The long run consequences of being graded in elementary school *

by

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Abstract

Exploiting time variation in when Swedish municipalities abolished school grades in elementary school between 1969 and 1982, a difference-in-difference strategy is used to compare long run educational attainments and earnings of graded and not graded children in the cohorts born 1954-1974. Grades appear to have benefitted the children of university graduates. We find a positive effect on the years of schooling of daughters of university graduates and on the earnings of the sons of university graduates. There is also evidence that being graded had a negative effect on the earnings of sons of less educated parents. The results are robust to high order municipal time trends. Analyses of cohorts that were out of school or too old to be directly affected by the grading reform lend partial support to a causal interpretation.

Keywords: School policy, Grades, Educational Attainment, adult earnings, family background, difference-in-difference

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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) and that the matching of pupils to teachers may matter (Grönqvist and Vlachos, 2008).

Another way in which teachers, schools or school systems may matter is in the way they set grades - or if they set grades at all. Although children in most countries are being graded on their school performance on a regular basis, there is little knowledge about the causal effects of being graded. In absence of scientific evidence, the pros and cons of grades are a subject of debate in some countries, e.g. Sweden where 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 variation, parents' ability to select schools based on their preferred grading policy makes causal inference difficult.

This paper is a first attempt to study the effects of being graded on pupils' long run educational attainment and earnings. To this end we exploit time variation in when Swedish municipalities abolished school grades for school children younger than 14 as a result of the 1982 grading reform. During the decade prior to the reform, municipalities were allowed to replace grades for 10 and 13 year olds by oral assessments, and many municipalities chose to do so. As a result, 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.

When we compare the outcomes of these children we find that being graded or not does matter. In particular, we find that elementary school grades had a positive effect on the years of schooling of girls with low parental education, but that there were no significant effects on adult earnings for this group. We find some negative effects for girls with university educated parents both on education and earnings. For boys, we find no effects on educational attainment. There is however evidence that boys with low parental education suffered from being graded since the grading reform appears to have significantly improved the earnings of this group. Boys with university educated

parents, instead, appear to have benefitted from being graded, since their adult earnings declined as a result of the reform.

Our findings are robust to including linear and non linear municipality specific time trends. In a placebo analysis we also find that the timing of effects supports a causal interpretation of our results, although the grading reform, and or other policy changes around the time of the reform seem to have had an impact on school and educational incentives in general and not only in the treated cohorts.

In economics, grades are seen as rewards. The general finding of the theoretical and empirical literature on grading standards, e.g. Becker and Rosen (1992), Costrell(1994), Betts (1998), Betts and Grogger (2004), and Figlio and Lucas (2004), is that tough grading or graduation standards can have beneficial effects on student performance, but that good students benefit more than poor student, who may even suffer. The proposed mechanism is that tough standards are motivating for those who believe that working harder will pay off in higher grades. Weaker students will instead give up because tougher standards push success out of reach. While economists typically view rewards as positive incentives for effort, educational psychology suggests a number of conditions under which rewards and feedback may either enhance or undermine motivation (Butler, 1987; Deci, Koestner and Ryan, 1999, Cameron et al 2001 and Mueller and Dweck, 1998). Many of these conditions relate to how the informational content in the grade or reward is perceived.

A second channel in which grades may affect children is by providing information. Grades can inform the child and the parents about performance, potential and return to effort. Grades can then potentially improve performance if they raise aspirations and convey that there are high returns to effort. Evaluation in the form of grades may 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), discuss the benefits of being uninformed, and in particular the motivational aspects of self-confidence and over-confidence in the presence of time inconsistency, or present biased preferences.

It is an empirical question to determine if the net long run effects of the mechanisms discussed in the literature are positive or negative and for whom. Although

the present paper does not unravel the actual mechanisms at play, we provide a first attempt to measure the long run consequences of being graded in school. The paper proceeds as follows. In the following section we provide background information on the Swedish school system and in particular of the 1970's grade reform. In section 3 we discuss the empirical strategy and present the data. Results are presented in section 4 which also includes some robustness checks and a placebo analysis. Section 5 concludes.

2 The Swedish school system and 1970's grading reform

2.1 The Swedish School System

Until the 1990s Sweden had a national school curriculum. During the period studied in this paper, curricula were regulated in National Compulsory School Curriculum of 1962 which was replaced by National Compulsory School Curriculum of 1980 in 1982. There were also annual decrees from the National School Authority. 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. As a result local variation in school organization, curricula and resources was limited.

There were nine years of compulsory schooling and children started school in the fall of the calendar year they turned seven. Schools were typically organized such that children would have one and the same teacher in most subject 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. From the 7th year on, children attended upper school where they had different teachers in different subjects. School classes were held together, with some changes as class size increased at the transitions to middle school and from elementary school (lower and middle school) to upper school. 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 written grades were to be given to pupils at the end of lower school, at the end of elementary school and at the end of the seventh years of school. From then on children were to be graded at the end of both fall and spring semesters until they completed compulsory schooling (National Curriculum 1962; SOU 1977:99). Grades were given on 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

The first steps towards the 1982 grading reform were taken already in 1969, when local, municipal school boards, were granted authority by the National School Authority to replace grades though elementary school with oral assessments communicated in end of year parent-teacher conferences.[1] With the "1980 National Compulsory School Curriculum", implemented from the academic year 1982/83, grades through the 7th year of school were abolished by law throughout the country.

At the time, the arguments for abolishing written grades ranged from the idea that grades in elementary school were unnecessary for selection to the idea that grades hamper learning, inspire unhealthy competition, and that grades unduly favored children from educated family background. (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 grades 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.

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. We have therefore conducted a survey to establish the timing of the grading reform in Swedish municipalities. All 290

[1] Swedish National School Authority 1969-1980.

municipalities were contacted and asked to report the years when grades were abolished in lower and middle school. After several follow up rounds, we have obtained complete answers from 187 municipalities out of 290. Of those that answered, only two provided information which is unlikely to be correct.[2] There is little reason to believe that municipalities that have failed to reply differ systematically for the others in any dimension which is relevant for this study. We will nevertheless provide some evidence to this fact. There is some 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 we have no way of matching children to schools, we 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.

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

Municipalities were free to choose how to abolish grades. A majority of the municipalities decided to abolish grades in both 3rd and 6th grade at the same time. In these municipalities, there are cohorts that were graded at age ten, but not at age thirteen. Some municipalities, instead, chose to abolish lower school grades several years earlier than the grades in middle school. As a results there are also in some municipalities, cohorts that were graded in 6th grade, but not in third grade. Table 1 shows how the reform was implemented. Table 1 is based on the responses to a survey conducted in 2005 asking municipalities to report when grades were abolished in lower and middle school. 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 abolished lower school grades by the time grades

[2] 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.

for these children became illegal, the new law forced a large number of municipalities to abolish grades at the end of elementary school.

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 Municipalities	First affected cohort	%	Cum.	Number of Municipalities	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

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. First, while the decision to replace grades with oral assessments was delegated to the local municipality school boards, there were no other major changes made to the national curriculum. The National Compulsory School Curriculum of 1962 was in place

during the whole period affected by the reform. 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 in to schools because of grades. The reason is that 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 nevertheless reasons to be concerned that the local decision to abolish grades was driven by factors and changes that may for various reasons also be correlated with the adult outcomes of the affected children. The abolition of grades was decided by the political majorities of the municipalities. 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 grades throughout compulsory school. The rest of the parties; liberals, social democrats and communists were more of less in favor of abolishing grades (Government proposition 1978/79:180 and Andersson 1999). There also appears to have been a city/country and/or educated/uneducated divide for and against grade abolishment. We will look further into how municipalities that abolished grades at different points in time differ in other dimensions that may have affected the long run outcomes of children affected by the grading reform in the next section when we describe the data used to analyze the effects of the grading reform.

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 particuar, our sample includes all individuals in the cohorts born 1954-1974. For the second half of the reform period, we

also have time varying municipality level information on demographics, expenditure and political majorities.

3.2 Empirical methodology

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, just as with the grading reform studied in the present paper, policy implementation is seldom random. Instead, the 1982 grading reform was, as we have described, implemented through local political decisions during a period of 12 years, between 1969 when grade abolishment was first allowed and 1982 when schools were no longer allowed to grade children through the seventh year of school.

The gradual abolishment of grades prior to the final implementation of grading reform in 1982 allows us to compare the difference in outcomes of children of cohorts that are exposed to different grading policy within a particular municipality to the difference in outcomes between children of these same cohorts in municipalities where grading policy was unchanged. Hence, we can investigate the effect of grading policy, while accounting for any factors affecting outcomes that are constant within a municipality across time and also factors that affect all individuals in a given cohort.

Identifying the causal effect of grading policy using such a differences-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 of future grading policy. A second assumption is that time trends in outcomes of children that went to school in municipalities that abolished grades at different points in time were the same, or that the effects of changes in grading policy can be distinguished from other systematic municipal time trends in outcomes with other underlying causes than grading policy. A causal interpretation also demands that the timing of effects of abolishing grades actually coincides with the timing of policy changes.

Although imperfect, our way of making sure that the first of these assumptions is satisfied is to let 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 that

Swedish families rarely move, 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 grading policy of choice, using the child's birth municipality should mitigate the risk of over-estimating positive effects of grades.

There is one obstacle to implementing this strategy and to assigning the correct grading policy based on birth municipality. During the period we study, 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, we 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), we have chosen to assign the grading policy of the present day municipality for children born in the smaller municipalities that were absorbed. We allow for separate fixed effects for the children born in absorbed municipalities in each municipality.

Because of the reforms to the municipal structure, we 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 our ability to control for time varying municipal characteristics that may have co-varied with the implementation of the reform. We do however have information on the parents of children also for the early cohorts. We use this information not only to investigate a presence of heterogeneous effects of grades, but also to control for demographic and social changes that may otherwise confound the results.

Because our data covers 21 cohorts, and because the reform is so spread out in time, we can 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. As a robustness check we allow for both linear and higher order municipal time trends. We 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 across 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 allows us to separately estimate effects of being graded at these different ages in elementary school, but since grades at age 14 were abolished at the same time everywhere, we are unable to discern the effect of abolishing grades at age 14 from a general cohort effect.

The differences in how grades were abolished also complicate the analysis of timing of reform effects since children of a particular cohort in a particular municipality may have been partially affected by the reform, while later cohorts did not get grades at all in elementary school. Moreover, while a partially affected cohort in one municipality was graded only at age 10, a partially graded cohort in another municipality was graded only at age thirteen. In the placebo analysis, we therefore investigate effects of the grading reform on cohorts born too early to be affected by any of these partial reforms, rather than by complete abolishment of grades in elementary school. We estimate the following basic equation:

$$y_{ijt}=\alpha+\beta_L\ NoGradesLS_{jt}+\ \beta_M\ NoGradesMS_{jt}+\ \beta_E\ NoGradesES_{jt}+\ Z_{ijt}\ +cohort_t\ +municipality_j+trend_j+\varepsilon_{ijt}, \qquad (1)$$

where y_{ijt} is an outcome for individual i going to school in municipality j and member of cohort t, $NoGradesLS_{jt}$, $NoGradesMS_{jt}$ and $NoGradesES_{jt}$ are indicator variables taking the value one if municipality j had abolished grades only in lower school, only in 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. We explore the sensitivity of the results to linear, squared and cubic municipal time trend. B_L, β_M and β_E are the difference-in-difference estimators of the effects of abolishing grades in lower school at age ten, in middle school at age thirteen and throughout elementary school (both lower and middle school) respectively.

Because the grading reform was implemented at the municipal level, we do not have any grade variation at the individual level. Yet we are interested in the possible presence

of heterogenous responses to grades depending on family background and in particular in differential effects for boys and girls. We also need to be able to account for possible demographic and social changes over time. In order to save on estimation capacity, we therefore compute the means for types of individuals, where type is defined by gender, detailed parental education categories, cohort and municipality. We then estimate the model separately for boys and girls using OLS on a data set containing the means of all variables for these individual types, weighting by the number of individuals in each type, rather than estimating on individual data. Throughout the standard errors are clustered on municipality.

3.3 Variables and measurement

The long-run outcomes of interest are years of education and earnings. We also consider high school graduation and the probability of obtaining a university degree.

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, we have imputed the corresponding years of education required to obtain them. Our measure underestimates the educational attainment of members of the youngest cohorts who may still be 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. Our measures of high school graduation and obtaining a university degree are based on the information on highest degree obtained and take value one if the individual has graduated from high school and university respectively.

The annual earnings measure used is the average of the individual's registered annual labor earnings for the years 2004 and 2006 from the tax registry. Also earnings are thus measured at different ages for the different cohorts, for the oldest cohorts at 50 years of age, and for the youngest cohorts at 30. In the estimations we use the log of the individual type's average annual earnings.

The vector of individual and municipal controls Z includes information on the parents of the individual and on the municipality of birth. We construct category variables, taking 8 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 we have used information in 1985. We construct a measure taking the value one if the parent is Swedish born and zero if the parent is born elsewhere. *Z* also includes the individual types' means of maternal age at the individuals birth as well as indicator variables capturing if either parent is missing.

Z also contains a set of time varying municipal controls. However, this information is only available from the year 1974 onwards. We include the municipal information on fraction left wing seats in the municipal council, size of the population, fraction of the population below 16 and a measure of local municipal expenditure pertaining to the year the individual turns 10. This means that we have time varying municipal information for the latter half of our reform period.

3.4 The exogeneity of the reform

Before we turn to the analysis of the effects of being graded on long run outcomes we present evidence of how background characteristics and trends in characteristics differ across municipalities with rapid or slow implementation of the grading reform. We have categorized the municipalities according to how rapidly they implemented the grading reform. Pioneers are municipalities that abolished lower and middle 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. We also present information for the municipalities that are excluded from the analysis for lack of grading data. It should be clear that this group is not different from the others in any compromising way.

Table 2: Individual background characteristics in the pre reform cohorts born 1954-1956, and municipal characteristics in 1974 by municipal category

		Pioneers	Reformers	Forced reformers	No grading information
	Cohort				
Proportion with at least one university educated parent	1954-1956	0.13	0.07	0.07	0.07
Proportion with Swedish born mother	1954-1956	0.88	0.92	0.94	0.90
Mother's age at birth	1954-1956	26.3	25.8	26.2	25.9
Cohort size	1954-1956	13014	255	93	218
	Year				
Proportion of municipal council seats held by left wing parties	1974	0.54	0.55	0.48	0.52
Per capital tax base, SEK ´1000	1974	0.467	0.437	0.410	0.453
Fraction of population below age 16	1974	20.7	20.1	20.2	20.6
Population	1974	103727	44249	29134	26968

Table 2 reveals that the level of education was higher among parents in the pioneer municipalities prior to the reform. Pioneer municipalities also had a slightly higher fraction of foreign born mothers. The big 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 grades. The second half of the table displays municipal averages for the first year in which they are available, 1974. 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.

To the extent that these differences across municipalities reflect permanent difference, they present no challenge to the identification of the effects of grading since they are captured by municipality fixed effects. If the dependence of the decision to implement the grading reform on these underlying municipal characteristics changed over time or if there were trends in these characteristics, e.g. the political power structure, that we for lack of data fail to account for, we have more of a challenge.

Examination of time trends in background characteristics, however show rather stable trends, or at least similar trends, over time. For instance, the fraction of children with university educated parents grew in a similar fashion in all the municipal types.

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

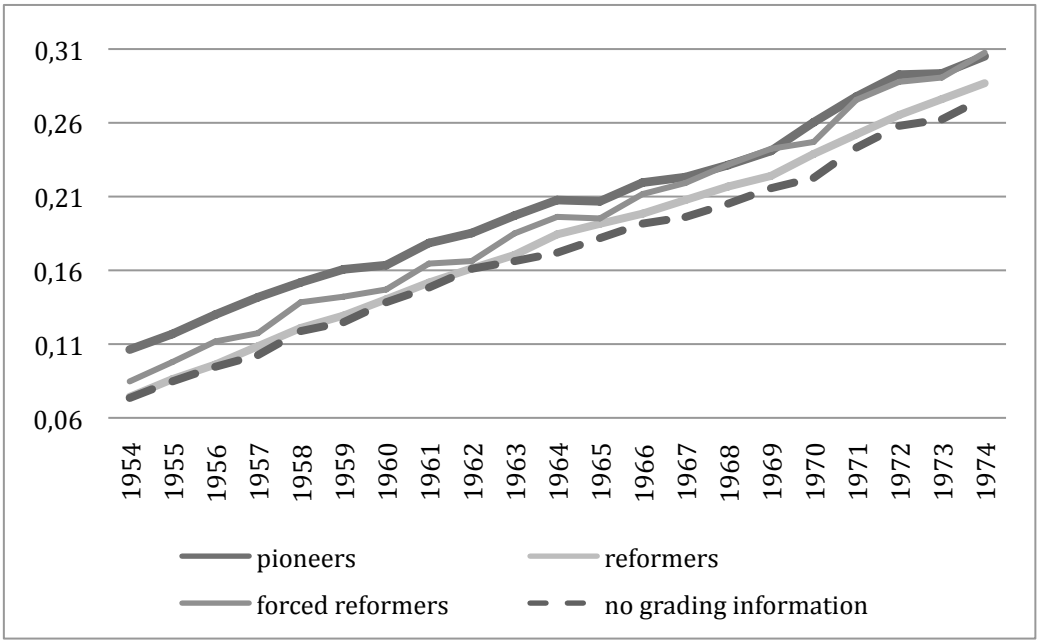


Figure 2 Trend in maternal age of children in cohorts 1954-1974 by reform category

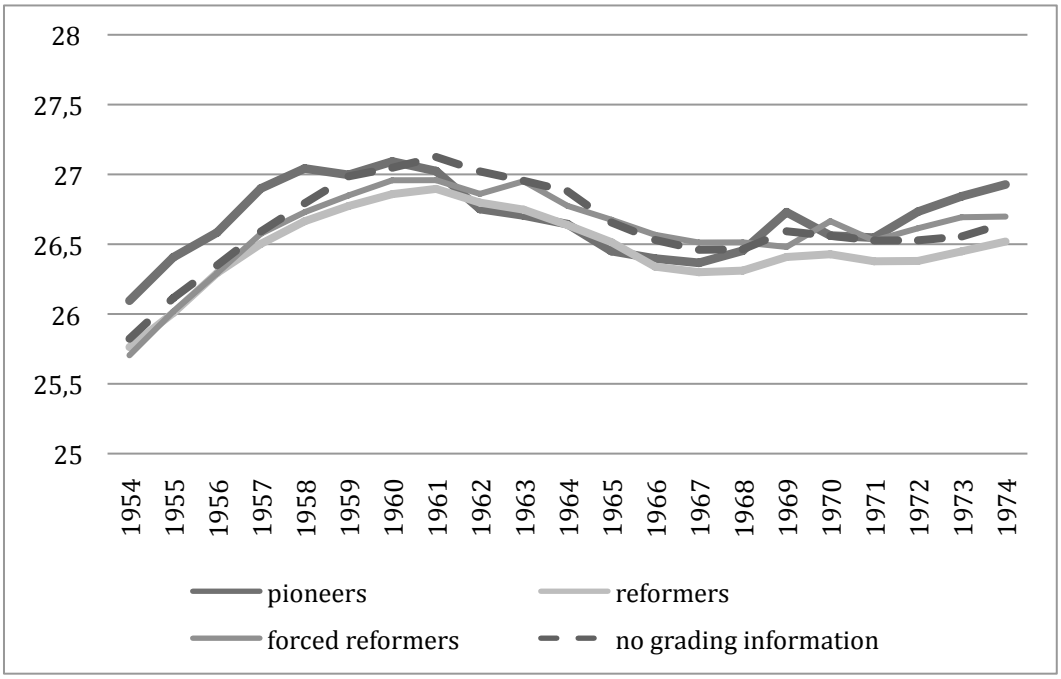
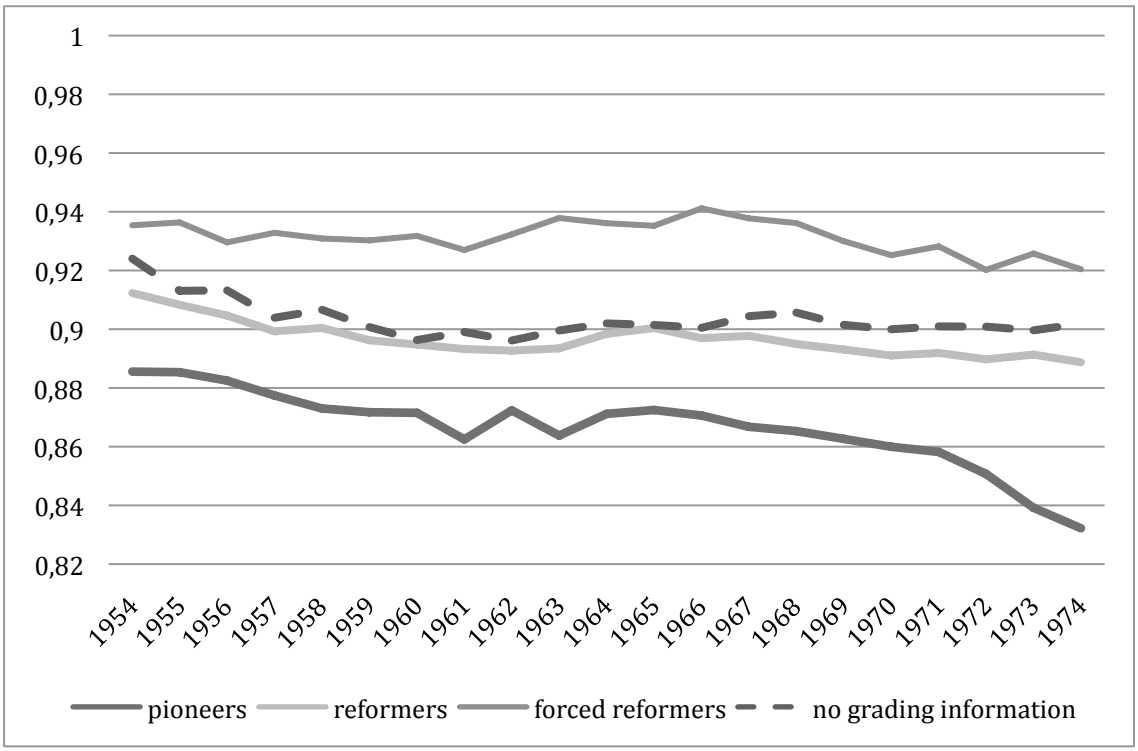


Figure 3 Trend in proportion of children with a swedish born mother 1954-1974 by reform category



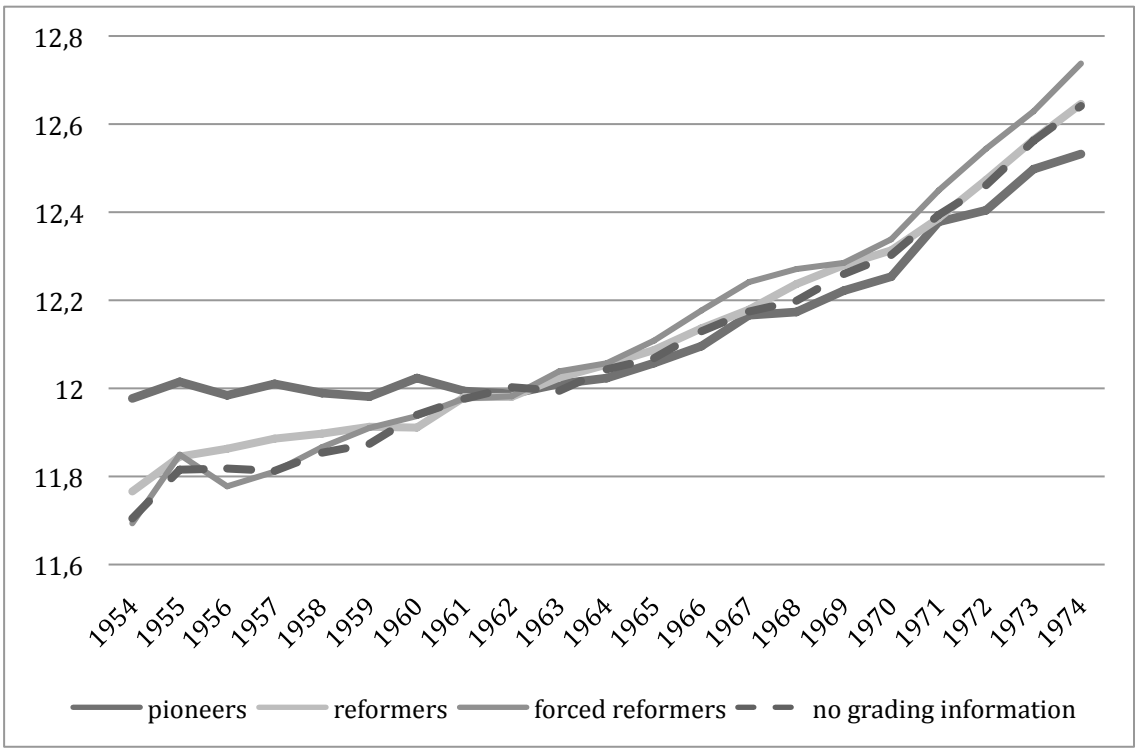
The time trend in maternal age was also similar across municipality types. However, when considering the proportion of children with a Swedish born mother, there is a tendency of a stronger negative trend for the Pioneer municipalities.

3.5 Trends in educational outcome

If we instead turn to the development over time of long run outcomes of children by municipality type, it is evident that there are strong time trends and that the trends look slightly different depending on reform category. In particular, the Pioneer municipalities have a long run development that deviates from the others also for the cohorts that were born too early to be affected by the reform. It is, however, very difficult in these aggregate figures to distinguish shifts that coincide with the timing of the grading reform from these strong time trends. This requires careful econometric analysis. The

presence of strong time trends is motivation for including controls for differential municipal time trends in the analysis.

Figure 4: Mean years of education for cohorts 1954-1974 by reform category



4 Results: Long run effects of being graded

4.1 Educational attainment and earnings

We 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 years of schooling and earnings, as we introduce controls, i.e. controlling for *Z*, and linear time trends, are presented in Tables 3 and 4.

Table 3: Effects of abolishing grades on years of schooling, average effects on girls and boys

	(1)	(2)	(3)	(4)	(5)
Girls					
No grades Lower School	-0.009	-0.007	-0.024	-0.022	-0.005
	(0.018)	(0.016)	(0.021)	(0.021)	(0.019)
No grades Middle School	-0.000	-0.014*	-0.039**	-0.038**	-0.016
	(0.018)	(0.009)	(0.018)	(0.017)	(0.012)
No grades Elementary School	-0.051***	-0.039***	-0.056*	-0.056*	-0.026
	(0.019)	(0.015)	(0.029)	(0.028)	(0.018)
Types	97784	97784	97784	97784	97784
R-squared	0.12	0.59	0.59	0.59	0.59
Boys					
No grades Lower School	0.016	0.017	0.003	0.004	0.015
	(0.025)	(0.023)	(0.026)	(0.025)	(0.020)
No grades Middle School	0.009	0.011	-0.011	-0.012	0.006
	(0.022)	(0.011)	(0.014)	(0.013)	(0.011)
No grades Elementary School	-0.031**	-0.005	-0.018	-0.017	-0.002
	(0.013)	(0.017)	(0.020)	(0.019)	(0.015)
Types	99694	99694	99694	99694	99694
R-squared	0.12	0.59	0.59	0.59	0.59
Cohort and municipal fe	yes	Yes	Yes	yes	Yes
Individual char, municipal x PE fe	no	Yes	Yes	yes	Yes
Municipal char	no	No	Yes	yes	Yes
Linear Education trend	No	No	No	Yes	No
Linear municipal trend	no	No	No	No	Yes

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

Table 3 shows consistently negative coefficients on the treatment variables No Grades in Lower, Middle and Elementary School for girls in the top panel and a mixed pattern for boys in the bottom panel. For girls, the coefficient on abolishing grades in elementary school ranges between -0.039 and -0.056 as individual and municipal controls and a linear pre reform educational time trend are included.[3] This suggests a negative effect of about 2.5 weeks of schooling of abolishing school grades, or about 5

[3] We use data for the pre reform years, 1954-1956, to fit a gender and municipality specific linear time trend in educational attainment and earnings by regressing years of schooling or annual earnings on municipal codes interacted with cohort and *boy*. The trends are then extrapolated for the subsequent years and then included as regressors.

per cent of a standard deviation of girls' average years of schooling for the studied cohorts (see summary statistics in appendix). In the last column, when a linear municipal time trend is included, the magnitude of the effect is reduced by 50 percent and is no longer significant. The bottom panel of Table 3 shows that there were no average effects on the educational attainment of boys from abolishing grades. Only in the first column, which controls only for municipal and cohort fixed effects, is there a significant negative effect of abolishing grades in elementary school. This effect disappears as we introduce controls for personal and municipal characteristics.

In Table 4 we analyze the average effect on annual adult earnings in a similar fashion and find no signs of average effects of abolishing grades on either girls or boys. Tables 3 and 4 indicate that there are no strong average effects of abolishing grades on long run outcomes for neither boys nor girls. However, the sensitivity of the estimated coefficients to the inclusion of controls is possible indication of a presence of heterogenous responses. Hence, before we settle for a conclusion that there were no long run effects of the Swedish grading reform, we investigate if children from different educational background may have responded in different ways. A reason for abolishing grades was precisely the idea that grades were negative for children from weak educational or social background.

Table 4: Effects of abolishing grades on the log of annual earnings, average effects on girls and boys

	(1)	(2)	(3)	(4)	(5)
Girls					
No grades Lower School	-0.000	0.001	-0.004	-0.003	-0.002
	(0.008)	(0.007)	(0.008)	(0.008)	(0.006)
No grades Middle	0.013	0.006	-0.003	-0.003	-0.000
School	(0.011)	(0.006)	(0.004)	(0.004)	(0.005)
No grades Elementary	0.005	0.006	-0.001	-0.001	0.001
School	(0.005)	(0.008)	(0.006)	(0.006)	(0.006)
Types	95146	95146	95146	95146	95146
R-squared	0.12	0.44	0.44	0.44	0.45
Boys					
No grades Lower School	0.009	0.004	-0.002	-0.002	0.001
	(0.007)	(0.007)	(0.006)	(0.006)	(0.005)
No grades Middle	0.012	0.009	-0.003	-0.003	0.001
School	(0.010)	(0.007)	(0.003)	(0.003)	(0.004)
No grades Elementary	0.009	0.011	0.003	0.003	0.005
School	(0.006)	(0.010)	(0.007)	(0.007)	(0.007)
Types	97266	97266	97266	97266	97266
R-squared	0.09	0.45	0.45	0.45	0.45
Cohort and municipal fe	yes	Yes	yes	yes	Yes
Individual char, municipal x PE fe	no	Yes	yes	yes	Yes
Municipal char	no	No	yes	yes	Yes
Linear earnings trend	No	No	no	Yes	No
Linear municipal trend	no	No	no	No	Yes

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

In Tables 5 and 6 we consider differential effects of grades depending on family background for educational attainment and adult earnings respectively. The first two columns in Table 5 display the effects of abolishing grades on girls whose parents do not have university education (Low parental education) and for girls of which at least one parent has a university degree. Column (1) controls for individual and time varying municipal characteristics and column (2) also allows for a linear municipality specific time trend. The negative effect of abolishing elementary school grades altogether is now robust to differential municipal time trends for girls with low parental education. The magnitude of the effect is small in economic terms and corresponds to less than 2

weeks, but represents almost 5 percent of a standard deviation for this group of girls. Also for girls with university educated parents do we find negative effects of abolishing grades that are robust to differential municipal time trends, but only for those that were exposed to the partial reforms. The effect of not getting grades in lower school is -0.103 and the effects of not getting grades in middle school is estimated to be -0.03, corresponding to 5 and less than 2 weeks respectively.

Table 5: Effects of abolishing grades on years of schooling by family background, girls and boys

	(1) Girls	**(2)** Girls	**(3)** Boys	**(4)** Boys
Low parental education				
No grades Lower School	0.002	0.020	0.015	0.026
	(0.023)	(0.021)	(0.027)	(0.020)
No grades Middle School	-0.034*	-0.012	-0.006	0.008
	(0.020)	(0.012)	(0.015)	(0.012)
No grades Elementary School	-0.059**	-0.031*	-0.013	0.001
	(0.025)	(0.017)	(0.021)	(0.018)
University educated parents				
No grades Lower School	-0.128***	-0.103**	-0.044	-0.026
	(0.049)	(0.049)	(0.047)	(0.048)
No grades Middle School	-0.062***	-0.033*	-0.035*	-0.007
	(0.019)	(0.020)	(0.019)	(0.021)
No grades Elementary School	-0.046	-0.013	-0.039	-0.015
	(0.049)	(0.037)	(0.029)	(0.023)
Observations	97784	97784	99694	99694
R-squared	0.59	0.59	0.62	0.62
Cohort and municipal x PE fe, municipal char	yes	Yes	Yes	yes
Municipal time trend	no	Yes	No	yes

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

Columns (3) and (4) display the corresponding results for boys. Although it is interesting to note in column (4) that the estimated coefficients on for boys with low parental education are positive, none of the coefficients are significantly different from

zero. The results suggest that there were no significant effects on the educational attainment of boys as measured by years of schooling from abolishing grades in lower and middle school.

Table 6: Effects of abolishing grades on log of annual earnings by family background, girls and boys

	(1) Girls	**(2)** Girls	**(3)** Boys	**(4)** Boys
Low parental education				
No grades Lower School	0.002	0.003	0.006	0.008
	(0.009)	(0.007)	(0.006)	(0.006)
No grades Middle School	-0.003	-0.001	0.001	0.004
	(0.004)	(0.005)	(0.003)	(0.004)
No grades Elementary School	-0.004	-0.002	0.017**	0.019***
	(0.007)	(0.005)	(0.007)	(0.007)
University educated parents				
No grades Lower School	-0.027***	-0.024**	-0.034***	-0.028***
	(0.008)	(0.010)	(0.012)	(0.010)
No grades Middle School	-0.002	0.003	-0.019**	-0.011
	(0.008)	(0.007)	(0.010)	(0.009)
No grades Elementary School	0.008	0.010	-0.051***	-0.045***
	(0.006)	(0.009)	(0.009)	(0.009)
Observations	95146	95146	97266	97266
R-squared	0.44	0.45	0.45	0.46
Cohort and municipal x PE fe, municipal char	yes	Yes	yes	Yes
Municipal time trend	no	Yes	no	Yes

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

In table 6, we take a closer look at the effects of the grading reform on the natural logarithm of adult earnings. It is interesting to note that the negative effect of abolishing lower school grades on girls with university educated parents is present also on adult earnings. The negative effect of not being graded in lower school corresponds to about 2.4 per cent of earnings, or 8 per cent of a standard deviation of log earnings. Otherwise, the results indicate that girls' adult earnings are not affected by grading

policy during the early school years. For boys the pattern looks different. For boys with low parental education, there is a positive and significant effect on earnings of not being graded throughout elementary school. The effect corresponds to 2 per cent of earnings or 7 per cent of a standard deviation. For the boys with university educated parents, there are negative and significant effects of abolishing grades in lower school, 2.8 per cent, and of abolishing grades altogether in elementary school, 4.5 per cent of earnings.

So far we have found that being graded had no significant effects on the average boy or girl. However, we do find evidence that grades have differential effects depending on family background. In particular, we find that elementary school grades had a positive effect on the years of schooling of girls with low parental education, but that there were no significant effects on adult earnings for this group. We find negative effects of the partial reforms for girls with university educated parents both on education and earnings, suggesting that these girls benefitted from being graded. For boys, we find no effects on educational attainment. There is however evidence that boys with low parental education suffered from being graded. The grading reform appears to have significantly improved the earnings of this group. Boys with university educated parents, instead, seem to have benefitted from being graded, since their adult earnings declined as a result of the reform.

4.2 Accounting for time trends

We argued in sections 3.4 and 3.5 that there are reasons to be concerned that pre-reform trends in e.g. educational attainments were not the same in municipalities that decided to implement the grading reform at different points in time. The results presented thus far account for linear municipality specific time trends, but as a robustness check we also allow for higher order trends, i.e. quadratic and cubic municipality specific time trends. The results, presented in the appendix tables A4-A5, show that the results presented thus far are robust to both quadratic and cubic municipality specific time trends.

4.3 The timing of effects – a placebo analysis

A causal interpretation of the results requires that the effects of the grading reform coincide in a systematic way with its implementation. In particular, we may worry that there were other reforms or changes to the local schools that were implemented at the same time as the grading reform was introduced. We need to ensure that we do not confound the effects of grading with such changes that would have affected not only the cohorts that stopped getting grades. It is also possible that the grading reform itself affected the way teachers behaved and schools functioned in ways that had consequences also for the school children that were not directly affected by the reform.

We perform a placebo analysis by estimating effects of the grading reform on two groups that have previously not been considered in the analysis. First we create a dummy variable taking the value one for the two cohorts in each municipality that had just completed compulsory when the grading reform was implemented in the schools in their municipality. If there were general changes going on locally that were correlated, or driven by the same factors that caused municipalities to implement the reform, these cohorts may have been affected too. If, however, the effects we have found are the causal effects of the grading reform, these cohorts ought not to have been effected. The second group to be investigated consists of those who were still attending school when grades were abolished, but who were too old to be directly affected. If we find effects of the grading reform on this group, a possible interpretation is that the grading reform had more general consequences for the teaching climate.

The results of the placebo analysis are presented in Table 7. The first two columns show the results of a placebo analysis of the reform effects on girls' years of schooling and annual earnings. Column (1) shows that daughters of university graduates who had just completed compulsory school when the grading reform was implemented did poorly relative to previous cohorts. Girls who were still in school and girls who were directly affected by the reform did even worse. The girls that were affected by the partial reform to abolish grades only in lower school did particularly poorly. This evidence points to that there were changes in the conditions for acquiring education that affected not only the cohorts that were directly affected by the reform, but that there was also a direct negative effect of the partial reform.

Column 2 displays the results for annual earnings. The only significant effect is for getting no grades in lower school for the girls with highly educated parents. In the analysis of earnings, there are no signs of effects on the cohorts going to school just before the reform.

Columns 3 and 4 show the corresponding results for boys. In table 4, we found no significant effects on boys' educational attainment. When the reference group consists of cohorts that finished compulsory school long before the reform, as it does in the present table, we find negative and significant effects on sons of university graduates. There is however little difference between the cohorts that were directly affected and those that were still in school when the reform was implemented. Again, as for the girls, there are signs that there were changes to the conditions for acquiring education that affected children that went to school before and during the reform. For boys, there is no evidence of a direct effect of being graded on educational attainment. In column 4, we display the results for boys' annual earnings. The positive effect on earnings for boys with low parental education still shows up, but it is also clear that boys with low parental education in cohorts going to school just before the reform, did particularly poorly compared to older cohorts. For the sons of university graduates, the reform effect is more straightforward. There appears to be a small negative effect of grade abolishment on the cohorts that were still in school, but a stronger negative effect on the children that were not graded at all in elementary school and on those that were not graded in lower school.

Next question to ask is if we can interpret the sum of these results as evidence for or against a causal interpretation of the estimated effects of the grading reform. The placebo analysis does not deliver a clear answer, but suggests the following. First, the grading reform, or changes that lead to or were simultaneous with the reform, appear to have affected the conditions for making schooling decisions also for cohorts that were not directly affected by the reform. Yet, it appears that the reform effects are strongest for the directly affected cohorts, which supports a causal interpretation at least for girls with university educated parents. The evidence for annual earnings suggests that abolishing grades had a negative effect on the earnings of the sons of university graduates, and possibly a positive effect on the sons of less educated parents.

Table 7: Placebo analysis of the effects of school grades..

	(1) Years of Schooling, girls	**(2)** Annual earnings, Girls	**(3)** Years of Schooling, Boys	**(4)** Annual earnings, Boys
Low parental education				
Cohorts out of School	-0.001	-0.005	-0.005	-0.013***
	(0.012)	(0.004)	(0.012)	(0.004)
Cohorts in School	0.020	-0.003	0.006	-0.006
	(0.016)	(0.005)	(0.022)	(0.006)
No grades Lower School	0.033	-0.001	0.025	-0.001
	(0.030)	(0.010)	(0.031)	(0.008)
No grades Middle School	-0.002	-0.004	0.006	-0.004
	(0.022)	(0.007)	(0.022)	(0.006)
No grades Elementary School	-0.019	-0.006	-0.002	0.010*
	(0.029)	(0.008)	(0.022)	(0.006)
University educated parents				
Cohorts out of School	-0.069***	0.005	-0.043	0.008
	(0.023)	(0.007)	(0.029)	(0.009)
Cohorts in School	-0.141***	-0.005	-0.117***	-0.022**
	(0.026)	(0.009)	(0.027)	(0.009)
No grades Lower School	-0.215***	-0.029**	-0.121**	-0.046***
	(0.060)	(0.013)	(0.059)	(0.014)
No grades Middle School	-0.131***	-0.001	-0.090**	-0.026**
	(0.037)	(0.012)	(0.037)	(0.011)
No grades Elementary School	-0.118**	0.006	-0.105***	-0.062***
	(0.054)	(0.013)	(0.040)	(0.011)
Observations	97784	95146	99694	97266
R-squared	0.59	0.45	0.62	0.46
Cohort and municipal x PE fe, municipal char	Yes	yes	Yes	Yes
Municipal time trend	Yes	yes	Yes	yes

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

4.4 Grades and Educational transitions

We have seen that being graded matters for the educational attainment of girls, but that years of schooling for boys appear to be unaffected. This section investigates at which educational transitions being graded matters. We estimate linear probability

models of the probability of graduating from high school and of getting a university degree. In the analysis we account for a linear municipal time trends.

Table 8: The effects of being graded on Educational transitions

	(1) Girls High school	**(2)** Girls University	**(3)** Boys High school	**(4)** Boys University
Low parental education				
No grades Lower School	0.004	0.003	0.005	0.004
	(0.004)	(0.003)	(0.004)	(0.004)
No grades Middle School	-0.001	-0.002	-0.001	0.002
	(0.003)	(0.002)	(0.002)	(0.002)
No grades Elementary School	-0.003	-0.008	-0.004	-0.001
	(0.003)	(0.005)	(0.003)	(0.004)
University educated parents				
No grades Lower School	-0.013**	-0.016	-0.014***	-0.002
	(0.006)	(0.011)	(0.005)	(0.010)
No grades Middle School	-0.010***	0.007	-0.004	0.004
	(0.002)	(0.006)	(0.003)	(0.005)
No grades Elementary School	-0.015***	0.034***	0.000	0.014**
	(0.004)	(0.007)	(0.004)	(0.006)
Observations	98064	98064	99985	99985
R-squared	0.50	0.49	0.45	0.53
Cohort and municipal x PE fe, municipal char	Yes	Yes	Yes	Yes
Municipal time trend	Yes	Yes	Yes	Yes

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

The results are presented in Table 8. The first two columns display the results for girls' high school graduation and university. We find a significant negative effect of grade abolishment on the probability of graduating from high school for daughters of university graduates, but that the probability of getting a university education for this group was positively affected by the grading reform. The results for boys point in the

same direction qualitatively. This result suggests that grading policy may affect individuals at different margin in different ways.

5 Conclusions

Exploiting time and regional variation when written elementary school grades where replaced by oral assessments in Sweden during the 1970's we have explored the effects of early grading on educational attainments, adult earnings and educational transitions. Comparing the outcomes of individuals who were graded or not depending on when and where they went to school, our differencec-in-differeces estimates show that being graded matters, although somewhat differently for boys and girls and depending on family background as measured by parental education.

First, we find no average effects of the grading reform on the years of schooling and annual earnings of boys and girls. It is when we analyse effects by family background that a pattern emerges. In particular, we find that elementary school grades had a positive effect on the years of schooling of girls with low parental education, but that there were no significant effects on adult earnings for this group. We find negative effects of the partial reforms for girls with university educated parents both on education and earnings. For boys, we find no effects on educational attainment. There is however evidence that boys with low parental education suffered from being graded, as had indeed been claimed by the proponents of the grading reform. The grading reform, which abolished grades, appears to have significantly improved the earnings of this group. Boys with university educated parents, instead, appear to have benefitted from being graded, since their adult earnings declined as a result of the reform.

Our findings are robust to including both linear and non-linear municipality specific time trends. An analysis of the timing of effects, however points to that the grading reform is likely to have had an impact on, or that it was accompanied by other changes to the schooling climate. Yet, the placebo analysis suggests that the grading reform, in particular the abolishing of grades in lower school, had a causal negative impact on the educational attainment of daughters of university graduates. The placebo analysis is less

clear as to whether abolishing grades was beneficial for the annual earnings of the sons of less educated parents, but abolishing grades does indeed seem to have been negative for the earnings of the sons of university graduates.

The presence of heterogenous effects, and in particular that it is the sons and daughters of the well educated that have more to gain from being graded, found in this study may play a role in explaining why grading debates often get hot and infected. Yet the evidence is far from conclusive and we need a better understanding of the mechanisms that are involved generating these long run effects of grades.

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SOU 1977:99, *Betygen i skolan*, Fritzes, Stockholm.

SOU 1992:86, *Ett nytt betygssystem*, Fritzes, Stockholm.

Appendix

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

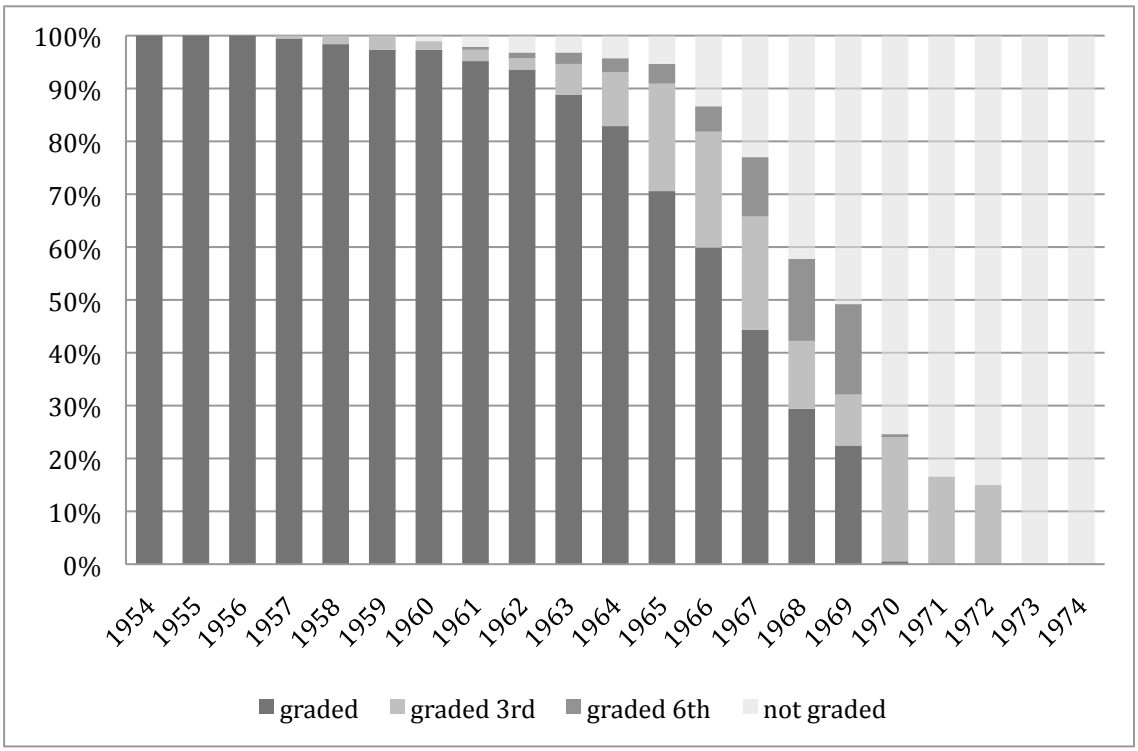

Table A1: Symmary statistics, outcome variables -All

Variable	Obs	Weight	Mean	Std. Dev.	Min	Max
Years of schooling	215860	1990471	12.20346	1.090576	8	19
High school graduation	215860	1990471	.8296152	.1566518	0	1
University degree	215860	1990471	.2046566	.1857174	0	1

Table A2: Symmary statistics, outcome variables –Boys,

Variable	Obs	Weight	Mean	Std. Dev.	Min	Max
All						
Years of schooling	108956	1013941	12.0359	1.086184	8	19
High school graduation	108956	1013941	.8229226	.1559821	0	1
University degree	108956	1013941	.1813498	.176888	0	1
University Educated Parents						
Years of schooling	34400	212731	13.36757	1.120486	8	19
High school graduation	34400	212731	.9390498	.1045607	0	1
University degree	34400	212731	.3725785	.2249918	0	1
Not university educated parents						
Years of schooling	74556	801210	11.68232	.7508928	8	19
High school graduation	74556	801210	.7920895	.1528282	0	1
University degree	74556	801210	.1305763	.1177684	0	1

Table A3: Symmary statistics, outcome variables –Girls

Variable	Obs	Weight	Mean	Std. Dev.	Min	Max
All						
Years of schooling	106904	976530	12.37744	1.067647	8	19
High school graduation	106904	976530	.8365642	.1570434	0	1
University degree	106904	976530	.2288563	.1914834	0	1
University Educated Parents						
Years of schooling	33754	200924	13.67727	1.017681	8	19
High school graduation	33754	200924	.952818	.0931966	0	1
University degree	33754	200924	.4335221	.2347209	0	1
Not university educated parents						
Years of schooling	73150	775606	12.04072	.784726	8	19
High school graduation	73150	775606	.8064481	.1561844	0	1
University degree	73150	775606	.1758367	.1350182	0	1

Table A4: Higher order time trends Years of schooling

	(1) Girls	**(2)** Girls	**(3)** Boys	**(4)**
Low parental education				
No grades Lower School	0.015	0.015	0.019	0.013
	(0.021)	(0.021)	(0.026)	(0.020)
No grades Middle School	-0.009	-0.010	0.006	0.006
	(0.011)	(0.011)	(0.012)	(0.012)
No grades Elementary School	-0.027	-0.029*	-0.002	-0.005
	(0.017)	(0.017)	(0.019)	(0.018)
University educated parents				
No grades Lower School	-0.110**	-0.110**	-0.025	-0.032
	(0.047)	(0.047)	(0.060)	(0.057)
No grades Middle School	-0.034**	-0.036**	-0.013	-0.013
	(0.017)	(0.017)	(0.020)	(0.020)
No grades Elementary School	-0.015	-0.017	-0.026	-0.029
	(0.035)	(0.035)	(0.027)	(0.025)
Observations	97784	97784	99694	99694
R-squared	0.60	0.60	0.62	0.62
Cohort and municipal x PE fe, municipal char	yes	Yes	yes	Yes
Municipal time trend, Quadr linear	Yes	Yes	Yes	Yes
Cubic	no	yes	No	yes

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

Table A5: Higher order time trends annual earnings

	(1) Girls	(2) Girls	(3) Boys	(4)
Low parental education				
No grades Lower School	-0.003	-0.000	0.018	0.008
	(0.019)	(0.007)	(0.337)	(0.006)
No grades Middle School	-0.007	-0.005	-0.005	0.001
	(0.022)	(0.015)	(0.191)	(0.004)
No grades Elementary School	-0.014	-0.010	-0.015	0.013**
	(0.036)	(0.023)	(0.906)	(0.006)
University educated parents				
No grades Lower School	-0.032	-0.030***	-0.021	-0.029***
	(0.022)	(0.011)	(0.253)	(0.010)
No grades Middle School	-0.004	-0.003	-0.024	-0.014
	(0.021)	(0.017)	(0.310)	(0.009)
No grades Elementary School	-0.003	0.000	-0.076	-0.051***
	(0.036)	(0.024)	(0.815)	(0.008)
Observations	95146	95146	97266	97266
R-squared	0.45	0.45	0.19	0.46
Cohort and municipal x PE fe, municipal char	yes	Yes	yes	Yes
Municipal time trend,Quadratic linear	Yes	Yes	Yes	Yes
Cubic	no	yes	No	yes

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