Does Education Foster Voter Participation?

An Empirical Study Using a Norwegian School Reform

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Writing this thesis has been interesting and enlightening, but also challenging. It has been motivating to investigate such a debated area of research and to dive into the ocean of politics. The close teamwork has been an enjoyable and advantageous experience.

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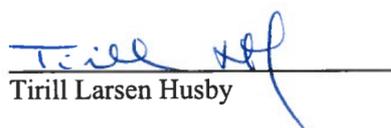
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Abstract

There is a common agreement that education is an essential part of a stable democracy. This is based on the belief that education encourages citizens to participate in democratic processes, and provides them with the knowledge to understand and accept political principles, as well as necessary skills to become politically engaged. A vast body of research has established that there is a correlation between education and voter participation. However, few have been able to estimate the causal effect. We contribute to accumulating research on the causal effect of education on voter participation using the Norwegian school reform from 1960 that extended the years of compulsory schooling from seven to nine, creating an exogenous shock in school attendance among Norwegian pupils. First, we apply the method of instrumental variables (IV). We use Norwegian Election Surveys as our main data source. We find no significant effect of reform implementation on level of education (first stage estimates). Second, we exploit the staggered implementation of the reform in a differences-in-differences approach using municipality level data. We do not find a causal effect of education on voter participation. This is possibly due to the absence of registration laws that represent voting barriers in Norway. Heterogeneity tests reveal a negative effect of education on voter participation in the municipalities with relatively high unemployment and low taxable income per tax payer. These effects are possibly due to citizen migration.

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1 Introduction

Creating a quality democracy is important both for the individual and the society as a whole. It is therefore important that we increase our understanding of what mechanisms that forms our democracy. There is a common agreement that *education* is an essential part of a stable democracy. This is based on the belief that education encourages citizens to participate in democratic processes, and provides them with the knowledge to understand and accept political principles, as well as necessary skills to become politically engaged (Wolfinger and Rosenstone, 1980, Nie et al., 1996, Milligan et al., 2004, Dee, 2004). Friedman (1962) argues that without a minimum degree of literacy and knowledge, as well as acceptance of a common set of values among citizens, a stable and democratic society is impossible.

A wide range of empirical literature has provided evidence of a strong correlation between education and various civic behaviours, in particular is education associated with increased voter participation (Wolfinger and Rosenstone, 1980, Rosenstone and Hansen, 1993, Putnam, 1995).[1] Smets and Van Ham (2013) provide a meta-analysis and find age and education as two of the most important determinants of voter participation across different democratic countries. However, these are not necessarily causal relationships. These results might be biased if both education and voter participation are correlated with unobserved factors specific to individuals, such as ability, family background or childhood culture. For instance, children who grew up in engaged families and communities that stressed civic responsibility might also be more likely to attend to higher education. If such factors exist, to make causal inference would be to misinterpret the results. The aim of this paper is to investigate the causal effect of education on political behaviour by citizens in Norway. More precisely:

"Does the length of education have a causal impact on voter participation?"

Democracy is interpreted in several different ways, but Abraham Lincoln made a definition of democracy as "the government of the people, by the people and for the people" that is commonly used. Political parties are freely created, and constitutes as spokesmen for ideals and political objectives of the population. Citizens are free to exercise their sovereignty by voting at these parties (Becker and Raveloson, 2008). Hall and Jones (1998) argue elections are vital

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[1] See Nie et al. (1996, p.3) for an extensive overview of empirical literature on this issue.

benchmarks of the democratic process because they educate and socialize, and generate dialogue and debate. Pelkonen (2012) argue that although voter participation legitimizes political outcomes and democracy, it should not be presented as a perfect measure of the quality of democratic process. How informed the voters are and their ability to make good choices is also important in this context. However, voter turnout is a measure that receives great attention from the media and social scientists, as it reflects the engagement of people living under democracy.

Statistics Noway (SSB) (2015) reports the official voter participation in total was 78.2% in 2013, thus an increase from 76.4% in 2009 and 77.4% in 2005. SSB (2016) reports descriptive statistics of a higher voter participation among citizens with a university or college degree in the national election in 2013 in Norway. 90% of these citizens voted, against 79% among citizens with only high school education. Among those who only attended primary school the voter participation was 65%.

If education has an effect on citizens voting behaviour, it has important policy implications. The belief in civic returns to education has been one of the most important arguments of government intervention in the education market (Dee, 2004). In Norwegian politics, education is a ground pillar. The Ministry of Education and Research aim at having one of the best educational systems in the world when it comes to participation, implementation and accomplishment. They emphasize that quality of education is crucial for which civic qualities the inhabitants develop (Kunnskapsdepartementet, 2007). If education increases voter participation, one could expect an improvement in the quality of the democratic decision-making in the long run (Pelkonen, 2012).

Milligan et al. (2004) investigate the effect of compulsory schooling laws on the likelihood of becoming politically involved in the United States and the United Kingdom. The researchers exploit compulsory schooling laws and child labour laws in an instrumental variables approach. They find a strong and robust relationship of education on voting in the U.S., but not in the United Kingdom, possibly indicating that U.S. registration rules are obstacles to participation and that education might contribute to citizens overcoming these obstacles. Pelkonen (2012) conducts a similar study by using Norwegian data and exploiting the Norwegian school reform from 1960 in an instrumental variables approach. He finds no effect of additional education on voter turnout.

We conduct a study similar to Milligan et al. (2004) and Pelkonen (2012) but in addition to an instrumental variables approach, we apply a staggered differences-in-differences approach in order to contribute to accumulating research on the relationship between education and voter turnout in Norway. We exploit the Norwegian school reform from 1960 that extended compulsory schooling from seven to nine years in both the instrumental variables approach and the staggered differences-in-differences approach. The Norwegian school reform is very attractive to researchers for several reasons: The reform was implemented at different times in different municipalities and is not systematically correlated with municipality specific characteristics. In addition, the reform increased compulsory schooling by two years, providing a large individual level variation in lower levels of educational attainment, relative to reforms exploited in other studies (Pelkonen, 2012). To the best of our knowledge, a study using the staggered differences-in-differences approach in this context has not been done before.

Given the evidence presented in the similar studies by Milligan et al. (2004) and Pelkonen (2012), we expect no causal effect of education on voter participation. In Norway, as in the United Kingdom, there are no registration laws implying voting barriers. Consequently, education may not lower the cost of voting in Norway. The existence of registration laws would probably make education, providing relevant knowledge and skills to overcome such barriers, an important determinant of voting. If we find no causal effect, our study will supplement the research by Pelkonen (2012).

The findings of this study show no causal effect of education on voter participation. These results are found using a staggered differences-in-differences approach on municipality level, controlling for municipality fixed effects, election year fixed effects and a linear time trend. Our robustness checks strengthen the internal validity of our model.

The remainder of the paper continues as follows. We present background information on the school reform utilized, voting and registration, and compulsory schooling in section 2. Section 3 presents relevant theoretical models, namely the absolute education model and the rational voter model. In section 4 we provide a review of relevant previous literature. We describe our data in section 5, followed by a presentation of our empirical approach in section 6, including the empirical framework, a discussion of the randomness of the reform and the specification of our models. Our results are presented in section 7, which also includes heterogeneity tests.

Section 8 provides robustness checks of our main specification. In section 9 we discuss our results and limitations of our analysis. Finally, section 10 concludes.

2 Background Information

To understand the context of the Norwegian school reform that we exploit in our analysis, we first shortly introduce information on compulsory schooling. Subsequently, we present background information on the reform. Last, we present information about voting and registration rules.

2.1 Compulsory Schooling

During the latter half of the 19th century, school reforms were carried out in many Western European countries (Murtin and Viarengo, 2011).[2] Galor (2005) emphasizes the timing was not random: In the beginning of the industrial revolution, human capital had only a limited role in the production process. Education was motivated by various reasons, such as social control, religion, military control and moral conformity, and supply of public education differed between countries due to political, historical, cultural and institutional factors. In the second part of the industrial revolution, compulsory schooling was institutionalized, reflecting higher demand for skilled workers in the production process in many countries.

2.2 Description of the Norwegian School Reform

In 1959, the Norwegian Parliament legislated a mandatory school reform that increased the minimum level of education in society.[3] The reform can be viewed as a political answer to the problems the Norwegian school system suffered after World War II. In particular, there were differences between the primary education provided in urban and rural parts of the country, social and regional differences in the intake to secondary schools and poor coordination between the different types of secondary education. The objective of the reform was to increase educational attainment, but also to reduce regional disparities and broaden access to further education (Lie, 1973).

Prior to the reform, there were seven mandatory years of primary education. Thus, with a starting age of seven, students finished at the age of fourteen. In addition, a possibility to

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[2] Most of the implemented school reforms extended compulsory schooling by one year or longer. Interestingly, reforms were undertaken by countries with different experiences and traditions in education policy.

[3] See Lie (1973) for a full description of the reform.

continue primary school in continuations schools was provided in some municipalities, lasting for one or two years. Secondary education was designed to prepare students for an academic education, lasting for three or five years. Commonly, secondary education was provided in central places and cities. Vocationally oriented schooling was also available, lasting for one or two years. The new comprehensive system increased the years of compulsory education from seven to nine years. The starting age was still seven years, but the finishing age was now sixteen years. Primary school now consisted of two parts: Lower primary school from class one to six, and higher primary school from class seven to nine. The curriculum of the two additional years concentrated on general education. This reflected increased demand for skilled workers, as in other countries with similar reforms (Lie, 1973, Pelkonen, 2012).

The reform was to be implemented between 1955 and 1975. Originally the reform started as a designed experiment (forsøk), until it was made compulsory by the central government in 1970. [4] Economic subsidies were provided to encourage adoption, primarily to even out differences between rich and poor municipalities. The reform implied drastic reorganizations. While some municipalities already had well developed school systems with secondary schools, others had nothing beyond the primary level. The Parliament recognized this variation in capacity, and gave the municipalities the liberty to decide exact timing themselves (Lie, 1973). As a result, although the reform was officially started in 1960, the implementation was not complete until 1972 (Pelkonen, 2012). This implies that the Norwegian schooling system were divided in two separate systems during the implementation period. The system you were in depended on birth year, and which municipality you grew up in.

The total set of birth cohorts that attended to education in the two different systems corresponds to birth cohorts 1946-1961 (See figure 1). The majority of the municipalities implemented the reform between 1961 and 1972, affecting birth cohorts 1947-1958. Our study is based on birth cohorts 1940-1958, in order to get variation in our data. The experiment municipalities are then also included.

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[4] Twenty-eight municipalities experimented with a school model in the period 1955-1959, affecting birth cohorts 1941-1945. The Norwegian word, "forsøk", which has no adequate English equivalent, is translated to "experiment" in its broadest meaning in this thesis. The model was a merger of the continuation school and lower secondary education. These municipalities can be viewed as the innovators of the reform, and politicians may have used this compromise action in order to gain wider support (Lie, 1973). Only a small number of schools were involved in the experiment, thus the reform was applied to a negligible number of students.

Figure 1: The Rollout of the Norwegian School Reform by the First Affected Birth Cohort
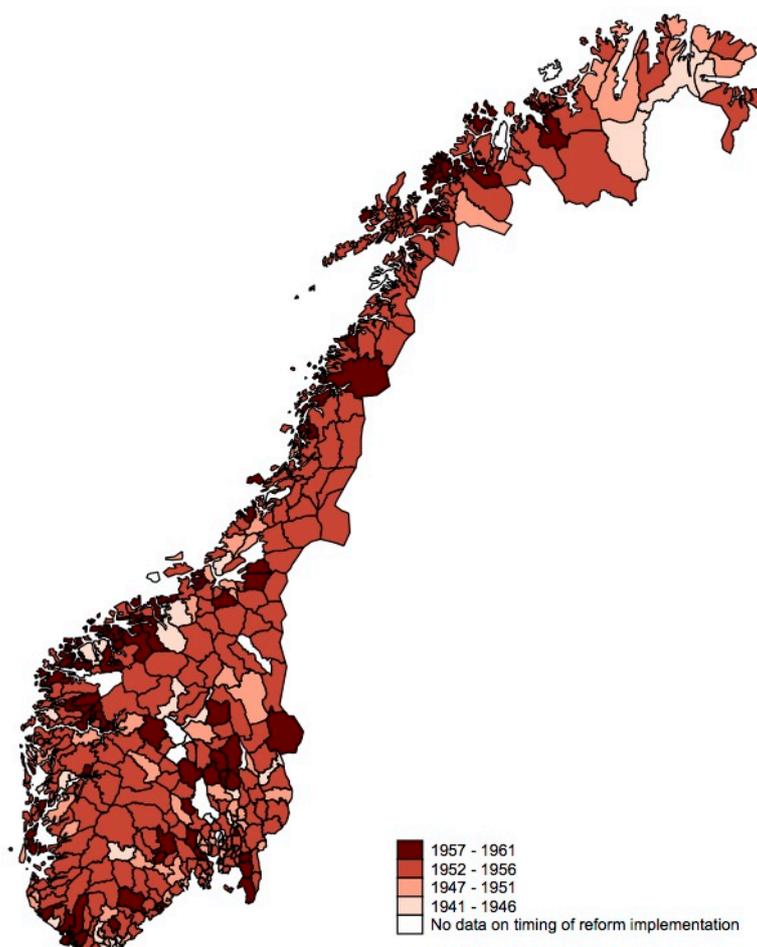
Source: Norwegian Centre for Research Data (NSD)

The school reform we exploit inn our empirical study has been used several times in previous empirical research. Pelkonen (2012) utilize it in an instrumental variables approach, estimating the impact of education on voter turnout. Machin et al. (2012) used the reform to show that the length of compulsory education has a causal impact on labour mobility, whereas Black et al. (2005) utilized the reform in order to identify a causal intergenerational transmission of education from mothers to their children in Norway. Aakvik et al. (2010) measured private returns to education, and Black et al. (2008) found evidence that increased compulsory schooling reduce teenage childbearing in Norway using the same school reform.

2.3 Voting and Registration

The decision of voting is made within an institutional context. To understand this context, we will provide details on the voting system in Norway at the time our data were collected, and highlight how it differed from the system in the United States.

The election system at National Elections in Norway was primarily regulated by The Constitution of Norway (Grunnloven) and The National Election Law (Stortingsvalgloven).[5] Everyone entitled to vote were automatically registered in a municipal register of electors in the municipality they were registered. In order to be entitled to vote, citizens had to fulfil the following requirements: You had to be registered as a Norwegian citizen, you had to reach the legal voting age by the end of the year and you could not have lost your right to vote (Tofte, 1981). In 1979 the legal voting age was lowered from 20 to 18 years (Aardal, 2010). It was important that the decision regarding locations and numbers of polling stations were taken in the electorates interest. The act of voting normally took place in the following manner: At the polling station, voters got a cross in the electoral register and were delivered a ballot paper. The voters then chose a polling booth where they voted privately and unobserved. Further, they folded their papers in order to keep their vote secret and put the paper into the ballot box (Tofte, 1981). The election term was four years. The procedures of voting in Norway has not changed considerably since the time our data were collected.

In contrast to the automatically registration in Norway, in the US, registration was an individual responsibility prior to election day. If a citizen moved, he or she had to re-register on the new address. Registration was often more difficult than the actual act of voting, usually involving obscure information and travelling at inconvenient times. Consequently, registration laws raised the cost of voting. However, each state determined its own registration laws, subject to limitations set by the court, the U.S. Constitution and national legislation. Taxes as a condition for registration, literacy tests, evening and Saturday registration, and required re-registration periodically are some examples of the regulations of registration from 1960 to 1973. Since the latter half of the 19th century there has been a broad liberalization in registration laws, lowering the cost of voting (Wolfinger and Rosenstone, 1980). However, despite substantial

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[5] The fundamental principles of elections are given in The Constitution of Norway (Grunnloven) from 1814. More precise regulations regarding national elections are given in The National Election Law (Stortingsvalgloven) from 1920 (Tofte, 1981).

improvements in balloting and registration procedures, registration laws are still an obstacle for voting in the United States, especially for young and low-income adults (Patterson, 2009). Difficult and complicated registration will matter mostly to citizens with low education because they are less able to cope with bureaucratic challenges (Wolfinger and Rosenstone, 1980).

3 Theoretical Framework

In this section, we will provide a theoretical framework for our analysis by describing the absolute education model and the rational voter model.

3.1 The Absolute Education Model

According to the absolute education model, education has a causal effect on political participation. What individuals learn at school positively affects cognitive ability such as civic skills and political knowledge, which function as the causal mechanisms triggering participation (Persson, 2015). Jackson (1995) argue that education in addition triggers individuals' political efficacy since education increases the belief that they can play a role in the political process by having the competence to understand and participate, as well as an opportunity to influence government actions. According to the absolute education model, individuals are more likely to vote, the more education they have. The effects of education are independent of the level of education in the environment. Most of the literature supporting the view that education directly causes political participation does not present evidence on exactly how and through which mechanisms education affects participation. There is no evidence in the literature on whether the effect is linear or not. Some researchers argue that only higher education is an important determinant of participation (Persson, 2015).

3.2 The Rational Voter Model

Throughout the 1970s, the rational voter model became a popular model of voting. According to this model, electors decide to vote or not, and who to vote for, based on some rational basis, i.e. which action gives the greatest expected benefits. Only if they perceive greater benefits than costs from voting, they will vote (Niemi and Weisberg, 2001). Among other externalities, education has been shown to reduce the cost of voting. Education provides individuals with cognitive skills that make it easier to process complex information. Furthermore, education might improve the socioeconomic position of individuals, which in turn increase voter participation because such groups typically have a great interest in election outcomes. In addition, education might develop an acknowledgement of civic duty by fostering democratic beliefs and values, encouraging participation (Wolfinger and Rosenstone, 1980). Education can also serve to increase the benefits of voting by increasing a feeling of civic duty (Denny and

Doyle, 2008). However, few people decide to vote based on the belief that their vote will make a difference between a candidate´s victory and defeat. Rather, many voters are motivated by an instrumental benefit, meaning they are motivated by the effect of the act of voting on their own immediate wellbeing. Moreover, the marginal effect on the probability of voting is not constant across individuals. For individuals who are almost certain not to vote, the marginal effect of a variable is most likely very small. As the probability of voting increases, the marginal effect of costs and benefits also starts to increase. For individuals who are very certain to vote, the effect of additional benefits and costs on the probability of voting starts to diminish. Thus, individuals who are very certain on their decision regarding to vote or not, are relatively unaffected by small changes in benefits and costs (Wolfinger and Rosenstone, 1980).

4 Literature Review

Education is frequently alleged to be an explanation of a person's civic behaviour. Wolfinger and Rosenstone (1980) notes that educated people are more likely to express interest in politics and a high sense of citizen duty, follow political campaigns in the mass media and be well informed about politics. Similarly, Putnam (1995) states that "Education is by far the strongest correlate that I have discovered of civic engagement[6] in all its forms, including social trust and membership in many different types of groups". His analysis shows that education correlates exponentially with civic engagement in the United States. Going from a bachelor degree to a master degree makes U.S. citizens ten times more likely to engage in civic activity than going from the first to the fourth year in primary education.

Individuals with higher education generally have a greater propensity to vote (Campbell et al., 1960). They are also more likely to feel a sense of civic responsibility about voting, regardless of the cause and how small their vote is against the total number of casts. Wolfinger and Rosenstone (1980) found that education is the most important determinant of voter turnout adding income and occupational measures as control variables in their multiple regression model. They state that education can proxy for other unobservable characteristics like ability and family background. This will be the case if, in example, individuals with high ability or individuals who have grown up in a wealthy neighbourhood select into education and also tend to vote. Then, what seems to be the effect of education on voter turnout is really the effect of ability and family background. This is the case of selection bias.

Several studies from the Unites States have attempted to answer *why* highly educated people have a higher voter participation. One explanation is that education gives the individuals knowledge and skills to surmount the barriers to participation, such as information about politics and the skills to deal with the bureaucracy of voting (Rosenstone and Hansen, 1993). Researchers have argued that it is the relative rather than the absolute education level that has implications for political participation. In other words, it is the individual's level of education relative to the education level of the environment, rather than the skills provided by the

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[6] Putnam (2001) refers to civic engagement as "people's connections with the life of their communities, not merely with politics", noting that civic engagement is correlated with political participation.

education, that matters. Applying Swedish data, Persson (2011) finds supportive evidence of this. Verba et al. (1995) found that once political engagement was controlled for, the correlation between education and voter turnout disappeared. Political interest and partisanship was found to be especially strong predictors. This lead them to conclude that the effect of education on voting occurs through engagement. Interest further increases the likelihood for the individuals to acquire knowledge on political issues, and thus increases likelihood to vote (Tilley et al., 2004). According to Campbell (2006), it is an individual's involvement in the school community, rather than the formal curriculum, that influences the student's decision to vote. Campbell (2008) shows that open classrooms lead to an increase in civic engagement. Open classrooms might foster exchange of ideas between the students, which strengthens the idea that the school community, in particular nature of the political discussion in the classroom, is what affects civic engagement.

Most studies of political participation in western democracies are conducted using cross-sectional data where the causal effect is difficult to isolate due to factors affecting both education and voter turnout (confounding factors), such as family background and individual ability. It might be that in previous research where education is shown to affect voter participation, education is a proxy for such factors. The causal link between education and voter participation is seldom directly investigated (Persson, 2015). Instead, some researchers attempt to explore the direct link from the confounding factors to voter participation.

Pre-adult factors such as socioeconomic status and political socialization in the home environment might be predictors of political activity. With the civic voluntarism model, Verba et al. (1995) finds that activity enhancing factors such as resources and political engagement can be traced back to characteristics acquired at birth and early experiences in family and school, family social class being highly important. These factors are also associated with education. If these factors predict selection of individuals into education, education might be a proxy for such pre-adult factors in explaining political participation.

Researchers have questioned whether voter turnout is affected by individual factors such as ability and personality rather than education, and that this can explain the underlying relationship between education and voter participation. Verba et al. (1995) states that skills relevant for politics are an outcome of education, rather than a proxy. They argue that individuals through education develops the skills that enhances political participation,

especially the skills of speaking and writing. Luskin (1990) finds education to be unimportant and interest and intelligence to be highly important in predicting whether an individual is political sophisticated, described as his or her political cognition being numerous, complex and well organized. He applies a subjective opinion of the interviewer's intelligence taken contemporaneously with the participation data. Here, misreporting might be an issue, and the measure of intelligence might therefore be unreliable. Denny and Doyle (2008) overcome this problem when modelling how education, ability and personality traits affects voter turnout in Britain using ability and personality measures taken of the respondents at a young age. [7] They are the first ones to include personality in modelling individual's decision to vote. First, they find a correlation between education and voter participation when not controlling for ability and personality. They find a weaker relationship in Britain, compared to previous studies using American data. Further, their estimates show that cognitive ability and personality plays a larger role than education in determining voter turnout. Using ability and personality measures taken at a young age, they argue that their measures of ability better reflect innate rather than acquired ability and the personality measure reflect the true personality, hence overcoming the problem of misreporting. Since Denny and Doyle (2008) find that cognitive ability and personality has greater effect on voter participation than education, it suggests that ability and personality might be a proxy for education, rather than an outcome. In other words, voter participation might be affected by pre-adult factors that also correlates with education. This shows that omitting ability and personality seems to have caused bias in earlier research aiming to prove a causal link between education and voter participation.

In order to make causal inference, the comparison of individuals who vote and do not vote must on average be ceteris paribus, i.e. all else equal. Failure to include all control variables that is correlated with both education and voter participation result in biased estimates. There is no guarantee that a relationship has a causal force even when regressions are insensitive to the inclusion of additional control variables (Angrist and Pischke, 2015). The ideal research design would be to randomly allocate individuals into different lengths of education. This could, however, never be implemented due to practically and ethically challenges (Persson, 2015).

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[7] Denny and Doyle (2008) use a measure of cognitive ability based on a test which is taken at age 11, and argues that this measure in a better way reflects innate rather than acquired ability. The personality variable is based on evaluations the respondent's teacher made when the respondents were 16 years old. The researchers argue that a personality measure taken at this age is likely to show the individual's true personality since measures taken later in life might in addition reflect more idiosyncratic attitudes and life experiences.

More recent research utilizes sophisticated regression techniques in order to identify a causal relationship between education and civic participation, consequently isolating the effect of education. Dee (2004), Milligan et al. (2004) and Pelkonen (2012) apply an instrumental variables approach, in studies similar to ours. Dee (2004) uses variation in teen exposure to restrictive child labour laws as an instrument applying data from the General Social Surveys (GSS). He finds that one extra year of schooling increases voter participation by 6.8 percentage point in the United States. He finds that other measures of civic engagement, in particular newspaper readership, membership in groups and support of free speech, also are largely and positively affected by education. In addition, he investigates the effect of post-secondary/college education on voter participation in the United States by utilizing distance to college as an instrument using data from High School and Beyond (HA&B), a major longitudinal study that contains interviews of a cohort that was high school sophomores in 1980. He finds that college entrance increases voter participation by 17 percentage points. However, the instruments that Dee (2004) utilizes might be problematic. Distance to college might be correlated with other variables affecting voter participation that are not measured. One such factor might be the parent's circle of acquaintances. The change in child labour laws might not be a good instrument because it might not provide an exogenous variation in individual's education level that is evenly spread out in the population. The change in child labour laws might affect individuals from low socio-economic background the most. Because of this, the estimates might be biased (Persson, 2015). Milligan et al. (2004) investigates the effect of extra schooling induced through increases in years of compulsory schooling on voter turnout in the United States and the United Kingdom. They exploit changes in compulsory attendance laws and child labour laws as instrument variables. The timing of the law changes within each state is unrelated to any individual characteristics that affects voting, conditional on state of birth, cohort of birth and election year, thus these instruments are better than the instruments applied by Dee (2004) in terms of causality. They use data from the annual National Election Studies and the November Voting Supplements to the Current Population Study for the US analysis, and the British General Election Studies and the Eurobarometer Surveys for the UK analysis Applying this instrument, they identify the local average treatment effect. In other words, they identify the effect for would-be-dropouts. In the US, the effect of education on voter turnout is found to be strong. When restricting the sample to include only registered citizens, the increase in voter turnout was minor in comparison to the whole sample. These findings lead them to conclude that increases in school leaving age leads more youth to register. In other words,

registration laws act as a barrier to participation. Estimates for the United Kingdom, where there are no such registration barriers, show no similar effect of education on voter turnout. These findings emphasize the role of registration laws on voter turnout.

The instrumental variables approach using increase in years of compulsory schooling is also applied in a study by Pelkonen (2012), to investigate the effect of education on voter participation and other civic activities in Norway. The reform implemented was not correlated to any socio-economic characteristic, thus the implementation appears to be quasi-random. It creates an exogenous shock in the individual's level of schooling that is evenly spread in the population, coming close to the ideal randomization design. Pelkonen (2012) argues that the Norwegian reform is the most convincing compulsory schooling reform available to social scientists. This is the same school reform that we apply in our analysis. Pelkonen (2012) estimates the effect of education on voter participation and other civic outcomes using both individual and municipality level data. The data source for the individual level analysis is the National Election Studies interviewing random samples in correspondence to the five national elections in the years 1977 to 1993. The municipality level data source is the national Census of Norway from the years 1960, 1970 and 1980, and matching this to voting data from the Norwegian municipality database. He finds no effect of education on voter participation in either of the estimated models. Regarding other civic outcomes,[8] only the effect on signing a petition was significant showing a positive effect of education. Persson (2015) argues that, given the solid research design, the results of no causal impact of education on voter participation is a "persuasive evidence" of the idea that education is a proxy for pre-adult characteristics. However, these empirical evidences are not able to confirm how education affects voter participation.

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[8] Other civic outcomes that Pelkonen (2012) models in his analysis is: interested in politics, easy to decide a candidate, discuss politics, contacted representative, written a complaint, taken an issue to party/union, written in paper, demonstrated and signed a petition.

5 Data

This section provides a description of the data material applied in the instrumental variables approach and in the staggered differences-in-differences approach, as well as challenges with the data.

5.1 Data Applied in the Instrumental Variables Approach

Our primary source of data is the National Election Surveys (NES) from 1977 and 1989 prepared and made available by The Norwegian Centre for Research Data (NSD).[9]

The NES provide information on whether the respondents voted or not in the Norwegian National Elections in 1977 and 1989. We have data on the dates the reform was implemented in each municipality, obtained by Black et al. (2005), and we link this to the National Election Surveys, in order to ascertain whether the respondents have been affected by the reform. The NES data only provide information on county level, thus we cannot tell which municipality the respondents grew up in. This makes us unable to state for certain whether the respondents have been directly affected by the reform. Still, we are able to create a probability of whether the respondents have been affected by the reform by constructing the share of individuals in each birth cohort in each county which were directly affected by the reform. This treatment intensity serves as our instrument for education.

We include birth cohorts for the years 1940 to 1958. Using this many cohorts should provide us with a lot of variation in our dataset. Each individual is linked to their childhood county. Each county consists of municipalities according to 1960 classification.

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[9] We apply "Norwegian Election Study, 1977" and "Norwegian Election Study, 1989". The surveys were financed by The Research Council of Norway and Kommunal- og Arbeidsdepartementet. Data are provided by Henry Valen, Bernt Aardal, Insitute for Social Research (ISF) and Statistics Norway (SSB), and prepared and made available by Norwegian Social Science Data Services (NSD). Neither Henry Valen, Bernt Aardal, Kommunal- og Arbeidsdepartementet, ISF, SSB nor NSD are responsible for the interpretation and presentation of the data.

National Election Surveys

The NES consists of a wide range of questions regarding voting behaviour, motivation for participation and political preferences. The sample is randomly selected from the population of Norwegians eligible to vote, ranging from age 18 to 79. The data is collected by personal interviews. The aim of the surveys is to study trends in the Norwegian politics and unique characteristics of the National Elections. NSD is one of the largest archives for research data in Norway and provides data to researchers and students both in Norway and internationally.

There are two variables of particular interest: education and electoral register. Summary statistics of these variables are provided in table 1. Education corresponds to years of schooling, ranging from primary to higher education. Details on the education variable is shown in table A1. The electoral register variable describes whether the respondents voted or not, 1 meaning voted. At election day, every person who voted was crossed off in the electoral register before he or she put the ballot paper in the ballot box. A common challenge with election surveys in general is the case of misreporting, since respondents may be reluctant to admit they did not vote (Milligan et al., 2004). By applying the electoral register, this challenge is not a problem to us.

Figure 2 reveal that the more educated the respondents are, the higher the voter participation tends to be. In particular, the difference between respondents with seven and nine years of schooling is of interest. However, whether the increase in voter participation is a result of education or other factors, cannot be stated without further empirical research.

Table 1: Summary Statistics of Education and Electoral Register

	Observations	Mean	Standard deviation	Min	Max
Education	1,102	12.226	3.311	7	24
Electoral register	1,102	0.870	0.336	0	1

Figure 2: Mean of Voter Participation by Years of Education
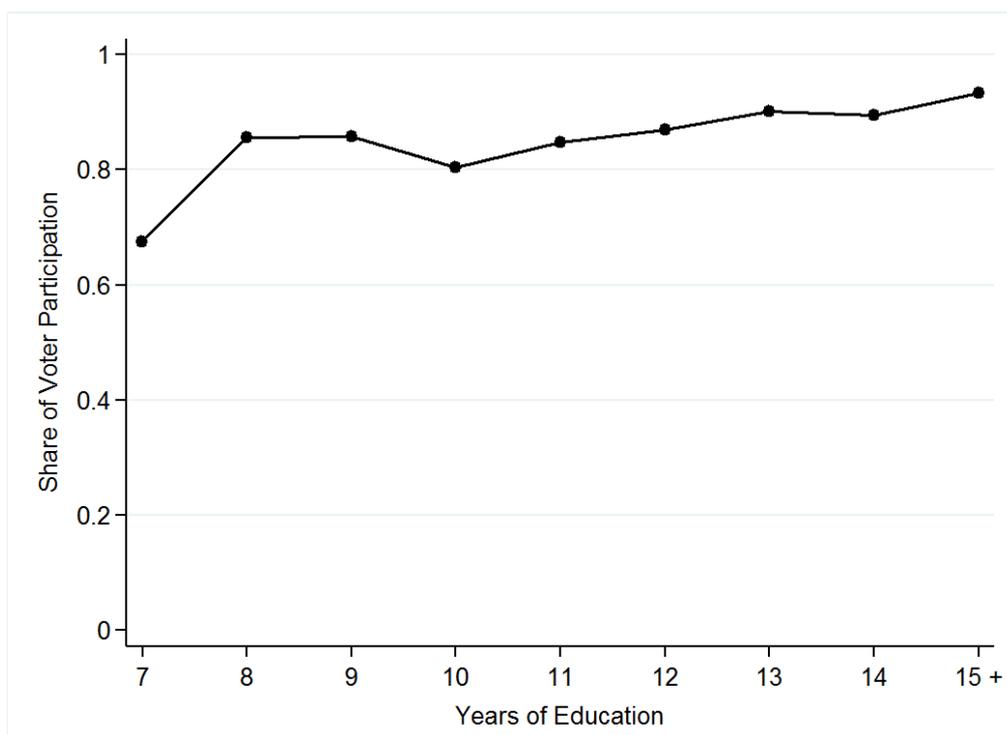
Source: NSD

5.2 Data Applied in the Staggered Differences-in-Differences Approach

The voting registry provided by NSD is our primary data source. For our main specification, we have panel data on Norwegian municipalities in the years of the National Elections every fourth year from 1953 to 1981. This contains number of votes and inhabitants eligible to vote, allowing us to calculate the share of eligible citizens that votes in each municipality, specified as voter participation. Summary statistics of the variable voter participation, grouped by election year is presented in table 2. We take into account that from 1953 to 1977, citizens were eligible to vote at the age of 20, whereas from the election in 1981 the legal voting age was lowered to 18 years. We link this information to data on when each municipality implemented the reform. Thus, we are able to identify whether there were citizens in the electorate of each municipality that had been enrolled in the reform. This allows us to analyse whether increased years of education had an impact on voter participation. The municipalities are specified according to 1960 classification.

Table 2: Summary Statistics of Voter Participation by Election Year

Election Year	Observations	Mean	Standard deviation	Min	Max
1953	706	0.761	0.073	0.390	0.908
1957	706	0.754	0.074	0.450	0.889
1961	706	0.759	0.075	0.487	0.911
1965	706	0.832	0.056	0.625	0.924
1969	706	0.821	0.052	0.621	0.929
1973	706	0.787	0.050	0.388	0.891
1977	706	0.815	0.042	0.652	0.902
1981	706	0.817	0.038	0.630	0.897

Also, we have data from NSD on unemployment rates, taxable income per tax payer, voter participation by gender and inhabitants in each municipality that we use in order to separate our sample in the heterogeneity tests. Unemployment by definition is citizens actively searching for a job. For our robustness checks we add the data applied in our main specification for the National Elections in 1985 and 1989. For our event study we apply data on voter participation from the National Election in 1949.

We do not have data on number of citizens in the years 1953, 1957 and 1961, which is solved by using population census from the corresponding decade. We assume the number of citizens is fairly stable over such a short time span, thus this should not be problem.

5.3 Challenges

26 municipalities implemented the reform during the merger of municipalities. We do not have data on the timing of the reform implementation of these municipalities, thus they are not included in the analysis. If these differ from the rest of the municipalities, not including them in our sample could lead to selection bias. However, these missing municipalities constitute a small share of the sample. A challenge of greater importance is that our data do not account for migration. Therefore, in our analysis, we assume the individuals still live in the county they grew up.

There are three challenges that are specific to the NES data. First, the NES data does not contain information on childhood county of 350 respondents. Therefore, we are unable to identify their treatment intensity and they are eliminated from our analysis. The majority of the eliminated observations are from the lower end of the educational distribution possibly indicating that any results using the NES data will suffer from selection bias. Second, individuals can decline the inquire to respond to the survey. In the NES from 1989 the response rate was 73.2 % of the drawn sample, while it was 72.5% in the NES from 1977. If the individuals that refuse to respond differ from the rest of the sample by characteristics that determine voter participation and are correlated to education, the problem of selection bias arises. We do not have the opportunity to test this. Third, since we are estimating the effect of an increase from seven to nine years of education induced by the reform, we might meet a challenge of finding an effect because there are few respondents with only seven years of education. In other words, there are few possible compliers in our data.

6 Empirical Approach

In this section, we provide a theoretical overview of the empirical framework relevant for our analysis, followed by a discussion of the randomness of the reform and a presentation of our models.

6.1 Empirical Framework

In order to estimate the effect of education on voter participation, the method of ordinary least squares (OLS) that minimizes the sum of squared residuals, can be applied. This tells us how the expected average value of political participation changes with an increase in compulsory schooling. The causal effect is obtained when comparing the treatment and the control group ceteris paribus, all else equal. This is best achieved with random assignment of individuals into the treatment and control group. The crucial assumption for causal inference, zero conditional mean, is then satisfied.[10] Due to practical and ethical reasons, random assignment of individuals into different lengths of education is impossible. When individuals with certain characteristics that determines the outcome of interest are more likely to select into education than others, the problem of selection bias arises. Hence, variables correlated with both the dependent and the independent variable are left in the error term. Examples of such variables are ability, family background and environmental factors. The OLS estimator will then suffer from omitted variable bias, causing the problem of endogenous independent variables in the regression model. Thus, when estimating the effect of education on voter participation using OLS, we are only able to estimate the correlation, not the causal effect (Wooldridge, 2014).

In order to overcome the problem of selection bias, and thus estimate the causal effect of education on voter participation, we apply the method of instrumental variables and the method of staggered differences-in-differences as our identification strategies. We apply these methods exploiting the staged Norwegian school reform that extended the years of compulsory schooling from seven to nine. This created an exogenous shock in level of schooling, which enables the application of our identification strategies. In addition, compared to reforms exploited in other studies, this Norwegian school reform provides a large individual level variation in lower levels

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[10] The zero conditional mean assumption states that the error term must be normally distributed with a mean value of zero and the average value of error term must not depend on the value of the independent variable, $E(u|x) = 0$. In addition, causal inference requires a model that is linear in parameters and a error term that is homoscedastic and not serially correlated.

of educational attainment. Finally, it was implemented at different times across different municipalities.

Method of Instrumental Variables

The method of instrumental variables (IV) can be applied in order to solve the problem of the endogenous explanatory variables in OLS regressions, hence leaving us with the causal effect. Simply put, the IV approach leaves the unobserved variable in the error term, but still recognizes the presence of it. The simple regression is in general written as

$$y = \beta_0 + \beta_1 x + u$$

In order to obtain consistent estimates of β_0 and β_1 we need a new variable, z, that satisfies the following two assumptions,

$$(1) \ z \text{ is uncorrelated with } u : \ Cov(z, u) = 0$$
$$(2) \ z \text{ is correlated with } x : Cov(z, x) \neq 0$$

where z is the instrument variable for x. When assumption (1) is satisfied, we often refer to *instrument exogeneity,* meaning that z have no partial effect on y and is uncorrelated with the error term. This assumption cannot be tested, but must be argued with economic behaviour and theory. Satisfaction of assumption (2) is referred to as *instrument relevance*, meaning that z is correlated with the endogenous variable x, thus relevant in explaining variation in x. This assumption can be tested, given a random sample from the population, simply by regressing x on z,

$$x = \theta_0 + \theta_1 z + v$$

Only if there is a correlation between the instrument variable and the endogenous variable, $\theta_1 \neq 0$, instrument relevance is obtained. Both instrument relevance and instrument exogeneity must be attained in order for z to serve as an instrument variable for x (Wooldridge, 2014).

Rollout

As a second approach to estimate the causal effect of education on voter participation, we utilize a staggered differences-in-differences (DID) setup. This method is previously explored by, among others, Akerman et al. (2015) and Bütikofer et al. (2015). We exploit the rollout of the Norwegian school reform that increased compulsory education by two years. Rollout refers to the reform being implemented at different times in different municipalities across the country. This is an advanced form of the DID approach. In order to explain the staggered DID setup, we find it useful to first deduce the DID approach.

The DID approach is based on exploring the differences in outcome between a treatment and a control group, where the treatment hits the whole treatment group simultaneously. The ideal control group is the true counterfactual to the treatment group, that is the treated group in the absence of treatment (the outcome of the treated group in the absence of treatment is referred to as potential outcome of the treatment group). In our case that would be the children that were affected by the reform had they not been affected by the reform. However, this is impossible since the true counterfactual is not observable. Instead, the DID approach is based on comparing the treatment group to a control group that displays what would have happened to the treatment group in the absence of treatment. The crucial assumption is that the treatment group would follow the same trend in outcome as the control group, in the absence of treatment. Thus, the control group creates a path that displays the potential outcome of the treated group. A post-treatment divergence from the path would indicate a treatment effect (Angrist and Pischke, 2015).

The deduction of the DID approach in its simplest form can be displayed using the following framework: D = 1 denotes the treatment group and D = 0 denotes the control group. Y(0) is the outcome variable at time $t = 0$ while Y(1) is the outcome variable at time $t = 1$. Y_1 is the outcome Y when treatment has occurred, while Y_0 is the outcome in the absence of treatment.
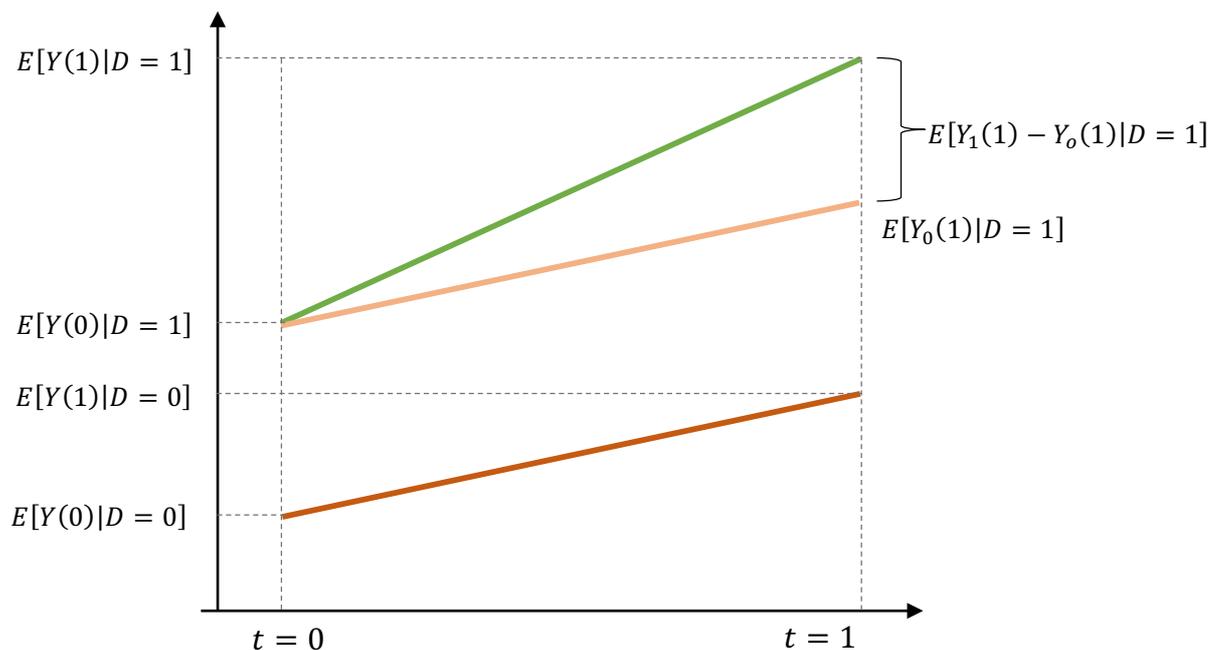
We want to estimate $E[Y_1(1) - Y_o(1)|D = 1]$. This is the difference between the outcome and the potential outcome of the treated group, indicating a treatment effect.
Since $[Y_0(1)|D = 1]$ is unobserved, the estimator of interest is derived in the following manner:

$$E[Y_1(1) - Y_o(1)|D = 1] = \{E[Y(1)|D = 1] - E[Y(1)|D = 0]\}$$
$$- \{E[Y(0)|D = 1] - E[Y(0)|D = 0]\}$$

This is illustrated in figure 3.

Figure 3: Differences-in-Differences Illustration

The chart shows on the vertical axis, from top to bottom, the labels $E[Y(1)|D=1]$, $E[Y(0)|D=1]$, $E[Y(1)|D=0]$, and $E[Y(0)|D=0]$. On the right side, a bracket denotes $E[Y_1(1) - Y_o(1)|D=1]$ and the label $E[Y_0(1)|D=1]$. The horizontal axis is marked at $t=0$ and $t=1$.

Source: Angrist and Pischke (2015)

The staggered DID approach follows the same idea as the basic DID approach, comparing treatment and control subjects. The difference is that here, all subjects are exposed to treatment during the relevant period, but at different times. The approach exploits the variation in the timing of treatment, allowing for findings of an average treatment effect. The control group consists of subjects that at a given time during the research period had not received treatment. The treatment group consists of subjects that had received treatment at that same point in time. In order to ensure internal validity of the staggered DID setup, the key assumption is that the timing of treatment is uncorrelated to other determinants of the outcome variable. In other words, the reform implementation must be random. For example, this assumption would be violated if the richest municipalities implemented the reform earlier than the poorest. When this assumption holds, the control group works on average as a good counterfactual for the treated group. In other words, each election year those municipalities that have implemented the reform are comparable to those municipalities that have not yet implemented the reform. For example, if reform implementation is random with respect to income in the municipality, it ensures that the treatment and control group are similar in terms of income.

Linear Probability Model

When testing the robustness of our results, we estimate the probability of high voter participation in the municipalites. To perform this analysis, we utilize a linear probability model (LPM). The LPM lets us explain a binary outcome, that is when the dependent variable takes the values of zero and one. This enables us to explain a qualitative event. In the case of the LPM, it is always true that $P(y = 1|x) = E(y|x)$, the probability that $y = 1$ is the same as the expected value of y. This gives:

$$P(y = 1|x) = \beta_0 + \beta_1 x_1 + \cdots + \beta_k x_k$$

The probability that the explanatory variable is one, is a linear function of the x_j. This is conditional on assuming that the zero conditional mean assumption holds. The mechanisms of the OLS are still as explained above. The coefficients β_j are interpreted as the *probability* that the outcome variable $y = 1$ when the x_j changes holding other factors fixed, in other words the probability of "success".

$$\Delta P(y = 1|x) = \beta_j \Delta x_j$$

Thus, the LPM measures the predicted difference in probability relative to the base group.

There are two shortcomings of the LPM. First, predictions that are either less than zero or greater than one are problematic to interpret, since probabilities can only take on values between one and zero. Second, a probability cannot be linearly related to the independent variables for all possible values. Third, there must be heteroscedasticity in a LPM, except in the case where the probability does not depend on any of the independent variables. This does not lead to bias in the estimates, but it leads the standard errors to be invalid in general (Wooldridge, 2014).

Clustering

A municipality-year panel consists of repeated observations of municipalities over time, raising the issue of serial correlation. Serially correlated data are persistent, meaning that values of variables close to each other in time are likely to be similar. This will be the case if the error terms of the municipalities correlate from one period to the next. In example, if voter participation is high one year, it is likely to be high in the next year. It is likely that ignoring serial correlation will result in misleading statistical conclusions, due to bias in the standard

errors. The solution is to apply clustered standard errors. By clustering on group level, we allow for correlation within group (Angrist and Pischke, 2015). Thus, by clustering on municipality level, any serial correlation within each municipality is taken care of.

6.2 Reform Randomness

As presented in the previous section, valid results in the IV approach depend on the exogeneity assumption, whereas in the DID approach, it depends on that the timing of the reform implementation is random with respect to determinants of voter participation. The crucial question in order to ensure that these assumptions are fulfilled is to ask whether the treatment group differ from the control group in factors determining the outcome. In other words, if those who went through the old education system is different from those who went through the new education system with respect to determinants of voter participation, the problem of selection bias arises. Although we can never test whether these assumptions are completely satisfied, checking for a correlation between reform implementation and observable characteristics of the municipalities gives indicative evidences.

Lie (1973) finds no apparent relationship between reform implementation and municipality characteristics such as taxable income, average earnings and education levels. Black et al. (2005) examine this issue further by regressing the year of reform implementation on municipality averages in background variables including parental income, level of education, average age, fraction of individuals with less than nine years of schooling, urban/rural status, industry and labour force composition, municipality unemployment rates in 1960 and the share of individuals who were members of the Labour Party. The researchers find evidence of no systematic correlation between reform implementation and these background variables. Furthermore, Pelkonen (2012) finds evidence supporting the randomness assumption when regressing the number of years that it took the municipalities to implement the reform on socio-economic and political factors, including proportions of municipality representatives of the five largest parties and political affiliation of the mayors.

However, it is challenging to fully test the randomness since some factors can be difficult to measure. Factors describing political interest and decision making can be difficult to find data on. Pelkonen (2012) used voter turnout and political affiliation as sources of identifying such

factors, but these do probably not sufficiently capture all features of political interest and political decision making. Ability is commonly known as a source of selection bias when estimating the effect of education. Both Black et al. (2005) and Pelkonen (2012) finds that the timing of the reform implementation is uncorrelated to level of education prior to reform implementation. Level of education can work as a proxy for ability, but it is debatable whether this is satisfactory. These are examples of that randomness is difficult to measure. Consequently, we are not able to state for certain that the reform implementation was perfectly random.

6.3 Our Model

6.3.1 Model I: IV

Our first model applied to measure the effect of education on voter participation, is an instrumental variables approach. We exploit the Norwegian school reform that extended the years of compulsory schooling from seven to nine. The reform provides exogenous variation in education. We follow a similar approach as Pelkonen (2012) who utilize this reform to study the effect of education on voter participation using an instrumental variables approach.
We estimate the following model:

$$Y_{it} = \alpha + \beta Education_i + \gamma Electionyear_t + \theta D_{bc} + \lambda D_c + \varepsilon_{it} \tag{1}$$

where Y_{it} represent our outcome variable voter participation for individual i at time t. $Education_i$ is the total number of years the individual attended school. The model controls for $Electionyear_t$ which indicates the year of the National Election, year of birth dummies (D_{bc}) that controls for birth cohort fixed effects and childhood county dummies (D_c) that controls for county fixed effects. Thus, year specific shocks and unobserved time invariant factors in each county are controlled for. An example of a year specific shock is a widespread pandemic hitting the population in a specific year, whereas an example of county fixed effects is geographical features. Failing to control for such factors would lead to omitted variable bias if they correlate with the outcome variable, voter participation.

In this model, $Education_i$ might be endogenous if there are variables correlated with education that also affects voter participation, Y_{it}. If this is the case, β will be biased. To overcome this, we apply the instrumental variables approach instrumenting $Education_i$ with $Intensity_{it}$. The $Intensity_{it}$ variable is constructed using the Norwegian school reform. Pelkonen (2012), Black et al. (2005), Black et al. (2008) and Machin et al. (2012) also exploit this reform to construct instrument variables.

The estimated first stage is:

$$Education_i = \eta + \delta Intensity_{it} + \pi Electionyear_t + \varphi D_{bc} + \psi D_c + v_{it} \tag{2}$$

$Intensity_{it}$ indicates the share of individuals in each birth cohort in each county that have enrolled in school for two more years due to the reform. This can be interpreted as the likelihood that individual i has been affected by the reform. In the first stage of the instrumental variables model (equation 2), δ shows how much an increase in the probability that the individual has gone through the reform increases years of education.

Our variable of interest is $Education_i$, and the estimate β shows the causal effect of education on voter participation. Given that we control for municipality fixed effects, and the indicative evidence of no correlation between reform implementation and municipality characteristics described in section 6.2, we argue that this assumption is met.

In general, the IV measures the effect of compliers, whose outcome is solely determined by their treatment status, that is whether they were treated or not. In our analysis, compliers are those whose number of years in school is determined by whether they were affected by the reform or not. In the absence of the reform, they would drop out after seven years in school, but when affected by the reform, they complete nine years of education.

The Effect of the Compulsory Schooling Law on Education
The first stage (ref. Equation 2) tests the relevance assumption, that the reform had an impact on educational attainment. Previous research by Black et al. (2005), Black et al. (2008) and Machin et al. (2012) show statistical significant evidence of this. However, we do not find a statistical significant relationship between the school reform and educational attainment when

adding control variables. Thus, relevance assumption is not fulfilled. The results are shown in table 3.

Table 3: First Stage

	(1)	(2)
Intensity	1.142***	0.117
	(0.299)	(0.884)
Year of birth fixed effects	No	Yes
Childhood county fixed effects	No	Yes
Election year	No	Yes
N	1102	1102

Note: The dependent variable is education. Intensity is the instrument, indicating the share of individuals in each birth cohort in each county that have enrolled in school for two more years due to the reform. Included birth cohorts are 1940 to 1958. The data includes election years 1977 and 1989. Robust standard errors in parentheses. Standard errors are clustered on childhood county level. $^{*} p < 0.05,$ $^{**} p < 0.01,$ $^{***} p < 0.001$

It can be beneficial to narrow the focus on the lower end of the educational distribution, since this is where most of the change took place. We run the first stage (ref. Equation 2) when excluding respondents with more than twelve years of schooling, i.e. higher education. We must acknowledge the possibility that some individuals when pushed to nine years would attend to higher education even if they would not in the absence of the reform. It may be that these individuals exit the sample. If these individuals are more likely to vote, the results would suffer from a downward bias (Pelkonen, 2012). The results, shown in table A2, are similar to when applying the unrestricted sample, there is no statistical significant relationship.

The insignificant results might be due to that our sample consists of few respondents with only seven years of education prior to reform implementation, which makes it difficult to estimate the effect of two additional years of education induced by the reform (ref. table A1). A possible improvement to our model would be to include more data by adding several election years. This would give us a larger sample and possibly more variation in the data. However, this is not feasible because our model requires information on the respondents that are not available for additional election years.

Since our instrument does not fulfil the relevance condition, it is not a valid instrument and cannot be applied further in the instrumental variables approach. Thus, the application of the IV model will not proceed any further in our analysis.

6.3.2 Model II: Staggered DID

Main Specification

Our second identification strategy to measure the effect of education on voter participation, is a staggered DID approach. This setup is inspired by Akerman et al. (2015) and Bütikofer et al. (2015). Increased education is identified as extended years of compulsory schooling by the Norwegian school reform. We exploit that the rollout of the reform took place at different times in different municipalities. The control group is the municipalities that at a point in time had not yet implemented the reform, and the treatment group is the municipalities that had implemented the reform at that time.

We estimate the following model:

$$Y_{mt} = \alpha + \gamma Reform_{mt} + \lambda D_m + \theta D_t + \rho t + \varepsilon_{mt} \tag{3}$$

Where Y_{mt} is our dependent variable, voter participation, which is the share of citizens who voted of all citizens eligible to vote in municipality m at time t. $Reform_{mt}$ is equal to 1 if there are citizens in the electorate who have been enrolled in the reform, and equal to 0 otherwise. In other words, individuals must be born late enough to have been enrolled, and early enough to be eligible to vote.

D_m and D_t is a set of dummies, controlling for municipality fixed effects and election year fixed effects. Thus, time invariant unobserved factors specific to each municipality and shocks specific to the election years are controlled for. If we fail to control for such factors, and they are correlated to the outcome variable, voter participation, the model will suffer from omitted variable bias. By including t, we control for a linear time trend.

$Reform_{mt}$ is our variable of interest, and β represents the causal effect of the treatment. Random rollout of the reform is required for causal interpretation. Since we control for municipality fixed effects, any correlation between the timing of the reform implementation

and time constant municipality characteristics should be taken care of. Moreover, evidence of no systematic correlation between timing of reform implementation and several municipality characteristics as describes in section 6.2, is reassuring for our assumption of a random rollout.

7 Results

In the following section results from our staggered DID analysis is presented. First, we present our findings from the main specification, where we regress voter participation on reform implementation in all municipalities (ref. equation 3). Subsequently, we investigate heterogeneity effects by dividing the sample by gender, rural areas, taxable income per tax payer and unemployment rate.

7.1 Main Specification

Table 4 presents the estimated effect of education on voter participation. The dependent variable is voter participation. Voter participation is specified as a share of the number of votes to the total electorate. There are eight observations per municipality, corresponding to the election years every fourth year from 1953 to 1981. Each column represents a separate regression and each coefficient represents the change in voter participation from the increase in years of education induced by the change in the compulsory schooling law. Standard errors are robust and clustered by municipality, allowing for correlation within the municipalities.

Column 1 presents the effect of education on voter participation, when not including municipality fixed effects, election year fixed effects nor a linear time trend. The estimate is significant and positive, indicating that voter participation increases as years of education is increased. As the reform is implemented, voter participation increase by 2.78 percentage points. When controlling for municipality fixed effects, election year fixed effects and a linear time trend, there is no longer a significant effect. This is shown in column 2. Consequently, the effect in column 1 likely suffers from omitted variable bias, thus the estimate is not trustworthy.

Column 3 presents the relationship between compulsory schooling and voter participation when not controlling for municipality fixed effects. Consequently, time invariant municipality characteristics are left in the error term. Compared to our main specification in column 2, the estimate changes from negative to positive, but is still insignificant. Hence, municipality specific characteristics, rather than compulsory schooling, seems to have an impact on voter participation.

Table 4: Voter Participation Using the Full Sample (Baseline Estimates)

	(1)	(2)	(3)
Reform	0.0278***	-0.00272	0.00569
	(0.00158)	(0.00203)	(0.00307)
Municipality fixed effects	No	Yes	No
Election Year fixed effects	No	Yes	Yes
Time	No	Yes	Yes
N	5648	5648	5648

Note: The dependent variable is voter participation which is the number of votes as a share of the total electorate in each municipality. Column 2 is our main specification. There are eight observations per municipality, corresponding to the election years every fourth year from 1953 to 1981. Robust standard errors in parentheses. All standard errors are clustered on municipality level. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

We estimate the intention-to-treat (ITT) effect. In our analysis, every citizen who voted is included and we cannot state for certain how many of these individuals were affected by the reform. There can be various reasons that children might not complete the compulsory years of schooling, such as sickness, death or simply by choice. It is only the intention to treat that has been randomly assigned to the different municipalities, not whether the person actually enrols in the school. We therefore estimate the effect on average across individuals with higher and lower likelihood of enrolling in education.

7.2 Heterogeneity

Gender

Level of education increased in general in Norway after the 1960s, but especially for women (Jensen, 2000). Our hypothesis is that extended compulsory schooling may have had a greater impact on women than men. We therefore divide our sample by gender in order to investigate whether education affected voter participation differently for women and men. Results are

presented in table 5, column 1 and 2. The effect of education on voter participation is negative and insignificant on a 5% level for both men and women, as in our main specification. Consequently, we cannot state that education induces an increase in voter participation for neither women nor men. We note that the estimate is significant on a 10% level for women. However, this estimate is not sufficiently trustworthy because of its small value and weak significance.

Rural areas

The provision of education prior to the reform varied between urban and rural areas. Typically, secondary education was provided in cities and central places. The reform made nine years of schooling compulsory in all municipalities, implying increased access to schools on a national basis (Lie, 1973, Machin et al., 2012). It is therefore reasonable to believe that the reform implied a more drastically change in the provision of education in rural areas than in urban areas, possibly leading to a bigger impact of extended compulsory schooling in rural areas.

We investigate the effect of education on voter participation in rural areas (see table 5, column 3). We define rural areas as all areas except the five biggest cities in terms of inhabitants in Norway in the relevant time period, hence excluding Oslo, Bergen, Stavanger, Trondheim and Bærum. This identification of rural areas is not perfectly adequate because it does not capture factors such as infrastructure and industry structure. Unfortunately, we do not have data available which would better capture these factors. The findings are similar to our main specification, that there is a negative and insignificant effect of education on voter participation. Thus, we cannot state that education has an effect on voter participation in rural areas. We choose not to restrict our sample to urban areas due to few observations.

Economic Situation and Unemployment

There are competing claims in the literature on how economic stress affects voter turnout. One point of view is that economic stress increase political participation because people blame politicians for their situation, creating civic engagement (Schlozman and Verba, 1979). In contrast, Brody and Sniderman (1977) found that those facing personal economic problems have a lower voter participation than those facing other problems in the United States. The researchers argue that people who experience economic difficulty as an acute personal concern do not hold the government responsible for their situation. Wolfinger and Rosenstone (1980)

found that in the United States Presidential Election in 1972, the poor and unemployed were less likely to vote. Rosenstone (1982) found that voter turnout is lower when short-term unemployment is high, based on the Presidential Election in 1974.

We find it interesting to investigate whether education affects voter participation differently in rich and poor municipalities. We use taxable income per tax payer as a proxy for the economic situation of the inhabitants in each municipality. Unfortunately, we do not have satisfying data on taxable income for all years in the relevant time period. Thus, we operate with taxable income in 1974 in our proxy variable. We divide our sample by quartiles. The results are presented in table 6, column 1-4. We find that in the poorest municipalities (first quartile), education decreases voter participation by 1.11 percentage points. We note that the sample size is smaller and the standard errors higher than in our main specification. In richer municipalities we find no significant effects of education on voter participation, similar to our main specification.

Based on the existing literature, it seems reasonable to acknowledge that education might have different effects on voter turnout in municipalities with different rates of unemployment. We therefore want to investigate whether there is a different effect of education on voter participation in municipalities with relatively high and low unemployment. Unemployment is per definition citizens actively searching for a job, accordingly it does not capture all individuals without a job. We define high and low unemployment rates as share of unemployment above and below mean, respectively. The results are presented in table 6, column 4 and 5. We find similar effects as in our main specification regarding municipalities with an unemployment rate below mean. However, there is a significant negative effect of education in municipalities with an unemployment rate above mean. The estimate indicates that education, identified through extended years of compulsory schooling, induces a decrease in voter participation of 1.22 percentage points in municipalities with relatively high unemployment. We note that the sample size is smaller and that the standard errors increase somewhat when restricting the sample to municipalities with an unemployment rate above mean, compared to the main specification.

Table 5: Voter Participation When the Sample is Restricted to Men, Women and Rural Areas

	(1)	(2)	(3)
	Men	Women	Rural
Reform	-0.000834	-0.00433	-0.00246
	(0.00231)	(0.00237)	(0.00204)
N	5648	5648	5608

Note: The dependent variable is voter participation. All regressions control for municipality fixed effects, election year fixed effects and a linear time trend (main specification). The sample is restricted to men and women,and rural areas (cities excluding Oslo, Bergen, Trondheim, Stavanger and Bærum). There are eight observations per municipality, corresponding to the election years every fourth year from 1953 to 1981.Robust standard errors in parentheses. All standard errors are clustered on municipality level. $^{*}p < 0.05$, $^{**}p < 0.01$, $^{***}p < 0.001$

Table 6: Voter Participation When the Sample is Restricted to Quartiles of Taxable Income and Unemployment Above and Below Mean

	(1)	(2)	(3)	(4)	(5)	(6)
	First quartile	Second quartile	Third quartile	Fourth quartile	Unemployment above mean	Unemployment below mean
Reform	-0.0111*	-0.000716	0.00229	0.00169	-0.0122*	-0.000182
	(0.00546)	(0.00409)	(0.00310)	(0.00300)	(0.00517)	(0.00254)
N	1352	1352	1360	1584	2095	3553

Note: The dependent variable is voter participation. All regressions control for municipality fixed effects, election year fixed effects and a linear time trend (main specification). The sample is restricted to quartiles of taxable income per tax payer and unemployment above and below mean. There are eight observations per municipality, corresponding to the election years every fourth year from 1953 to 1981. Robust standard errors in parentheses. All standard errors are clustered on municipality level.
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

8 Robustness Checks

In order to verify the robustness of our findings, we perform several specification checks on our main analysis. If these tests give the same results as our main specification, it strengthens the internal validity of our main specification. First, we perform a test on the exogeneity of the reform implementation by performing an event study. Second, we test whether the baseline estimates are robust to the inclusion of two more election years. Third, we allow for non-linear form of voter participation by applying a quadratic time trend to the regression. Furthermore, we add share of unemployment as a control variable. Last, we re-specify the dependent variable to a dummy variable that indicates high and low voter participation.

Event Study

The key assumption for the DID identification strategy is that the implementation of the reform is not correlated to any characteristics of the municipality, in other words that it is random. In section 6.2, suggestive evidences of such randomness is presented. However, we want to test this assumption further by checking for correlation between the timing of reform implementation across municipalities and lead values of our variable of interest, voter participation. We apply voter participation in 1949 as our dependent variable. This National Election was accordingly held prior to reform implementation. This gives us a further indication of whether the assumption of random rollout of the reform holds. The result is shown in table A3. We find a statistically significant and weak negative correlation. In fact, the effect is close to zero, and we therefore consider this as further evidence of the exogeneity of the reform implementation.

Extending the Data by Two Election Years

We extend our analysis by including data on the election years 1985 and 1989. By doing this, we investigate whether the baseline estimates in table 4 are robust when a larger share of the electorate has been affected by the reform. The results when using the extended data set are presented in table A4. The estimated coefficient showing the effect of education on voter participation (column 1) is a little larger than the baseline estimates. The estimate of our main specification (column 2) is slightly smaller but still insignificant, strengthening the results of not finding a causal relationship between education and voter participation. Accordingly, including two more election years does not change our findings.

Quadratic Time Trend

By including a quadratic time trend to our main specification, we test whether the functional form of the regression model is specified correctly. We then allow for a non-linear time trend in voter participation in the data. Observing figure A1, showing the mean of voter participation by election year, we suspect that the development of voter participation over time might be non-linear. The result is presented in table A5, column 1. The estimate is similar to results from our main specification, and we therefore conclude that no such trend affects the development of voter participation.

We could also control for municipality specific time trend, but this would eliminate much of the variation in our data. We therefore do not find this expedient.

Controlling for Unemployment

We want to check the robustness of our analysis by including share of unemployment as a control variable. Sørensen (2015) studied the relationship between unemployment and voter turnout in her master thesis, and found that unemployment has a negative effect on voter participation in Norway. It is reasonable to believe that unemployment is also correlated to education. The result from the regression controlling for share of unemployment is presented in table A5, column 2. The result strengthens the findings from our main specification. Since municipality fixed effects are included in our main specification, time invariant factors specific to each municipality are controlled for. From figure A2, we see that the share of unemployment varies over time. However, the variation is very small. This suggests that unemployment is controlled for by fixed effects, and this might be the reason that the regression controlling for unemployment is similar to our main specification.

Re-specification of the Dependent Variable

We re-specify the dependent variable to a dummy variable that indicates high and low voter participation, measured as the mean across all municipalities. Consequently, we investigate whether there is a higher probability of attaining high voter participation when the electorate consists of citizens affected by the reform, applying the same data material. We consider a mean of 80% as a threshold for high voter participation. In figure A1, we observe that in the National Election in 1973 the mean of voter participation increased somewhat and stabilized above 80 percent. At this time, a growing share of the electorate had been affected by the reform (ref.

figure 1).[11] We regress a threshold of high voter participation on the same independent variables as in the main specification, utilizing a linear probability model.

We estimate the following model:

$$Threshold_{mt} = \alpha + \gamma Reform_{mt} + \lambda D_m + \theta D_t + \rho t + \varepsilon_{mt} \qquad (4)$$

The dependent variable $Threshold_{mt}$ is a dummy variable indicating whether the municipality was above or below 80 percent voter participation at time t. The independent variables are the same as in our main specification.

Table A6 presents the results of this model that is the estimate of the probability of a voter participation above 80 percent in each municipality, as an effect of education. The relationship between reform implementation and high voter participation in the municipalities is significant and positive when not including control variables, indicating that reform implementation increased the probability of high voter participation. When controlling for municipality fixed effects, election year fixed effects and a linear time trend, the estimate is negative and insignificant. Thus we are not able to state a causal effect of education on the probability of attaining at least 80 percent voter turnout. The results correspond to the findings from our main specification, supporting the conclusion of not stating a causal relationship between compulsory schooling and voter participation.

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[11] All individuals born between 1941 and 1953 had been affected by the reform at the National Election in 1973. This corresponds to reform implementation between 1955 and 1967.

9 Discussion

In the following, we will provide a discussion of our results and limitations of our analysis.

9.1 Discussion of the Results from our Main Specification

In our analysis, we estimate the intention-to-treat (ITT) effect of education on voter participation in Norway. The result from our main specification is statistically insignificant, and robust to several specification checks. Based on this evidence, we find no causal relationship between education and voter participation in Norway. This corresponds to findings by Pelkonen (2012) and Milligan et al. (2004) who finds no effect of education on voter participation in respectively Norway and the United Kingdom. However, our evidence goes against the vast majority of research, especially in the U.S., and stands in contrast to the absolute voter model.

Previous studies have faced difficulty in estimating the causal effect. Generally speaking, people tend to select into education. More educated people might also be more keen voters. This might lead to selection bias, possibly revealing the positive relationship between education and voter participation that so many researchers have claimed. If this is the case, estimating the causal effect should show a weaker or no effect. Since we utilize an exogenous increase in education, we are able to eliminate selection bias from our analysis. This might explain why we find no relationship between education and voter participation. Our estimates reveal that other municipality characteristics than access to the school reform seem to have an impact on voter participation.

In contrast to the insignificant results found in Norway and the United Kingdom, Milligan et al. (2004) found that education increases voter participation in the United States causally. The different results can possibly be attributed to the differences in the election systems where the U.S. citizens faces registration rules, while there are no similar rules that apply in Norway (see section 2) or in the United Kingdom. Registration rules has been presented as a barrier to voter participation, making it cumbersome for the electorate to vote. Thus, in relation to the rational voter model, registration rules can be considered a cost to participation (Wolfinger and Rosenstone, 1980). The positive relationship in the U.S. discovered by Milligan et al. (2004) might indicate that education contributes to enhancing the skills that the electorate need in order

to overcome the registration barriers. Thus, education can contribute to lowering the costs of participation and increasing voter turnout in countries with such voting barriers. Furthermore, the rational voter model suggest that education can contribute to increased perceived benefits of voting by inducing acknowledgement of civic duty. Our result is evidence against such a mechanism in Norway. It is possible that education does not lead to an acknowledgement of civic duty for Norwegian citizens, and accordingly does not increase their perceived benefits. On the other hand, education might lead individuals to acknowledge their civic duty, but still they do not experience any increase in perceived benefits from voting. In other words, increased acknowledgement of civic duty might not be a mechanism leading them to vote. The Norwegian electorate might not have perceived their vote as particularly important since Norway had a well-functioning and stable democracy, hence they did not experience increased benefits from voting.

A possible explanation of our null result in this study is that two additional years of compulsory education might not be a sufficient amount of schooling to affect voter participation. In relation to the rational voter model, it might be the case that many individuals are very certain on their decision on whether to vote or not. They might therefore be relatively unaffected by possibly small changes in costs and benefits induced by the two years of education. In addition, Pelkonen (2012) suggests that additional education affects voting differently, depending on education level. Based on reported voter turnout by self-reported years of education, it is shown that additional education has the biggest impact on voting at the very lowest, and possibly at the highest level of education. According to Putnam (1995), there is an exponential relationship between education and voter participation. Since the reform affected students at the lower end of the educational distribution, there might not be an effect. The reform implied not only a 2-year increase, but an increase from 7-9 years. A 2-year increase at a higher level of education, for example from 12-14, might have different effects. Another possible explanation of the findings of no significant effect of education on voter participation in our main specification, is that it is relative, rather than absolute education that matters. This corresponds to the relative education model. The voter behaviour of an individual might be affected by perceived social standing relative to others. Although the minimum level of education increased by the reform, the affected individuals still have the minimum years of education, thus their relative standing

remains unchanged.[12] A potential downward bias is low quality of the additional two years of education, but Pelkonen (2012) argues this is not likely due to the nature of the reform.[13]

9.2 Limitations

For the rollout strategy to reveal an effect, the reform implementation needs to be a strong predictor of increases in years of education. If only a small share of the electorate prior to the reform implementation have no more than seven years of education, revealing an effect might be difficult.

When testing for heterogeneity, we find significant negative effects of education on voter participation in the poorest municipalities and in the municipalities with relatively high unemployment. A possible explanation of these results is that education provides knowledge and skills that make people realize that politicians are not to blame for their situation. Therefore, the civic engagement of the poorest and unemployed decrease. In addition, in municipalities with relatively high unemployment, citizens might lose their motivation to vote if they experience that even with education, they stay unemployed. However, there probably is a more structural explanation to the negative effects: We do not have individual data, thus our data do not account for migration. It is reasonable to believe that citizens with increased levels of education induced by the reform living in municipalities suffering from poverty and unemployment, migrate to a municipality that is better in terms of living conditions and job opportunities when they grow up. Consequently, although these individuals were affected by the reform in a specific municipality, the effect of education on voter participation might turn out negative in this municipality if they migrated and voted in a different municipality.

The key assumption of a random rollout is satisfied if there are no shocks in voter participation that are correlated with the rollout. Many events that trigger political participation is specific to municipalities. Such shocks would not be held constant by election year fixed effects or municipality fixed effects. If these shocks affect whether the citizens voted in the National Election, it could affect our results. However, we have not been able to identify any such shocks.

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[12] Persson (2011) finds evidence supporting that relative education matters in explaining voting and political participation in Sweden.

[13] The Norwegian school reform is described in section 2

10 Conclusion

Voting behaviour is one of the most explored topics among researchers. Many previous studies have found a positive relationship between education and voter participation, but in many cases, a causal link is indecisive. Milligan et al. (2004) and Dee (2004) explored exogenous variation in level of education as a source to causal inference and found that education have a positive impact on voter participation in the United States, but Milligan et al. (2004) found no such relationship in the United Kingdom. Pelkonen (2012) found no effect of education on voter participation in Norway.

This paper provides an empirical study of the causal effect of education on voter participation. We exploit the Norwegian school reform from 1960 that increased compulsory schooling from seven to nine years. Characteristics of the reform imply that it is an attractive source of identification of the effects of education. In particular, it was implemented in different municipalities at different times and there is no systematic correlation between the implementation and municipality characteristics. Also, it increased compulsory schooling by a large amount relative to other reforms exploited by researchers. First, we apply an instrumental variables approach, exploiting the Norwegian school reform as an instrument variable of education. However, as the relevance assumption is not satisfied, the instrumental variable approach cannot be proceeded throughout the analysis. Second, we apply a staggered differences-in-differences approach, exploiting the rollout of the Norwegian school reform.

We find no causal effect of education on voter participation in Norway. The results are robust to a number of specification checks. Heterogeneity tests reveal a negative effect of education on voter participation in the municipalities with relatively high unemployment and poverty. However, these effects are possibly due to citizen migration. In fact, assuming immobile citizens represents an important limitation of our analysis.

Our findings contribute to accumulating research that investigates a causal link between education and voter participation, exploiting exogenous increases in compulsory schooling. It strengthens the findings of no causal relationship, as found in previous research in Norway and the United Kingdom. However, it stands in contrast to findings from the United States. This suggest no conformity across countries, one cannot generalize findings of a relationship

between education and voter participation globally. Furthermore, we encourage to more research on this topic, both on an individual and on an aggregate level.

While writing this thesis, specifically two political events reflecting isolationist interests has dominated the news internationally: United Kingdom leaving the European Union, known as Brexit, and Donald Trump winning the U.S. Presidential Election 2016. Many low educated citizens voted for Brexit and Trump, and accordingly, had their way (Kirk and Dunford, 2016, Tyson and Maniam, 2016). This might motivate low educated to vote in the future, hence the positive relationship between education and voter participation found in the U.S. might be revised in years to come. We would argue that a changing political environment in a global world suggests the determinants of voter participation are not given, but rather dynamic in the long run. Consequently, continued research on this topic is of great importance and interest. We encourage future research to analyse whether the school reform had an effect on the rise of isolationist politics such as EU membership or the rise of right wing parties in Norway.

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12 Appendix

Table A1: The Education Variable

Years of education	Frequency	Percent
7	37	3.36
8	97	8.80
9	126	11.43
10	143	12.98
11	131	11.89
12	115	10.44
13	81	7.35
14	85	7.71
15	90	8.17
16	57	5.17
17	55	4.99
18	42	3.81
19	21	1.91
20	15	1.36
21	2	0.18
22	1	0.09
23	2	0.18
24	2	0.18
Total	1,102	100.00

Table A2: First Stage When Restricting the Sample to Lower Education

	(1) Education
Intensity	0.136
	(0.386)
N	649

Note: The dependent variable is education. Intensity is the instrument indicating the share of individuals in each birth cohort in each county that have enrolled in school for two more years due to the reform. Control variables are year of birth fixed effects, childhood county fixed effects and election year. The sample is restricted to respondents with 12 years or less of education. The data includes election years 1977 and 1989. Included birth cohorts are 1940 to 1958. Robust standard errors in parentheses. Standard errors are clustered on childhood county level. $^{*}p<0.05,$$^{**}p<0.01,$$^{***}p<0.001$

Table A3: Event Study

|	(1)
Reform	-0.000670*
	(0.000287)
N	5648

Note: The dependent variable is voter participation in 1949, which is number of votes as a share of the total electorate in each municipality. Standard errors in parentheses. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table A4: Voter Participation Using Extended Data Set

	(1)	(2)	(3)
Reform	0.0336***	-0.00183	0.00569
	(0.00153)	(0.00167)	(0.00307)
Municipality fixed effects	No	Yes	No
Election year fixed effects	No	Yes	Yes
Time	No	Yes	Yes
N	7060	7060	7060

Note: The dependent variable is voter participation which is the number of votes as a share of the total electorate in each municipality. Column 2 shows the results of our main specification. There are ten observations per municipality, corresponding to the election years every fourth year from 1953 to 1989. Robust standard errors in parentheses. All standard errors are clustered on municipality level. $^*p < 0.05$, $^{**}p < 0.01$, $^{***}p < 0.001$

*Figure A1: Mean of Voter Participation by Election Year*
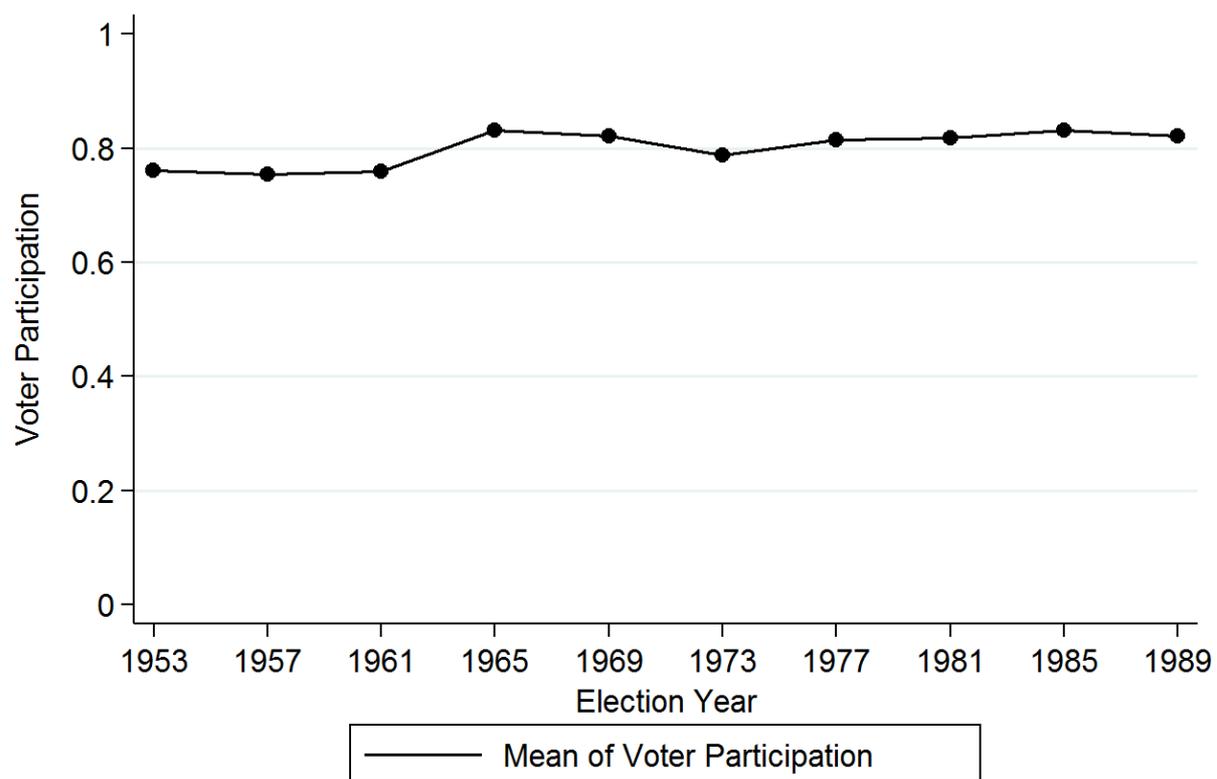
Source: NSD

Table A5: Voter Participation Controlling for Quadratic Time Trend and Unemployment

	(1)	(2)
Reform	-0.00272	-0.00277
	(0.00203)	(0.00203)
Municipality fixed effect	Yes	Yes
Election year fixed effects	Yes	Yes
Time	Yes	Yes
Time^2	Yes	No
Unemployment	No	Yes
N	5648	5648

Note: The dependent variable is voter participation, which is the number of votes as a share of the total electorate in each municipality. There are eight observations per municipality, corresponding to the election years every fourth year from 1953 to 1981. Robust standard errors in parentheses. All standard errors are clustered on municipality level. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Figure A2: Mean of Unemployment by Election Year
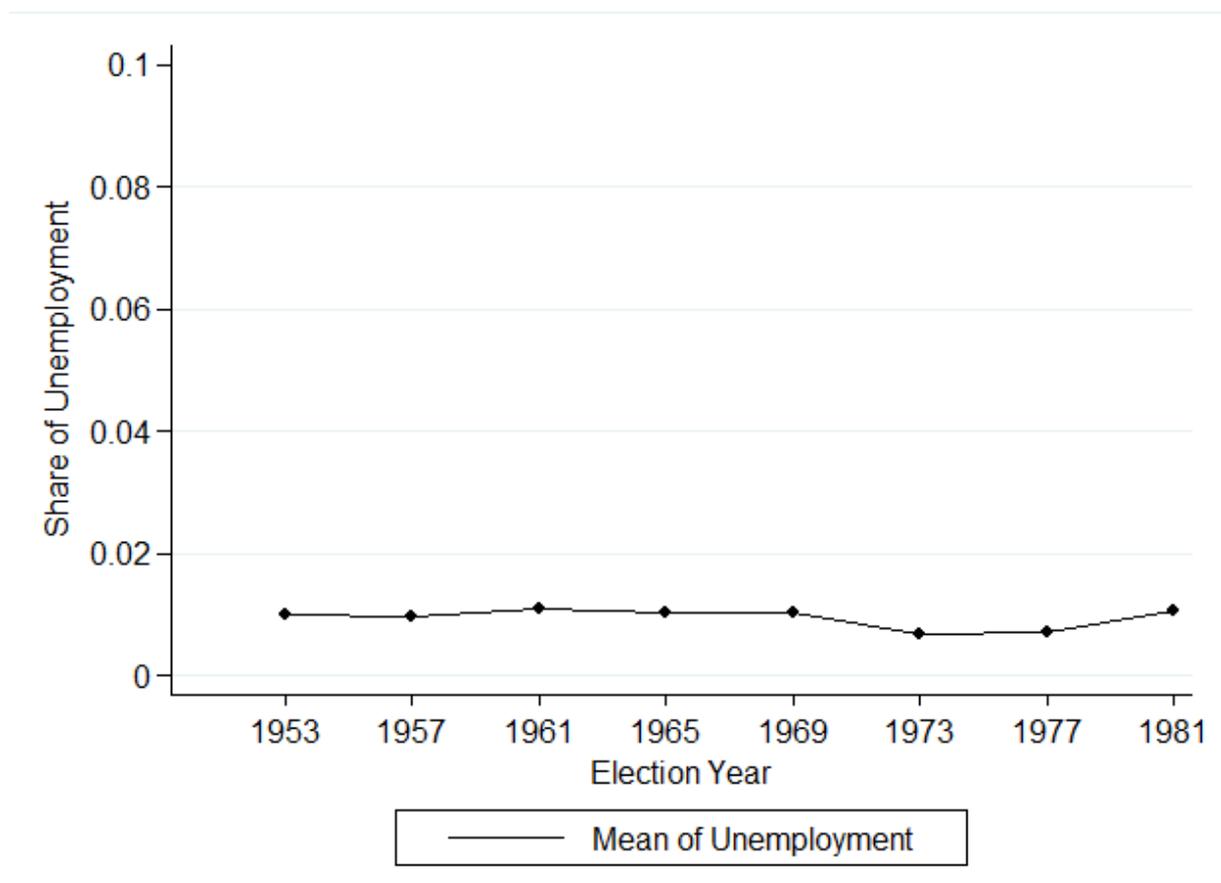
Source: NSD

Table A6: Re-specification of the Dependent Variable

	(1)	(2)
Reform	0.206***	0.0179
	(0.0128)	(0.0230)
Municipality fixed effects	No	Yes
Election year fixed effects	No	Yes
Time	No	Yes
N	5648	5648

Note: The dependent variable is a threshold of 80 percent voter participation. There are eight observations per municipality, corresponding to the election years every fourth year from 1953 to 1981. Robust standard errors in parentheses. All standard errors are clustered on municipality level. $^{*} p < 0.05,$ $^{**} p < 0.01,$ $^{***} p < 0.001$