital accumulation is affected by tests and exams (Lazear, 2006; Dee and Jacob, 2006; Neal and Schanzenbach, 2008).

Another way in which teachers, schools or school systems may matter is to the extent that they set grades or if they set grades at all. Although children in most countries are being graded on a regular basis, there is not much knowledge about the consequences of being graded. In absence of scientific evidence, the pros and cons of being graded on a report card are a subject of debate in some countries, e.g. Sweden where written grades are being reintroduced (Ministry of Education, 2008), and in the US (Kohn, 1994). A reason for the lack evidence is that there, within a school system, usually is little variation in whether and when children are graded. To the extent that there is such variation, parents' ability to select schools based on their preferred grading policy makes causal inference difficult.

There is, however, some recent evidence on the role of feedback from individual educational institutions. Based on differential feedback policies across departments at a UK university, Banderia, Larcinese and Rasul (2009) find that interim feedback improves future performance. Azmat and Iriberri (2009) study the introduction of relative performance evaluations, in addition to absolute evaluations at a Basque high school, and show that the new form of feedback had short lived positive effects on the performance of the students.

This paper is a first attempt to study the effects of being graded on pupils' long run educational attainment and earnings in adulthood. It is also the first study of effects of grades on young children and the first evidence stemming from a large scale reform of grading practices. I exploit time variation in when Swedish schools stopped giving written end of year school report cards to children younger than 14 as a result a grading reform in 1982. More specifically, the time variation is the result of an option for local school boards to stop the practice of giving end of year report cards for ten and thirteen year olds with the implementation of the 1969 National School Curriculum. Hence, children in the cohorts born 1957–1972 were graded at age ten and thirteen, at either age or not at all depending on when and where they went to school. I employ a difference-in-differences strategy to investigate the effects of being graded on educational attainment as measured by years of schooling, high school and university graduation and adult annual earnings using register data from Statistics Sweden covering all individuals born 1954–1974 and survey data on the reform implementation.

Accounting for municipal time trends and tracing out the reform dynamics, I find that being graded lengthens girls' years of schooling, but has no significant average effects on boys. An analysis of heterogeneous effects by family background suggests that getting grades increases the probability of graduating from high school for girls and for boys with compulsory school educated parents. Sons of university graduates, however, earn less and are less likely to have a university degree if they were graded in elementary school compared to ungraded boys of the same background. These results suggest that being graded is beneficial for girls and for boys at the lower end of the achievement distribution, i.e. on the children that are on the margin of being successful in high school. The effects on university graduation also suggest that grades were discouraging for boys at the top of the distribution.

The timing of the reform effects lends support for a causal interpretation of these results. A plausible interpretation of the dynamic pattern of the effects is that abolishing elementary school grades had some impact on children that were not directly affected, but who were still attending school when the reform took place. Also, the impact of the reform was not as strong on the first ungraded cohorts as on cohorts that were younger when they found out they would not get grades. This suggests that anticipating being graded may have a positive effect on human capital accumulation in school.

Economic analyses of grades have often focused on effects on effort and performance of how hard it is to obtain a given grade or reach a given standard required for graduation, assuming that children (or future employers) attach a value to higher grades and successful graduation (Becker and Rosen, 1992; Costrell, 1994; Betts, 1998; Betts and Grogger, 2003; Figlio and Lucas, 2004; Dee and Jacob, 2006). A general finding this literature is that tough grading or graduation standards can have beneficial effects on student performance, but that good students benefit more than poor students, who may even suffer. The proposed mechanism is that tough standards and requirements are motivating for those who believe that working harder will pay off in higher grades or a higher likelihood of graduation. Weaker students may instead give up if tougher standards push success out of reach. With a fine enough scale, most students can, however, be on a margin where effort can pay off.

An important aspect of grades is that they may also affect children’s performance because they reveal information about ability and about the learning technology. Grades can inform children and their parents about performance, potential and, importantly on the return to effort. Getting grades may thus benefit learning if grades raise aspirations and convey a message that there are high returns to effort. Evaluation may however also discourage effort and motivation if the information conveyed hurts self-confidence and lowers the child's perceived returns to effort (Benabou and Tirole, 2002). For the informational content of grades to have either motivational or demotivational effect on future performance, they need to convey some *new* information. It is therefore plausible that children whose parents are less able to assess their child’s ability or evaluate performance are more positively or negatively affected by grades.

In educational psychology there has been a heated debate on the effects of reward, praise and feedback on motivation, and in particular the extent to which intrinsic motivation is crowded out by the presence of rewards and praise (Deci, Koestner and Ryan, 1999; Cameron et al, 2001). There is also a literature focusing on the effects of different forms of feedback suggesting that feedback can be either enhancing or discouraging (Butler, 1987; Mueller and Dweck, 1998).

It is an empirical question to determine if the long run effects of the mechanisms discussed in the literature are positive or negative and for whom. In particular, in the Swedish context where parents and children are given qualitative bi-annual oral feedback, it is not clear that the written report cards convey any additional incentives or information. Although the present paper does not unravel the actual mechanisms at play, it provides a first attempt to measure the long run consequences of being graded in school. The paper proceeds as follows. In the following section I provide background information on the Swedish school system and in particular of the 1970's grade reform. In Section *3*, I discuss the empirical strategy and present the data. Results are presented in Section *4* which also investigates the dynamics of the effects and robustness. In Section *5*, I explore differential affects by parental education and at different educational transmissions. Section *6* concludes.

2 The Swedish school system and 1970's grading reform

2.1 The Swedish school system

Until the 1990s Sweden had a highly centralized school system, with detailed curricula specifying what and how to teach and how to organize schools.[1] Although schools were operated by the municipal governments, teachers and headmasters were employed by the central government and schools were financed over the central government budget. As a result local variation in school organization, curricula and resources was limited.

Compulsory schooling was, and still is, comprised of nine years starting from the fall of the calendar year a child turns seven. Schools were typically organized such that elementary school children would have one and the same teacher in most

[1] During the period studied in this paper, National Compulsory School Curriculum of 1962 was replaced by the National Compulsory School Curriculum of 1969 and later on by a new curriculum (Lgr80) in 1982. There were also annual decrees from the National Board of Education that schools were to follow.

subjects in lower school, from the first year through the third year of school when the children were 7 to 10 years of age. Another teacher would take over the class in middle school from year 4 through 6. In secondary school, from the 7th year through the 9th year of school children had different teachers in different subjects. With rare exceptions, children attended the school in their catchment area of residence.

Prior to the grading reform studied in this paper, the national school legislation mandated that teachers give children written grades in each subject on a report card at the end of lower school (age 10), at the end of middle school (age 13) and at the end of the seventh year of school (age 14). From then on children were to be graded at the end of both fall and spring semesters until they completed compulsory schooling (National Board of Education 1962; SOU 1977:99) after 9 years of school. Grades were set by the teachers on a scale from 1 to 5, 5 being the highest. Grades were norm based, with 3 as the national average, pinned down by national standardized tests in the core subjects Mathematics, Swedish and English.

2.2 The reform – a change in feedback regime

With the implementation of the "1980 National Compulsory School Curriculum", in the academic year 1982/83, grades through the 7th year of school were abolished by law throughout the country. The first steps towards the 1982 grading reform were however taken already in 1969. With the 1969 school curriculum, local municipal school boards were granted authority by the National Board of Education to end the practice of written grades and report cards in elementary school.[2]

A reason for granting this option was the view that parents' need for information was satisfied by the introduction of oral assessments in schools which took place in 1969/1970. Teachers were to invite parents (and later on also pupils) to bi-annual parent-teacher conferences during which parents were informed of the child's progress (National Board of Education, 1970). Hence, two major changes to the nature of information on child performance from schools to parents took place during the reform period. First, bi-annual qualitative orally communicated information was introduced for everyone. Second, written quantitative grades and report cards at the

[2] See National Board of Education (1969).

end of 3rd and 6th grade were gradually abandoned. It is worth noting that although, no longer necessary for pinning down the grading norm, the practice of nationwide standardized testing in 5th grade continued.

The bi-annual parent teacher conferences became known as "15-minute talks" and provided an opportunity for teachers to inform parents of child performance and parents to ask questions. Over time it became more common also for the child to participate, but there was no obligation for parents to show up. Once written grades were no longer permitted, it was made explicit that no comparison of the child's performance to classmates' was to be made and that "grade like" statements were prohibited. Instead, the talks should focus on the child's progress.

During the reform years, it is less clear what type of information was actually conveyed. There are no records on how conferences were implemented or to what extent parents actually attended. It is possible that these conferences were taken more seriously in schools where they were the only source of information for parents. However, since the parent-teacher conferences were bi-annual events and hence provided information more frequently than the written reports which only came at the end of the third and sixth year, it is unlikely that parents in grade giving schools ignored the parent teacher conferences for three years because they knew they would get a report card home at the end of the third year. It is therefore more relevant to view the change in feedback regime as moving from frequent oral assessment and infrequent written grades to only frequent oral assessment.

At the time of the reform, the arguments for abolishing written grades ranged from the idea that grades in elementary school were unnecessary for selection to higher levels in school to the idea that grades hampered learning by inspiring unhealthy competition, and that grades unduly favored academically strong children from educated family backgrounds (Andersson 1999; SOU 1977:99; SOU1992:86). In fact, the ideas explored later by economists go a long way in capturing the trade-off between stimulating achievements for the high performers and the discouraging effects on the weaker students that were at the heart of the Swedish policy debate leading up to the 1970's reform. Now, several decades later, when written report cards are on their way back, the informational aspect of grades is in focus. The main argument for grades is that parents and children need and have a right to know how the child is doing and that oral assessments are not enough.

2.3 A survey of the reform implementation

There are no official records documenting the local implementation of the grading reform. Neither are there any centrally kept records of school grades from this period that could have been used to infer if grades were given. A survey to establish the timing of the grading reform in Swedish municipalities was therefore conducted. All 290 municipalities were contacted and asked to report the years when grades were abolished in lower and middle school. After several follow up rounds, complete answers were obtained answers from 187 municipalities out of 290. Of those that answered, only two provided information which is unlikely to be correct.[3] There is no way of verifying the correctness of the responses received, and given that information in some instances had to be recovered from forgotten archives or in some cases recalled from the memory of old teachers there is bound to be some measurement errors in the survey responses. It is also not certain that schools complied with the grading policy. Measurement errors will make it harder to find effects of the reform.

There is little reason to believe that municipalities that have failed to reply differ systematically from the rest in any dimension which is relevant for this study. A comparison of mean characteristics of non-response to response municipalities, verify this belief. Another source of measurement error is that there is evidence from a few municipalities that grades were abolished in some catchment areas first and then later in the rest of the schools. Because, this is rare and because I have no way of matching children to schools, I have instead chosen to assume that the first abolishment dates or, when possible, the dates relevant for a majority of the schools apply to the whole municipality.

The survey shows that some local school boards acted immediately; the first cohort to be affected by the reform the cohort born in 1957 of which some did not receive grades at the end of elementary school. In the years to come, other municipalities followed suite and about a third of all children in the cohort born 1964 were af-

[3] They have reported that grades were abolished in 1963. It is possible that the response is correct in terms of which cohort that did not get grades, but we have chosen to exclude these municipalities from the analysis.

fected by the reform in one way or another. Some did not get grades at all in elementary school, some got grades only at age 10 and some only at age 13, see *Figure A1* in the Appendix.

Table 1 The implementation of the grading reform

Year of abolishment	End of Lower school grades, 10 year olds				End of elementary school grades, 13 year olds			
	Number of Munici-palities	First affected cohort	%	Cum.	Number of Munici-palities	First affected cohort	%	Cum.
1969	2	1960	1.07	1.07	1	1957	0.53	0.53
1970	3	1961	1.60	2.67	2	1958	1.07	1.60
1971	3	1962	1.60	4.28	2	1959	1.07	2.67
1972	2	1963	1.07	5.35	0	1960	0	2.67
1973	3	1964	1.60	6.95	3	1961	1.60	4.28
1974	4	1965	2.14	9.09	2	1962	1.07	5.35
1975	17	1966	9.09	18.18	7	1963	3.74	9.09
1976	30	1967	16.04	34.22	10	1964	5.35	14.44
1977	44	1968	23.53	57.75	21	1965	11.23	25.67
1978	19	1969	10.16	67.91	18	1966	9.63	35.29
1979	15	1970	8.02	75.94	17	1967	9.09	44.39
1980	14	1971	7.49	83.42	20	1968	10.70	55.08
1981	3	1972	1.60	85.03	10	1969	5.35	60.43
1982	28	1973	14.97	100	74	1970	39.57	100
Total	187		100		187		100	

Source: own data collection.

Municipalities were free to choose how to abolish grades. A majority of the municipalities decided to abolish grades at both at age ten and age thirteen at the same time. Some municipalities, instead, chose to abolish lower school grades several years earlier than the grades in middle school. *Table 1* shows how the reform was implemented. It is clear that the late 1970's was the more active reform period, although some pioneer municipalities implemented the reform already in the early 1970's. While most municipalities had already abolished lower school grades, the implementation of the new grading law in 1982 forced a large number of municipalities to abolish grades at the end of elementary school.

The gradual abolishment of grades provides us with a valuable opportunity to investigate the effects on educational attainment and adult earnings of being graded in school. There are several reasons. First, while the decision to stop giving written grades was delegated to the local municipality school boards, schools were still obliged to follow same national curriculum. Second, because schools were financed over the national budget, there was little scope for local authorities to influence the allocation of resources to schools. Third, there is little reason to be concerned that children (or their parents) selected into schools depending on grading policy since there was no free school choice at the time. Children were automatically enrolled in the school closest to home. Only moving to a different municipality would have made possible such a choice.

There are, however, reasons to be concerned that the local decision to abolish grades was driven by factors that may be correlated with the adult outcomes of the affected children. The abolition of grades was decided by the political majorities of the municipalities, and there was a political divide between parties for and against grades. Most notably, the right wing (Moderaterna) and the center right (Center party) were the parties in favor of maintaining written grades throughout compulsory school. Liberals, social democrats and communists were more of less in favor of abolishing grades (Government proposition 1978/79:180 and Andersson 1999). To the extent that differences in implementation were driven by permanent differences between municipalities, it poses no challenge to identification since such differences are captured in municipal fixed effects. It is however also possible that long run trends in outcomes were different in municipalities that implemented the reform. I will look further into how municipalities that abolished grades at different points in time differ in dimensions that are relevant for long run outcomes of children affected by the grading reform in the next section.

3 Data and methodology

3.1 Register and survey data

In addition to the survey information on grade abolishment just described, the present study uses register based information from Statistics Sweden on individual long run educational outcomes, i.e. educational attainment and earnings, place of birth and family background for the cohorts that were exposed to or that were slightly too old or too young to be exposed to the grading reform. In particular, the sample includes all individuals in the cohorts born 1954–1974. For the second half of the reform period, from 1974 onwards, I also have time varying municipality level information on demographics, expenditure and political majorities.

3.2 Empirical strategy

An ideal set up for studying the long run effects of being graded would be to randomly assign grading policy to schools or municipalities and then compare the long run outcomes of the children that had attended schools with different policies. However, policy implementation is rarely random. The 1982 grading reform was implemented through a series of local political decisions over a period of 12 years, between 1969 when grade abolishment was first allowed and 1982 when schools were no longer allowed to give written grades to children through the seventh year of compulsory school.

The gradual abolishment of grades allows us to compare the difference in outcomes of children of cohorts that are exposed to different grading policies within a particular municipality to the difference in outcomes between children of these same cohorts in municipalities where grading policy remained unchanged.

Identifying the causal effect of being grading using such a difference-in-differences-approach, however rests on a number of assumptions. First, children, or their parents, must not have selected their municipality of residence based on present or future grading policy. A second assumption is that the time trend in outcomes of children that went to school in municipalities that abolished grades at different points in time would have been the same, absent the grading reform, or that the effects of changes in grading policy can be distinguished from systematic differences in municipal time trends in outcomes with other underlying causes than grading policy. A causal interpretation also demands that the timing of any observed effects in a plausible way matches the timing of policy changes.

I make sure that the first of these assumptions is satisfied by letting the child's birth municipality define where the child went to school and assume that the child was subject to the grading policy in that municipality. Given low mobility among Swedish families, residential selection due to grading policy is in any case likely to be a minor issue, but to the extent that parents do select municipality of residence depending on their preferences for grading policy, using the child's birth municipality mitigates the risk of over-estimating effects of grades.

There is one obstacle to implementing this strategy and to assigning the correct grading policy based on birth municipality. During the time period studied, there were several merger waves that reduced the number of municipalities dramatically from over a thousand to today's 290. The survey of grading policy conducted provides information on the dates when today's municipalities abolished grades. Hence, I lack information on smaller municipalities that were absorbed into the larger ones. Because most of today's municipalities did not implement the grading reform until the merging process was over (93 per cent of the sample), I assign the grading policy of the present day municipality for children born in the smaller municipalities that were absorbed. However, I keep track of children that were older than ten when their birth municipality was absorbed since there is a risk that they are assigned the wrong grading policy.

Because of changes in the municipal structure, I do not have access to data on aggregate municipal characteristics for the pre-reform and early reform period. Such data is available from the year 1974 onwards. This limits my ability to control for time varying municipal characteristics that may have co-varied or influenced the implementation of the reform. I do however have information on the parents of children also for the early cohorts. This information is used not only to investigate a presence of heterogeneous effects of grades by family background, but also to control for demographic and social changes that may otherwise confound the results.

Thanks to a data set covering 21 cohorts, and because the reform is so spread out in time, I can, and need to, deal explicitly with the risk of confounding the effects of grading policy changes with other long run trends in outcomes by including municipality specific time trends in a number of ways. I also investigate whether the timing of the effects on outcomes matches the timing of the reform in a plausible way.

While all municipalities were mandated by law to abolish grades for 14 year olds at the same time, in 1982, grades in lower and middle school were not abolished in the same way in all municipalities. Of the municipalities that abolished elementary school grades earlier than 1982, some abolished grades for 10 year olds and 13 year olds at the same time, while others abolished grades for younger children first. This makes it possible, in principal, to separately estimate effects of being graded at these different ages in elementary school. However, because municipalities that graded only at age ten are very few it is in practice hard to distinguish these effects with any precision. I therefore present the effect of *partially* implementing the reform (i.e. abolishing grades at ten or thirteen) and fully implementing the reform. Since grades at age 14 were abolished at the same time everywhere, I am unable to discern the effect of abolishing grades at age 14 from a general cohort effect.

The following basic equation is estimated:

$$y_{ijt}=\alpha+\beta_{pr}\, Partial\ Reform_{jt}+ \beta_{ng}\, No\ Grades_{jt}+ Z_{ijt}+cohort_t + municipality_j + trend_j +\varepsilon_{ijt}, \quad (1)$$

where y_{ijt} is an outcome for individual i going to school in municipality j and member of cohort t, $Partial\ Reform_{jt}$ and $No\ Grades_{jt}$ are indicator variables taking the value one if municipality j had abolished grades in lower school *or* middle school or throughout elementary school for the cohort born in year t. Z_{ijt} is a vector of individual and time varying municipal level controls, $cohort_t$ and $municipality_j$ capture cohort and municipal fixed effects and $trend_j$ captures municipality specific time trends.

$B_{pr,}$ and β_{ng} are the difference-in-differences estimators of the effects of *partially* abolishing grades at age ten or age thirteen, and *fully* abolishing grades throughout Elementary school respectively.

Because the grading reform was implemented at the municipal level, there is no grade variation at the individual level, only across cohorts within a municipality, and across municipalities in a given cohort. To allow for possible serial correlation of the error terms at the municipal level, standard errors are clustered on municipality (Bertrand, Duflo and Mullainathan, 2004).

I control for municipal specific time trends in several ways. Given the length of the period studied, imposing linear trends is rather restrictive. I hence allow also for quadratic time trends. However, including time trends at the municipal level is done at the risk of controlling away effects of the reform, in particular for municipalities that reformed early and where the time trend therefore is estimated largely off post-reform years (see Wolfers, 2006 and Böhlmark and Lindahl, 2007 for discussions of similar concerns). This is particularly problematic if the full reform effect is not immediate, but gradual. Including a municipal time trend will control for average changes in the outcome variable and capture changes both before and after the reform. One way of handling this problem is to control for pre-reform trends rather than an average trend. I construct municipal trends by extrapolating estimated linear pre-reform trends to the post reform years and including the predicted time trends as controls.[4] Another approach is suggested in Wolfers (2006) and aims at explicitly accounting for the dynamics around the reform.

The implementation of the grading reform implied that some of the children that did not get graded, went to school for five years believing they would be graded, others got the news of the reform at an earlier age and some never expected grades at all. Hence, for the cohorts in school around the time of the reform, the reform treatment is not identical and it is possible that it takes a few years before the full reform effect is realized. In order to account for these dynamics, I augment equation (1) with a full set of lags and leads of the reform capturing dynamic effects of the reform before and after the reform. In particular, letting the cohort that was just old enough to be graded in both lower and middle school be the reference cohort, I include a dummy for cohorts 2–8 years older than the first cohort to experience the partial re-

[4] This is also done in Böhlmark and Lindahl (2007). In specifications for groups, I include group specific pre-reform trends.

form. I also include dummies for each of the eight consecutive cohorts that experienced the full reform:

$$y_{ijt}=\alpha +\sum^{8}_{i=2} \beta_{t-i}\ Pre\ reform_{jt-i} + \beta_{pr}\ Partial\ Reform_{jt}+\sum^{8}_{i=0} \beta_{t+i}\ No\ grades_{\ jt+i}\ +Z_{ijt} +cohort_t +municipality_j+trend_j+\varepsilon_{ijt}. \qquad (2)$$

The leads and lags of the reform variables included in this model capture systematic dynamics around the reform years common to all reform municipalities and hence reveal the timing of the reform effects. Estimating the effects of the leads of the reform can be regarded as placebo experiments.

3.3 Variables and measurement

The long run outcomes of interest are years of education and annual earnings, but in order to study effects at different ability margins, I also consider high school graduation and the probability of obtaining a university degree. Because a majority of children graduate from high school, effects on the probability of graduating from high school are interpreted to capture effects in the lower end of the ability distribution, while effects on the probability of attaining a university degree are assumed to capture effects on individuals at the high end of the ability distribution.

Based on register information on the highest educational degree attained by the individual in 2004, i.e. when the first cohorts in the sample were 50 years old and the youngest cohorts were 30, I impute the corresponding years of education required to obtain the degree.[5] Indicators of high school graduation and obtaining a university degree are based on the information on highest degree obtained. At the time the studied cohorts attended high school, the length of vocational and technical programs was two years, while most academic programs were three years. I code all individuals with at least a two year high school diploma as high school graduates. The annual earnings measure used is the average of the individual's registered annual labour earnings for the years 2004 and 2006 from the tax registry.

[5] This measure may underestimate the educational attainment of some members of the youngest cohorts who are still in education. Because this is likely to affect graded and not graded in the same way, it is unlikely to generate biases in the results, but will instead be captured in the cohort fixed effects.

The vector of individual and municipal controls *Z* includes information about the parents of the individual and about the municipality of birth. I construct category variables, taking 6 different values, from less than compulsory education to graduate degrees, for the educational attainment of both parents based on their highest degree attained measured in 2004 for parents younger than 65 in that year. For the older parents information pertaining to the year 1985 is used. I construct a measure taking the value one if the parent is Swedish born and zero if the parent is born elsewhere. The age of each parent is also included.

Z also contains a set of time varying municipal controls. This information is only available from the year 1974 onwards.[6] I include the municipal information on fraction left wing seats in the municipal council, size of the population, fraction of the population below 16 and above 65, and also a measure of local municipal expenditure and a measure of the size of the tax base (sum of taxable income in the municipality) pertaining to the year the individual turns 10, which is the year the children complete lower school and the first time children got grades in the pre-reform regime.

3.4 The exogeneity of the reform

Before I turn to the analysis of the effects of being graded on long run outcomes, I present evidence of how background characteristics and trends in characteristics differ across municipalities with early or late implementation of the grading reform. The municipalities have been categorized according to how early they implemented the grading reform. Pioneers are municipalities that abolished elementary school grades before 1975, reformers are municipalities that abolished grades before they had to, i.e. no later than 1981 but not early enough to qualify as pioneers, and forced reformers are the municipalities that kept grades as long as they could. I also present information for the municipalities that are excluded from the analysis for lack of reform data. This group of municipalities does not differ from the rest in any compromising way.

[6] The reason for poor data availability for the early reform period is a major reform to the municipal structure.

Table 2 Municipal means of individual background characteristics in the pre reform cohorts born 1954–1956 (Panel A), and municipal characteristics in 1974 by reform category (Panel B).

	Pioneers	Reformers	Forced reformers	No grading inform-ation	Total
Panel A					
Mean Proportion with at least one university educated parent	0,09	0,08	0,07	0,08	0,08
	(0,04)	(0,04)	(0,04)	(0,04)	(0,04)
Mean Proportion with Swedish born mother	0,89	0,91	0,93	0,92	0,91
	(0,07)	(0,06)	(0,07)	(0,06)	(0,06)
Mean Mother's age at birth	26,09	26,08	26,22	26,22	26,14
	(0,59)	(0,59)	(0,63)	(0,66)	(0,62)
Mean cohort size	1101	448	295	266	413
	(3231)	(616)	(352)	(222)	(943)
Panel B					
Mean Proportion of municipal council seats held by left wing parties	0,52	0,50	0,44	0,49	0,49
	(0,11)	(0,11)	(0,10)	(0,13)	(0,12)
Mean Per capita tax base. SEK ´1000	0,16	0,15	0,14	0,15	0,15
	(0,03)	(0,02)	(0,02)	(0,03)	(0,03)
Mean Fraction of population below age 16	23,74	22,66	21,84	22,56	22,62
	(3,91)	(3,33)	(2,86)	(3,85)	(3,43)
Mean Population	62703	32773	20467	21048	31295
	(160062)	(47419)	(25780)	(18587)	(58522)

Standard deviation in parenthesis.

Panel A in *Table 2* reveals that the level of education was somewhat higher among parents of children in the pioneer municipalities prior to the reform, 9 per cent compared to 7 or 8 for the other reform types (the difference is significant against the other reform types at the 5 per cent level). Pioneer municipalities also had a slightly higher fraction of foreign born mothers. The large difference is however the size of a cohort. The difference is largely driven by the fact that the capital city Stockholm was among the first municipalities to abolish grade. The second half, Panel B, of the table displays municipal characteristics for 1974, when they are first available. These figures confirm that pioneer and reform municipalities had a higher fraction of left wing seats in the municipal council than did municipalities that were eventually forced to implement the grading reform (the difference is significant at 5 per cent level).

If these differences across municipalities reflect permanent difference, they present no obstacle to the identification of effects of grading since they are captured by municipal fixed effects. If the dependence on municipal characteristics of the decision to implement the grading reform changed over time or if there were trends in these characteristics, e.g. the political power structure, that I for lack of data during the first part of our sample period fail to account for, there is more of a challenge.

In *Table 3*, I estimate a linear probability model of grade abolishment. The dependent variable is a dummy variable taking the value one if grades have been abolished in a municipality at time t and zero otherwise. As explanatory variables I include municipal average family background characteristics of 10 year olds and municipal characteristics at time t. The model is estimated for the latter part of the reform period, when municipal data are available.

The first two columns point to significant average differences across municipalities that have abolished grades in a given year. In particular they show that a higher fraction of low educated parents makes it less likely that a municipality has implemented the grading reform. The opposite is true for high fractions of educated parents. A high fraction of Swedish born fathers is also associated with low likelihood of having implemented the reform. It is also evident that a higher fraction of left wing seats in the local government is associated with a high probability of having implemented the reform in a given year. Comparing the models presented in columns 1 and 2 gives at hand that adding municipal characteristics to the model does not increase our ability to explain the variation in the reform variable although some of the municipal characteristics, notably the share of left wing seats, are significant.

Table 3 OLS estimates of the probability of not being graded

	(1)	(2)	(3)	(4)
Share low Parental Ed	-0.452***	-0.397**	-0.454	-0.375
	(0.148)	(0.156)	(0.327)	(0.329)
share medium low P Ed	-0.009	-0.034	-0.441	-0.488
	(0.126)	(0.129)	(0.320)	(0.322)
share medium Parent Ed	0.678***	0.555***	0.048	-0.030
	(0.187)	(0.209)	(0.388)	(0.392)
share high Parent Ed	0.142	0.144	-0.694*	-0.673*
	(0.207)	(0.219)	(0.374)	(0.378)
share graduate Parent Ed	0.090	0.173	-0.783	-0.675
	(0.234)	(0.241)	(0.491)	(0.494)
Swedish born mother	0.031	0.046	-0.278	-0.247
	(0.210)	(0.210)	(0.367)	(0.368)
Swedish born father	-0.670**	-0.599**	-0.343	-0.393
	(0.262)	(0.270)	(0.383)	(0.383)
Mother birth year	0.050**	0.045**	0.028	0.021
	(0.022)	(0.023)	(0.022)	(0.022)
Mother birth year	-0.010	-0.013	0.002	0.004
	(0.019)	(0.020)	(0.020)	(0.020)
Share leftwing seats		0.219**		0.001
		(0.087)		(0.383)
Taxbase		-0.000		-0.000**
		(0.000)		(0.000)
Municipal expenditure		-0.000		0.000
		(0.000)		(0.000)
Population		0.000*		0.000
		(0.000)		(0.000)
Fraction age0-15		0.020***		0.022**
		(0.006)		(0.010)
Fraction age 65+		0.011**		0.026*
		(0.004)		(0.014)
Time fixed effect	Y	Y	Y	Y
Municipal fixed effect	N	N	Y	Y
Observations	2057	2057	2057	2057
R-squared	0.47	0.48	0.67	0.67

Robust standard errors clustered on municipality in parentheses
* significant at 10%; ** significant at 5%; *** significant at 1%

In Columns 3 and 4, municipal fixed effects are included, and hence the focus is on how *changes* in the explanatory variables within a municipality over time can explain if the reform is implemented. Note that the effect of local politics is picked up by the municipal fixed effect. Although, some municipal characteristics have significant coefficients in column 4, including municipal characteristics have no additional explanatory power for the probability of being graded once municipal fixed effects and individual characteristics are accounted for (i.e. when compared to column 3). This is reassuring for the analysis of grading effects since it suggests that failure to include municipal characteristics during the first part of the sample period when they are not available is not that problematic. I will, however, use all available information and control for municipal characteristics in the analysis of the grading reform for the years when these data are available.

Examination of time trends in background characteristics, also show rather stable trends, or at least similar trends, over time. For instance, the fraction of children with university educated parents presented in *Figure 1* grew in a similar fashion in all the municipal types. However, when considering the proportion of children with a Swedish born mother in *Figure 2*, there is a tendency of a stronger negative trend for the Pioneer municipalities indicating a systematic difference in change in demographic structure over time across the different reform groups.

Figure 1 Trend in the proportion of children with at least one university educated parent. cohorts 1954–1974.

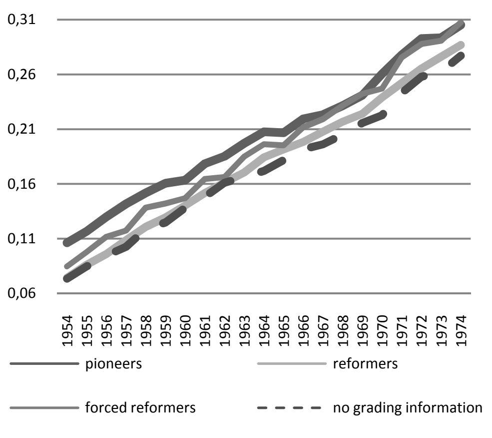
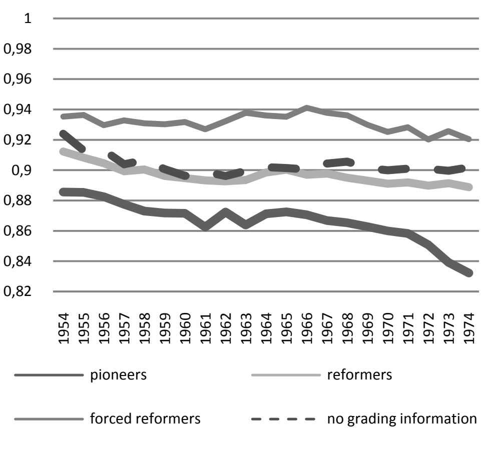


Figure 2 Trend in proportion of children with a swedish born mother 1954–1974 by reform category



4 Results: Long run effects of being graded

4.1 Educational attainment and earnings

I first explore the effect of being graded in lower and middle school on long run educational attainment and adult earning by estimating equation (1) with years of schooling and the log of annual earnings in adulthood as outcome variables, controlling for both municipal and cohort fixed effects. The first results, showing the average effects of abolishing grades on years of schooling and earnings as I introduce controls, i.e. controlling for individual and municipal time varying characteristics, and municipal time trends, are presented in *Table 4* and *5*.

Table 4 shows consistently negative coefficients on the reform variable *No Grades in Elementary School* for all children (top panel), and for boys and girls separately in the mid and bottom panel respectively. The first three columns of *Table 4* suggest that that abolishing grades in elementary school had negative effects on the educational attainment of both boys and girls. The estimated coefficients on the full reform presented in Column (3), which controls for individual and municipal characteristics suggest an effect of -0.04 for boys and -0.066 for girls. This corresponds to 2 and 3.4 weeks respectively, or 1.8 % and 2.6 % of a standard deviation (see Summary statistics in the Appendix 0.04/2.28=1.8 and 0.066/2.27=2.6).

The coefficients on the full reform are stable, at least qualitatively, as controls are introduced. When municipal controls are added the coefficient increases in magnitude for both boys and girls.[7]

The models presented in the last three columns of *Table 4* allow for linear, quadratic and estimated pre reform municipality specific time trends respectively. Adding linear and quadratic trends estimated off the whole sample period effectively reduces the reform coefficients for boys to zero and more than halves the coefficients for all children and for girls. None of the reform effects are now significant and it does not

[7] Although not reported separately, note that the coefficients do not change much when I add a control for children born in the municipalities that were absorbed into present day municipality after they turned ten years old, but that standard errors are reduced. This is reassuring since it suggests that failure to assign the correct grading policy to the children born in absorbed municipalities does not introduce systematic biases in the estimations. I have also allowed for a separate municipal fixed effect for these children. Again, results are not sensitive to this. These results are not presented here.

seem to make a great difference to add quadratic trends, suggesting that municipal trends are in fact linear. However, controlling for an extrapolated time trend estimated off the pre reform years only (Column 6), reduces the magnitudes of the estimates much less. But standard errors increase and again, the reform coefficients are insignificant.

Table 4 Effects of abolishing grades on years of schooling, average effects on all children, boys and girls

	(1)	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**
All						
Partial grade reform	0.004	-0.005	-0.031**	-0.006	-0.001	-0.006
	(0.017)	(0.011)	(0.012)	(0.008)	(0.007)	(0.008)
No grades in	-0.041***	-0.047***	-0.053***	-0.013	-0.006	-0.010
elementary school	(0.013)	(0.010)	(0.020)	(0.011)	(0.012)	(0.011)
Obs	2009604	2009604	2009604	2009604	2009604	2009604
R-squared	0.02	0.14	0.14	0.14	0.14	0.14
Boys						
Partial grade reform	0.010	0.004	-0.022*	0.003	0.006	0.002
	(0.019)	(0.015)	(0.012)	(0.010)	(0.011)	(0.010)
No grades in	-0.032**	-0.038***	-0.040***	-0.002	0.004	0.001
elementary school	(0.013)	(0.011)	(0.015)	(0.016)	(0.016)	(0.016)
Obs	1024068	1024068	1024068	1024068	1024068	1024068
R-squared	0.02	0.14	0.14	0.14	0.14	0.14
Girls						
Partial grade reform	-0.002	-0.015	-0.040**	-0.015	-0.009	-0.014
	(0.017)	(0.010)	(0.016)	(0.011)	(0.010)	(0.011)
No grades in	-0.051***	-0.057***	-0.066**	-0.025	-0.017	-0.022
elementary school	(0.019)	(0.016)	(0.029)	(0.017)	(0.018)	(0.017)
Obs	985536	985536	985536	985536	985536	985536
R-squared	0.03	0.13	0.13	0.13	0.13	0.13
Year and municipal fe	Y	Y	Y	Y	Y	Y
Individual characteristics	N	Y	Y	Y	Y	Y
Municipal time varying char. and control for absorbed municipalities	N	N	Y	Y	Y	Y
Linear mun. time trend	N	N	N	Y	Y	N
Quadratic municipal time trend	N	N	N	N	Y	N
Pre reform time trend	N	N	N	N	N	Y

Robust standard errors clustered on municipality in parentheses
* significant at 10%; ** significant at 5%; *** significant at 1%

These results have two possible interpretations. It is possible that the reform had no effects on outcomes, and that the initial negative coefficients were picking up differential long run municipal trends in outcomes that for some reason correlate with the implementation of the reform. Another possibility is that the long run linear or quadratic time trends pick up some of the reform effects by accounting for average changes in years of education that happened after the reform. The fact that the pre-reform trend reduces the coefficients less suggests that this may be the case.

Before I further investigate the relationship and trace out pre and post reform patterns to make sure that the municipal time trends do not pick up a true reform effect. I present the results for log annual earnings in *Table 5*. Only in the third column is there a weakly significant negative estimate for girls.

Table 5 Effects of abolishing grades on log average annual earnings, on all children, boys and girls

	(1)	(2)	(3)	(4)	(5)	(6)
All						
Partial grade reform	0.010	0.007	-0.003	0.000	-0.000	0.000
	(0.009)	(0.007)	(0.003)	(0.004)	(0.003)	(0.004)
No grades in	0.005	0.005	-0.005	-0.001	-0.002	0.000
elementary school	(0.005)	(0.006)	(0.004)	(0.005)	(0.005)	(0.005)
Obs	1823586	1823586	1823586	1823586	1823586	1823586
R-squared	0.01	0.08	0.08	0.08	0.08	0.08
Boys						
Partial grade reform	0.015	0.012	-0.000	0.003	0.002	0.003
	(0.011)	(0.008)	(0.004)	(0.005)	(0.005)	(0.005)
No grades in	0.010	0.010	-0.001	-0.000	-0.003	-0.000
elementary school	(0.008)	(0.010)	(0.007)	(0.008)	(0.008)	(0.008)
Obs	936154	936154	936154	936154	936154	936154
R-squared	0.01	0.05	0.05	0.05	0.05	0.05
Girls						
Partial grade reform	0.004	0.001	-0.007*	-0.003	-0.003	-0.002
	(0.007)	(0.006)	(0.004)	(0.005)	(0.005)	(0.005)
No grades in	-0.002	-0.001	-0.009	-0.002	-0.003	0.000
elementary school	(0.005)	(0.005)	(0.006)	(0.006)	(0.007)	(0.006)
Obs	887432	887432	887432	887432	887432	887432
R-squared	0.01	0.04	0.04	0.04	0.04	0.04
Year and municipal fe	Y	Y	Y	Y	Y	Y
Individual characteristics	N	y	Y	Y	Y	Y
Municipal time varying characteristics and control for absorbed municipalities.	N	N	Y	Y	Y	Y
Linear municipal time trend	N	N	N	Y	Y	N
Quadratic municipal time trend	N	N	N	N	Y	N
Pre reform time trend	N	N	N	N	N	Y

Robust standard errors clustered on municipality in parentheses
* significant at 10%; ** significant at 5%; *** significant at 1%

4.2 Dynamics and causality

There are reasons to believe that a grading reform, such as the one studied here, affects outcomes gradually, rather than having a discrete impact immediately at the time of the reform. When this is the case, there is a risk that accounting long run time trends takes away a true reform effect.

First, when the grading reform was implemented some of the children that had been graded were still attending school. If the grading reform had a general impact on the learning climate in schools, it is possible that these children were in a sense indirectly affected by grade abolishment. Second, the children that did not get grades as a result of the reform became aware that they would not be graded at different ages. The first children to be affected by the reform went through their first years of school expecting that they would be graded. Younger cohorts, however, did not expect to be graded. It is possible that such differences in expectations matter, and that the full impact of abolishing grades appears after a few years, rather than for the first treated cohort.

Because there are reasons to worry that the time trends included in the above analysis pick up a gradually emerging reform effect. I turn to estimation of Equation 2, which traces out the dynamics of the reform for the cohorts too old to be affected by the partial implementation, and for the cohorts young enough to have been affected by the full reform. I present the results when accounting for linear municipal time trends.[8]

The results are presented in graphical form in *Figure 3* including a 90-per cent confidence interval. The last cohort to receive grades at both ages ten and thirteen is the reference cohort.[9] Whenever the horizontal axis is visible, the results are at least marginally significant. I will indicate in the text if results are significant also at conventional (5%) levels. Tables presenting the full results and more stringent significance levels are available in the appendix.

The top two graphs show the dynamics of the reform effects on years of schooling for boys (left) and girls (right). The bottom panel displays the effects on earnings for boys (left) and girls (right). The results suggest that abolishing grades had no significant

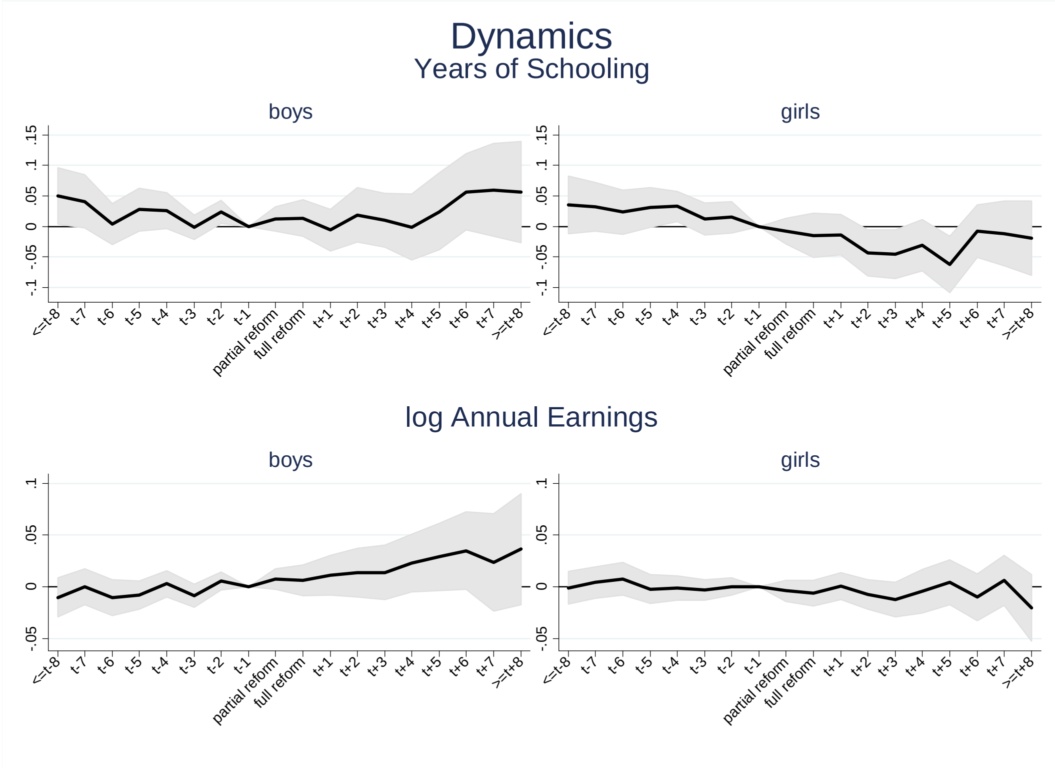
[8] Adding a quadratic time trend has only a minor effect on estimated, but increases standard deviations. See the appendix for tables including results using alternative ways of controlling for municipal time trends.

[9] In interpreting the results, caution is warranted for the early and late cohorts since the panel used for estimation is unbalanced around the year of implementation of the reform. For years distant from the reform year, there is data for only a limited number of municipalities, which also explains why standard errors are larger.

average effects on boys, but that there were negative and (marginally) significant effects on girls' years of schooling. The magnitude of the negative effect on the years of schooling of girls is at most -0.063 of a year at t+5, which is the only coefficient which is significant at the 5%-level. This estimate corresponds to 3.2 weeks or 2.8 % of a standard deviation. There are no significant average effects on log annual earnings of neither boys nor girls.

A causal interpretation of the effect on girls would have been more straightforward if the results had shown zero-effects for the pre reform cohorts and a sharp decline for the cohorts that were first affected by the partial reform, followed by a further decline at the implementation of the full reform, but as discussed above there are reasons for why we should expect reform effects to emerge gradually and for why we may even expect non-zero effects before the reform. A possible interpretation of the positive pre-reform estimates found in the data is that the cohorts that had completed compulsory school by the time the reform was implemented, (t-8 through t-4), did better in terms of educational attainment than the cohorts that were in school at the time of the reform is that the reform had *indirect effects* on these cohorts. I explore this hypothesis further below. Alternatively, although I account for linear municipal time trends, it is still possible that municipalities implemented the reform in a way that for some reason correlated with time trends that I fail to capture.

Figure 3 Dynamic effect of the grading reform on years of schooling and earnings of boys and girls.

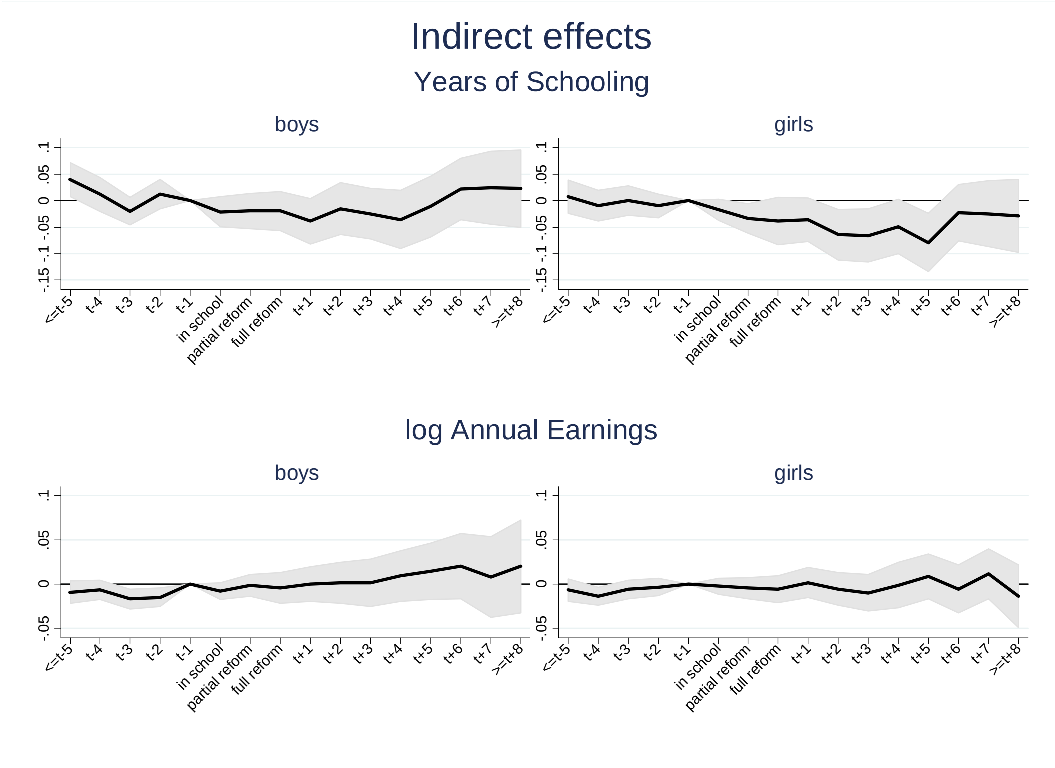


Estimates include controls for cohort and municipal fixed effects, individual characteristics, time varying municipal characteristics, and linear municipal time trends. Shaded area represents 90-percent confidence interval. Standard errors clustered on municipality. The reference cohort, t-1, is the last cohort that was graded both in lower and middle school.

4.3 Indirect effects

I investigate the hypothesis that the positive pre-reform effects found, in particular for girls' years of schooling are the result of indirect reform effects on children who were themselves graded, but who were still attending compulsory school at the time of the reform. I create a specific dummy for these graded "in school"-cohorts and dummies for the cohorts that had left school one to five years before the reform was implemented. Moving too far ahead of the reform is problematic because there are observations only for the late reformers. With this in mind, the results, displayed in *Figure 4*, lend some support for this hypothesis.

Figure 4 Dynamic effect of the grading reform on years of schooling and earnings of boys and girls, accounting for indirect effects on ungraded cohorts in school.



Estimates include controls for cohort and municipal fixed effects, individual characteristics, time varying municipal characteristics, and linear municipal time trends. Shaded area represents 90-percent confidence interval. Standard errors clustered on municipality. The reference cohort, t-1, is the first cohort that had completed compulsory school when grades were first abolished.

Compare the results of estimating the effects of being graded on the years of schooling of girls, the top right graph in *Figure 4*, to the corresponding graph in *Figure 3*. Note that, when a separate effect is estimated for the children that were graded, but still attended school when the reform was implemented, there are zero effects for the cohorts that had graduated from compulsory school at the time of the reform. Moreover, the negative reform effects on girls are now significant at the 5%-level for cohorts t+2, t+3 in addition to t+5.

The downward sloping pre-reform pattern found in *Figure 3*, hence, appears to have been largely due to an indirect negative, but insignificant, reform effect on the in school cohorts. This finding supports a causal interpretation of the negative effect on girls and suggests that the grading reform may have impacted negatively on the learning climate also for girls that were too old to be directly affected. Turning to the boys,

Figure 4 shows that there are negative point estimate of the “in-school”-cohort interaction term and for some of the cohorts affected by the grading reform, although none of the estimates are significant.

It is not possible to say whether this indirect effect on girls is the result of a motivational spill over-effect due to the absence of grades for the younger cohorts, perhaps because teachers down played the importance children should attach to the grades they had got, or if other changes were introduced simultaneously that affected all school girls.

The dynamic pattern for the post-reform cohorts, i.e. estimated reform effects for cohorts t+1 through >=t+8, suggest that the full negative effect of the reform kicked in for the cohorts that knew they would not be graded already some years before they came of age. An interpretation of this result is that the expectation of being graded shielded the interim cohorts from some of the negative effect of not being graded, perhaps because anticipation of being graded stimulated school effort. Another possibility is, again, that the reform induced other changes to the learning climate and that the later cohorts got full exposure to these changes.

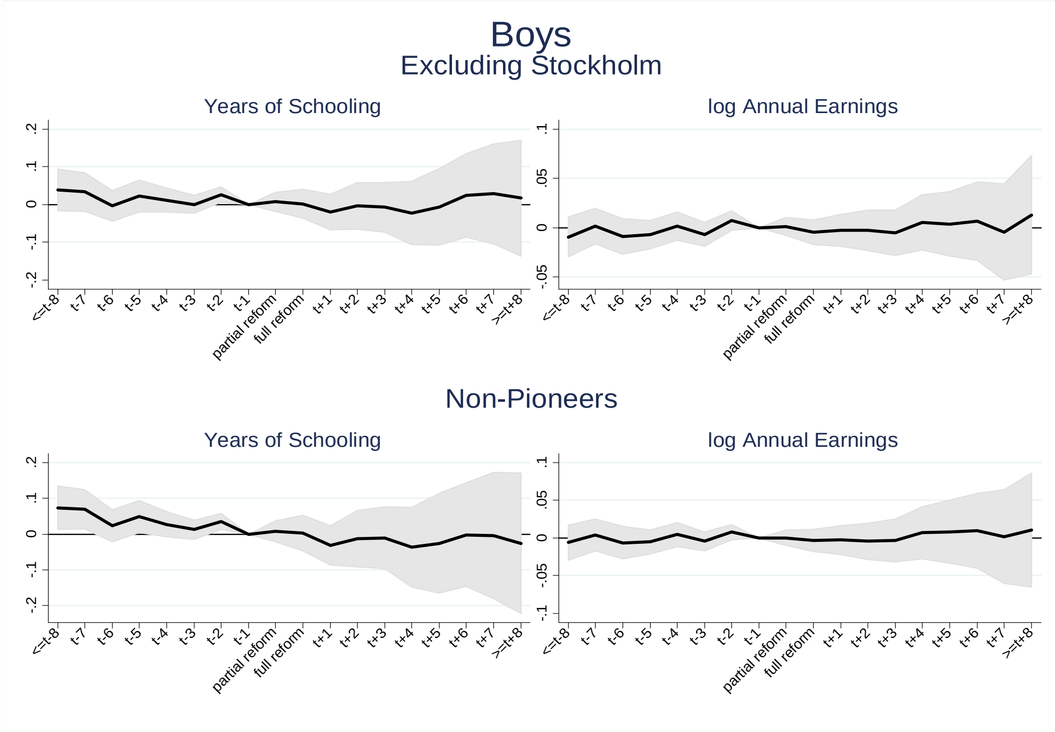
Comparing the outcomes of the girls out of school by the time the reform was implemented (the reference cohort t-1 in *Figure 4*) to those who had not yet started school (t+5), gives a maximum estimate for the total effect of the reform of -0.079. The effect is significant at the 5%-level. This corresponds to a decline in years of schooling with 4 weeks or 3.5 % of a standard deviation.

4.4 Robustness

Before I investigate further if there are signs of heterogeneous effects of grades depending on family background, I investigate the robustness of the pattern found for years of schooling for girls. I investigate the extent to which the results I have presented thus far are driven by Stockholm or by the pioneer reformers that were first at implementing the grading reform. The analysis is presented in *Figure 5* for boys and *Figure 6* for girls.

Just as in the previous analysis, there are no clear effects of the reform on boys. There is, however, a more marked sign of indirect reform effects on boys, when pioneers are excluded from the analysis.

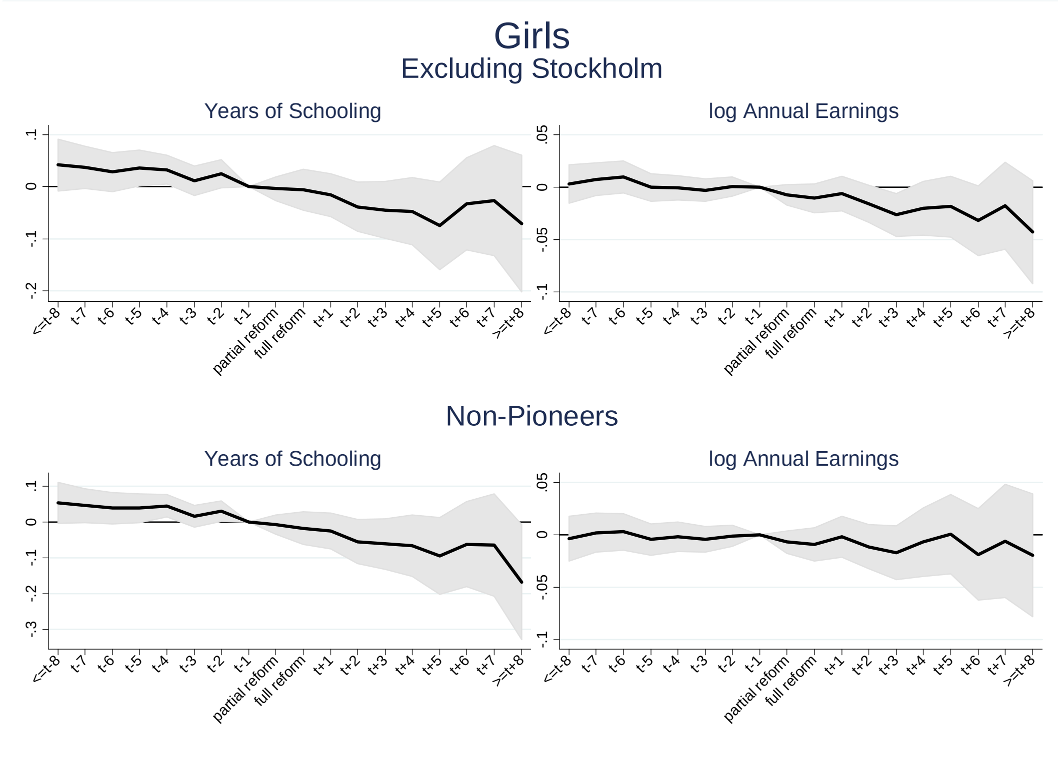
Figure 5 Dynamic effect of the grading reform on years of schooling and earnings of boys excluding Stockholm and Pioneer reformers



Estimates include controls for cohort and municipal fixed effects, individual characteristics, time varying municipal characteristics, and linear municipal time trends. Shaded area represents 90-percent confidence interval. Standard errors clustered on municipality. The reference cohort, t-1, is the last cohort that was graded both in lower and middle school.

When Stockholm or all Pioneer reformers are excluded from the analysis, the estimated effects on girls' years of schooling are no longer significant. However, a comparison of the *Tables A3*, *A4* and *A6* in the appendix reveals that the estimated coefficients are still rather similar to when all municipalities are included, but standard errors are larger rendering estimates insignificant. Note, that there is a negative and significant effect on girls' log annual earnings at cohort t+3 when Stockholm is excluded from the analysis.

Figure 6 Dynamic effect of the grading reform on years of schooling and earnings of girls excluding Stockholm and Pioneer reformers



Estimates include controls for cohort and municipal fixed effects, individual characteristics, time varying municipal characteristics, and linear municipal time trends. Shaded area represents 90-percent confidence interval. Standard errors clustered on municipality. The reference cohort, t-1, is the last cohort that was graded both in lower and middle school.

5 Parental education and the effects of being graded

A reason for abolishing grades was the belief that grades were negative for children from weak educational or social background. Grades were also viewed as stigmatizing for weak students. Economic theory also predicts that we might expect heterogeneous effects. In this section, I investigate differential effects by family background. I also attempt to get at the issue of weaker or stronger students by extending the investigation to include also educational transitions, i.e. the probability of graduation from high school, which is likely to capture effects on weaker students and university graduation, which captures effects on high achievers. On average for the years studied here some 80 per cent of all children graduated from high school, while only 20 per cent got a university degree.

I estimate the effects of the grading reform on years of schooling, adult earnings, high school graduation and university graduation for boys and girls of different parental background, running separate regressions for all groups, controlling for cohort and municipal fixed effects, individual characteristics, time varying municipal characteristics, children born in absorbed municipalities, and including a linear municipal time trends.

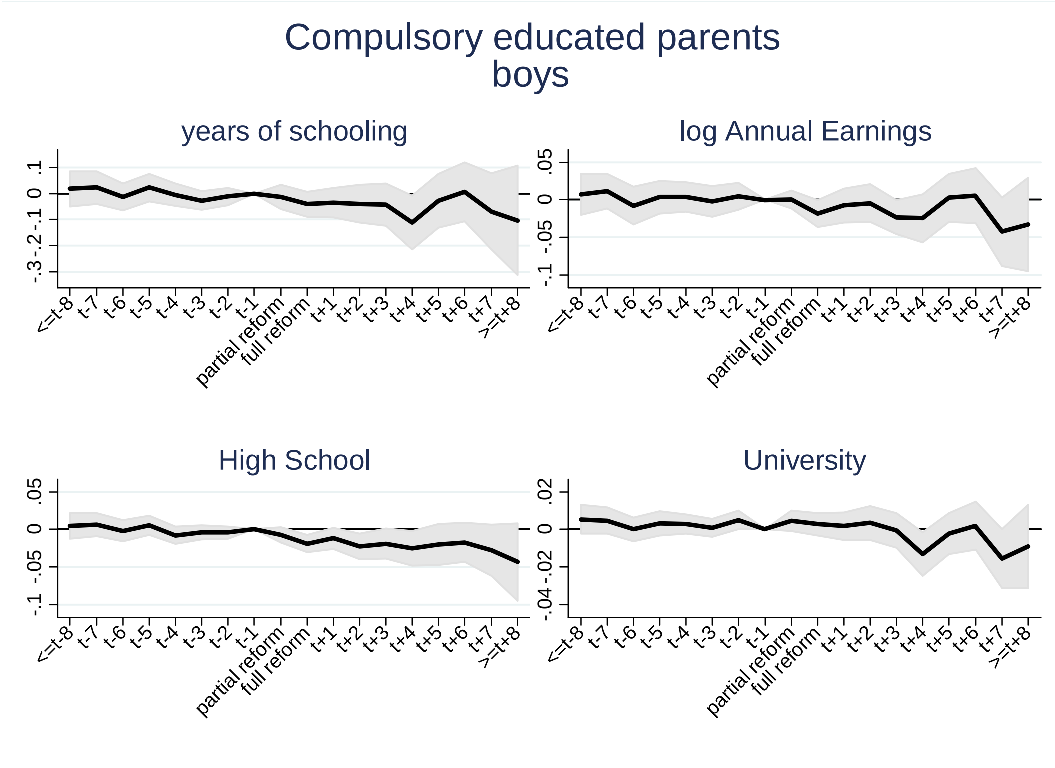
Children are grouped into three categories defined by parental education. The groups are children whose parents have at most compulsory schooling (26 per cent of the sample), high school (38 per cent), and a university degree (19 per cent).[10] The groups also differ in average attainment, where the children with compulsory school parents reach on average 11.4 years of education, three quarters graduate from high school and only 10 per cent get a university degree. The corresponding attainments for children with university educated parents are on average 13.5 years of schooling, 94 percent high school graduates and 40 per cent university graduates. *Table A1* in the appendix contains means and standard deviations of the outcome variables for all groups.

5.1 Children with compulsory school educated parents

I first consider the effects of being graded on the children with compulsory school educated parents. The results are presented in graphical form in *Figures 7* and *8* and in *Table A6* in the appendix. The previous analysis showed no significant general effect on boys. When restricting the analysis sons of parents with compulsory schooling, there is a marginally significant decline in the mean years of schooling at t+4 of -0.11, which corresponds to 6 weeks or 6 per cent of a standard deviation of mean years of schooling for these boys. There is no significant effect on earnings.

[10] Children with missing parental education are excluded from this analysis. The university category includes parents with shorter academic (two year) degrees.

Figure 7 Dynamic effect on sons of compulsory school educated parents

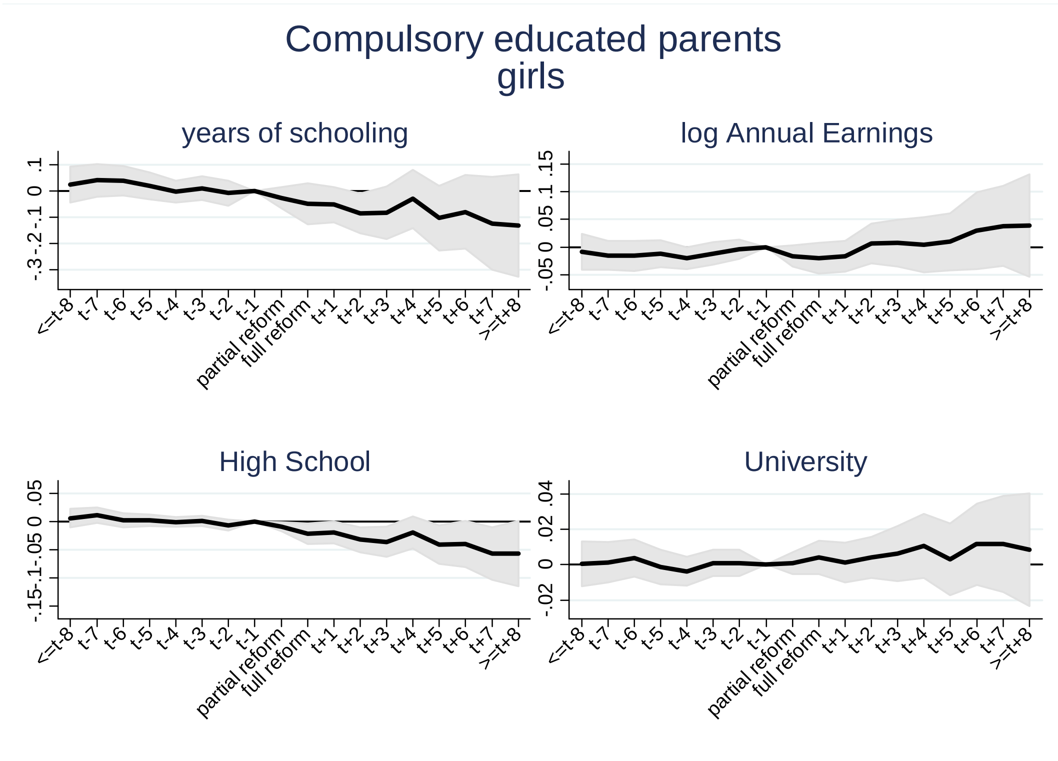


Estimates include controls for cohort and municipal fixed effects, individual characteristics, time varying municipal characteristics, and linear municipal time trends. Shaded area represents 90-percent confidence interval. Standard errors clustered on municipality. The reference cohort, t-1, is the last cohort that was graded both in lower and middle school.

The bottom panel takes a closer look at educational transitions. The results for high school and university graduation suggest that it was boys at the lower end of the achievement distribution that were negatively affected by grade abolishment. The estimated decline in high school graduation probability ranges between 2 and 2.5 percentage points which corresponds to 4–6 percent of a standard deviation. The estimates for cohorts t and t+2 are significant at the 5% level. The bottom right graph suggests that there were negative effects also on university graduation for cohorts that started school by the time grades were already abolished, the marginally significant (at the 10%-level) estimate at t+4 of -0.013 corresponds to a decline of 5 per cent of a standard deviation in the probability of getting a university degree.

Next, consider the effects of being graded on the daughters of parents with compulsory schooling. The results are shown in *Figure 8*. The pattern found for all girls is evident when looking at this category of girls. However, only at t+2 is there a significant estimate (at the 10 percent level). The point estimate of -0.085 corresponds to a 4.4 week decline in schooling, 4.4 of a standard deviation. The estimated effects on earnings are insignificant. The dynamic effects of the reform on high school graduation and university graduation suggest that the negative effect on mean years of schooling was a result of a decline in the probability of graduation from high school, while the girls at the top of the distribution appear to have been unaffected as suggested by the dynamics of university graduation. The effects on high school graduation, range between -0.02 and -0.057 suggesting that the probability of high school graduation declined by 2–6 percentage points, or 3–13 percent of a standard deviation. The estimate for the partial reform and for t, t+2, t+3, t+5 and t+7 are statistically significant at the 5%-level.

Figure 8 Dynamic effect on daughters of compulsory school educated parents

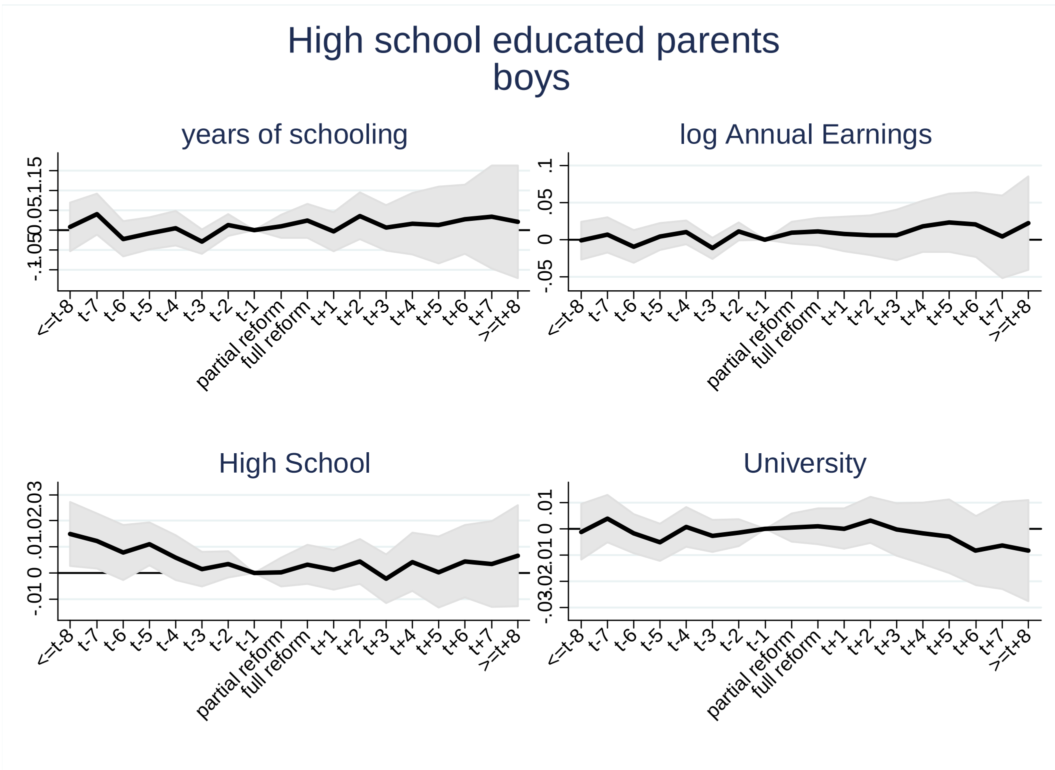


Estimates include controls for cohort and municipal fixed effects, individual characteristics, time varying municipal characteristics, and linear municipal time trends. Shaded area represents 90-percent confidence interval. Standard errors clustered on municipality. The reference cohort, t-1, is the last cohort that was graded both in lower and middle school.

5.2 Children of parents with high school education

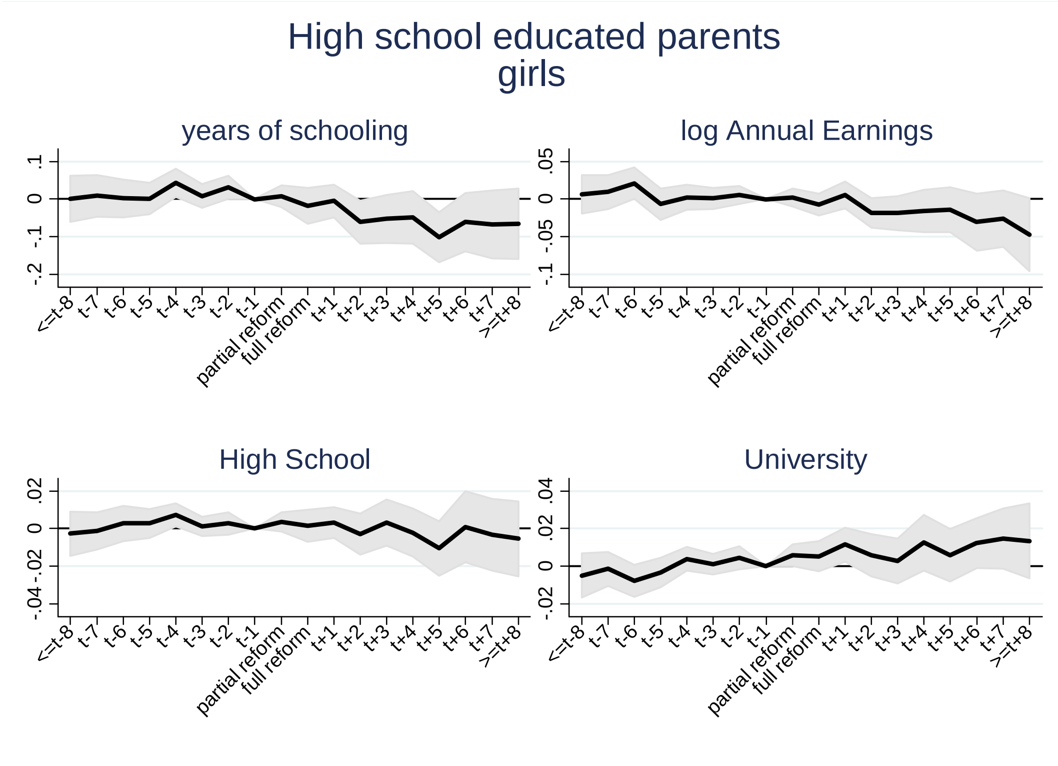
Next, I analyse the effects of grading policy on the children whose parents have high school education. The results for boys and girls are shown in *Figures 9* and *10* and in *Table A7* in the appendix. For boys, the results suggest no direct effects of being graded, but the time pattern of results for high school graduation suggests negative indirect effects of the type discussed in section *4.3* on graded cohorts that had not completed compulsory school when the reform was implemented. For girls there are negative and significant effects on years of schooling at t+2 (at 10%-level) och t+5 (at 5%-level) of -0.06–-0.102 suggesting effects of 3–5 weeks (3-5 per cent of a standard deviation). The evidence is no effect on high school graduation, but there is a positive and significant effect (at 5%-level) on university graduation at t+1. The point estimate of 0.012 corresponds to an increase in the probability of getting a university degree by 3 percent of a standard deviation.

Figure 9 Dynamic effect on sons of High school educated parents



Estimates include controls for cohort and municipal fixed effects, individual characteristics, time varying municipal characteristics, and linear municipal time trends. Shaded area represents 90-percent confidence interval. Standard errors clustered on municipality. The reference cohort, t-1, is the last cohort that was graded both in lower and middle school.

Figure 10 Dynamic effect on daughters of high school educated parents

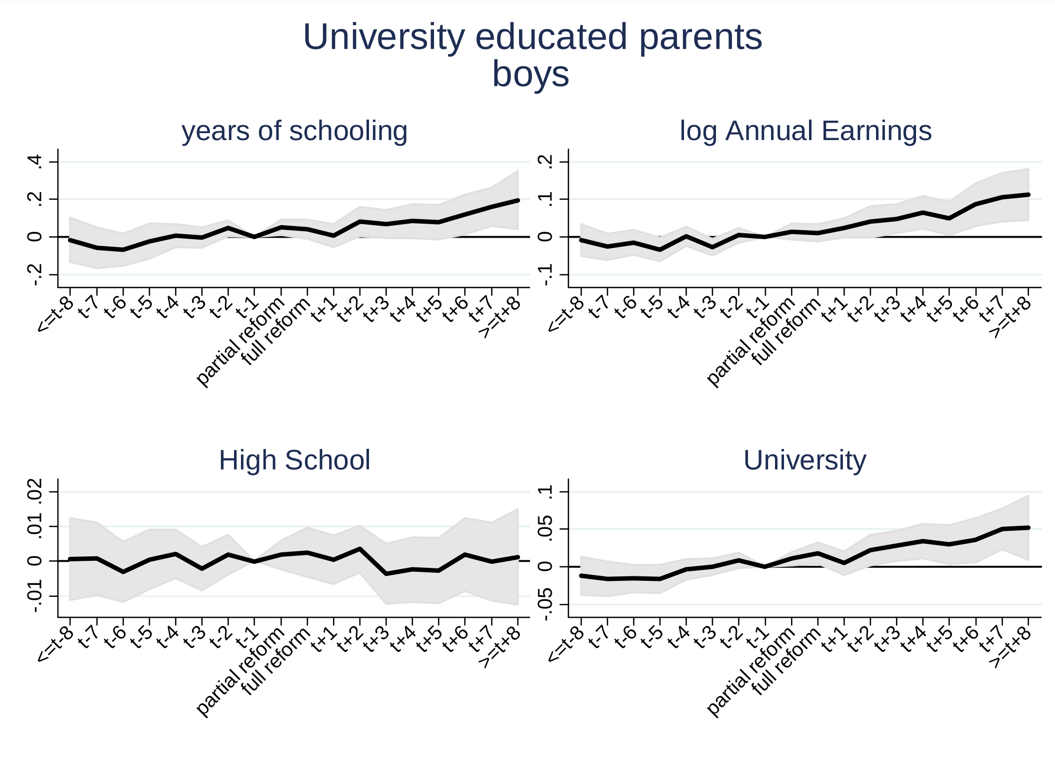


Estimates include controls for cohort and municipal fixed effects, individual characteristics, time varying municipal characteristics, and linear municipal time trends. Shaded area represents 90-percent confidence interval. Standard errors clustered on municipality. The reference cohort, t-1, is the last cohort that was graded both in lower and middle school.

5.3 Children of university graduates

The last set of results concern the children of university graduates. *Figures 11* and *12* and *Table A8* in the appendix present the results. First, sons of university educated parents appear to have benefitted from the grading reform. There are positive and significant effects (at 5%- and 10%-level) on years of schooling, earnings and university graduation, while there is no effect on high school graduation. The positive coefficients of 0.08–0.12 for years of schooling correspond to 4–7 weeks, or an increase in years of schooling of 3.6–5.2 per cent of a standard deviation. The effects on earnings of 0.049–0.087 log points, correspond to an earnings increase of 5-9 percent or 6-10 percent of a standard deviation of log earnings. The probability of graduating from university increased by 1–5 percentage points. This corresponds to an increase of 2–10 per cent of a standard deviation. Hence, for sons of university graduates being graded had negative effects on outcomes at the high end on the ability distribution.

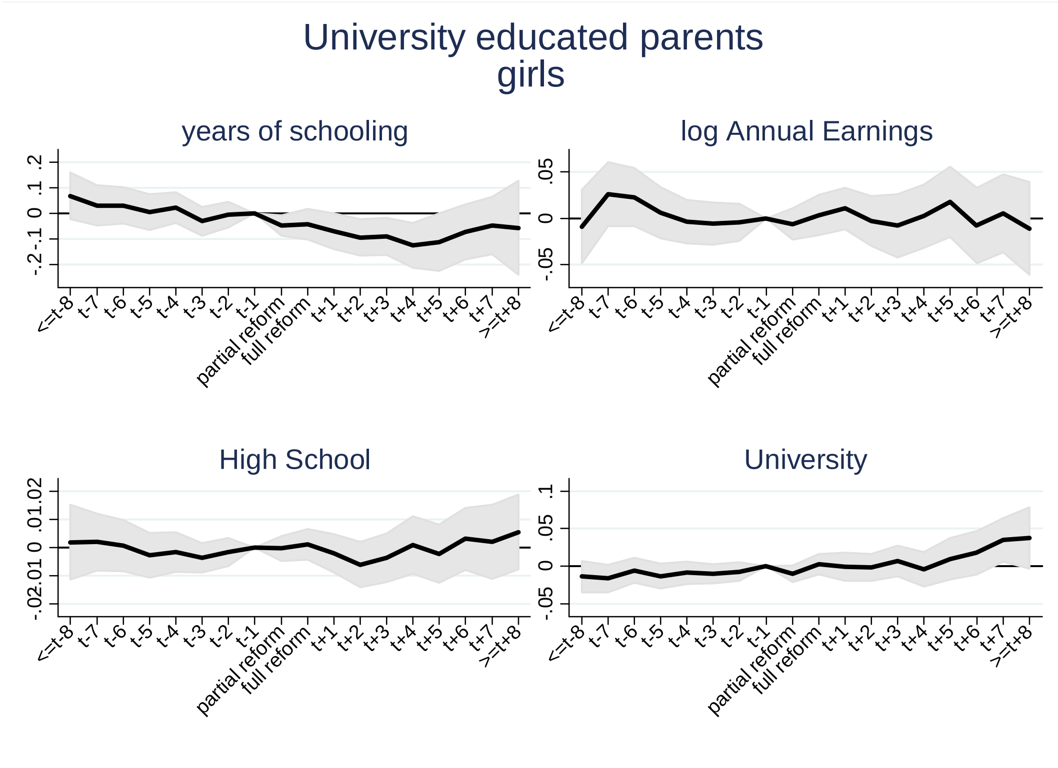
Figure 11 Dynamic effect on sons of university educated parents



Estimates include controls for cohort and municipal fixed effects, individual characteristics, time varying municipal characteristics, and linear municipal time trends. Shaded area represents 90-percent confidence interval. Standard errors clustered on municipality. The reference cohort, t-1, is the last cohort that was graded both in lower and middle school.

Turning to the daughters of university graduates, the pattern is different from that of sons. Years of schooling declined when grades were abolished. The estimated coefficients ranging from -0.049–-0.127 suggest that schooling was shortened by 3–7 weeks or 2–6 percent of a standard deviation. The coefficients for t+2 – t+5 are significant at the 5%-level. There are, however, no effects on earnings, nor any clear effects on high school. For university graduation, there is a positive and significant (at 5%-level) effect at t+7, but I am reluctant do draw too firm conclusions based on estimates for cohorts too distant from the reform. The absence of effects on high school graduation, combined with a negative effect on years of schooling indicates that the negative effects on these girls were concentrated at the middle of the ability distribution.

Figure 12 Dynamic effect on daughters of university educated parents



Estimates include controls for cohort and municipal fixed effects, individual characteristics, time varying municipal characteristics, and linear municipal time trends. Shaded area represents 90-percent confidence interval. Standard errors clustered on municipality. The reference cohort, t-1, is the last cohort that was graded both in lower and middle school.

The analysis of heterogeneous effects of being graded by gender and family background has revealed an interesting pattern. First, girls benefit from being graded regardless of family background. Second, the negative effect of the grading reform on high school graduation of daughters of compulsory educated parents suggest that it is girls at the lower end of the achievement distribution that were affected. Third, the results for boys differ from the results for girls. While low achieving sons of compulsory educated parents were negatively affected by grade abolishment, boys at the other end of the distribution did better when they were not graded.

6 Conclusions

Exploiting time and regional variation when stopped giving children written end of year report cards in Swedish elementary schools during the 1970's. I have explored the effects of early grading on educational attainments, adult earnings and educational transitions.

I find evidence of negative average effects on years of schooling of girls of the reform, suggesting that on average girls benefit from being graded. For boys, I find no average effects. When I estimate separate effects by family background and explore effects on high school and university graduation, I find that being graded increases girls' years of schooling regardless of family background. Moreover, for girls with compulsory educated parents, the effect is driven by the low achievers. The negative effect of abolishing grades on children of poorly educated parents from the lower end of the achievement distribution is present also for boys. At the other end of the distribution of boys, I find that boys with highly educated parents at the top of the achievement distribution did better, both in terms of education and earnings when they were not graded.

The estimates of the negative effects on high school graduation of sons and daughters of parents with compulsory schooling suggest effects in the order of magnitude 2–6 percentage point decline in the probability of graduating from high school or 2–13 percent of a standard deviation for girls and a decline of 2–2.5 percentage points or4–6 percent of a standard deviation for boys. These magnitudes can be compared to the 20 percentage point gap in high school graduation between children with university educated parents and those whose parents had only compulsory schooling during this time period. Another relevant comparison is to the size of gender gap in high school graduation among children with compulsory educated parents. This widened from 3 to 6 percentage points to the favor of girls over the period studied. Absent the reform, the gap would have widened further.

The time pattern of effects supports a causal interpretation of this result, but also suggests that the reform may have had an indirect impact on graded children who were still attending compulsory school when the reform was implemented. Moreover, it appears that children who expected to get grades, but never did, were not as affected by the reform as children who started school when grades were already abolished.

The findings contrast with the motivation for the grading reform. It was argued that grades were potentially harmful for weak, disadvantaged children. I find that being graded benefitted academically weak, socially disadvantaged children. My results are therefore more in line with the recent findings on short run outcomes from smaller scale natural experiments that find positive effects of grades. (Bandeira, Larcinese, and Rasul, 2009 and Azamt and Iriberri, 2009) In addition, unlike the previous studies, however, the present analysis also finds evidence of a discouraging effect of being graded for some boys at the top of the ability distribution.

A possible interpretation of the negative effects on children from weak educational background is that the quantitative information contained on a written report card is particularly important for children whose parents are less confident in or able to extract the relevant information from subtle qualitative oral assessments. It is also possible that the parents of these weak students were more reluctant to show up for the parent teacher conferences. Anecdotal evidence suggests that that may be the case (Andersson, 1999). Since the positive effect of being graded is present for all groups of girls, but not for boys with high school or university educated parents, there is also a gender dimension to the effects of being graded. One interpretation is that girls benefit from being graded because they otherwise underestimate their capacity, while the grades lower the expectations and motivation of the boys at the high end of the ability distribution. A reason may be that grades given to these boys are for some reasons negatively biased, while the girls' grades may be positively biased. Such gender biases in grades have been found in e.g. Grönqvist and Vlachos (2008), Lavy (2008) and Lindahl (2007) but are absent in a recent experimental study by Hinnerich et al (2010).

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Appendix

Figure A1 Proportion municipalities that had implemented the reform, by cohort.

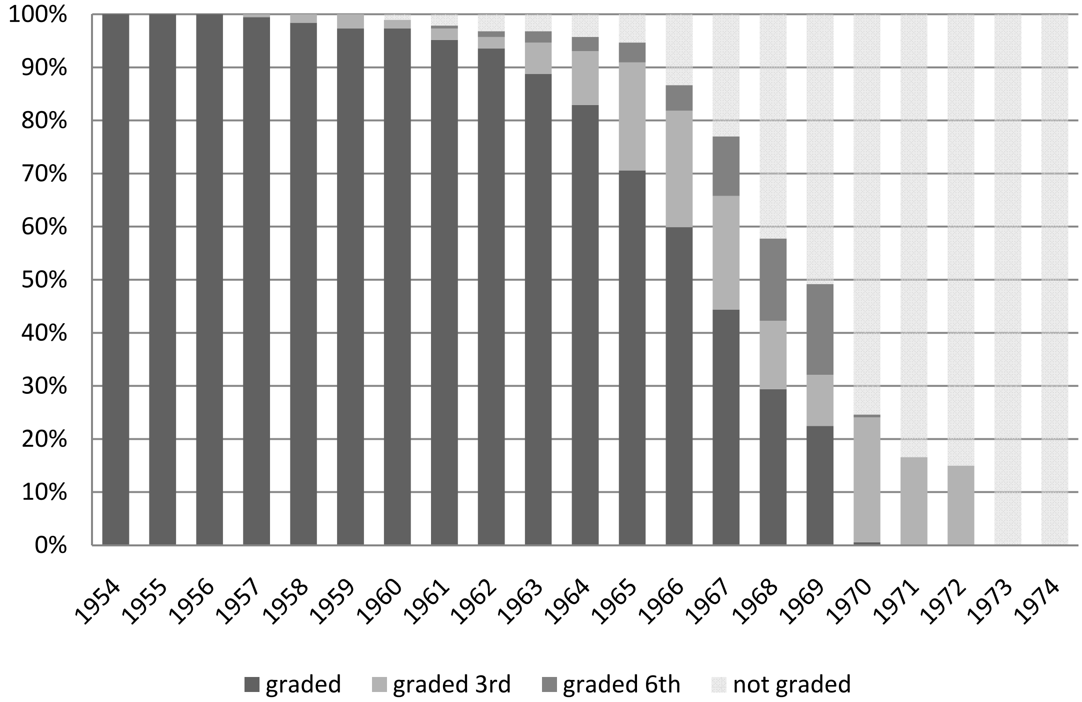

Table A1 Symmary statistics, outcome variables

All

Variable	Obs	Mean	Std. Dev.
Years of schooling	2009604	12.124	2.281
High School graduation	2009604	0.811	0.392
University graduation	2009604	0.197	0.398
Log annual earnings	1823586	7.624	0.893

All

	Boys			Girls		
Variable	Obs	Mean	Std. Dev.	Obs	Mean	Std. Dev.
Years of schooling	1024068	11.966	2.281	985536	12.288	2.269
High School graduation	1024068	0.806	0.395	985536	0.815	0.388
University graduation	1024068	0.176	0.381	985536	0.218	0.413
Log annual earnings	936154	7.780	0.871	887432	7.460	0.887

Parents Compulsory schooling

	Boys			Girls		
Variable	Obs	Mean	Std. Dev.	Obs	Mean	Std. Dev.
Years of schooling	266754	11.193	1.837	254310	11.632	1.936
High School graduation	266754	0.738	0.440	254310	0.763	0.425
University graduation	266754	0.074	0.262	254310	0.112	0.316
Log annual earnings	246060	7.766	0.782	231662	7.453	0.829

Parents High school

	Boys			Girls		
Variable	Obs	Mean	Std. Dev.	Obs	Mean	Std. Dev.
Years of schooling	393487	11.876	1.991	374880	12.282	1.994
High School graduation	393487	0.847	0.360	374880	0.861	0.346
University graduation	393487	0.137	0.344	374880	0.186	0.389
Log annual earnings	369984	7.832	0.780	349355	7.486	0.821

Parents University

	Boys			Girls		
Variable	Obs	Mean	Std. Dev.	Obs	Mean	Std. Dev.
Years of schooling	193617	13.377	2.316	183172	13.667	2.089
High School graduation	193617	0.937	0.243	183172	0.948	0.221
University graduation	193617	0.374	0.484	183172	0.430	0.495
Log annual earnings	184010	7.966	0.849	173324	7.617	0.828

Parents missing education information

	Boys			Girls		
Variable	Obs	Mean	Std. Dev.	Obs	Mean	Std. Dev.
Years of schooling	170210	11.779	2.740	173174	11.804	2.775
High School graduation	170210	0.670	0.470	173174	0.652	0.476
University graduation	170210	0.202	0.402	173174	0.220	0.414
Log annual earnings	136100	7.413	1.137	133091	7.198	1.136

Table A2 Dynamic effects on years of school – controlling for municipal trends

	Girls				boys			
	No trend	linear	Quadr.	Pre ref	No trend	linear	Quadr.	Pre ref
<=t-8	0.037	0.035	0.051	0.027	0.055*	0.050*	0.083**	0.039
	(0.028)	(0.029)	(0.033)	(0.029)	(0.028)	(0.028)	(0.041)	(0.027)
t-7	0.048**	0.032	0.045	0.025	0.062**	0.041	0.072**	0.032
	(0.022)	(0.024)	(0.029)	(0.024)	(0.028)	(0.026)	(0.035)	(0.025)
t-6	0.036*	0.024	0.034	0.018	0.022	0.004	0.031	-0.003
	(0.021)	(0.022)	(0.024)	(0.023)	(0.021)	(0.020)	(0.029)	(0.020)
t-5	0.043**	0.031	0.040*	0.027	0.045**	0.028	0.051*	0.021
	(0.020)	(0.020)	(0.022)	(0.020)	(0.021)	(0.021)	(0.028)	(0.020)
t-4	0.051***	0.033**	0.036**	0.029*	0.047**	0.026	0.044*	0.020
	(0.017)	(0.015)	(0.017)	(0.015)	(0.021)	(0.018)	(0.024)	(0.019)
t-3	0.027	0.012	0.014	0.009	0.016	-0.001	0.010	-0.005
	(0.018)	(0.016)	(0.017)	(0.016)	(0.012)	(0.012)	(0.015)	(0.012)
t-2	0.022	0.015	0.016	0.013	0.033***	0.024**	0.030**	0.022*
	(0.015)	(0.016)	(0.016)	(0.016)	(0.011)	(0.011)	(0.012)	(0.011)
Partial reform	-0.022	-0.008	-0.008	-0.005	-0.005	0.012	-0.000	0.014
	(0.014)	(0.013)	(0.013)	(0.013)	(0.012)	(0.012)	(0.014)	(0.012)
T	-0.038	-0.015	-0.014	-0.009	-0.016	0.014	-0.008	0.020
	(0.028)	(0.022)	(0.023)	(0.023)	(0.017)	(0.018)	(0.022)	(0.017)
t+1	-0.044*	-0.014	-0.013	-0.006	-0.045**	-0.006	-0.035	0.002
	(0.025)	(0.020)	(0.024)	(0.020)	(0.020)	(0.021)	(0.025)	(0.019)
t+2	-0.080**	-0.043*	-0.040	-0.032	-0.030	0.019	-0.019	0.030
	(0.032)	(0.023)	(0.028)	(0.025)	(0.022)	(0.027)	(0.035)	(0.024)
t+3	-0.072**	-0.046*	-0.032	-0.033	-0.032	0.010	-0.038	0.024
	(0.029)	(0.024)	(0.030)	(0.026)	(0.025)	(0.027)	(0.037)	(0.023)
t+4	-0.073**	-0.031	-0.025	-0.015	-0.058*	-0.001	-0.055	0.016
	(0.033)	(0.025)	(0.034)	(0.028)	(0.031)	(0.033)	(0.044)	(0.029)
t+5	-0.086**	-0.063**	-0.035	-0.044	-0.026	0.024	-0.042	0.045
	(0.035)	(0.028)	(0.035)	(0.031)	(0.035)	(0.038)	(0.055)	(0.032)
t+6	-0.040	-0.008	0.026	0.012	-0.002	0.057	-0.028	0.080**
	(0.035)	(0.026)	(0.038)	(0.030)	(0.040)	(0.038)	(0.056)	(0.034)
t+7	-0.049	-0.012	0.046	0.011	-0.004	0.060	-0.029	0.091**
	(0.041)	(0.032)	(0.045)	(0.039)	(0.047)	(0.046)	(0.065)	(0.041)
>=t+8	-0.063	-0.020	0.035	0.009	-0.023	0.056	-0.049	0.092**
	(0.047)	(0.037)	(0.045)	(0.043)	(0.049)	(0.050)	(0.075)	(0.044)
Obs	985536	985536	985536	985536	1024068	1024068	1024068	1024068
R2	0.13	0.13	0.13	0.13	0.14	0.14	0.14	0.14

Estimates include controls for cohort and municipal fixed effects, individual characteristics, time varying municipal characteristics. Standard errors clustered on municipality. The reference cohort, t-1, is the last cohort that was graded both in lower and middle school.
* significant at 10%; ** significant at 5%; *** significant at 1%

Table A3 Dynamic effects on adult earnings – controlling for municipal trends

	Girls				Boys			
	No trend	linear	Quadr.	Pre ref	No trend	Linear	Quadr.	Pre ref
<=t-8	-0.005	-0.001	-0.008	-0.007	-0.001	-0.010	-0.011	-0.006
	(0.008)	(0.010)	(0.015)	(0.010)	(0.010)	(0.011)	(0.016)	(0.011)
t-7	0.004	0.004	-0.002	-0.002	0.008	0.000	0.001	0.003
	(0.008)	(0.009)	(0.014)	(0.010)	(0.010)	(0.011)	(0.014)	(0.010)
t-6	0.007	0.007	0.002	0.003	-0.004	-0.011	-0.010	-0.008
	(0.009)	(0.010)	(0.013)	(0.010)	(0.010)	(0.011)	(0.013)	(0.010)
t-5	-0.001	-0.002	-0.007	-0.006	-0.003	-0.008	-0.008	-0.006
	(0.008)	(0.008)	(0.011)	(0.009)	(0.007)	(0.008)	(0.010)	(0.008)
t-4	0.001	-0.001	-0.004	-0.005	0.007	0.003	0.005	0.004
	(0.008)	(0.007)	(0.009)	(0.007)	(0.008)	(0.008)	(0.010)	(0.008)
t-3	-0.002	-0.003	-0.006	-0.006	-0.006	-0.009	-0.008	-0.008
	(0.006)	(0.006)	(0.007)	(0.006)	(0.006)	(0.007)	(0.008)	(0.007)
t-2	0.001	0.000	-0.001	-0.001	0.007	0.006	0.006	0.006
	(0.005)	(0.005)	(0.006)	(0.005)	(0.005)	(0.005)	(0.006)	(0.005)
Partial reform	-0.005	-0.004	-0.002	-0.002	0.005	0.008	0.006	0.007
	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)
T	-0.009	-0.006	-0.003	-0.001	0.002	0.006	0.002	0.005
	(0.008)	(0.008)	(0.008)	(0.007)	(0.007)	(0.009)	(0.009)	(0.008)
t+1	-0.002	0.001	0.005	0.008	0.005	0.011	0.006	0.009
	(0.007)	(0.008)	(0.010)	(0.007)	(0.009)	(0.012)	(0.011)	(0.011)
t+2	-0.011	-0.007	-0.001	0.002	0.007	0.014	0.007	0.011
	(0.008)	(0.009)	(0.012)	(0.008)	(0.011)	(0.014)	(0.015)	(0.013)
t+3	-0.012	-0.012	-0.004	-0.000	0.008	0.014	0.002	0.011
	(0.009)	(0.010)	(0.014)	(0.009)	(0.012)	(0.016)	(0.016)	(0.014)
t+4	-0.007	-0.004	0.005	0.010	0.015	0.023	0.011	0.019
	(0.010)	(0.013)	(0.018)	(0.011)	(0.012)	(0.017)	(0.019)	(0.015)
t+5	0.007	0.004	0.015	0.022*	0.022	0.029	0.009	0.025
	(0.010)	(0.013)	(0.019)	(0.011)	(0.014)	(0.020)	(0.023)	(0.017)
t+6	-0.007	-0.010	0.001	0.010	0.025	0.035	0.012	0.030
	(0.010)	(0.014)	(0.022)	(0.013)	(0.016)	(0.023)	(0.026)	(0.019)
t+7	0.010	0.006	0.019	0.026**	0.014	0.023	-0.000	0.019
	(0.011)	(0.015)	(0.021)	(0.013)	(0.020)	(0.029)	(0.032)	(0.024)
>=t+8	-0.021	-0.021	-0.008	0.007	0.025	0.036	0.009	0.032
	(0.014)	(0.020)	(0.026)	(0.017)	(0.021)	(0.033)	(0.035)	(0.026)
Obs	887432	887432	887432	887432	936154	936154	936154	936154
R2	0.04	0.04	0.04	0.04	0.05	0.05	0.05	0.05

Estimates include controls for cohort and municipal fixed effects, individual characteristics, time varying municipal characteristics. Standard errors clustered on municipality. The reference cohort, t-1, is the last cohort that was graded both in lower and middle school.
* significant at 10%; ** significant at 5%; *** significant at 1%

Table A4 Dynamic effects on years of schooling and earnings with indirect effects on in–school cohorts controlling for linear municipal trends

	(1)	(2)	(4)	(5)
	Years of Schooling		Log annual earnings trends	
	Boys	Girls	Boys	Girls
<=t-5	0.040**	0.008	-0.009	-0.006
	(0.019)	(0.019)	(0.008)	(0.008)
t-4	0.012	-0.009	-0.006	-0.014**
	(0.019)	(0.018)	(0.007)	(0.006)
t-3	-0.020	0.000	-0.017**	-0.006
	(0.016)	(0.017)	(0.007)	(0.006)
t-2	0.012	-0.010	-0.015**	-0.003
	(0.017)	(0.014)	(0.006)	(0.006)
in school	-0.021	-0.017	-0.007	-0.002
	(0.017)	(0.012)	(0.006)	(0.005)
Partial	-0.020	-0.034**	-0.001	-0.004
reform	(0.020)	(0.017)	(0.007)	(0.007)
T	-0.020	-0.038	-0.004	-0.006
	(0.022)	(0.027)	(0.011)	(0.009)
t+1	-0.039	-0.036	0.000	0.002
	(0.026)	(0.025)	(0.012)	(0.010)
t+2	-0.015	-0.064**	0.002	-0.005
	(0.030)	(0.029)	(0.014)	(0.011)
t+3	-0.025	-0.066**	0.002	-0.010
	(0.029)	(0.031)	(0.016)	(0.013)
t+4	-0.035	-0.049	0.009	-0.001
	(0.033)	(0.031)	(0.017)	(0.016)
t+5	-0.011	-0.079**	0.015	0.009
	(0.035)	(0.034)	(0.019)	(0.015)
t+6	0.022	-0.022	0.021	-0.005
	(0.035)	(0.032)	(0.023)	(0.017)
t+7	0.025	-0.025	0.008	0.012
	(0.042)	(0.038)	(0.028)	(0.017)
>=t+8	0.023	-0.028	0.020	-0.014
	(0.044)	(0.042)	(0.032)	(0.022)
Obs	1024068	985536	936154	887432
R2	0.14	0.13	0.05	0.04

Estimates include controls for cohort and municipal fixed effects, individual characteristics, time varying municipal characteristics, and linear municipal time trends. Standard errors clustered on municipality. The reference cohort, t-1, is the cohort that had just completed compulsory school when grades were abolished in lower and middle school.

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

Table A5 Robustness to excluding Stockholm and pioneer reformers

	Girls				Boys			
	Years of Schooling		Log annual earnings		Years of Schooling		Log annual earnings	
	Excluding Stockholm	Non-pioneers	Excluding Stockholm	Non-pioneers	Excluding Stockholm	Non-pioneers	Excluding Stockholm	Non-pioneers
<=t-8	0.041	0.053	0.003	-0.004	0.038	0.074**	-0.009	-0.006
	(0.030)	(0.035)	(0.011)	(0.013)	(0.034)	(0.037)	(0.012)	(0.014)
t-7	0.037	0.046	0.007	0.002	0.033	0.069**	0.002	0.004
	(0.025)	(0.029)	(0.009)	(0.011)	(0.031)	(0.033)	(0.011)	(0.013)
t-6	0.028	0.038	0.010	0.003	-0.004	0.024	-0.009	-0.006
	(0.023)	(0.027)	(0.009)	(0.011)	(0.024)	(0.027)	(0.011)	(0.013)
t-5	0.036*	0.038	-0.000	-0.005	0.023	0.049*	-0.007	-0.005
	(0.021)	(0.024)	(0.008)	(0.009)	(0.026)	(0.027)	(0.009)	(0.010)
t-4	0.033*	0.045**	-0.001	-0.002	0.012	0.027	0.002	0.005
	(0.017)	(0.019)	(0.007)	(0.009)	(0.019)	(0.021)	(0.009)	(0.010)
t-3	0.011	0.016	-0.003	-0.004	0.000	0.013	-0.007	-0.004
	(0.017)	(0.018)	(0.007)	(0.007)	(0.014)	(0.016)	(0.007)	(0.008)
t-2	0.025	0.030*	0.000	-0.001	0.027**	0.035**	0.007	0.008
	(0.016)	(0.017)	(0.006)	(0.006)	(0.013)	(0.013)	(0.006)	(0.006)
Partial reform	-0.004	-0.007	-0.008	-0.007	0.007	0.008	0.001	0.000
	(0.014)	(0.016)	(0.006)	(0.006)	(0.015)	(0.018)	(0.005)	(0.006)
t	-0.006	-0.018	-0.011	-0.009	0.002	0.003	-0.004	-0.003
	(0.024)	(0.028)	(0.008)	(0.010)	(0.023)	(0.030)	(0.008)	(0.009)
t+1	-0.016	-0.026	-0.006	-0.002	-0.020	-0.031	-0.003	-0.003
	(0.025)	(0.030)	(0.010)	(0.012)	(0.029)	(0.034)	(0.010)	(0.012)
t+2	-0.039	-0.055	-0.016	-0.011	-0.004	-0.013	-0.003	-0.004
	(0.029)	(0.037)	(0.011)	(0.013)	(0.038)	(0.048)	(0.012)	(0.015)
t+3	-0.045	-0.061	-0.027**	-0.017	-0.007	-0.010	-0.005	-0.003
	(0.033)	(0.043)	(0.012)	(0.015)	(0.040)	(0.053)	(0.014)	(0.017)
t+4	-0.047	-0.066	-0.020	-0.007	-0.022	-0.036	0.005	0.007
	(0.039)	(0.052)	(0.015)	(0.020)	(0.051)	(0.068)	(0.017)	(0.021)
t+5	-0.075	-0.095	-0.018	0.000	-0.006	-0.026	0.004	0.008
	(0.051)	(0.065)	(0.018)	(0.023)	(0.062)	(0.085)	(0.020)	(0.025)
t+6	-0.033	-0.062	-0.032	-0.019	0.024	-0.002	0.007	0.009
	(0.054)	(0.072)	(0.020)	(0.027)	(0.067)	(0.088)	(0.024)	(0.030)
t+7	-0.027	-0.065	-0.018	-0.006	0.029	-0.004	-0.004	0.002
	(0.064)	(0.087)	(0.025)	(0.033)	(0.081)	(0.107)	(0.030)	(0.038)
>=t+8	-0.071	-0.168*	-0.043	-0.020	0.017	-0.026	0.013	0.010
	(0.080)	(0.098)	(0.030)	(0.035)	(0.093)	(0.119)	(0.036)	(0.046)
Obs	850558	792146	771146	718527	888493	826975	817686	761277
R2	0.13	0.13	0.04	0.03	0.15	0.15	0.05	0.05

Estimates include controls for cohort and municipal fixed effects, individual characteristics, time varying municipal characteristics, and linear municipal time trends. Standard errors clustered on municipality. The reference cohort, t-1, is the last cohort that was graded both in lower and middle school.
* significant at 10%; ** significant at 5%; *** significant at 1%

Table A6 Compulsory educated parents

	Girls				Boys			
	Years of Schooling	Log Annual Earnings	High school graduation	University	Years of Schooling	Log Annual Earnings	High school graduation	University
<=t-8	0.025	-0.008	0.006	0.001	0.018	0.007	0.005	0.005
	(0.042)	(0.020)	(0.010)	(0.008)	(0.041)	(0.017)	(0.010)	(0.005)
t-7	0.041	-0.015	0.011	0.001	0.023	0.011	0.006	0.005
	(0.038)	(0.016)	(0.008)	(0.007)	(0.038)	(0.014)	(0.009)	(0.004)
t-6	0.039	-0.015	0.002	0.004	-0.013	-0.008	-0.002	-0.000
	(0.034)	(0.016)	(0.008)	(0.006)	(0.031)	(0.015)	(0.008)	(0.004)
t-5	0.020	-0.011	0.001	-0.001	0.023	0.004	0.006	0.003
	(0.031)	(0.015)	(0.006)	(0.006)	(0.032)	(0.013)	(0.008)	(0.004)
t-4	-0.002	-0.020*	-0.001	-0.004	-0.005	0.004	-0.008	0.003
	(0.026)	(0.012)	(0.005)	(0.005)	(0.026)	(0.012)	(0.007)	(0.003)
t-3	0.011	-0.011	0.000	0.001	-0.027	-0.002	-0.004	0.001
	(0.027)	(0.012)	(0.006)	(0.004)	(0.022)	(0.012)	(0.006)	(0.003)
t-2	-0.008	-0.004	-0.007	0.001	-0.011	0.005	-0.004	0.005
	(0.029)	(0.011)	(0.006)	(0.004)	(0.020)	(0.011)	(0.005)	(0.003)
Partial reform	-0.025	-0.016	-0.010**	0.001	-0.013	0.000	-0.007	0.004
	(0.024)	(0.011)	(0.005)	(0.004)	(0.028)	(0.007)	(0.006)	(0.003)
t	-0.048	-0.020	-0.022**	0.004	-0.041	-0.019*	-0.019***	0.003
	(0.047)	(0.016)	(0.011)	(0.006)	(0.029)	(0.011)	(0.007)	(0.004)
t+1	-0.052	-0.016	-0.020*	0.001	-0.035	-0.008	-0.012	0.002
	(0.041)	(0.017)	(0.012)	(0.007)	(0.035)	(0.014)	(0.009)	(0.004)
t+2	-0.085*	0.007	-0.032**	0.004	-0.039	-0.004	-0.023**	0.003
	(0.046)	(0.022)	(0.014)	(0.007)	(0.044)	(0.015)	(0.010)	(0.005)
t+3	-0.083	0.007	-0.036**	0.006	-0.043	-0.023*	-0.019	-0.001
	(0.061)	(0.026)	(0.016)	(0.009)	(0.049)	(0.014)	(0.012)	(0.006)
t+4	-0.030	0.005	-0.020	0.011	-0.112*	-0.024	-0.025*	-0.013*
	(0.068)	(0.030)	(0.017)	(0.011)	(0.062)	(0.020)	(0.014)	(0.007)
t+5	-0.103	0.010	-0.041**	0.003	-0.028	0.003	-0.020	-0.002
	(0.075)	(0.031)	(0.021)	(0.012)	(0.062)	(0.019)	(0.016)	(0.007)
t+6	-0.079	0.030	-0.040	0.012	0.007	0.006	-0.017	0.002
	(0.085)	(0.042)	(0.025)	(0.014)	(0.069)	(0.022)	(0.016)	(0.008)
t+7	-0.124	0.038	-0.057**	0.012	-0.069	-0.042	-0.028	-0.016
	(0.107)	(0.044)	(0.028)	(0.016)	(0.089)	(0.028)	(0.021)	(0.009)
>=t+8	-0.131	0.039	-0.057	0.009	-0.103	-0.033	-0.043	-0.009
	(0.119)	(0.056)	(0.035)	(0.019)	(0.127)	(0.038)	(0.031)	(0.013)
Obs	254310	231662	254310	254310	266754	246060	266754	266754
R2	0.02	0.02	0.03	0.01	0.02	0.02	0.03	0.01

Estimates include controls for cohort and municipal fixed effects, individual characteristics, time varying municipal characteristics, and linear municipal time trends. Standard errors clustered on municipality. The reference cohort, t-1, is the last cohort that was graded both in lower and middle school.
* significant at 10%; ** significant at 5%; *** significant at 1%

Table A7 High School educated parents

	Girls				Boys			
	Years of Schooling	Log Annual Earnings	High school graduation	University	Years of Schooling	Log Annual Earnings	High school graduation	University
<=t-8	0.001	0.006	-0.003	-0.005	0.008	-0.001	0.015**	-0.001
	(0.037)	(0.016)	(0.007)	(0.007)	(0.037)	(0.016)	(0.007)	(0.006)
t-7	0.009	0.009	-0.001	-0.001	0.041	0.006	0.012*	0.004
	(0.033)	(0.014)	(0.006)	(0.005)	(0.032)	(0.015)	(0.006)	(0.006)
t-6	0.002	0.021	0.003	-0.008	-0.022	-0.009	0.008	-0.002
	(0.030)	(0.013)	(0.006)	(0.005)	(0.027)	(0.013)	(0.006)	(0.004)
t-5	0.001	-0.007	0.003	-0.003	-0.007	0.004	0.011**	-0.005
	(0.026)	(0.013)	(0.005)	(0.005)	(0.024)	(0.011)	(0.005)	(0.004)
t-4	0.043*	0.002	0.007**	0.004	0.005	0.010	0.006	0.001
	(0.023)	(0.010)	(0.004)	(0.004)	(0.027)	(0.010)	(0.005)	(0.005)
t-3	0.008	0.001	0.001	0.001	-0.029	-0.012	0.001	-0.003
	(0.019)	(0.008)	(0.003)	(0.003)	(0.018)	(0.009)	(0.004)	(0.004)
t-2	0.031	0.006	0.003	0.004	0.013	0.011	0.003	-0.002
	(0.019)	(0.007)	(0.004)	(0.004)	(0.017)	(0.007)	(0.003)	(0.003)
Partial reform	0.008	0.002	0.004	0.006	0.010	0.009	0.000	0.000
	(0.017)	(0.007)	(0.003)	(0.004)	(0.017)	(0.009)	(0.003)	(0.003)
t	-0.018	-0.007	0.001	0.005	0.024	0.011	0.003	0.001
	(0.029)	(0.009)	(0.005)	(0.005)	(0.026)	(0.011)	(0.005)	(0.004)
t+1	-0.005	0.006	0.003	0.012**	-0.003	0.008	0.001	0.000
	(0.027)	(0.011)	(0.005)	(0.005)	(0.030)	(0.014)	(0.005)	(0.005)
t+2	-0.061*	-0.018	-0.003	0.006	0.036	0.006	0.004	0.003
	(0.035)	(0.012)	(0.007)	(0.007)	(0.036)	(0.016)	(0.005)	(0.005)
t+3	-0.052	-0.019	0.003	0.003	0.006	0.006	-0.002	-0.000
	(0.039)	(0.014)	(0.007)	(0.007)	(0.035)	(0.021)	(0.006)	(0.006)
t+4	-0.048	-0.016	-0.002	0.012	0.017	0.018	0.004	-0.002
	(0.042)	(0.017)	(0.008)	(0.009)	(0.047)	(0.021)	(0.007)	(0.007)
t+5	-0.102**	-0.014	-0.011	0.006	0.013	0.023	0.000	-0.003
	(0.040)	(0.018)	(0.009)	(0.008)	(0.059)	(0.024)	(0.008)	(0.009)
t+6	-0.061	-0.031	0.001	0.012	0.028	0.020	0.004	-0.008
	(0.047)	(0.023)	(0.011)	(0.008)	(0.053)	(0.027)	(0.008)	(0.008)
t+7	-0.067	-0.026	-0.003	0.015	0.034	0.004	0.003	-0.006
	(0.055)	(0.023)	(0.012)	(0.010)	(0.079)	(0.034)	(0.010)	(0.010)
>=t+8	-0.066	-0.047	-0.005	0.013	0.021	0.023	0.007	-0.008
	(0.057)	(0.030)	(0.012)	(0.012)	(0.087)	(0.038)	(0.012)	(0.012)
Obs	374880	349355	374880	374880	393487	369984	393487	393487
R2	0.05	0.02	0.03	0.03	0.06	0.02	0.03	0.03

Estimates include controls for cohort and municipal fixed effects, individual characteristics, time varying municipal characteristics, and linear municipal time trends. Standard errors clustered on municipality. The reference cohort, t-1, is the last cohort that was graded both in lower and middle school.
* significant at 10%; ** significant at 5%; *** significant at 1%

Table A8 University educated parents

	Girls				Boys			
	Years of Schooling	Log Annual Earnings	High school graduation	University	Years of Schooling	Log Annual Earnings	High school graduation	University
<=t-8	0.068	-0.009	0.002	-0.014	-0.015	-0.008	0.001	-0.012
	(0.055)	(0.024)	(0.008)	(0.013)	(0.071)	(0.026)	(0.007)	(0.016)
t-7	0.030	0.026	0.002	-0.016	-0.057	-0.026	0.001	-0.016
	(0.047)	(0.021)	(0.006)	(0.011)	(0.066)	(0.021)	(0.006)	(0.014)
t-6	0.030	0.023	0.001	-0.005	-0.066	-0.014	-0.003	-0.016
	(0.043)	(0.019)	(0.006)	(0.010)	(0.052)	(0.020)	(0.005)	(0.011)
t-5	0.004	0.006	-0.003	-0.013	-0.022	-0.033*	0.001	-0.016
	(0.043)	(0.017)	(0.005)	(0.010)	(0.057)	(0.019)	(0.005)	(0.011)
t-4	0.021	-0.004	-0.002	-0.009	0.009	0.002	0.002	-0.003
	(0.036)	(0.014)	(0.004)	(0.009)	(0.037)	(0.015)	(0.004)	(0.008)
t-3	-0.032	-0.006	-0.004	-0.010	-0.003	-0.027**	-0.002	-0.000
	(0.034)	(0.014)	(0.003)	(0.008)	(0.033)	(0.014)	(0.004)	(0.007)
t-2	-0.006	-0.004	-0.002	-0.007	0.047*	0.005	0.002	0.009
	(0.031)	(0.012)	(0.003)	(0.008)	(0.026)	(0.012)	(0.004)	(0.006)
Partial reform	-0.049*	-0.006	-0.000	-0.010	0.053**	0.014	0.002	0.011**
	(0.025)	(0.010)	(0.003)	(0.007)	(0.025)	(0.013)	(0.003)	(0.005)
t	-0.043	0.003	0.001	0.003	0.042	0.011	0.003	0.018**
	(0.036)	(0.013)	(0.003)	(0.008)	(0.031)	(0.014)	(0.004)	(0.008)
t+1	-0.071*	0.011	-0.002	-0.001	0.007	0.024	0.000	0.005
	(0.043)	(0.014)	(0.004)	(0.011)	(0.037)	(0.015)	(0.004)	(0.010)
t+2	-0.096**	-0.003	-0.006	-0.001	0.083*	0.041	0.003	0.022*
	(0.043)	(0.016)	(0.005)	(0.011)	(0.047)	(0.025)	(0.004)	(0.012)
t+3	-0.091**	-0.008	-0.004	0.007	0.068	0.049**	-0.004	0.028**
	(0.044)	(0.021)	(0.005)	(0.012)	(0.045)	(0.024)	(0.005)	(0.012)
t+4	-0.127**	0.002	0.001	-0.004	0.085	0.065**	-0.002	0.034**
	(0.053)	(0.021)	(0.006)	(0.014)	(0.055)	(0.026)	(0.006)	(0.014)
t+5	-0.115*	0.018	-0.002	0.010	0.080	0.050*	-0.003	0.030*
	(0.068)	(0.023)	(0.006)	(0.017)	(0.056)	(0.027)	(0.006)	(0.016)
t+6	-0.074	-0.008	0.003	0.018	0.120*	0.087**	0.002	0.035**
	(0.065)	(0.025)	(0.007)	(0.017)	(0.065)	(0.035)	(0.006)	(0.018)
t+7	-0.049	0.005	0.002	0.035**	0.162***	0.106***	-0.000	0.050***
	(0.068)	(0.026)	(0.008)	(0.018)	(0.062)	(0.040)	(0.007)	(0.017)
>=t+8	-0.058	-0.011	0.005	0.037	0.196**	0.112***	0.001	0.052**
	(0.112)	(0.030)	(0.008)	(0.025)	(0.094)	(0.041)	(0.008)	(0.026)
Obs	183172	173324	183172	183172	193617	184010	193617	193617
R2	0.08	0.02	0.02	0.06	0.09	0.03	0.02	0.07

Estimates include controls for cohort and municipal fixed effects, individual characteristics, time varying municipal characteristics, and linear municipal time trends. Standard errors clustered on municipality. The reference cohort, t-1, is the last cohort that was graded both in lower and middle school.
* significant at 10%; ** significant at 5%; *** significant at 1%