Education and Voter Turnout in a Quasi-Experimental School Reform

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Abstract

It is often argued that an educated electorate is essential for the functioning of democracy, and that this relationship represents an important externality of educational investment. This paper revisits the question by measuring the impact of additional schooling at the lower end of the educational distribution on voter turnout. We instrument the supply of education with a school reform that created large, quasi-random variation in the minimum years of mandatory schooling across cohorts and municipalities in Norway. The impact of the reform on turnout is measured at two levels: individual level and municipality level. The individual level data is based on electoral surveys, while the municipality level analysis utilises Norwegian register data and municipal voter turnout rates. Both levels of analysis point to a similar conclusion. We find that additional education at low levels of attainment does not in general have a causal impact on the turnout rates. We nevertheless find a significant and large effect on men in the individual level, suggesting that the voting behaviour of men and women may respond differently to education.

JEL Codes: I21, I28, H23, H42

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1. Introduction

Much has been written about private returns to education. It is also believed that education has social benefits or positive externalities, which provide one justification for public funding. However, a consensus is emerging in the literature on human capital externalities that direct, productivity enhancing knowledge flows due to education are either non-existent, very small or not well understood (Lange and Topel, 2005). It may however be that human capital produces externalities indirectly, via non-market channels such as health, crime or via different inter-generational channels (Wolfe and Zuvekas, 1997). One of the areas attracting research interest is the link between education and civic participation. If education raises voter turnout, its benefits can extend beyond private, implying that voter turnout could be considered an externality of education. This study examines the impact of education on voter turnout by utilising a Norwegian school reform as an instrument for education.

A vast body of research supports the conclusion that educated people a have higher tendency to vote in political elections (Wolfinger and Rosenstone 1980, Helliwell and Putnam 1999). This has led to a conclusion that education is possibly the most important determinant of voter turnout. However, since the level of education is not assigned randomly, but is a conscious choice, the possibility remains that both education and political participation are determined by unobserved personal qualities or by unobserved parental influences.

Very few studies have been able to find and utilise an institutional change that would have produced quasi-random variation in the amount of schooling across individuals or groups of people and match it to data on voter turnout or other variables of civic engagement. A couple of recent studies that attempt this, end up with partly conflicting results.

A study on voter turnout in the United States and United Kingdom by Milligan et al (2003) uses variations in compulsory schooling laws and state level child labour laws as a source of identification. The differences in these laws produce marginally higher probabilities of school completion across different segments of population. Their results suggest that in the UK, there is no causal relationship between education and voter turnout, and that in the US, being a high school graduate has an impact on voter turnout, but the effect vanishes once registering to vote is controlled for. Their OLS estimates suggest that in the US, high school graduation increases the probability to vote by 26 percentage points, while the instrumental variable estimates based on the laws suggest slightly larger point estimates of 28-30 percentage points.

A similar study by Dee (2003) measures the effect of college attendance on voter turnout and instruments college education with proximity of two-year colleges as a teen. As argued by Dee, and previously by Card (2001), the proximity of colleges reduces the costs of college attendance, though mostly for low-income households. However, the extent to which unobservable characteristics of households are correlated with the proximity of two-year colleges remains unknown, undermining the identification strategy. Dee estimates that in an OLS framework college attendance increases turnout in elections by about 10 percentage points, while the instrumental variable estimates point to twice as large an effect. Furthermore, Dee estimates that one additional year of education increases voter turnout by 6-7 percentage points, using state-specific child labour laws as instruments for schooling. The corresponding OLS estimates pointed to effects roughly half of this size. As these laws mainly affected the years of education for people likely to drop out of high school, they have to be interpreted as local effects specific to lowest educational attainment.

Both of the above studies suggest that in the US, schooling has a positive effect of voter turnout. As emphasised by Milligan et al, most of the effect may be due to the US procedure of voter registration, which can pose an obstacle for voting for less educated people. This view is supported by their result that in the UK where registration is simpler, education does not affect voter turnout.

A recent working paper by Thomas Siedler (2006) assesses the effect of education on civic participation in Germany, and uses German school reform as an instrument to education. The reform increased minimum years of compulsory schooling from seven to nine, and was implemented in different federal states in different years. Using this variation and individual level data on outcomes, he finds no causal effect from education to voting, or other variables of civic engagement.

This study contributes to the empirical evidence based on institutional changes by using the timing of a Norwegian school reform as an instrument for education. In contrast to previous studies based on child labour and school leaving laws, the Norwegian reform created relatively large individual level variation in years of schooling at lower levels of attainment. The timing if the reform varied by location and we show that this variation is quasi-random.

To provide a comprehensive view of the effects of the reform, the analysis is carried out at two levels. Firstly, by using electoral surveys, we test for the impact of the reform on turnout in parliamentary elections using individual level data. To the extent that the individuals treated by the reform can be identified from the surveys and the survey responses trusted, this provides an ideal setting for the evaluation of the reform. Since some assumptions regarding accuracy of the survey data cannot be fully tested, the analysis is also carried out at municipality level. Municipality level turnout data is representative and accurately measured and also caters for potential behavioural externalities in voting behaviour.

The outline of the paper is as follows. Section 2 briefly describes the data sources. Further details of the data are presented as required, and are mostly located in tables and appendices. Section 3 focuses on the details of the school reform, and its exogeneity. Section 4 presents the individual level analysis and section 5 the municipality level analysis. Conclusions are drawn in section 6.

2. Data

The data source for the individual level voter participation is the Norwegian Electoral Survey. This is a rotating panel conducted after parliamentary elections, and provided by Norwegian Social Science Data Services. It includes data on individual characteristics and political behaviour.

The first data source for the municipality level data is the national Census. We use data from three censuses: those for 1960, 1970 and 1980, which include all Norwegians. The censuses provide data on gender, year of birth, education, sector of employment and municipality of residence, which will be aggregated to municipality level for panel data analysis. The census data is matched to voter turnout data from a Norwegian municipal database (NSD kommunedatabase). Data on voter turnout is based on two election types: parliamentary elections of 1961, 1969 and 1981, and local elections of 1959, 1971 and 1979, each of which are just one year apart from the censuses. The units of observation are municipalities with

1980 municipality structure[1]. The votes cast by men and women are counted separately, which allows for an analysis of turnout for both genders. Table 1 provides summary statistics of the municipality averages.

Further, we use a combination of administrative register datasets described in Møen et al. (2003). The register datasets include all Norwegians aged 16-74 over the period 1986-2002, and provides information about labour market status, income, educational attainment, age, marital status as well as municipality residence. This data is mainly used to estimate the impact of the reform on educational attainment and geographic mobility, as detailed below.

3. The school reform

The school reform in Norway is similar to many reforms carried out in European countries over the latter half of the 20^{th} century. The initiation of the reform dates to 1959, when the Norwegian Parliament passed the required legislation. The purpose of the policy was not only to increase educational attainment, but also to unify education at the expense of tracking, reduce regional disparities and broaden access to further education. Specifically, in the old system, there were seven mandatory years of primary education. In addition to these, some municipalities provided an opportunity to continue primary school in so called continuation schools, for one or two years. The secondary education was either a 3 or 5-year track of general education preparing for academic education, or a vocationally oriented middle school, lasting between 1-3 years. The new system increased the years of compulsory education to 9, and provided options for further studies, either an academically oriented high school with an expanded intake, or a vocational schooling.

The municipalities were originally required to implement the reform by 1974, but were given the liberty to decide the exact timing by themselves. The earliest municipalities reformed in 1955, and the latest in the early 1970s. Thus for more than a decade, Norwegian school children were attending two different systems, depending on their municipality of residence. The cohorts that faced either 7 or 9 years of compulsory schooling depending on their municipality of residence were born during 1946-1961. Everyone from cohort 1962 onwards went through the reformed system, and had 9 years of compulsory schooling. From here on, references to “affected cohorts” refer to these cohorts.

The school reform utilised here has been used as an instrument for education previously by Black, Devereux and Salvanes (2005), who used it to assess intergenerational transmission of education.

3.1 Were the municipal differences in timing of the school reform random?

To use the reform as a quasi-exogenous source of variation in education, it is essential that the timing of reform across municipalities is not correlated with any variable that could potentially affect voter turnout. Previous work by Lie (1973) finds no relation between the timing of the reform and municipal characteristics such as average earnings and education level. Lie however found that in a subsample of rural municipalities, politically left-leaning and demographically young municipalities implemented the reform earlier than others. Further, she found that municipalities' year of reform depends on the timing of their

1 Over the period 1960 to 1980 the number of municipalities reduced from 732 to 454 through mergers. Of these, 370 are eventually used in this study, as will be described below.

neighbouring municipalities. Black et al (2005) regress the year of reform on education, income, mean age, rate of unemployment, total population, industrial structure, county dummies[2] and share of votes received by the Norwegian Labour party in elections at the beginning of the reform. They find that only the county dummies are significant. This suggests that within counties, the timing of the municipalities' reform is quasi-exogenous. Inclusion of the share of Labour party vote was important since the Labour party was the most active party promoting the reform. Testing for political determination of the timing of the reform is also of high importance to the current study, which concentrates on voter turnout.

We replicate here the exogeneity test along the lines of Black et al (2005), but add even more variables depicting pre-reform political outcomes. Table 2 shows a municipality level cross sectional estimation, where municipal characteristics are used to predict the timing of the reform[3]. The dependent variable is the birth year of the first reformed cohort, and the explanatory variables are as listed in the table. We find the same result as Black et al. - that only the county dummies are significant, reflecting the spatial correlation in the timing of the reform. It is worth also noting the low proportion of the variance attributable to the regressors.

The impact of the reform on the educational distribution can be seen from Table 4 that tabulates the final educational attainment of cohorts 1952-1954 by treatment status, i.e. whether they went through the old or new school system[4]. The bottom of the educational distribution has shifted upwards by two years, and this has affected roughly 10-15% of people. We also see that the categories with 12 or 16+ years of education have increased slightly, but whether this is due to the reform or trend growth in education is unclear, since in Table 4 the treated cohorts are on average a bit younger than the untreated, due to the gradual progression of the reform. The question of whether the reform had spillover effects to higher levels of educational attainment will be addressed more rigorously below.

4. Individual level analysis

It is possible to identify the individuals affected by the reform from some sweeps of the Norwegian Electoral Survey. Specifically, the surveys have a question on the type of schooling received, and the year of birth. Some sweeps also ask about the county in which the respondent grew up. This is useful since the analysis of the previous section suggested that the reform is quasi-exogenous only within counties. Appendix 1 reports how the sample is constructed, and how the treated individuals are identified from the survey data. It also provides summary statistics for the sample used. Only parliamentary elections are analysed.

The reform increased the effective number of years only for those individuals who would have not continued schooling after 7 or 8 years under the old school system. This brings us to the question of defining the treatment and control groups. In what follows, the “control group” is defined as the people under the new school system who received nine years of schooling, which is the minimum. We thus need to assume that had the reform not taken place, these people would have received 7-9 years of schooling in the old system (the “treatment group”), and that these two groups are comparable.

2 Norway has 19 mainland counties, on average including more than 20 municipalities each.
3 Figure 1 shows the progression of the reform across Norway’s municipalities.
4 Rather than use all affected cohorts 1946-61, we can improve the visual comparability of the treatment and control group by narrowing down the number of cohorts, so that the effect of rising education levels across cohorts is minimised.

Therefore, we must assume that if an individual attained no secondary education (ie. beyond 9 years) in the old school system, she wouldn't have attained it in the new system either. Conversely, we also assume that if the individual attained no secondary education in the new system, she wouldn't have attained it in the old system. The assumption is not implausible, since the reform was mainly focused on raising the minimum attainment. It is still possible that once low attainment individuals are pushed to higher attainment by the reform, they continue to study. If this was the case, the treatment group would lose some - most likely a non-random set - of its members. One way to test this is to test whether the years of education in a higher attainment group is affected by the reform. In appendix 2 we test for this and find that the treatment effect of the reform for individuals whose final educational attainment is at least 10 years, is statistically significant, but very small.

Survey data holds many possible caveats. Firstly, it is possible that since we assign treatment status to individuals based on their on reported education (see appendix 1), it is possible that the treatment status is mismeasured. The individuals report to belonging to any of the four categories "7 years", "7+1 years", "7+2 years" or "9 years", where the first three refer to pre-reform system and the last one to the reformed system. In the second and third category, "7+1" and "7+2", the additional years of education refer to voluntary continuation school, available in some municipalities prior to the reform. As expected, over the cohorts 1947-1958 we see in table A1 a steadily declining proportion of people reporting to have been in the pre-reform system. There still remains a possibility that especially the categories "7+2 years" and "9 years" have been mixed up by some.

A second possible problem is misreporting of voting. It is commonly acknowledged that respondents may lie about voting, which is valued as a "social responsibility". It is not possible to test for this, since in the survey the proportion of respondents choosing not to respond is too large to make a meaningful comparison with the national turnout rates. In terms of identification, a problem arises if the misreporting of the turnout is correlated with whether the individual attended the new or the old school system. Considering that the school system one attends is not a choice, but simply a function of the municipality one lives in, this seems unlikely. While the treated individuals did receive on average almost 1 year more schooling than the untreated, they both still belong to groups with less than secondary schooling, and it seems implausible that an odd additional year of education would have made the respondent more sensitive to misreport her voting behaviour.

Since we have restricted the data to those cohorts that were born during 1946-1961, the sample size remains fairly small for an individual level dataset. As explained in Appendix 1, we have a sample of 500 individuals, of which 311 correspond to the "treatment" group that received post-reform education, and 189 to the "control" group of the pre-reform school system.

We estimate a simple linear probability instrumental variable (IV) model of the following form:

(1) $T_i = \alpha_0 + \alpha_1 edu_i + \alpha_2 female_i + D_{cohort} + D_{survey} + D_{county} + e_i$

Where i refers to the individual, T_i to turnout, edu_i to years education and the first stage for education is as follows

(2) $edu_i = \beta_0 + \beta_1 reform_i + \beta_2 female_i + D_{cohort} + D_{survey} + D_{county} + v_i$

Turnout T_i is defined as a binary variable, classified as 1 if the respondent reported to having voted, and zero otherwise. Education is measured as years of education, classified as 7, 8 or 9, depending on the self-reported schooling as in explained in appendix 1. $Reform_i$ is a dummy variable indicating the treatment and control group, as explained in the same appendix. D_{cohort}, D_{survey} and D_{county} refer to dummy sets.

4.1 Potential biases in the school reform IV estimate due to curriculum effects

Since the school reform not only increased the number of years of education at the bottom of the distribution, but also unified the curriculum to groups that were previously separated by streaming, it is possible that the curriculum, as well as the years of education have affected voting behaviour. This may lead to a bias in the IV estimate as is shown below.

Assume that at the individual latent probability to vote (V) is simply determined by years of schooling (E), and unobserved curriculum quality (C), both affected by the reform (R):

$$V = E(R) + C(R) + \mathrm{u},$$

then the first stage regression of education on reform is:

$$E = \mathrm{d}R + \mathrm{v}.$$

The first stage should be unbiased since the timing of the reform independent of the municipal level education. In the second stage regression, where predicted Education of the first stage (E') is used, there will be a problem of correlation between E' and the error term e, due to the reform:

$$V = \mathrm{b}E'(R) + \mathrm{e}(R).$$

Thus, if the reform affected not only the quantity of education but also the curriculum in a way that it affects students' voting intentions in the future, the 2-step IV estimator may be biased.

To allow for this possibility, we estimate a reduced form model as well:

$$(3)\quad T_i = \delta_0 + \delta_1 reform_i + \delta_2 female_i + D_{cohort} + D_{survey} + D_{county} + e_i$$

This specification will capture effects from both the quality and quantity of education, and answers a different question that the instrumental variable specification. This specification allows us best to assess how much this particular reform has affected voter turnout of the affected individuals – without discriminating *why* it may have affected it, and serves the analysis as a robustness check.

4.2 Results of the individual level analysis

The first stage of the instrumental variables estimation is reported in Table 5. Unsurprisingly, the years of education is strongly affected by the reform status, and increases the years of schooling on average by 0.85 years.

The results of the IV estimation (Table 6a) show that while the effect of education is not statistically significant neither in the full sample, nor in the gender-specific samples, the point estimate for men is much larger. The coefficient 0.175 for males implies that a year of education would increase the probability of voting by about 17 percentage points.

The reduced form estimation in Table 6b mirrors the results of the IV estimation. Again the point estimate for women is close to zero, while for men the estimate is larger. Since the reform raised education on average by 0.85 years, the reduced form estimate for men, 0.148, translates to roughly 17 percentage points higher probability to vote, per extra year of education (0.148/0.85 = 0.174).

The estimate seems incredibly large, and raises the question whether men especially have a tendency to report both their education and voting activity in a positive light. This is not supported by the summary statistics in Table A2 of Appendix 1, where 78.2 % of men report having voted while the comparable figure for women is 78.6 %. Considering the large standard errors of the estimates, and uncertainty with survey responses, one should take a conservative view of these point estimates. The finding that nevertheless raises interest is the large difference between men and women.

It is also worth noting that the treatment effects implied by the IV and the reduced form estimations are very close to each other. This suggests that the treatment effect of the reform, if any, is mostly due to the quantity, not quality of education.

5. Municipality level analysis

This chapter focuses on the models we estimate at the municipal level. The main benefit of this analysis is that the turnout rates can be measured without uncertainty. An interesting feature of the Norwegian data is that the turnout rates are recorded separately for men and women. This will allow us the possibility to confirm or disprove the gender-specific treatment effects of the individual level analysis.

The main issues regarding the estimation are how to define the level of education in the electorate in a way that is meaningful for the instrumental variable estimation, and how to define the instrument at the municipality level so that it can still be argued to be quasi-exogenous. The municipality level analysis allows us to construct a panel data set and utilise the time dimension of the reform. Due to the progression of the reform, different proportions of the electorate had experienced the reformed system in municipalities at a given point in time.

5.1 School reform as an instrument at the aggregate level

The benefits of using aggregate level voter turnout data are that firstly, the measurement errors and biases typical of the survey data are not present. Secondly, in the presence of behavioural externalities (“I vote because my neighbour votes”), the effects will be captured in aggregate data, but not in the individual level data. The main disadvantage will be the lower level of precision that can be expected from an aggregate data.

A key issue with the aggregate level analysis is how to define the school reform instrument in a way that produces heterogenous treatment intensities across municipalities and time, and keeps these treatment intensities quasi-exogenous. We will argue that while the exogeneity cannot be maintained, it is possible to produce two alternative definitions of the instrument, of which one will be upward, and the other one downward biased.

To use the instrument at municipality level we need to assess how much variation in municipality level educational attainment in our data is due to this reform. The panel data of municipalities consists of census years 1960, 1970 and 1980, and the school reform will affect voters in these censuses in the following way:

- In census year 1960, none of the cohorts affected by the reform (cohorts 1946-61) were in voting age, and thus all voters were educated in the old school system.

- By census year 1970, cohorts 1946, 1947, … ,1952 were aged 24, 23, … ,18, and thus eligible to vote. Due to the differential timing of the reform, only some municipalities had voters treated by the school reform in these age groups.

- By the census year 1980, all of the cohorts affected by the reform were in voting age, but again, the proportions of voters affected by the reform are different in different municipalities. All cohorts from 1962 onwards experienced the reformed school system.

Thus the instrument used is *the share of voting age population that has gone through the reformed school system.* The data allows us to construct this instrument, as well as voter turnout for both sexes.

Importantly, whether the treatment intensity, defined as above, remains quasi-exogenous, depends on assumptions we make about the mobility of the individuals. Below we show that with reasonable assumptions, it is not possible to maintain the quasi-exogeneity, but it is possible to construct two definitions of the instrument, which are biased in different directions.

First we make the assumption that in each census, people received their schooling in the same municipality where they currently live. Defined in this way (definition 1), the instrument will be biased if those treated by the reform would be more likely to be geographically mobile and more likely to move to a big city, for example. Appendix 2 shows that this indeed is the case.

The above definition of the instrument (definition 1) failed to take into account the fact that individuals treated by the reform have a higher tendency to move, creating a bias into the treatment intensity of municipalities.

The alternative definition of the instrument presented now (definition 2) will take into account the mobility of people by pinpointing where the individuals went to school. This is possible since the treated cohorts 1946-1961 can be linked to their mothers' municipalities of residence in 1960 via the register data from 1986.

Roughly 98 percent of the individuals born in years 1946-1961 are present in the 1986 register data. The remaining 2 percent may have emigrated or died. For 80% of individuals, the register data in 1986 shows the municipality where their mother lived in 1960[5]. In 1960 the treated cohorts were aged 0-14, or were just about to be born. Thus here we assume that the individuals went to school in the same municipalities as where their mother lived when they were of this age.

Even if we can correctly assess the treatment status of each individual, it is not the case that the aggregated share of treated voters within municipalities is quasi-exogenously distributed. This is because the treated individuals are more likely to move, and are migrating towards cities characterised by unobserved "attractiveness".

In Appendix 3 we assess the direction of the bias using both definitions of the instrument. To summarise, it is possible to aggregate the treated and non-treated individuals to municipality level. This aggregation measures the proportion of electorate that have gone through the reformed school system, and can be used as an instrument with variable treatment intensity across municipalities and time. The quasi-exogeneity of the instrument will however fail at the municipality level if the individuals who are treated have higher tendency to move. This is the case as is shown in Appendix 2. However, it is possible to define the instrument in two different ways, other of which leads to upward bias in the IV estimator, and one which lead to a downward bias. These two definitions of the school reform can act as robustness checks to each other.

5.2 Model for municipal data

The model to be estimated is a municipality level panel of the following form:

$$(4) \quad T_{it} = \alpha + \beta edu_{it} + X_{it}^{'}\gamma + g_t + f_i + e_{it}$$

where T_{it} refers to turnout, i refers to municipality, t to years (t = 1960, 1970, 1980). The municipality level control variables are denoted by X_{it}, while f_i refers to municipality fixed effects and g_t to year dummies. It will be estimated separately for men and women. Even though the dependent variable is a fraction, it is never close to the boundary values 0 and 1 (see Table 1), making an untransformed linear model a fairly good approximation.

The focus of interest is the effect of education on voter turnout, or parameter β. The level of education must be measured at municipality level, but is constructed from individual level data. The model will be estimated separately for both genders so that turnout and education are gender specific, while control variables X_{it} may be specific to gender or general population[6].

Using the average years of schooling in a municipality as the measure for education is problematic, because it is systematically downward biased for young voters who may still be in school. The register data only shows the achieved qualifications. For example, a voter still attending high school might in reality have 11.5 years of education while the data reports 9. Due to this, the measures least likely to be affected by bias are the shares of population that have attained at least 9 or 12 years of education, which roughly correspond to compulsory and

5 In general, a smaller proportion of older cohorts can be matched to their mothers. We assume that the share of treated people in each municipality-gender-cohort cell is the same for the unmatched people as for those that can be matched.

6 In the above equation (4), we include the proportion of voters whose education is missing into the set of controls X_{it}. In the case of Norway, a great majority of people whose education is missing are foreigners.

secondary degrees. Table 3 shows the distribution of years of schooling for all years, and the proportions classified into “below 9 years”, “9-11 years” and “12 or more years”. In this case, the model above would include two variables for education: the shares of voters who have attained at least 9 and who have attained at least 12 years of education.

In non-instrumented regressions both of these measures for education will be used, but as they both are potentially endogenous, we would like to find an instrument for both. This will not be possible, as the school reform that this study utilises affected education only in the lower end of the educational distribution, and can only be used to instrument the proportion of voters with at least 9 years of education. It will however be shown that omitting the share of voters with 12 or more years of education will not change the point estimates for the former variable.

5.3 Reduced form models for municipal data

Again, as in the individual level analysis of section 4, we estimate reduced form models as an alternative, and a robustness check. In these specifications, we will simply measure the impact of the reform on voter turnout, where reform is defined like the instrument, as the share of municipal voting age population who have gone through the reformed school system:

(5) $$T_{it} = \alpha + \beta reform_{it} + g_t + f_i + w_{it}$$

(6) $$T_{it} = \alpha + \beta reform_{it} + X_{it}^{'}\gamma + g_t + f_i + w_{it}$$

The model (5) includes only the proportion of treated as an explanatory variable, accompanied only by municipality fixed effects and year dummies. Model (6) includes a larger set of controls, as described below. Since the reform affects the proportion of voters with lowest educational qualifications, in X_{it} we control the proportion of voters with at least 12 years of education. The reduced form estimate can be considered as a robustness test. The results in the following chapter report these estimates along with IV estimates.

5.4 Results

All results presented are carried out with a balanced panel data set covering 370 municipalities and three years: 1960, 1970 and 1980. The reduction in the number of the municipalities in the final sample is due to municipality mergers, which make the aggregation of individual level characteristics uncertain for some municipalities, and which are thus left out. The details of the sample formation and summary statistics of the instrument can be found from appendix 4.

The summary Table A below presents the coefficients of interest from 24 full model specifications. It reports coefficients of interest for two fixed effects specifications, IV second stage and reduced form results using both definitions of the instrument.

The full results can be found in Tables 7-11 and are ordered as follows: Table 8 presents three non-instrumented fixed effects specifications for both genders using parliamentary election turnout as the outcome variable. Table 9 repeats this for local elections. Tables 10 and 11 show IV second stage in the first column and then two reduced form regressions, for both genders. Table 10 uses parliamentary election turnout and Table 11 the local election turnout.

Tables 10 and 11 use only the second definition of the instrument to save space. In all cases the reported standard errors are corrected for heteroskedasticity and serial correlation. The coefficients of interest are highlighted.

Table A. Summary of results

					Definition 1		Definition 2	
			FE 2	FE 3	IV	Reduced Form 2	IV	Reduced Form 2
PARLIAMENTARY ELECTIONS	MEN	% Treated				-0.027		0
						[0.042]		[0.046]
		% 9+ yrs edu	0.15	0.152	0.157		0.015	
			[0.043]**	[0.042]**	[0.279]		[0.188]	
		% 12+ yrs edu	0.086			0.106		0.102
			[0.092]			[0.093]		[0.092]
	WOMEN	% Treated				-0.066		-0.076
						[0.048]		[0.068]
		% 9+ yrs edu	0.102	0.091	-0.517		-0.308	
			[0.052]+	[0.053]+	[0.376]		[0.246]	
		% 12+ yrs edu	-0.294			-0.243		-0.247
			[0.127]*			[0.129]+		[0.128]+
MUNICIPAL ELECTIONS	MEN	% Treated				0.035		0.052
						[0.049]		[0.056]
		% 9+ yrs edu	0.098	0.103	0.298		0.270	
			[0.051]+	[0.052]*	[0.327]		[0.228]	
		% 12+ yrs edu	0.351			0.355		0.352
			[0.112]**			[0.111]**		[0.111]**
	WOMEN	% Treated				-0.047		-0.081
						[0.061]		[0.085]
		% 9+ yrs edu	0.064	0.075	-0.235		-0.217	
			[0.061]	[0.061]	[0.424]		[0.297]	
		% 12+ yrs edu	0.324			0.358		0.360
			[0.184]+			[0.184]+		[0.184]+

The difference between FE2 and FE3 columns is simply that the share of voters with 12+ years of education has been excluded from the latter regression. The purpose is to show that the coefficient for 9+ years of education is not altered significantly. The instrumented regression is then performed based on the FE3 model.

Results from FE2 column reveal that the share of voters with 9+ years of education is in both cases significant for men. Women's coefficients are lower, and non-significant in local elections. The coefficient for 12+ years of education seems to be important in local elections, but not in parliamentary elections. It may be the case that the issues in local elections appeal relatively more to highly educated electorate. This may also tell us that excluding this variable may be a cause for concern in the local election regressions, but not in the parliamentary election models.

When the share of voters with 12+ years of education is excluded from the specification, we end up with the point estimates in FE3 column. The previously noted result concerning the difference between the genders remains, with almost exactly the same point estimates as in FE2 column. Without any instruments available, it would be appealing to conclude that education matters for men's turnout at the lower end of the educational distribution, while for women the picture is less straightforward. In the case of men and parliamentary elections, the size of the coefficient implies that if 10% of the electorate would be educated from below 9 years to 9 or more years of education, voter turnout would rise by 0.1*0.15 = 0.015, meaning 1.5 percentage points.

The IV results are presented in the third and fifth columns, using two different definitions of the instrument as described in the previous section. The first stages of the 2SLS are significant for both men and women, as can be seen from Table 7. The second stage estimates generally have high standard errors, and point to no statistically significant effects. Again the point estimates are consistently larger for men than women, supporting the conclusion that if any effects exist, they are likely to be larger for men.

It is noteworthy that the results in general are little affected by the definition of the instrument, as can be seen by comparing columns 3-4 to columns 5-6 in table 6. This suggests that the mobility of the treated people is not significantly affecting results.

As we argued in previous section, the content of the curriculum can bias the instrumental variable estimates. It is also well known that even small departures from the strict exogeneity assumptions of the IV can lead to large biases in instrumental variable estimation (Bound et al 1995). Columns 4 and 6 present the reduced form estimates that measure the impact of the reform itself on voter turnout. The interpretation of the coefficient is slightly different. The coefficient tells how much turnout would rise if the proportion of voters going through the new school system would rise from 0 to 1, keeping in mind that in this case only about 15% of the voters would actually receive more education due to the reform. In both parliamentary and local elections the estimated effects are close to zero.

5.5 Robustness checks

We have applied the following robustness checks to the municipality level results.

(1)Including an imperfect income measure

Controlling for municipality level average income should be very important in the analysis of voter turnout. Unfortunately, we do not have mean municipal incomes for 1960, the first year in the panel. However, if we accept the second best option and proxy the 1960 income with the earliest available, 1967, we can include a measure of income into the panel. This variable is however never statistically significant, and has only slight effects the parameters of interest. Controlling for sector of employment, city size and other controls already caters for all the income variation that is significant in voting behaviour.

(2) Altering sample sizes

Since the reformed cohorts are the youngest voters, it is arguable that selecting a sample by excluding 10%-25% of municipalities with oldest age structure, we should end up with a better signal-to-noise ratio in the instrumental variable estimates. Reducing the sample size towards younger municipalities makes the men's and women's results converge somewhat toward each other, while increasing the standard errors as the sample size decreases. The conclusion of no universal turnout effects however clearly remains. The fact that sample selection does not affect the conclusions is not surprising considering that the regressions already control for age structure.

6. Conclusions

It is important to emphasise the context in which this study was made. This is an analysis of the effects of an educational reform that, while affecting everyone, increased years of schooling only at the bottom of educational distribution. Secondly, the results must be interpreted in a framework of a mature democracy. The effect of the reform on average years of schooling at low levels of attainment was considerable: The lowest educated 15% of the population received on average 0.75 years of additional schooling. The strength of the reform as an exogenous source of educational variation is notable compared to earlier studies.

The main results of the paper are firstly, that the school reform did not increase voter turnout generally. The individual level analysis, which due to data limitations concentrated on voter turnout only at parliamentary elections, found that there were no significant effects on voter turnout of women. For men the point estimates are large, and statistically significant. The results suggest that the men treated by the reform were 15 percentage points more likely to vote, corresponding to 17 percentage points greater likelihood for an additional year of education. This effect is substantially very large, but due to the small sample size (500 individuals), the precision of the estimate remains low.

In the municipality level analysis we find no statistically significant effects when instrumental variables are used. This is largely due to the difficulty of using the instrument at municipality level, leading to imprecision. The fixed effects estimates however point to significant, positive effects, which are larger for men than women.

A result that emerges from all of the specifications in this study is that the estimated effect of education on turnout is larger for men than women. It is interesting since the votes cast by women and men are not counted separately in most countries, making similar municipality level analysis impossible.

We have also learned that the type of elections (or possibly the political issues at hand) matter for voter turnout, and might appeal to voters at different parts of the educational distribution. It emphasises the need for researchers to make robustness checks with different types of elections and at different points in time. This issue also highlights the need to gather evidence from many countries, where political systems and issues differ.

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Table 1. Summary Statistics

Variable	Description	Obs.	1960 mean	1960 min	1960 max	1970 mean	1970 min	1970 max	1980 mean	1980 min	1980 max
Turnout M/P	Turnout of men in parliamentary elections	370	**0.79**	0.46	0.93	**0.84**	0.63	0.95	**0.83**	0.62	0.91
Turnout M/L	Turnout of men in local elections	370	**0.74**	0.45	0.92	**0.75**	0.47	0.91	**0.75**	0.55	0.92
Turnout W/P	Turnout of women in parliamentary elections	370	**0.73**	0.51	0.89	**0.81**	0.56	0.93	**0.81**	0.64	0.90
Turnout W/L	Turnout of women in local elections	370	**0.68**	0.24	0.90	**0.73**	0.33	0.90	**0.73**	0.51	0.89
Educ Yr M	Mean years of education in the voting age male population	370	**8.08**	7.24	10.43	**8.63**	7.69	11.48	**9.35**	8.33	11.99
Educ 9+ M	Share of voting age male population with at least 9 years of education	370	**0.33**	0.07	0.64	**0.44**	0.20	0.79	**0.59**	0.36	0.85
Educ 12+ M	Share of voting age male population with at least 12 years of education	370	**0.06**	0.01	0.36	**0.09**	0.02	0.47	**0.16**	0.07	0.53
Educ Mis M	Share of voting age male population whose education is missing	370	**0.00**	0.00	0.00	**0.00**	0.00	0.22	**0.01**	0.00	0.14
Educ Yr W	Mean years of education in the voting age female population	370	**7.74**	7.16	9.10	**8.26**	7.54	10.11	**8.91**	8.06	10.70
Educ 9+ W	Share of voting age female population with at least 9 years of education	370	**0.27**	0.05	0.53	**0.39**	0.16	0.71	**0.54**	0.30	0.78
Educ 12+ W	Share of voting age female population with at least 12 years of education	370	**0.04**	0.01	0.22	**0.07**	0.02	0.32	**0.12**	0.05	0.39
Educ Mis W	Share of voting age female population whose education is missing	370	**0.00**	0.00	0.00	**0.00**	0.00	0.14	**0.01**	0.00	0.10
Age 0-17	Share of municipal population of this age	370	**0.32**	0.23	0.44	**0.30**	0.20	0.42	**0.28**	0.17	0.38
Age18-34	- .. -	370	**0.19**	0.12	0.28	**0.21**	0.13	0.30	**0.24**	0.19	0.33
Age35-64	- .. -	370	**0.37**	0.26	0.44	**0.35**	0.24	0.42	**0.32**	0.25	0.38
ln Pop	Natural log of municipality population	370	**8.37**	5.88	13.04	**8.41**	5.67	13.08	**8.47**	5.58	13.02
Married M	Share of male voting age population that is Married/Divorced/Widower	370	**0.70**	0.53	0.83	**0.71**	0.55	0.84	**0.70**	0.52	0.80
Agric M	Share of voting age male population that works in primary production	370	**0.36**	0.01	0.67	**0.23**	0.00	0.61	**0.16**	0.01	0.51
Industry M	Share of voting age male population that works in manufacturing	370	**0.15**	0.00	0.67	**0.18**	0.01	0.66	**0.18**	0.00	0.63
Services M	Share of voting age male population that works in services	370	**0.34**	0.15	0.64	**0.40**	0.17	0.72	**0.43**	0.22	0.72
Married W	Share of female voting age population that is either Married/Divorced/Widow	370	**0.81**	0.64	0.91	**0.82**	0.63	0.92	**0.81**	0.66	0.89
Agric W	Share of voting age female population that works in primary production	370	**0.02**	0.00	0.08	**0.11**	0.00	0.44	**0.07**	0.00	0.25
Industry W	Share of voting age female population that works in manufacturing	370	**0.03**	0.00	0.20	**0.05**	0.00	0.25	**0.06**	0.00	0.31
Services W	Share of voting age female population that works in services	370	**0.12**	0.02	0.30	**0.21**	0.06	0.44	**0.37**	0.21	0.62

Table 2. Exogeneity of the reform

DEPENDENT VARIABLE: Birth year of the first reformed cohort		
	Coefficient	P-Value
Income	0.000	0.108
Edu Tertiary	-13.174	0.416
Edu Secondary	8.609	0.364
Edu Basic	-1.497	0.499
% in Services	-0.006	0.999
% in Manufacturing	-2.797	0.432
% out of Labour F.	-5.618	0.527
% Married	-4.649	0.468
Ln Population	-0.051	0.786
% Labour Party 1959	-0.335	0.689
% Centre Party 1959	-0.622	0.464
% Right Wing 1959	-1.232	0.491
% Communist 1959	0.026	0.993
% Christian PPLs Party 1959	0.345	0.809
Turnout 1959	-0.064	0.967
% age 0-17	-7.891	0.281
% age 18-34	7.241	0.471
% age 35-64	17.677	0.107
County 1	2.326	0.030*
County 2	-0.304	0.778
County 3	[.]	[.]
County 4	1.220	0.208
County 5	2.073	0.032*
County 6	1.797	0.091+
County 7	1.448	0.187
County 8	0.483	0.643
County 9	1.159	0.248
County 10	3.221	0.001**
County 11	2.452	0.012*
County 12	1.707	0.077+
County 13	-5.301	0.058+
County 14	1.758	0.068+
County 15	2.836	0.002**
County 16	0.937	0.299
County 17	3.081	0.001**
County 18	2.131	0.012*
County 19	3.145	0.000**
Constant	1956.3	0.000**
Observations	644	
R-squared	0.18	

All explanatory variables are aggregated from the 1960 census, unless otherwise stated.
(**) significant at 99% level, (*) at 95% level, (+) at 90% level.

Figure 1. Progression of the school reform across municipalities

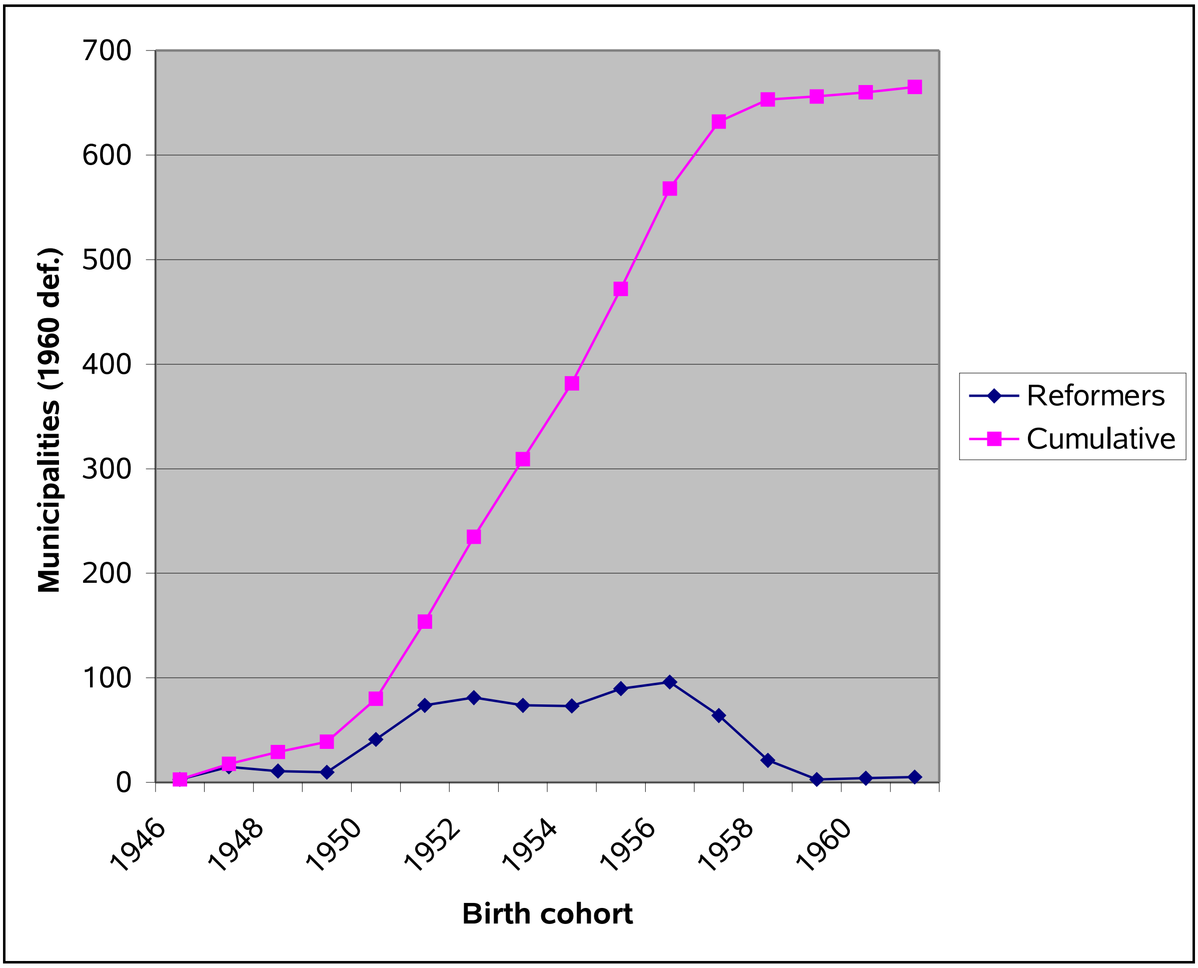

Table 3. Distribution of years of education in censuses 1960, 1970 and 1980. Includes all people aged 18 and above.

Years of Education	1960	1970	1980
7	64.7	42.4	29.7
8	0.0	8.9	7.6
Total "Below 9"	**64.7**	**51.2**	**37.3**
9	15.1	17.5	18.5
10	9.7	14.6	18.5
11	1.6	3.6	5.3
Total "9-11"	**26.4**	**35.6**	**42.3**
12	4.5	6.2	8.8
13	1.6	2.5	4.2
14	1.1	1.8	2.6
15	0.1	0.4	1.1
16	0.1	0.5	1.3
17	1.0	1.1	1.3
18	0.4	0.6	1.1
19	0.0	0.0	0.1
20	0.0	0.0	0.0
Total "12 or more"	**8.8**	**13.1**	**20.4**

Table 4. Impact of reform on educational attainment

FINAL EDUCATIONAL ATTAINMENT OF COHORTS 1952-1954 (from 2002, when subjects were 48-50 years old)			
YEARS EDU	OLD SYSTEM	NEW SYSTEM	EFFECT
7	2.7%	0.9%	down
8	8.9%	1.1%	down
9	2.6%	12.6%	up
10	29.5%	26.3%	
11	9.5%	8.9%	
12	17.6%	18.9%	
13	6.3%	6.3%	
14	5.4%	6.0%	
15	3.0%	3.2%	
16+	14.8%	15.9%	
N	64.149	73.982	

Table 5. The first stage results of the instrumental variable estimation.

	Everyone	Men	Women
	Years of Edu.	Years of Edu.	Years of Edu.
Reformed school system dummy	0.852	0.774	0.896
	[0.060]**	[0.084]**	[0.089]**
Female	-0.015		
	[0.035]		
Year of birth dummies	Yes	Yes	Yes
County of growing up dummies	Yes	Yes	Yes
Survey sweep dummies	Yes	Yes	Yes
Observations	500	257	243
R-squared	0.64	0.68	0.66

Table 6a. Instrumental Variable Probit estimations, marginal effects.

	Everyone	Men	Women
	Turnout	Turnout	Turnout
Years of Education	0.030	0.175	-0.039
	[0.049]	[0.083]*	[0.065]
Female	0.034		
	[0.032]		
Year of birth dummies	Yes	Yes	Yes
County of growing up dummies	Yes	Yes	Yes
Survey sweep dummies	Yes	Yes	Yes
Observations	500	257	243

Table 6b. Reduced form Probit estimates, marginal effects.

	Everyone	Men	Women
	Turnout	Turnout	Turnout
Treated	0.044	0.148	-0.032
	[0.049]	[0.074]*	[0.057]
Female	0.041		
	[0.036]		
Year of birth dummies	Yes	Yes	Yes
County of growing up dummies	Yes	Yes	Yes
Survey sweep dummies	Yes	Yes	Yes
Observations	500	257	243
Pseudo R-squared	0.27	0.34	0.37

In all tables: (**) significant at 99% level, (*) at 95% level, (+) at 90% level. All standard errors (in brackets) are adjusted for heteroscedasticity. Note that Instrumental Variable Probit fails to compute unless some counties are merged together, since some counties have a very small number of individuals. In men's regression, counties 3 and 4, 9 and 10, and 13 and 14 must be merged, leaving a total of 17 counties. In women's regression, also counties 5 and 6, 2 and 3, 7 and 8, and 17 and 18 must be merged, leaving a total of 14 counties. A linear probability model with all counties would give an IV estimate of 0.12 [0.07] for men and –0.03 [0.07] for women. With similarly adjusted county dummy sets, the linear probability IV estimates would change to 0.11 [0.07] for men and –0.04 [0.07] for women.

Table 7. First stage IV estimations, for both genders and definitions of the instrument

	Definition 1	Definition 1	Definition 2	Definition 2
	MEN	WOMEN	MEN	WOMEN
Dependent	% Edu 9+	% Edu 9+	% Edu 9+	% Edu 9+
% Treated	0.240	0.288	0.152	0.145
	[0.045]**	[0.062]**	[0.042]**	[0.046]**
% Edu NA	-1.254	-0.634	-1.225	-0.628
	[0.159]**	[0.244]**	[0.165]**	[0.249]*
% Age 0-17	0.865	0.801	0.899	0.830
	[0.187]**	[0.167]**	[0.191]**	[0.169]**
% Age 18-34	0.974	0.834	1.037	0.918
	[0.159]**	[0.167]**	[0.162]**	[0.165]**
% Age 35-64	0.887	0.605	0.902	0.678
	[0.163]**	[0.158]**	[0.165]**	[0.157]**
ln Popul	0.005	-0.036	0.005	-0.029
	[0.024]	[0.023]	[0.024]	[0.023]
% Married	-0.242	-0.221	-0.230	-0.232
	[0.092]**	[0.090]*	[0.092]*	[0.091]*
% Agric M	-0.326	-0.002	-0.325	-0.010
	[0.071]**	[0.071]	[0.072]**	[0.073]
% Manuf M	-0.319	-0.011	-0.321	-0.022
	[0.076]**	[0.079]	[0.077]**	[0.080]
% Serv M	-0.242	-0.023	-0.241	-0.028
	[0.083]**	[0.086]	[0.085]**	[0.087]
% Agric W	-0.113	-0.026	-0.116	-0.023
	[0.043]**	[0.040]	[0.043]**	[0.040]
% Manuf W	0.087	0.068	0.093	0.086
	[0.072]	[0.079]	[0.073]	[0.081]
% Serv W	0.123	0.145	0.121	0.138
	[0.063]*	[0.060]*	[0.064]+	[0.061]*
Constant	0.105	0.222	0.074	0.128
	[0.147]	[0.145]	[0.146]	[0.146]
Year Dummies	YES	YES	YES	YES
Observations	1100	1100	1110	1110
R-squared	0.94	0.95	0.94	0.95

(**) significant at 99% level, (*) at 95% level, (+) at 90% level. All standard errors are adjusted for heteroscedasticity and serial correlation.

Table 8. Fixed effects regressions with parliamentary election turnout

	(1)	(2)	(3)	(4)	(5)	(6)
	FE 1	FE 2	FE 3	FE 1	FE 2	FE 3
	MEN	MEN	MEN	WOMEN	WOMEN	WOMEN
Dependent	turnout	turnout	turnout	turnout	turnout	turnout
% Edu 9+	0.130	0.150	0.152	0.089	0.102	0.091
	[0.040]**	[0.043]**	[0.042]**	[0.047]+	[0.052]+	[0.053]+
% Edu 12+	-0.078	0.086		-0.583	-0.294	
	[0.072]	[0.092]		[0.093]**	[0.127]*	
% Edu NA	-0.392	-0.063	-0.076	-0.557	-0.391	-0.390
	[0.217]+	[0.239]	[0.237]	[0.213]**	[0.241]	[0.243]
% Age 0-17		-0.234	-0.242		0.042	0.065
		[0.191]	[0.193]		[0.187]	[0.188]
% Age 18-34		0.045	0.045		0.201	0.206
		[0.161]	[0.161]		[0.179]	[0.179]
% Age 35-64		0.028	0.027		0.360	0.373
		[0.166]	[0.167]		[0.167]*	[0.168]*
ln Popul		-0.016	-0.008		-0.020	-0.035
		[0.023]	[0.021]		[0.025]	[0.024]
% Married		0.182	0.182		0.109	0.086
		[0.093]+	[0.093]+		[0.106]	[0.107]
% Agric M		0.081	0.091		-0.128	-0.162
		[0.070]	[0.070]		[0.084]	[0.081]*
% Manuf M		0.214	0.219		0.011	-0.009
		[0.083]**	[0.082]**		[0.091]	[0.089]
% Serv M		0.118	0.130		-0.037	-0.069
		[0.083]	[0.082]		[0.093]	[0.091]
% Agric W		-0.029	-0.029		0.079	0.077
		[0.037]	[0.037]		[0.043]+	[0.044]+
% Manuf W		-0.105	-0.118		-0.021	0.001
		[0.088]	[0.086]		[0.085]	[0.084]
% Serv W		-0.115	-0.110		-0.069	-0.095
		[0.057]*	[0.058]+		[0.068]	[0.068]
Constant	0.792	0.750	0.688	0.816	0.756	0.886
	[0.016]**	[0.158]**	[0.139]**	[0.018]**	[0.173]**	[0.162]**
Year Dummies	YES	YES	YES	YES	YES	YES
Observations	1110	1110	1110	1110	1110	1110
R-squared	0.80	0.81	0.81	0.86	0.87	0.87

(**) significant at 99% level, (*) at 95% level, (+) at 90% level. All standard errors are adjusted for heteroscedasticity and serial correlation.

Table 9. Fixed effects regressions with municipal election turnout

	(1)	(2)	(3)	(4)	(5)	(6)
	FE 1	FE 2	FE 3	FE 1	FE 2	FE 3
	MEN	MEN	MEN	WOMEN	WOMEN	WOMEN
Dependent	turnout	turnout	turnout	turnout	turnout	turnout
% Edu 9+	0.052	0.098	0.103	0.023	0.064	0.075
	[0.048]	[0.051]+	[0.052]*	[0.061]	[0.061]	[0.061]
% Edu 12+	0.013	0.351		-0.079	0.324	
	[0.083]	[0.112]**		[0.136]	[0.184]+	
% Edu NA	-0.629	-0.213	-0.266	-0.719	-0.203	-0.204
	[0.205]**	[0.246]	[0.251]	[0.315]*	[0.298]	[0.301]
% Age 0-17		0.426	0.394		0.284	0.259
		[0.226]+	[0.230]+		[0.266]	[0.265]
% Age 18-34		0.422	0.422		0.397	0.391
		[0.195]*	[0.196]*		[0.238]+	[0.241]
% Age 35-64		0.551	0.546		0.551	0.537
		[0.182]**	[0.185]**		[0.224]*	[0.225]*
ln Popul		-0.106	-0.075		-0.088	-0.072
		[0.029]**	[0.028]**		[0.036]*	[0.033]*
% Married		0.294	0.292		0.170	0.195
		[0.124]*	[0.125]*		[0.144]	[0.142]
% Agric M		-0.053	-0.010		-0.043	-0.005
		[0.094]	[0.094]		[0.114]	[0.110]
% Manuf M		0.032	0.053		0.159	0.182
		[0.106]	[0.106]		[0.126]	[0.124]
% Serv M		-0.102	-0.055		0.127	0.163
		[0.120]	[0.121]		[0.132]	[0.133]
% Agric W		0.087	0.085		0.129	0.131
		[0.055]	[0.055]		[0.058]*	[0.059]*
% Manuf W		-0.170	-0.222		-0.251	-0.275
		[0.091]+	[0.092]*		[0.102]*	[0.103]**
% Serv W		-0.018	0.002		-0.116	-0.088
		[0.074]	[0.075]		[0.085]	[0.084]
Constant	0.729	0.997	0.747	0.732	0.887	0.683
	[0.021]**	[0.190]**	[0.173]**	[0.034]**	[0.247]**	[0.208]**
Year Dummies	YES	YES	YES	YES	YES	YES
Observations	1110	1110	1110	1110	1110	1110
R-squared	0.73	0.75	0.75	0.78	0.80	0.80

(**) significant at 99% level, (*) at 95% level, (+) at 90% level. All standard errors are adjusted for heteroscedasticity and serial correlation.

Table 10. Instrumented regressions with parliamentary election turnout

	(1)	(2)	(3)	(4)	(5)	(6)
Dependent	IV 2nd stage MEN turnout	Reduced Form 1 MEN turnout	Reduced Form 2 MEN turnout	IV 2^{nd} Stage WOMEN turnout	Reduced Form 1 WOMEN turnout	Reduced Form 2 WOMEN turnout
% Treated		-0.041	-0.027		-0.124	-0.066
		[0.038]	[0.042]		[0.043]**	[0.048]
% Edu 9+	-0.157			-0.517		
	[0.279]			[0.376]		
% Edu 12+			0.106			-0.243
			[0.093]			[0.129]+
% Edu NA	-0.441		-0.230	-0.772		-0.453
	[0.387]		[0.230]	[0.337]*		[0.230]*
% Age 0-17	0.022		-0.112	0.571		0.130
	[0.293]		[0.190]	[0.382]		[0.178]
% Age 18-34	0.377		0.213	0.798		0.324
	[0.333]		[0.151]	[0.420]+		[0.169]+
% Age 35-64	0.335		0.195	0.842		0.482
	[0.309]		[0.161]	[0.356]*		[0.165]**
ln Popul	-0.004		-0.014	-0.045		-0.018
	[0.022]		[0.023]	[0.027]+		[0.024]
% Married	0.113		0.150	-0.077		0.062
	[0.114]		[0.095]	[0.155]		[0.105]
% Agric M	-0.004		0.035	-0.168		-0.135
	[0.106]		[0.070]	[0.085]*		[0.082]
% Manuf M	0.120		0.165	-0.031		-0.002
	[0.121]		[0.081]*	[0.098]		[0.090]
% Serv M	0.062		0.087	-0.082		-0.042
	[0.102]		[0.083]	[0.101]		[0.093]
% Agric W	-0.066		-0.047	0.065		0.078
	[0.048]		[0.037]	[0.047]		[0.043]+
% Manuf W	-0.080		-0.079	0.069		0.006
	[0.095]		[0.089]	[0.102]		[0.085]
% Serv W	-0.073		-0.099	-0.012		-0.061
	[0.070]		[0.058]+	[0.093]		[0.068]
Constant		0.792	0.719		0.812	0.748
		[0.002]**	[0.158]**		[0.002]**	[0.166]**
Year Dummies	YES	YES	YES	YES	YES	YES
Observations	1110	1110	1110	1110	1110	1110
R-squared		0.37	0.40		0.59	0.64

(**) significant at 99% level, (*) at 95% level, (+) at 90% level. All standard errors are adjusted for heteroscedasticity and serial correlation.

Table 11. Instrumented regressions with municipal election turnout

	(1)	(2)	(3)	(4)	(5)	(6)
Dependent	IV 2nd stage MEN turnout	Reduced Form 1 MEN turnout	Reduced Form 2 MEN turnout	IV 2nd Stage WOMEN turnout	Reduced Form 1 WOMEN turnout	Reduced Form 2 WOMEN turnout
% Treated		-0.023	0.035		-0.128	-0.047
		[0.050]	[0.049]		[0.060]*	[0.061]
% Edu 9+	0.298			-0.235		
	[0.327]			[0.424]		
% Edu 12+			0.355			0.358
			[0.111]**			[0.184]+
% Edu NA	-0.035		-0.337	-0.399		-0.242
	[0.469]		[0.239]	[0.438]		[0.298]
% Age 0-17	0.227		0.520	0.517		0.339
	[0.370]		[0.226]*	[0.434]		[0.263]
% Age 18-34	0.213		0.518	0.693		0.476
	[0.405]		[0.194]**	[0.448]		[0.236]*
% Age 35-64	0.351		0.626	0.776		0.631
	[0.372]		[0.183]**	[0.378]*		[0.226]**
ln Popul	-0.078		-0.107	-0.077		-0.087
	[0.028]**		[0.029]**	[0.034]*		[0.037]*
% Married	0.336		0.270	0.112		0.139
	[0.144]*		[0.124]*	[0.187]		[0.143]
% Agric M	0.051		-0.087	-0.009		-0.047
	[0.138]		[0.092]	[0.111]		[0.114]
% Manuf M	0.115		0.001	0.171		0.150
	[0.151]		[0.104]	[0.127]		[0.127]
% Serv M	-0.012		-0.130	0.156		0.125
	[0.144]		[0.119]	[0.138]		[0.132]
% Agric W	0.109		0.076	0.124		0.128
	[0.066]+		[0.055]	[0.058]*		[0.058]*
% Manuf W	-0.246		-0.164	-0.240		-0.233
	[0.098]*		[0.092]+	[0.119]*		[0.103]*
% Serv W	-0.021		-0.006	-0.046		-0.111
	[0.083]		[0.075]	[0.106]		[0.085]
Constant		0.756	1.015		0.750	0.879
		[0.010]**	[0.184]**		[0.011]**	[0.244]**
Year Dummies	YES	YES	YES	YES	YES	YES
Observations	1110	1110	1110	1110	1110	1110
R-squared		0.03	0.13		0.26	0.35

(**) significant at 99% level, (*) at 95% level, (+) at 90% level. All standard errors are adjusted for heteroscedasticity and serial correlation.

APPENDIX 1 - Construction of the sample using the Norwegian Electoral Survey Data.

The Norwegian Electoral Survey is a rotating panel carried out every four years by phone, after the parliamentary elections. The sample used in this study is first restricted only to individuals who are in any of the survey sweeps 1977, 1981, 1985 or 1989 and are born between 1946-1961. The selection of the sweeps is determined by the availability of the variable that identifies the type of education as described below. This leaves us with 1707 unique individuals, but a total of 2442 observations due to the rotating panel. Then we exclude all individuals for whom we cannot infer the county in which they grew up, since we need to control for this. This question is asked only in the survey sweeps for the years 1977, 1981 and 1989. As multiple answers are possible due to the rotating panel, the earlier answer is preferred. If there are multiple values for education per individual, the higher attainment is chosen. This leaves us with 1147 unique individuals, 1143 for whom we have the type of secondary education, which reveals the treatment status. These individuals are tabulated below by year of birth and type of education. Respondents are asked about their secondary education, and choose only one category out of the eight options.

Table A1. Type of secondary education by birth cohort

	Only Primary Education				Secondary Education,if any				
Year of	OLD			NEW	Vocational	Middle	Vocational	High	
Birth	7 years	7+1 years	7+2 years	9 years	1 year prg	School	2 year prg	School	Total
1946	7	23	6	4	6	15	3	17	81
1947	5	12	2	6	2	22	4	30	83
1948	5	26	1	9	5	15	2	20	83
1949	5	11	4	6	4	19	4	24	77
1950	1	10	5	9	4	18	2	19	68
1951	2	5	3	9	4	14	1	27	65
1952	0	9	6	15	5	13	4	22	74
1953	3	3	3	19	1	15	4	22	70
1954	1	7	2	22	5	9	2	22	70
1955	0	5	3	27	0	7	2	27	71
1956	0	0	3	33	4	6	2	27	75
1957	1	1	4	32	11	5	2	30	86
1958	0	0	1	31	2	2	0	21	57
1959	0	0	0	31	3	1	0	20	55
1960	1	0	0	36	2	0	3	27	70
1961	2	1	0	22	2	1	0	30	58
Total	33	113	43	311	60	162	35	385	1,143

The first four columns show individuals without secondary education, and they are further divided into those who had their schooling under the old system (columns 1-3) and the new system (column 4). The data shows how the proportion of people that go through the new system increases with later cohorts.

As the data is self-reported, it is possible that some misclassifications regarding the exact schooling system remain. For example, the proportion of the sample in the new school system for the 1946 cohort is larger than expected. With these reservations in mind, the 311 individuals of the column 4 are used as the treatment group, while the 189 individuals in columns 1-3 are the control group.
Table A2 below summarises the variables used in the individual level regression by treatment status and gender.

Table A2. Summary statistics of the regression sample.

	All Observations (N=500)			Untreated (N=189)			Treated (N=311)		
	mean	min	max	mean	min	max	mean	min	max
Voted	0.784	0	1	0.810	0	1	0.768	0	1
Treatment status	0.622	0	1						
Years of Schooling	8.642	7	9	8.053	7	9	9.000	9	9
Female	0.486	0	1	0.497	0	1	0.479	0	1
Year of birth	53.570	46	61	49.857	46	61	55.826	46	61
In 1977 Survey	0.168	0	1	0.249	0	1	0.119	0	1
In 1981 Survey	0.120	0	1	0.053	0	1	0.161	0	1
In 1985 Survey	0.312	0	1	0.296	0	1	0.322	0	1
In 1989 Survey	0.400	0	1	0.402	0	1	0.399	0	1

	Men (N=257)			Women (N=243)		
	mean	min	max	mean	min	max
Voted	0.782	0	1	0.786	0	1
Treatment status	0.630	0	1	0.613	0	1
Years of Schooling	8.654	7	9	8.630	7	9
Female						
Year of birth	53.681	46	61	53.453	46	61
In 1977 Survey	0.144	0	1	0.193	0	1
In 1981 Survey	0.105	0	1	0.136	0	1
In 1985 Survey	0.323	0	1	0.300	0	1
In 1989 Survey	0.428	0	1	0.370	0	1

APPENDIX 2 - Impact of the school reform on individual years of schooling and mobility

To estimate how much more education the treated individuals eventually received, and whether the individuals who go through the reformed school system are more mobile, we estimate respectively a simple OLS and a linear probability models of the following type for cohorts 1946-1961:

$$Education_i = \alpha_i + \beta_1 treated_i + X_i^{'}\beta_2 + D_{cohort} + D_{county} + e_i$$
$$Mobile_i = \alpha_i + \beta_1 treated_i + X_i^{'}\beta_2 + D_{cohort} + D_{county} + e_i$$

$Education_i$ refers to final years of education for the reformed cohorts by 2002. $Mobile_i$ is a dummy variable indicating whether the person changed municipality of residence during 1980-1990. The choice of the period is partly determined by data availability, but is suitable since the treated cohorts (1946-1961) are aged 19-34 at the start of the 10-year period. $treated_i$ is a dummy variable indicating whether the person has gone through a reformed school system, based on information about mother's municipality of residence in 1960. The vector of control variables is denoted by X_i and consists of mother's and father's log income and their years of education in 1980, dummies for both parents' sectors of employment and an urban/rural dummy. D_{cohort} refers to cohort dummies, and D_{county} to dummies for the county of growing up. Mainland Norway consists of 19 counties.

As the reform increased only the minimum years of compulsory schooling, we estimate the above models for two samples: those whose final educational attainment by 2002 (ages 41-56) was less or equal to 9, and for those whose final attainment was more than 9 years. The split reflects the assumption that individuals who acquired more than 9 years of education (i.e. more than the minimum) would have acquired it anyway, had the reform not taken place.

The results of these estimations are shown in Table A3 below. They show that for the group with 9 or less years of final attainment, the years of schooling are 0.75 years higher for those who went through the reformed school system. The estimated figure is similar to what we obtained using different data source, the Electoral Survey in Table 5. There also seem to be a spillover effect to higher levels of education as the second column reveals. In the group whose final educational attainment was more than 9 years, the treatment effect of the school reform was to increase the average years of schooling by 0.08 years. The effect is small, but it is likely that some people have decided to educate themselves beyond the minimum due to the reform, or that some people of higher educational attainment have decided to study even further due to having attended the reformed school system.

For those with 9 or less years of final education, the people treated with the school reform have been 4,4 percentage points likelier to change the municipality of residence during 1980 to 1990, than those not treated by the reform. The fact that there are similar, but smaller effects for the group with higher education levels, suggests two potential explanations. The first is a worry that the model is simply mis-specified, reflecting the possibility that the reformed municipalities were on average more peripheral, agricultural or industrial from which people moved to cities. Objections to this view are that the reform was related neither to industrial composition nor town size, as demonstrated in table 2, and that among others, the regressions control for parental sector of employment and a rural dummy. The second explanation could be that the school reform had some type of curriculum effects or other effects relating to the change in the school institutions, and that these effects extend to those whose time spent in school never changed due to the reform. Still, as expected the mobility effects of the reform are about two times larger for that part of the sample that gained in the years of education due to it.

Table A3: The impact of the reform on years of education and individual mobility

	Sample 1		
	Final educational attainment: 9 or less years		
Dependent variable	Impact of school reform		
Years of Education	**0.7532**	Obs.	32872
	[0.008]**	R2	0.52
Change municipality over 1980-90	**0.0440**	Obs.	32536
	[0.008]**	R2	0.08

	Sample 2		
	Final educational attainment: more than 9 years		
Dependent variable	Impact of school reform		
Years of Education	**0.0849**	Obs.	291267
	[0.014]**	R2	0.18
Change municipality over 1980-86	**0.0289**	Obs.	287959
	[0.003]**	R2	0.06

The regressions control for:
Father's log income 1980, Mother's log income 1980
Father's and Mother's years of Education 1980
Rural dummy 1980
Mother works in primary sector 1980 dummy
Mother works in secondary sector 1980 dummy
Mother works in tertiary sector 1980 dummy
Father works in primary sector 1980 dummy
Father works in secondary sector 1980 dummy
Father works in tertiary sector 1980 dummy
Dummies for year of birth, Dummies for mother's county 1960

Sample:
All Norwegians born over 1946-1961 for whom the treatment status can be recovered using mother's municipality of residence in 1960.

(**) significant at 99% level
(*) significant at 95% level
(+) significant at 90% level

APPENDIX 3 – Biases in the IV estimator due to aggregation

Downward biased definition of the instrument (Definition 1)

Since we assume that people do not move, we essentially mismeasure the proportion of the treated individuals:

$$R_a = R_t + m$$

Where R_a is the assumed proportion of treated, R_t is the true value and m is the proportion of movers such that $m > 0$ for municipalities that lose people and $m < 0$ for municipalities that gain people.

Let T = turnout, E = education. The (simplified) true first stage of the IV estimation is as follows:

$$E = \mathrm{d}R_t + \mathrm{v},$$

but since R_a is observed instead of R_t, it expands to:

$$E = \mathrm{d}R_a + (\mathrm{v} - \mathrm{d}m).$$

This leads to a classical attenuation bias for parameter d. Larger values of R_a will be associated with municipalities losing people, as well as with low values of the error term. In addition, it may be the case that municipalities with high level of education attract more movers. If this is the case, there will be negative correlation with v and m, leading to further downward bias.

Second stage is of the IV is

$$V = \mathrm{c}E' + \mathrm{e},$$

where V is a measure of turnout and E' is the predicted education from the 1st stage. The probability limit of the instrumental variable estimator in this case is

$$\mathrm{Plim}\ \mathrm{b}_{\mathrm{IV}} = \mathrm{b} + \mathrm{S}(E', V\text{-b}E)/\mathrm{S}^2(E),$$

where $\mathrm{S}(E', V\text{-b}E)$ is the covariance between the first stage predicted education (E') and the prediction error of the uninstrumented OLS model of V on E, and $\mathrm{S}^2(E)$ is the variance of education. The assumed proportion of treated, R_a, is overstated in cities that lose people, and understated in cities that gain people, since we assumed that people received their education in the municipality where they live, while in reality the treated people have been more likely to leave. This leads to E' being under predicted for destination cities. If these “attractive” cities have a tendency to vote more (less) actively, the IV is biased downwards (upwards).

A further note is in place. If we assume that the individuals who move, refrain from voting due to any reason, for example not knowing where the polling station is in their new city, or not being familiar with local politics, the IV estimator will be downward biased, since E' will be negatively correlated with e in the second stage. This should not be of concern since the analysis focuses on national, not local elections. National elections were the focus of the individual level analysis

Upward biased definition of the instrument (Definition 2)

Using the same notation as above, consider the first stage of the IV:

$$E = \mathrm{d}R_t + \mathrm{v}.$$

Since the movers have gone to "attractive" cities, we know that *E'* will be over predicted for them. Note however that if the unobserved "attractiveness" of a city is not correlated with level of education, the estimate of d is unbiased. If destination cities have higher level of education, d will be upward biased.

In a similar argument as before, if *E'* is over predicted for attractive cities, and attractive cities vote more (less) actively, the instrumental variable estimator is biased upwards (downwards).

Further, the analysis of bias above assumes a cross-sectional data. In the analysis we will use panel data with municipality fixed effects. If the unobserved "attractiveness" of the cities remains relatively constant over 1960-1980, the biases in the two definitions should be small, and we should expect similar results using both definitions.

APPENDIX 4 - Municipality mergers affect the sample

In 1980 there were 454 municipalities in Norway. Our sample will be smaller for the following reasons. Firstly, in 1960, when the reform started, Norway consisted of 732 municipalities, for which 545 the year of school reform is known, based on work by Ness (1971). For the remaining 187 municipalities the reform was either staged over several years or it is unclear when the reform took place. Secondly, an additional complication arises as a number of municipal mergers took place from 1960 to 1980, reducing the total number of municipalities to 454. Thus in the new municipality structure some municipalities consist of a number of old ones with potentially different years of reform, or undetermined year of reform. In cases like this we have decided to drop municipalities for which a proportion of the population does not have a well-defined year of reform due to a municipality merger. In cases where a 1980 municipality consists of several municipalities with different reform years, there is no problem, since the share of voters who were reformed is calculated from individual level data, and individuals can be assigned with the correct treatment status based on their birth cohort and residential status of their mothers in 1960. This procedure leaves us with 370 municipalities. Increasing the sample size beyond this would compromise the quality of the instrument.

The Table below reports summary statistics of the instrument (second definition)[7] for the 370 municipalities and for both genders [8]. In 1970 the mean value across municipalities is only about 2 %, but it varies from zero to beyond 10 %. By 1980, it averages 13.9% for men and 18.6 % for women. For 1960, the value of the instrument is zero in each municipality, since the treated cohorts are not in voting age yet.

Table A4. The proportion of voting age population treated by the reform, across municipalities and years.

	MEN		WOMEN	
	1970	1980	1970	1980
Observations	370	370	370	370
Mean	0.021	0.139	0.019	0.186
S.D.	0.029	0.046	0.025	0.040
Min	0.000	0.030	0.000	0.072
Max	0.137	0.273	0.110	0.339
Percentile				
5th	0.000	0.062	0.000	0.126
10th	0.000	0.079	0.000	0.137
25th	0.001	0.046	0.002	0.158
50th	0.004	0.138	0.006	0.183
75th	0.034	0.170	0.031	0.209
90th	0.067	0.200	0.056	0.239
95th	0.084	0.219	0.077	0.256

7 Second definition of the instrument, as explained in chapter 5. The two definitions correlate 0.98 for women and 0.95 for men in the panel data sample we use.

8 The fact that larger share of voting age men have gone through the reformed school system in most municipalities reflects the differential age structures of genders. When a municipality has relatively larger gender imbalance among older people, generally favouring women, the share of reformed is larger for men, since the reformed belong to youngest generations.