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Length of Compulsory Education and Voter Turnout – Evidence From a Staged Reform

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Executive Summary

It is possible that human capital produces positive externalities to the society indirectly, through non-market channels such as health or crime. Another such channel could be the effect of education on the functioning of democratic decision-making. Measures of the functioning of democracy are bound to be controversial, but one such measure – voter turnout – reflects the engagement of people to democracy, and also receives a considerable amount of attention from social scientists as well as the media.

A vast body of empirical research supports the conclusion that educated people have a higher tendency to vote in political elections. 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 parental influences. So far few studies have been able to find and utilise an institutional change that would have produced experimental 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. The few recent studies that attempt this, end up with partly conflicting results.

This study estimates the effect of education on voter turnout in the long run. It contributes to the empirical evidence based on institutional changes by using the timing of a Norwegian staged school reform as an instrumental variable for education. In contrast to previous studies, the Norwegian reform created relatively large individual level variation in the years of schooling at lower levels of attainment, as the minimum number of compulsory years was raised from seven to nine. The timing of the reform varied across Norway in a scattered fashion, and this variation appears to be quasi-random as it is difficult to find socio-economic correlates for it.

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

The results of the study are, at both levels of analysis, that education does not affect voter turnout. Further, using survey data, the causal impact of education on several measures of civic activity is estimated. Again, all effects are found to be non-significant, except for the likelihood of signing a petition. The findings of this study support the view that political activity of people is driven by other things than education.

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Acknowledgments

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1 Introduction

Empirical search for human capital externalities has not been fruitful, prompting recent surveyors to conclude that empirical evidence for important human capital externalities is "at best, weak" (Lange and Topel, 2006). Such externalities typically refer to direct, productivity increasing externalities, often labelled „technological externalities". However, it may be that human capital produces positive externalities to the society indirectly, through non-market channels such as health, crime or via different inter-generational channels (Wolfe and Zuvekas, 1997). Another such channel could be the effect of education on the functioning of democratic decision-making (Hanushek, 2002, Milligan *et al*., 2004). For instance, if education raises voter turnout or the awareness of the electorate in terms of economic and social issues, one could expect the quality of democratic decision-making process to improve in the long run. While empirical work has not found significant connections between democracy and economic growth (Haan and Siermann 1995, Barro 1996), the impact of the quality of the democratic decision making process on economic performance remains largely an unexplored area.

Voter turnout is far from a perfect measure of the quality of the democratic process. Larger voter turnout legitimises democracy and political outcomes, but it could also be argued that if voters differ in terms of their ability to make good choices, a lower turnout could even lead to better outcomes if the average voter is more informed.[1] Still, voter turnout is a measure, which receives a considerable amount of attention from social scientists as well as the media, as it reflects the engagement of people to democracy.

It is not obvious why additional education would make a person more likely to vote. The central questions are, what motivates people to vote in the first place despite non-trivial costs, and how could education affect these mechanisms? A recent survey of theories of voter turnout by Dhillon and Peralta (2002) categorise the potential objectives of voters as either instrumental, or expressive. In the instrumental models, the voters want to affect the outcome of the election and weigh their costs and benefits in deciding about whether to vote or not. The expressive motives have to do with attaining some type of psychological utility from the act of voting (Schuessler 2000).

[1] Feddersen and Pesendorfer (1996) show that it can be in the interests of less informed voters to abstain from voting.

2 The effect vanishes once registering to vote is controlled for. Registration to vote is not automatic in the United States.

In instrumental models, factors that can affect the costs of voting can be such as weather or distance to a polling station, but importantly, also the time needed to gather information about candidates, their policies, and how their policies might affect the voter. Feddersen and Pesendorfer (1996, 1999) note that if education provides people with better access to information, the costs of voting will be lower, and one should expect higher turnout among educated people.

The instrumental motives are not entirely satisfactory, since the costs of voting are practically always larger than the expected marginal benefits at the individual level. As Dhillon and Peralta (2002) conclude: "All models seem agreed that some expressive factor is causing the relatively high levels of turnout". One potential reason for expressive voting, social pressure, is explored by Funk (2007). She finds that introduction of postal voting reduced voter turnout in small Swiss communities. Her explanation for the reduction in turnout is that voters have gained „a credible excuse" for not appearing on the polling stations on the day of election. Whether education contributes to such expressive motives, is unknown.

A vast body of empirical research supports the conclusion that educated people have a higher tendency to vote in political elections (eg. Wolfinger and Rosenstone 1980, Helliwell and Putnam 1999). 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 parental influences (Luskin 1990, Denny and Doyle 2008, Fowler and Daves 2008).

So far few studies have been able to find and utilise an institutional change that would have produced experimental 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. The few 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, Moretti and Oreopoulos (2004) uses the variation 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 the 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 the likelihood of voting[2]. A similar study by Dee (2003) measures the effect of college attendance on voter turnout and utilises the proximity of two-year colleges as a teen to infer the causal effect of education. As argued by Dee, and earlier by Card (1995), the proximity of colleges reduces the costs of college attendance. However, the extent to which unobservable characteristics of households are correlated with the proximity of two-year colleges remains unknown, undermining the identification strategy.

Both of the above studies suggest that in the US, schooling has a positive causal effect of voter turnout. As emphasised by Milligan *et al.* (2004), 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 study by Siedler (2009) assesses the effect of education on civic participation in Germany, and uses the German comprehensive school reform of 1949-1969 as an instrumental variable for education. The reform increased the 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 to other variables of civic engagement.

This study estimates the effect of education on voter turnout in the long run. It contributes to the empirical evidence based on institutional changes by using the timing of a Norwegian staged school reform as an instrumental variable for education. In contrast to previous studies, the Norwegian reform created relatively large individual level variation in the years of schooling at lower levels of attainment, as the minimum number of compulsory years was raised from seven to nine. As demonstrated in the next Section, the timing of the reform varied across Norway in a scattered fashion, and that this variation appears to be quasi-random in a sense that it is difficult to find socio-economic correlates for it. This makes the Norwegian reform arguably the most convincing compulsory schooling reform available to social scientist.

To provide a robust view of the effects of the reform, which was initiated in 1959, the analysis is carried out at two levels. Firstly, by using survey data, the impact of the reform on voting in

2 The effect vanishes once registering to vote is controlled for. Registration to vote is not automatic in the United States.

parliamentary elections is tested using individual level data from 1977 to 1993, or two to four decades after the reform. To the extent that the survey responses can be trusted, this provides an ideal setting for the evaluation of the impact of education on voting. Since some assumptions regarding accuracy of the survey data cannot be fully tested, the analysis is also carried out at the municipality level, using data from 1960 to 1980, up to two decades after the reform started. Municipality level turnout data is representative and accurately measured and also caters for potential behavioural externalities in voting behaviour. If political activity of individuals is affected by the activity of their friends and acquaintances, individual level analysis of education and voting would produce biased results. Secondly, the municipality level analysis gathers for potential behavioural changes of those voters whose educational attainment was not affected by the reform.

The results of the study are, at both levels of analysis, that education does not affect voter turnout. Further, using survey data, the causal impact of education on several measures of civic activity is estimated. Again, all effects are found to be non-significant, except for the likelihood of signing a petition.

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

2 The School Reform

The Norwegian compulsory school reform is similar to many reforms carried out in European countries over the latter half of the 20th century (Leschinsky and Mayer 1990, Viarengo 2007). Its goal was to increase educational attainment, but also to unify education at the expense of tracking and reduce regional disparities in educational attainment in Norway. The pre-reform system consisted of seven mandatory years of primary education. In addition, some municipalities provided a possibility to continue primary school for one or two years. Secondary education was either a three or a five-year track of general education preparing for academic education. Further, a vocationally oriented schooling was available, typically lasting one or two years. The post-reform system increased the years of compulsory education to nine, and provided an academically oriented

3-year high school with an expanded intake, or a vocational schooling. The curriculum for the two added years in the post-reform system would concentrate on general education. As in other countries with similar reforms, the curricular changes reflected the increased demand for skilled workers (Lie 1973).

The reform was launched as an experiment in six municipalities, chosen by the Ministry of Education, until it was made compulsory by the central government. Once the reform was legislated in 1959, the municipalities were required to implement the reform by the end of 1972, but were given the liberty to decide the precise timing by themselves. Prior to implementation, local governments were required to present a plan to the central government regarding needs for new teachers, buildings and other costs, which were to be covered by the central government. Due to this process, the timing of the reform across municipalities was partly determined by an interaction process between the central government and the municipalities. Aakvik, Salvenes and Vaage (2003) also mention that individual school directors may have been influential in the timing of the reform within municipalities. The reforms were implemented from 1960 onwards, and the cohorts that were differentially affected by the reform depending on their place of birth, were born over 1946 to 1961.

In 1960, when the reform started, Norway consisted of 732 municipalities, 665 for which the year of reform is known, or can be plausibly estimated, based on the work by Ness (1971), Aakvik, Salvanes and Vaage (2003) and Black, Devereux and Salvanes (2005). For the remaining 67 municipalities the reform was either staged over several years or it is unclear when the reform took place. [3]

Instrumental variable estimation (IV) is an econometric technique that provides a general solution to the problem of endogenous explanatory variables. In the literature on the effects of education on various outcomes of interest, it is generally acknowledged that education is an endogenous variable because unobservable characteristics of individuals can be correlated with their education.

To use the school reform as an instrumental variable, it should satisfy two conditions. Firstly, the reform should increase the level of education for the individual, or the group of interest, and

3 The cited studies use reform year data for somewhat smaller set of municipalities. The latest data of the municipal reform years was provided to the author by Professor Kjell Salvanes of Norwegian School of Economics and Business Administration

secondly, the reform should affect the outcome variable of interest only via this increase in the level of education. In terms of the first condition, the reform, by imposing the new minimum of nine years of schooling, clearly increases the years of education for those who in the absence of the reform would have attained less than nine years. In terms of the second condition, the critical question to ask is whether those who went through the reformed school system are, in some respects, different than those who went through the old system? In particular, in birth cohorts 1946-1961, pupils ended up to the different schooling systems based on their municipality of residence. But can it be the case that the municipalities that were early reformers, were in some respects fundamentally different? If this is the case, the second condition will not be satisfied (Wooldridge, 2002: Ch. 5).

Early investigation by Lie (1973) finds no obvious geographical or socio-economic correlates of the timing of the reform across municipalities. Her only statistically significant findings were that in a sub-sample of rural municipalities, politically left-leaning and demographically young municipalities implemented the reform earlier than others. Secondly, she noted that municipalities were more likely to reform quickly, if their neighbouring municipalities reformed early.

Table 1 presents the results of a municipality-level cross-sectional OLS estimation, in which various municipal socio-economic and political characteristics are used to predict the timing of the reform. The dependent variable is the birth year of the first reformed cohort, and the explanatory variables are as listed in the Table. Only the log mean income of the municipality is near to being significant at 95% level in explaining the timing of the reform. Adding regional controls for the 19 Norwegian counties would not make alter the statistical significance of the variables. It is also worth noting the low proportion of the variance attributable to the regressors. Similar exercise was carried out by Black, Devereux and Salvanes (2005), albeit with fewer variables depicting political characteristics of the municipalities.

These results suggest that there are no important socio-economic variables that would have predicted the timing of the reform across municipalities in Norway. As such, the reform provides an interesting case of "natural experiment", in which individuals have been subjected to an institutional reform in a fashion that appears random. This provides an interesting opportunity to evaluate the effects of education on individual or collective outcomes in Norway.

3 Data

The data source on individual voters is the Norwegian Election Study. This is a rotating panel conducted by phone after parliamentary elections, and provided by the Norwegian Social Science Data Services[4]. It includes data on individual characteristics and political behaviour. Summary statistics and other details of the data are presented in Section 4 and Appendix 1.

For municipality level data, the first source is the national Census of Norway. Data from three censuses are used: those for 1960, 1970 and 1980, which include all Norwegians. The censuses provide data on the year of birth, education, sector of employment and municipality of residence, which are aggregated to municipality level for panel data analysis. The census data is matched to voting data from a Norwegian municipal database[5]. Data on voter turnout is based on parliamentary elections of 1961, 1969 and 1981, each of which is one year apart from the censuses. The units of observation are municipalities with 1980 municipality structure[6].

4 Individual Level Analysis

The data

It is possible to identify the individuals affected by the reform from some sweeps, but not all, of the Norwegian Election Study. Specifically, the surveys have a question on the type of schooling received, and the year of birth. Appendix 1 reports how the sample is constructed, and how the treated individuals are identified from the survey data. The final sample is a cross-sectional dataset of individuals, who have been interviewed in at least one of the five electoral surveys over 1977-1993. Each individual is in the final dataset only once. Summary statistics for the sample used are

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4 http://www.nsd.uib.no/nsd/english/index.html

5 http://www.nsd.uib.no/nsd/english/regionaldata.html

6 Over the period 1960 to 1980 the number of municipalities reduced from 732 to 454 through mergers. Of the latter, 370 are eventually used in this study, as described in Appendix 3.

provided in Table 2. Only parliamentary elections are analysed, since the survey data is confined to these.

In practice, the reform increased the number of years only for those individuals who would not have continued schooling beyond 7 or 8 years under the old school system. In what follows, the "treatment group" is assumed to be the individuals who went through the new school system, but did not continue beyond 9 years, the new minimum. The "control group" is defined as the people under the old school system, who ended up attaining 7-9 years of schooling. It is thus assumed that had the reform not taken place, the "treatment group" would have received 7-9 years of schooling in the old system, and that once treated, they would have attained only the new minimum of nine years, and not continued beyond this.

The assumptions are not implausible, since the reform mainly focused on raising 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 control group would lose some - most likely a non-random set - of its members.

Survey data can hold some caveats. Firstly, it is possible that since we assign treatment status to individuals based on their reported education (see the Appendix 1), it is possible that the treatment status is mismeasured. The individuals who attended attained no further general or vocational education report belonging to any of the four categories: "*7 years*", "7+*1 years*", "7+*2 years*" or "*9 years*". The first three refer to the 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 1946-1961 we see in Table A1 a steadily declining proportion of people reporting to have been in the pre-reform system.

A second possible problem is misreporting of voting. It is acknowledged that respondents may lie about voting, which is valued as a "social responsibility". The individual level data suggests a turnout rate of 82% (Table 2), which is in line with turnout rates in Norway during the same period[7]. In terms of identification, a problem arises if misreporting of the turnout is correlated with whether the individual attended the new or the old school system. Considering that the school

7 Municipality level averages for voter turnout in 1961, 1969 and 1981 parliamentary elections are 76%, 83% and 82%, as reported in Table 5.

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 one 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 the data is restricted to 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 983 individuals, of which 610 correspond to the "treatment" group that received post-reform education, and 373 to the "control" group of the pre-reform school system.

Model specification for individual level data

An individual level Probit instrumental variable (IV) model of the following form is estimated:

(1) $T_i = \Phi(\alpha_1 Ed_i + \alpha_2 Female_i + D_{cohort} + D_{survey} + D_{region} + e_i)$

(2) $Ed_i = \beta_0 + \beta_1 Treated_i + \beta_2 Female_i + D_{cohort} + D_{survey} + D_{region} + v_i$

Turnout T_i is defined as a binary variable, classified as 1 if the respondent reports having voted, and zero otherwise. Education (Ed_i) is measured as the years of education, classified as 7, 8 (7+1) or 9 (7+2 or 9), depending on the self-reported schooling as is explained in Appendix 1. $Treated_i$ is a dummy variable indicating the treatment and control group. D_{cohort}, D_{survey} and D_{region} refer to dummy sets. Error terms e_i and v_i are assumed to be independently and identically distributed, and $\Phi(.)$ is a normal distribution function. Adding more individual level variables would necessarily introduce more endogenous variables. For IV-specification of this type, it is therefore not desirable to extend the model beyond the current controls.

The above model is estimated with a sample restricted to those with a maximum of 9 years of education. In addition to estimating the above model, both with and without instrumentation, the Probit model (1) will be estimated for the whole educational distribution for comparison. The highest primary/secondary education type, as described above, is available for all individuals. The data also includes a variable on self-reported years of education, which however suffers from large non-response, but also from unreliability. The sub-sample for which data on years of education is available can be used to produce a model for the "predicted" years of education, by regressing the self-reported years of education on the above schooling category dummies, gender, birth year dummies, and region dummies. Then this model can be applied to the full sample, to produce "predicted" years of schooling for everyone. The details of this exercise are in Appendix 2.

The *Years education (predicted)*, as summarised in Table 2, point to about one and a half years more of education, than one would expect on the basis of the primary/secondary schooling type[8]. The predicted years of education will be used only in the regressions for the full sample, the purpose of which is to act as a reference point for the local effects obtained with the instrumental variable estimation.

In addition to voter turnout, the model is estimated for a number of other outcomes: whether the respondent is interested in politics, whether it was easy to decide a candidate, whether the respondent discusses politics, and whether he has taken political action in form of contacting a representative, making a complaint, taking an issue to a political party meeting, writing an opinion piece, demonstrating, or signing a petition.

Results of the individual level analysis

The first stage of the instrumental variables estimation (Equation 2) is reported in Table 3. Not surprisingly, the reform status has a strong effect on the years of education, and increases it on average by 0.88 years.

Table 4 presents the results on the effect of education on voter turnout. The first column uses the predicted years of education to estimate the effect across the whole educational distribution. On average, one year of education is associated with 1.8 percentage points higher probability of voting.

8 It is not clear why the respondents report having on average more years of education than implied by the reported qualifications. They may have taken additional courses not reported here, or may have been trained by their workplaces. Some people may simply also exaggerate their years of education.

The second column suggests a much larger point effect at the bottom of the educational distribution, but the effect is not statistically significant at any conventional levels. Further, the instrumental variable estimation (IV-Probit) reduces the point estimate to 1.3%, and the effect is not statistically significant either.

Since the instrumental variable is exogenous, one should trust the estimates based on it more, than the non-instrumented estimates. The fact that the non-instrumented Probit estimate at the second column gives a larger coefficient, points to the possibility that unobservable factors, which are correlated with education, bias the estimate upwards.

Overall, Table 4 suggests that firstly, the statistically significant positive effects of education in the full sample (column 1) can not be confirmed, once the sample is restricted to the bottom of the distribution (column 2), possibly due to the smaller sample sizes. Secondly, once the endogeneity of education is addressed in column 3 with instrumental variable estimation, the positive effects look even more remote.

Most years of the Norwegian Election Study also ask the interviewees about other aspects of political activism, in addition to voting. The first three columns of Table 5 list and summarise some of these variables for the subsample of respondents with no more than 9 years of schooling. Further a Probit, and an instrumental variable Probit models are estimated for each of the measures of civic activism.

The results of Table 5 suggest that education in general appears to have no impact on political activism, or interest in politics. The only variable which is statistically significant at 95% confidence level is the likelihood of signing a petition.

5 Municipality Level Analysis

This section focuses on the models estimated at the municipality level. The main benefit of this analysis is that the turnout rates can be measured without uncertainty. Secondly, in the presence of behavioural externalities ("I vote because my neighbour votes"), the effects will be captured with aggregate data, but not with individual level data. The main disadvantage will be the lower level of precision that can be expected from an aggregate data.

Regarding the practical implementation of the estimation, the main issue is 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. In other words, to use the instrument at the 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 the 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 received their schooling in the reformed school system.

Thus the instrumental variable used is the share of voting age population that has gone through the reformed school system *(% went to reformed school*). Since the reform affects mostly relatively young cohorts until 1980, it is important to control for the municipal age structure when using this instrument.

Whether the treatment intensity, defined as above, remains quasi-exogenous, depends on assumptions we make about the mobility of individuals. Here we make the simplifying assumption that in each census year, individuals received their schooling in the same municipality where they currently live. This corresponds to assuming that the population is not mobile. Since this is a highly restrictive assumption, an alternative definition of the instrumental variable, and alternative set of results, which take into account the mobility of workers, is presented in Appendix 5. These results will be discussed later as a robustness check.

All results presented use a balanced panel data set covering 370 municipalities and three years: 1960, 1970 and 1980. The data from censuses is aggregated to municipality level using 1980 municipality structure. The definitions of variables and data sources are reported in Appendix 4, and summary statistics for the panel data are presented in Table 6. The reduction in the number of the municipalities in the final sample, compared to available municipalities, 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 can be found in Appendix 3.

Model specification for municipal data

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

$$(3)\quad T_{it}=\alpha+\beta Ed_{it}+X_{it}^{'}\gamma+g_t+f_i+e_{it},$$

$$(4)\quad Ed_{it}=\eta+\gamma PT_{it}+X_{it}^{'}\lambda+g_t+f_i+e_{it},$$

where T_{it} refers to turnout, i refers to municipality, t to years (t = 1960, 1970, 1980). Ed_{it} is mean years of schooling in the municipality i at year t. Municipality level control variables are denoted by a vector $\mathbf{X}_{it}$, while f_i refers to municipality fixed effect and g_t to year dummies. The control variables $\mathbf{X}_{it}$ include only controls for the age structure of the municipality.[9] Even though the dependent variable T_{it} is a fraction, it is never close to the boundary values 0 and 1, making an untransformed linear model a sufficiently good approximation. Controlling for municipality fixed effect is important for identification, because it allows us to control for all permanent characteristics of municipalities that may have affected the timing of the reform. As in the individual level analysis, introducing more covariates to predict voter turnout would necessary involve endogenous

9 Particularly, proportions of the municipal population, who are 0 to 17, 18 to 34, and 35 to 64 years old.

variables which would not be possible to instrument for. Thus, the model has been kept to its bare minimum.

In the first stage of the instrumental variable estimation (4), the instrument (PT_{it}) is the proportion of the municipal voting age population that has gone through the reformed school system. The focus of interest is the effect of education on voter turnout, or parameter □. The level of education must be measured at municipality level, but is constructed from individual level data.

The municipal level analysis differs from the individual level analysis in two important aspects. Firstly, the effect of education is measured from the complete educational distribution, not just the bottom, where the reform had its biggest impact. Secondly, the data is from an earlier period – 1960 to 1980 – whereas in the individual level analysis the data was from 1977 to 1993.

Results

The first stage estimate (Equation 4), is presented in Table 7, column 1. In column 2, an alternative specification is estimated where county-level[10] linear trends have been added. In the individual level analysis we estimated that the reform increased the years of schooling by 0.88 years, and that this increase applied to the proportion of the population who have at most nine years of schooling. The impact on the rest of the distribution should be close to zero. In this panel data, on average about half of the voters have at most nine years of education. Therefore, we would expect the first stage estimate to be roughly half of 0.88, or 0.44.

A comparison of columns 1 and 2 in Table 7 suggests that the model where county linear trends are included (column 2) corresponds more to these expectations. The results that follow are nevertheless carried out using both of these specifications.

Table 8 presents the results for voter turnout. Overall, there is no positive relation between voter turnout and education whether one uses instrumentation or not, or whether the model is specified

10 Norway consists of 19 mainland counties, which on average contain more than 20 municipalities each.

with county trends or not. If fact, the estimates for the IV models are negative, and even statistically significant at 95% level in the third column, where county trends are not controlled for.

It is difficult to compare the size of the IV estimates in the municipality level analysis directly with that of the estimates in the individual level analysis, since the latter measured the effect at the bottom of the educational distribution. However, both levels of analysis point to no significant positive effects from education to voter turnout.

Robustness check with an alternative formulation of the instrumental variable

As noted above, the design of the instrumental variable, by construction, assumed that people had received their education in the same municipality as where they currently reside. As an alternative, one can assign the treatment status of the individuals more accurately on the basis of information on their mothers‟ municipalities of residence in 1960, at the time when the reform took place. Once this individual data is aggregated to municipality level, one can gain more accurate figures for the proportion of the electorate who had received their education in the new school system.

Even if the alternative definition of the instrument leads to potentially better accuracy, the identification of the effects of education on voter turnout can still fail. This is due to the possibility that the treated and non-treated individuals may have become regionally mobile in different ways. One can still hope that municipality fixed effects and county level trends, once added to the models, will alleviate this possible source of bias.

Appendix 5 reports the alternative set of results. The correlations between the two definitions of the instrument are high (0.99 in 1970 cross section, and 0.84 in 1980, Table A3). The conclusions remain largely the same (Tables A4 and A5. The most notable difference is that the first stage estimation produces somewhat larger than expected effects of the reform on years of schooling (Table A4),

6 Conclusions

This study has measured the long-term effects of education on voter participation and various measures of political activity. A staged school reform was used as an instrumental variable for years of education. In interpreting the results, it is important to emphasise the context of the reform. Firstly, while affecting everyone, the reform effectively increased years of schooling only at the bottom of educational distribution. The impact of the reform on the average years of schooling at low levels of attainment was considerable: The segment of the population with lowest qualifications received on average nearly a full year of additional schooling. The strength of the reform as a quasi-exogenous source of educational variation is notable compared to earlier studies. Secondly, the reform not only increased the level of education, but imposed a more egalitarian compulsory system, in which the tracking of pupils begins after nine years, instead of seven.

The main result is, that the school reform and the increase in level of education implied by it, did not increase voter turnout. The conclusions can be confirmed at two levels of analysis. At the individual level, survey responses were used to measure the impact of additional years of education resulting from the reform on the voting behaviour of those at the bottom of the educational distribution. At the municipality level, the impact on voting patterns was measured using municipality level panel data with municipality fixed effects.

In relation to voter turnout, the results contribute to the accumulating evidence in which institutional changes are used, such as Milligan *et al*. (2004) and Siedler (2009). The results from these studies have not been in full agreement. The findings of this study support the view that political activity of people is driven by other things than education.

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Table 1 Timing of the school reform, municipality level

	Dependent: Birth year of the first reformed cohort
log Income	-2.265 (1.356)
log Income dispersion	1.689 (1.042)
% Tertiary education	-12.128 (15.088)
% Secondary education	9.082 (8.848)
% Primary education	.054 (2.096)
% working in Services	-2.436 (4.052)
% working in Manufacturing	1.524 (3.282)
% out of labour force	.607 (8.405)
% Married at least once	-.806 (5.568)
log Municipal population	-.023 (.168)
% voting for the Labour party	-.274 (.740)
% voting for the Centre party	-.045 (.825)
% voting for the Conservatives	-2.427 (1.509)
% voting for the Communist party	-3.325 (2.607)
% voting for the Christian people‟s party	-.925 (1.346)
Voter turnout	-1.246 (1.431)
% in age 0-17	3.131 (7.002)
% in age 18-34	9.790 (9.260)
% in age 35-64	22.55 (10.68)
Constant	1,950 (16.6)
Observations	650
R-squared	0.06

Notes: Standard errors in parenthesis. All explanatory variables are aggregated from the 1960 census, except for the electoral results and turnout, which are from 1959 elections, and income data, which is from 1967. The log Mean Income is calculated for males aged 20-65. The Income dispersion measure refers to difference between 90^{th} and 10^{th} percentile of the in the same group, within municipalities.

Table 2 Summary statistics of the individual-level sample

Sample:	Full	Subsample: Treatment defined		
		All	Nontreated	Treated
Observations:	2117	983	373	610
	Mean (S.D.)	Mean	Mean	Mean
Treated		.621	0	1
Female	.468 (.499)	.447	.442	.449
Age	33.27 (7.17)	33.17	36.23	31.31
Voted	.824 (.381)	.790	.804	.782
Highest reported Primary/Secondary education				
7 years	.032 (.175)	.068	.180	0
7+1 years	.100 (.300)	.215	.566	0
7+2 years	.045 (.207)	.097	.255	0
9 years	.288 (.453)	.621	0	1
Vocational 1-yr program	.054 (.226)	0	0	0
Middle school	.133 (.339)	0	0	0
Vocational 2-yr program	.014 (.118)	0	0	0
High school	.335 (.472)	0	0	0
Years education[1]		8.650	8.080	9.000
Years education (predicted)[2]	12.327 (2.326)	10.309	9.351	10.896
Data by year and region				
Survey: 1977	.215 (.411)	.198	.298	.138
Survey: 1981	.084 (.277)	.085	.054	.105
Survey: 1985	.155 (.362)	.157	.110	.185
Survey: 1989	.195 (.396)	.205	.196	.211
Survey: 1993	.351 (.478)	.354	.343	.361
Region: Oslo & Åkershus	.204 (.403)	.135	.110	.151
Region: Eastern Norway	.266 (.442)	.287	.327	.262
Region: Western Norway	.309 (.462)	.329	.295	.349
Region: Trondelag	.108 (.310)	.098	.088	.103
Region: Northern Norway	.112 (.316)	.152	.180	.134

Notes: Respondent will get value zero for variable „Voted" also if they refuse to answer. (1) The years are counted to be either 7, 8 or 9, if the person reported their highest primary/secondary schooling to be any of these. (2) Predicted years of education is based on a mapping of education categories to self reported years of schooling (see Appendix 2 for details).

Table 3 The effect of the reform on years of education

Dependent:	Years of education
Sample:	Educ. ≤ 9 / Obs = 983
Estimator:	1st stage of IV-Probit
New school system	.878 (.045)
Female	.014 (.026)
Birth cohort 1947	-.011 (.102)
Birth cohort 1948	-.122 (.094)
Birth cohort 1949	.053 (.107)
Birth cohort 1950	.099 (.101)
Birth cohort 1951	.081 (.106)
Birth cohort 1952	.075 (.093)
Birth cohort 1953	.089 (.089)
Birth cohort 1954	.033 (.088)
Birth cohort 1955	.095 (.081)
Birth cohort 1956	.089 (.092)
Birth cohort 1957	.071 (.091)
Birth cohort 1958	.037 (.085)
Birth cohort 1959	.058 (.081)
Birth cohort 1960	.033 (.083)
Birth cohort 1961	.020 (.083)
Survey 1981	.070 (.052)
Survey 1985	.086 (.046)
Survey 1989	.037 (.047)
Survey 1993	.015 (.044)
Region: Oslo & Åkershus	.070 (.038)
Region: Eastern Norway	.051 (.034)
Region: Trondelag	.005 (.041)
Region: Northern Norway	-.055 (.039)
Constant	8.008 (.078)
R-squared	.57

Notes: Standard errors in parentheses.

Table 4 Education and voter turnout

Dependent:	Voted	Voted	Voted
Sample:	Full	Educ. ≤ 9	Educ. ≤ 9
Estimator:	Probit	Probit	IV-Probit
Yearseducation*	.018 (.004)		
Years education		.027 (.025)	.013 (.041)
Female	.041 (.016)	.064 (.026)	.064 (.026)
Observations	2117	983	983

Notes: (*) Refers to predicted years of education. All models include Cohort dummies, Survey dummies and Region dummies. Reported coefficients are marginal effects. Standard errors are in parentheses and have been corrected for heteroscedasticity. The sample denoted "Educ. ≤ 9" refers to sample in which the treatment status is defined. Summary statistics for this sample are in the second column of Table 2.

Table 5. The effect of education on measures of civic outcomes.

Sample: Educ. ≤ 9	Mean	Obs.	Probit	IV-Probit
Interested in politics	.463	983	0.043 (.033)	0.015 (.053)
Easy to decide a candidate	.689	870	0.036 (.033)	-0.063 (.051)
Discuss politics	.577	633	-0.034 (.040)	-0.016 (.067)
Actions taken:				
Contacted representative	.129	703	0.030 (.025)	0.032 (.039)
Written a complaint	.040	703	0.007 (.013)	-0.002 (.024)
Taken an issue to party/union	.073	703	0.010 (.018)	0.028 (.028)
Written in paper	.023	703	-0.014 (.011)	-0.012 (.020)
Demonstrated	.071	703	0.014 (.017)	0.006 (.028)
Signed a petition	.185	703	0.083 (.032)	0.131 (.048)

Notes: Variable „Easy to decide a candidate" is missing from 1993 survey, and „Actions taken" variables are missing from 1977 and 1981 surveys. All models include a female dummy, cohort dummies, survey dummies and region dummies. Reported coefficients are marginal effects of years of schooling. Standard errors are in parentheses and have been corrected for heteroscedasticity. The sample is restricted to those whom the treatment status is defined (less than 9 years of education). Variable „Interested in politics" is coded as 1 if the respondent is "very" or "fairly" interested in politics. „Easy to decide" is coded to 1 if „very" or „fairly" easy to decide. „Discuss politics" is coded as 1 if the respondent discusses politics at least „a couple of times a week".

Table 6 Summary statistics for the municipal panel data

	1960	1970	1980
	Mean (S.D.)	Mean (S.D.)	Mean (S.D.)
% went to reformed school	.000 (.000)	.020 (.033)	.194 (.059)
Mean years of education	7.914 (.340)	8.445 (.419)	9.130 (.445)
% of population in ages 0-17	.321 (.033)	.302 (.035)	.276 (.032)
% of population in ages 18-34	.191 (.026)	.211 (.031)	.245 (.024)
% of population in ages 35-64	.369 (.029)	.346 (.027)	.320 (.023)
% of population in ages 65+	.119 (.028)	.142 (.037)	.159 (.041)
% Voter turnout	.763 (.073)	.825 (.057)	.819 (.042)

Notes: municipality averages of 370 municipalities each year.

Table 7 Effect of the reform on years of education, municipal panel data

	(1)	(2)
Dependent:	Mean years of education	Mean years of education
Model:	IV 1st stage	IV 1st stage
% went to reformed school	.624 (.131)	.425 (.122)
% of population in ages 0-17	4.364 (.313)	4.199 (.299)
% of population in ages 18-34	4.584 (.363)	4.406 (.344)
% of population in ages 35-64	3.887 (.364)	3.627 (.351)
Constant	4.204 (.255)	-119.383 (3.564)
Year effects	Yes	Yes
Municipality fixed effects	Yes	Yes
County trends		Yes
Observations	1110	1110
R-squared	.98	.98
Number of municipalities	370	370

Notes: Standard errors are in parentheses and have been corrected for heteroscedasticity.

Table 8 Education and voter turnout, municipality level

	(1)	(2)	(3)	(4)
Dependent:	Turnout	Turnout	Turnout	Turnout
Estimator:	Fixed effects	Fixed effects	IV	IV
Years education	.011 (.013)	-.002 (.012)	-.162 (.070)	-.091 (.086)
County trends		Yes		Yes
Observations	1110	1110	1110	1110

Notes: All models include municipal fixed effects, age controls and year dummies. Standard errors are in parentheses and have been corrected for heteroscedasticity.

Appendix 1 Construction of the sample using Norwegian Election Study data

The Norwegian Election Study is a nationally representative rotating panel carried out every four years by phone, after the parliamentary elections (for documentation, see Kiberg *et al*. 2000). The sample used in this study is first restricted only to individuals who are in any of the survey sweeps 1977, 1981, 1985, 1989 or 1993 and are born between 1946-1961. After dropping a few individuals who give conflicting information about their gender or year of birth in consecutive surveys, we are left with 2131 individuals.

Due to the rotating panel, most individuals in the data have data available in two surveys. Using individual fixed effects would be of no use in the study because the instrumental variable is defined at the individual level and treatment status does not change. Thus, only one observation per individual is selected. The educational information is always taken from the newest possible survey, since some respondents might still be in school. Data quality in terms of voting is the worst in survey sweeps 1981 and 1985. In 1981, information on voting is missing for roughly 40% of respondents, while in 1985 the voter turnout rates are suspiciously high at 98%. Thus, all outcome variables in this study are always taken from sweeps 1977, 1989 or 1993 if multiple responses per individual are available. If multiple responses are from 1981 and 1985, the 1985 response is selected. If responses are from 1989 and 1993, the 1993 response is selected.

Finally, observations for which any of the outcome variables used in the study is missing are dropped. One is left with 2117 individuals, who are interviewed in the following sweeps: 455 in 1977, 177 in 1981, 329 in 1985, 412 in 1989 and 744 in 1993. These individuals are tabulated below by their birth year and type of highest attained primary/secondary education.

Table A1 highest attained primary/secondary education by birth cohort

Cohort	[1] 7 Years	[2] 7+1 Years	[3] 7+2 Years	[4] 9 Years	[5] Vocational 1-yr prg.	[6] Middle School	[7] Vocational 2-yr prg.	[8] High School	Total
1946	14	40	17	8	7	30	2	37	155
1947	7	26	8	12	10	28	3	51	145
1948	12	40	6	10	10	25	2	30	135
1949	6	20	10	12	6	39	4	43	140
1950	5	19	11	19	6	29	3	34	126
1951	6	18	11	18	5	23	0	43	124
1952	3	15	7	34	11	21	2	42	135
1953	2	11	6	41	2	22	3	33	120
1954	3	9	3	47	7	20	3	46	138
1955	0	9	4	52	4	15	4	55	143
1956	3	1	5	54	5	15	3	51	137
1957	3	2	5	59	18	9	0	46	142
1958	1	0	1	64	5	1	0	48	120
1959	0	0	1	59	7	2	0	41	110
1960	1	0	0	66	5	1	1	48	122
1961	1	1	0	55	6	1	0	61	125
Total	67	211	95	610	114	281	30	709	2117

Notes: The survey respondent can report only on of the eight categories.

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 clearly shows how the proportion of people that go through the new system increases with later cohorts.

As the data are self-reported, it is possible that some misclassifications regarding schooling 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 610 individuals of the column 4 are used as the treatment group, while the 373 individuals in columns 1-3 are the control group.

Appendix 2 Predicted years of education

Of the 2117 individuals in the sample, 1547 have responded to a question about years of schooling. The non-response is more concentrated on some survey sweeps than others, particularly for the years 1985 and 1993. To obtain the years of education for everyone, we estimate the following model for the sub-sample of 1547 observations, from which the predicted education can be calculated for all 2117 persons.

$$\text{(A4.1)} \quad Years_i = \alpha + \beta_n \sum_{n=1}^{8} EdType_{n,i} + \delta Female_i + D_{cohort} + D_{region} + u_i$$

EdType refers to the type of highest primary/secondary education, as reported by the respondents (see Table A1). The results are in the Table A2 below.

Table A2 Predicting years of education

Dependent:	Self-reported years of education
Sample:	Full, if dependent available
Estimator:	OLS
7+1 years	.406 (.311)
7+2 years	1.935 (.358)
9 years	2.526 (.295)
Vocational 1-year program	2.839 (.337)
Middle School	3.739 (.302)
Vocational 2-year program	4.265 (.456)
High School	6.738 (.290)
Female	-.560 (.097)
Region: Eastern Norway	-.117 (.129)
Region: Oslo & Åkershus	.377 (.140)
Region: Trondelag	.030 (.168)
Region: Northern Norway	-.086 (.169)
Cohort 1947	.131 (.252)
Cohort 1948	.205 (.257)
Cohort 1949	.393 (.251)
Cohort 1950	.337 (.272)
Cohort 1951	.378 (.266)
Cohort 1952	.476 (.259)
Cohort 1953	.473 (.272)
Cohort 1954	-.001 (.264)
Cohort 1955	-.159 (.260)
Cohort 1956	.013 (.267)
Cohort 1957	.142 (.261)
Cohort 1958	-.279 (.289)
Cohort 1959	-.263 (.293)
Cohort 1960	-.361 (.278)
Cohort 1961	-.955 (.284)
Constant	8.688 (.324)
Observations	1547
R-squared	.61

Appendix 3 How 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, 665 for which the year of school reform is known, based on work by Ness (1971), and Black *et al.* (2005). For the remaining 67 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 it has been 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 former municipalities with different reform years, the share of voters who are reformed can be calculated from individual level data, and individuals can be assigned with the correct treatment status based on their birth cohort and residential status. This procedure leaves us with 370 municipalities. Increasing the sample size beyond this would compromise the quality of the instrument.

Appendix 4 Variable definitions and sources for municipality level data

% went to reformed school

This variable is calculated from census datasets for 1970 and 1980. For 1960 the values will be zero for all municipalities. Individual treatment status is based on the year of birth and the year in which the reform was started in the municipality The proportion of treated voting age population in each municipality is calculated from individual level data using the 1980 municipality structure. Since the reform years of some municipalities are unknown, some 1980 municipalities will be left out, as explained in Appendix 3.

Mean years of education
% of population in age 0-17
% of population in age 18-34
% of population in age 35-64
% of population in age 65+

The average years of schooling are aggregated from individual years of schooling using the 1980 municipality structure, for each census year 1960, 1970 and 1980. The procedure is the same for age groups.

% Voter turnout

The voter turnout is obtained from Norwegian municipal database (NSD kommunedatabase). The data are the official results of parliamentary elections of 1961, 1969 and 1981, each of which is one year apart from the censuses. The units of observation are municipalities with 1980 municipality structure.

Appendix 5 Alternative way to define the instrumental variable in the panel data

In constructing the instrumental variable in Section 5, we have assumed that people are immobile across time. The alternative definition of the instrument, presented here, 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[11]. 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.

The share of treated individuals is then aggregated to the municipality level using the 1980 municipality structure for years 1970 and 1980. For 1960 the values will be zero for all municipalities. Table A6 below summarises the two alternative instruments at the municipality level.

Table A3 Summary statistics of alternative definitions for the instrument

	1960	1970	1980
	Mean (S.D.)	Mean (S.D.)	Mean (S.D.)
% Treated (original definition)	.000 (.000)	.020 (.032)	.194 (.059)
% Treated (alternative definition)	.000 (.000)	.020 (.027)	.162 (.040)
Observations	370	370	370
Correlation across municipalities	1	.991	.841

Below, instrumental variable estimates corresponding to those in Tables 6 and 7 have been replicated, using the alternative definition of the instrument.

11 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.

Table A4 Effect of the reform on years of education, municipal panel data

	(1)	(2)
Dependent:	Mean years of education	Mean years of education
Model:	IV 1st stage	IV 1st stage
% went to reformed school	.843 (.154)	.701 (.159)
% of population in age 0-17	4.264 (.323)	4.077 (.300)
% of population in age 18-34	4.436 (.362)	4.246 (.341)
% of population in age 35-64	3.885 (.362)	3.557 (.351)
Constant	4.265 (.263)	-116.233 (3.821)
Year effects	Yes	Yes
Municipality fixed effects	Yes	Yes
County trends		Yes
Observations	1110	1110
R-squared	.98	.98
Number of municipalities	370	370

Notes: Standard errors are in parentheses and have been corrected for heteroscedasticity.

Table A5 Education and voter turnout, municipality level

	(3)	(4)
Dependent:	Turnout	Turnout
Estimator:	IV	IV
Years Education	-.111 (.063)	-.013 (.065)
County Trends		Yes
Observations	1110	1110

Notes: All models include municipal fixed effects, age controls and year dummies. Standard errors are in parentheses and have been corrected for heteroscedasticity.