



School spending and extension of the youth voting franchise: Quasi-experimental evidence from Norway[☆]

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ARTICLE INFO

JEL-codes:

D72

H10

H70

Keywords:

Youth voting franchise

Compulsory school spending

Local governments

ABSTRACT

This paper provides quasi-experimental evidence from Norway on the relationship between local government school spending and the age composition of the electorate. We exploit a reduction in the minimum voting age from 18 to 16 years in selected local governments in the 2011 local election and a difference in differences strategy to estimate causal effects on compulsory school spending. The results do not support the hypothesis that newly enfranchised young voters prefer higher compulsory school spending. Instead, the numerical estimates on school spending point toward negative impacts. Since the newly enfranchised voters had just finished compulsory school and will not receive any direct benefit from compulsory school spending, the absence of positive effects is broadly consistent with the hypothesis that the new young voters behave selfishly. Further research with other types of data is needed to confirm this interpretation.

1. Introduction

Governments all over the world provide services directed toward specific age groups. Examples are primary and secondary schooling and care for the elderly. Currently and even more so in the future, many countries experience substantial changes in the age composition of the population, with an increased share of elderly and a shrinking share of children and young people. An important question is to what extent the implied demand shifts and potential intergenerational conflict will affect the supply side of the public sector represented by policy choices of elected politicians. Two key questions arise. The first is to what extent the preferences of different age groups differ with respect to public spending policy. The second question is to what extent actual policy determined by the politicians and ultimately by the voters responds to changes in the age composition of the *electorate*.

This paper considers the latter question and is motivated by previous studies of the relationship between public spending and age composition and studies of public spending effects of extensions of the voting franchise. We provide quasi-experimental evidence on the relationship between local government school spending and the age composition of the

electorate. Exploiting a novel institutional change in the 2011 local elections in Norway when the voting age was reduced from 18 to 16 years in selected governments, we estimate the effect of a narrowly and well-defined change in the age composition of the *electorate* on public education spending. We argue that within our quasi-experimental research design, this can be interpreted as causal effects of an extended number of youths in the electorate.

There is a discussion in many countries on whether adolescents aged 16 to 18 should be included in the electorate. A central question is to what extent the political maturity of individuals in this age group is sufficient to make informed political decisions.¹ Bergh (2013) uses the change in voting age from 18 to 16 in 2011 described above to test the hypothesis that maturity will increase when young people gain the right to vote and concludes that maturity was unchanged. In contrast, we use the same institutional change to investigate to what extent a lowering of the voting age impacts *actual policy outcomes*.

The paper relates to traditional applied spending studies that include age composition measures as explanatory variables. Most studies find that a higher share of elderly decreases educational spending, while a higher share of the population having children increase spending. These

[☆] We are thankful for comments from two anonymous reviewers, Monique de Haan and Jonas Vlachos, as well as participants at seminars at the Institute for Social Research, and NIFU in Oslo 2019, the IWAEE 2019 in Catanzaro, IIPF 2018 congress in Tampere, and the 11th Norwegian-German Seminar on Public Economics in Munich in 2019 for helpful comments on earlier versions of the paper. Declarations of interest: none.

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¹ See Wagner et al. (2012) and Bergh (2013) for a review of the literature.

age composition effects are consistent with the hypothesis that people vote in their narrow self-interest and support spending on items that imply direct benefits for themselves.² Disentangling the role of the supply and demand-side factors is challenging in spending equations as age composition variables are potentially endogenous due to sorting of people across local jurisdictions with respect to preferences for private and public goods (“Tiebout sorting”). Many studies use additional evidence from voter surveys to judge whether age composition effects are consistent with supply-side responses.³ However, as the link between survey evidence and actual variables in spending equations is often very indirect, it is still an open question whether these results adequately reflect supply responses to age composition in the electorate.

The paper also relates to the growing literature on the relationship between public spending and democratization in terms of extensions of the voting franchise. Most of the literature in this area studies the effect of historical events such as the introduction of universal suffrage to males and females, the removal of socio-economic restrictions on voting, and other changes in pecuniary and nonpecuniary costs of voting.⁴ Few studies consider the effects of changes in the age composition of the electorate. One exception is a recent paper by Bertocchi et al. (2020), who provide quasi-experimental evidence exploiting the staggered introduction of pre-registration laws in US states that arguably reduced voting costs for young voters. They show that introducing such laws increased young voter registration and turnout and led to a substantial increase in state spending on higher education and no impact on compulsory school spending.⁵ This is consistent with evidence that young voters have strong preferences for higher education spending and that the policymakers respond to the shift in the electorate’s preferences in a predictable way.

Our quasi-experimental research design exploits that the Norwegian parliament (*Stortinget*) in 2008 decided to introduce a trial (“experiment”) in the next local elections to take place in September 2011, in which the voting age would be lowered from 18 to 16 years in selected local governments. All local governments were invited to apply for the trial, and in October 2009, 20 local governments (treated governments) were selected by the Ministry of Local Governments (*Kommunal- og regionaldepartementet*).

As compulsory schooling is provided free of charge for children aged 6-16, the additional eligible voters in the treated governments in local elections in September 2011 consisted of 16-17-year-olds that finished compulsory school in spring 2010 and spring 2011. Since the newly eligible voters in the treated governments had all finished compulsory school when the election took place, they would not be directly affected by compulsory school spending decisions in the local government in the period following the 2011 election. Importantly, most of the newly enfranchised voters in the treated local governments were enrolled in upper secondary schools owned and governed by county governments. The change in voting age did not apply to the election of county government counties. Thus, we have a combination of a quasi-experimental setup with a clearly defined new group of young voters that had just graduated from compulsory school and a clear definition of the responsibilities of the politicians to be elected. This makes it possible to

come close to the actual responses of local government school spending to changes in the age composition of the electorate.

Using a difference in differences strategy and panel data from 2006 to 2017 for Norwegian local governments, our results support the hypothesis that politicians act in a way consistent with the self-interest of the newly enfranchised young voters since compulsory school spending does not benefit them directly. Our main specifications suggest that reducing the minimum voting from 18 to 16 decreased compulsory school spending by approximately 2% in treated local governments relatively to control governments. Thus, our finding is broadly consistent with the finding in Bertocchi et al. (2020) that reducing voting costs for young voters in the US did not affect compulsory school spending. The evidence suggests that predictions about supply-side responses to age composition changes need to be based on clear definitions of the stakes for the age groups involved and the actual responsibilities of the politicians elected.

Our results are quite robust across several econometric specifications, robustness checks, and alternative definitions of control groups. Heterogeneity analyses suggest that the negative impact on school spending from the youth enfranchisement mainly appears among local governments with high political fragmentation and a low share of left-wing politicians in the local council in the pre-trial period. We also find that reducing the minimum voting age led to a significant increase in the probability of young candidates being elected to the local council. However, it had no impact on political fragmentation and the share of left-wing politicians.

The rest of the paper is organized as follows: Section 2 presents the theoretical background. Section 3 presents the trial and empirical strategy. Section 4 describes data, while section 5 presents the main empirical results. Section 6 contains robustness analyses, section 7 heterogeneous effects, and section 8 examines political outcomes. Section 9 concludes.

2. Theoretical background

The traditional point of departure in local public school spending analyses is that the local political units (school districts) choose the level of school spending and tax rate consistent with the preferences and constraints the median voter faces. Under these circumstances, an extension of the voting franchise will have counteracting income and price effects on the political equilibrium. Extension of the voting franchise usually shifts the location of the median voter down the income distribution while the tax price faced by the median voter decrease (Husted and Kenny, 1997). However, the political equilibrium is more challenging to handle in many European countries where school spending decisions are made by multipurpose local governments financed by central government grants and limited local tax rate discretion. In these situations, voter group decision models inspired by the framework in Shepsle (1979) and applied in Craig and Inman (1986) and Borge and Rattsø (1995) seem more relevant. In this framework, the political equilibrium is a weighted average of the preferred allocation of different interest groups.

While we do not go into details of the model, the main prediction is that in the event of an extension of the voting franchise, the political equilibrium shifts towards the preferences of the enfranchised group. Thus, when 16-17-year-olds get the right to vote in local elections, the political equilibrium should move closer to the preferences of this group. Thus, whether the franchise extension increases or decreases school spending crucially depends on whether 16-17 old voters have stronger or weaker preferences toward school spending than the rest of the voting population. A critical point is to what extent the choices made by the elected politicians affect the well-being of the additional voters. In our case, the elected politicians affected by the franchise extension only decide compulsory school spending, while spending on high school or higher education is decided by county-level and nationally elected politicians, respectively. On the one hand, the 16-17-year-old newly

² See Borge and Rattsø (1995), Harris et al. (2001), Figlio and Fletcher (2012), Ladd and Murray (2001), and Poterba (1997).

³ See Brunner and Balsdon (2004), Brunner and Johnson (2016), and Rattsø and Sørensen (2010).

⁴ Aidt et al. (2006), Carruther and Wanamaker (2015), Falch, Strøm, & Tovmo, 2022, Husted and Kenny (1997), and Vernby (2013) study the effects of suffrage extensions on the size and composition of public spending. Hodler et al. (2015) and Hoffman et al. (2017) study the effect of compulsory voting on voter turnout and subsequent public spending.

⁵ Their findings show that following the introduction of preregistration laws, state spending on higher education increased by 6% on average, while young voter registration and turnout increased by 4.6% and 8.2%, respectively.

enfranchised voters do not have children, have just left compulsory school, and may have weaker preferences for school spending than other voters. An extreme version of this selfish argument is that 16-17-year-olds may believe that increasing educational opportunities for younger cohorts harms their future well-being as it can increase competition for skilled jobs in the future.

On the other hand, 16-17-year-olds may exhibit intergenerational altruism or reciprocity. Altruism and reciprocity may induce these newly enfranchised youth voters to be more supportive of compulsory school spending. These considerations mean that the compulsory school spending effect of giving these young voters the right to vote cannot be determined theoretically, and the question can only be answered by empirical work.

3. The trial, schooling system, and empirical strategy

3.1. The trial⁶

The ordinary rule in Norway is that all inhabitants who are 18 years of age or older or are 18 years of age during the election year and living in the local government for a minimum of two years have the right to vote in local elections. In 2008, the Norwegian parliament (*Stortinget*) decided to introduce a trial in the local election in 2011 in which the voting age was lowered from 18 to 16. All local governments were invited to apply for participation in the trial in a letter sent from the Ministry of Local government (*Kommunal- og regionaldepartementet*) to the local governments in June 2009. 143 governments applied. By October 2009, the Ministry selected 20 of these as participants. The decision was announced in a press release on October 15, 2009.⁷ According to the Ministry, the selection was made to have a variety of governments in terms of size, geographical location, the political composition of the governing council, and the population's age composition. In addition to these objective criteria, the ministry actively looked for local governments with an activist policy toward getting the youth involved in political issues. The extension of the voting franchise applied only to the election of local government councils and not to the election of the county council held on the same date. According to Bergh and Ødegård (2013), the newly enfranchised 16- and 17-year-olds represented an increase in the voting franchise by approximately 3.4 percent in treated governments.⁸ The election for local and county councils was held on September 12, 2011. About two months earlier, on July 22, Norway was hit by a severe terrorist attack in Oslo and Utøya. It is likely that the terrorist attack affected political participation and voting in the election. However, since the selection of participating governments was made more than one year earlier, there is no apparent reason why the terrorist attack should affect political participation, voting, and subsequent school spending patterns systematically differently in treatment and control governments, as also argued in Bergh (2013).⁹ The general effects on outcomes from the terrorist attack are captured by the year fixed effects in the empirical model.

⁶ The description of the trial builds on Bergh (2013) as well as official information from the Ministry at the website "regjeringen.no".

⁷ The selected 20 local governments were Austevoll, Gjesdal, Grimstad, Hamar, Hammerfest, Kautokeino, Kåfjord, Luster, Lørenskog, Mandal, Marker, Namdalseid, Osen, Porsgrunn, Re, Sigdal, Stavanger, Tysfjord, Vågå, and Ålesund.

⁸ This is based on the numbers given in Bergh and Ødegård (2013) Appendix A, p. 50. The number of 16-17-year-olds eligible for voting in the treated governments in 2011 election was 9,406, while the number of voters 18 years or older was 275,894 in these governments.

⁹ Bharadwaj et al. (2021) describes the terrorist attack and analyze the short and long run consequences for the survivors, their families, and peers.

3.2. Institutional setup

We first describe local government financing and budgeting issues and then briefly present the schooling system in Norway.

3.2.1. Local government financing and budgeting

In the period we study, Norway had more than 420 local governments located in 19 different counties. They ranged in size from around 200 inhabitants (Utsira) to 680 000 inhabitants (Oslo). Norwegian local governments are multipurpose institutions, providing many services: Childcare (children 0-5), primary and lower secondary schooling (children 6-15), health care, care for the elderly, culture, and infrastructure.

Regulated income taxes and block grants are the primary revenue sources, and all local governments use the maximum allowed income tax rate. The rest consists of user fees and regulated property taxes. The empirical analysis treats the sum of regulated income taxes and block grants as local government revenue ("*Frie inntekter*") as exogenous. The block grants are based on objective criteria meant to reflect the local government's demographic and socio-economic situation (see also Rattsø and Sørensen (2010)).

The local government budget is prepared for the following calendar year during the fall, and the final decision on the local government budget is made in December. The local council is elected in September every fourth year. With few exceptions, the council elects the mayor (*Ordfører*) and an executive board (*Formannskap*).¹⁰ The mayor is the executive board chairman, consisting of senior council members with considerable agenda-setting power. Typically, all political parties are represented on the executive board. The local government administration implements the policies prepared by the executive board. The institutional setup means that budgets for calendar years 2010 and 2011 were prepared and decided by the representatives in the council elected in the 2007 election, while the budget for 2012 was decided by the local council elected in 2011.

County governments are responsible for upper secondary education and infrastructure services such as regional roads, cultural institutions, and dental care. The county council determines the county budget allocations. The county council is elected every fourth year, and elections are held at the same time and place as the election to local councils.

3.2.2. Schooling system

Compulsory education in Norway consists of primary and lower secondary schools and ends by grade 10, the year the students turn 16. Most students enroll in upper secondary education, which is divided into a three-year-long academic study track and different vocational study tracks. After a major reform in 1994, vocational study tracks typically last four years (including two years of apprenticeship training). Acceptance to an upper secondary school is based on the grades achieved in grade 10. However, all students have been guaranteed admission to upper secondary education since 1994.

There is no possibility of failing a class in primary or lower secondary education during the empirical period, implying that almost everyone finishes compulsory education on time.¹¹ The education is comprehensive, with no tracking and a common curriculum for all students. The

¹⁰ In a few trial local governments, the mayor was elected directly by the voters in the elections before 2011. Some of the larger cities have implemented a parliamentary system where the local council elects a city government "byråd" led by a government chairman. Currently, this is implemented in Oslo and Bergen. All other local governments use the executive board model.

¹¹ A few students do not start primary education at the expected age, which implies that they finish lower secondary education at a higher age. If a child is not considered to be mature enough, the parents together with the school and psychologists can postpone enrollment one year. In addition, some older students return to improve their grades, and immigrants are often over-aged at graduation.

cutoff between grades is birth on January 1.

3.3. Empirical strategy

We want to investigate the relationship between the age composition of the electorate and local compulsory school spending. Evaluation of the introduction of voting rights to the younger age group described above fits naturally into a difference in differences research design. The spending difference before and after the 2011 election in treated governments is compared with the same spending difference in control governments not participating in the treatment. Under the assumption that the change in spending in the control governments is a valid estimate of the counterfactual change in spending in the treated governments, this strategy gives the causal effect of the franchise extension. This is the usual common trend assumption required for the difference in differences estimates to be interpreted causally. Equation (1) formally represents the difference in differences strategy.

$$y_{it} = \beta_1 TREAT_i + \beta_2 TREAT_i \times POST_t + X_{it}\alpha + \delta_t + \gamma_i + u_{it}, \quad (1)$$

where i denotes local government, t is year, y_{it} is the logarithm of real compulsory school spending, $TREAT_i$ is a dummy equal to 1 if the local government is among the treated governments, $POST_t$ is a dummy equal to 1 if the observation is from a year with budget decisions taken by the local council elected in 2011. γ_i is local government fixed effects. When these are included, the variable $TREAT_i$ is omitted. δ_t is year fixed effects, and X_{it} is a vector of local government control variables specified below. As usual, β_2 represents the difference in differences treatment effect in this framework. In the empirical part, we estimate different versions of the baseline equation (1), including different definitions of both the treatment group and the control group and more general versions allowing for year-specific treatment effects in the post-treatment period and before implementation of the treatment.

A critical issue to consider is possible anticipation effects due to the fact that the treated governments were informed about their selection into the treatment group nearly two years before the local election in 2011 took place. One possibility is that the incumbent council members in the treated local governments stick to their initial political platforms throughout the election period while the political parties select their candidates for the next election (2011) so as to maximize the votes by nominating candidates with a political platform more in line with preferences of the newly enfranchised young voters. This is the implicit assumption made in the baseline version of the model.

Another possibility is that incumbent members in the treated local councils (elected in 2007) adjusted their political platform after receiving information about participation in the trial to increase the probability of reelection. In that case, we would observe spending change already in the election year or even from the moment the selection was announced. To take account of this possibility, we estimate a general version of the model as in equation (2).

$$y_{it} = \sum_{p=2006}^{2015} \beta_p TREAT_i \times POST_p + X_{it}\alpha + \delta_t + \gamma_i + u_{it}, \quad p \neq 2009 \quad (2)$$

This specification lets the treatment group dummy interact with all year dummies except for the reference year 2009. We can use this event-study approach to formally assess the common-trend assumption, the role of anticipation effects, and heterogeneous effects across time by testing three different hypotheses. The first hypothesis is that the change in voting age did not affect the treated local governments in the clean pre-period (2006-2009), which can be interpreted as an indication of parallel trends. Secondly, we can test the hypothesis that the treatment effects of 2010 and 2011 are jointly zero, which can be interpreted as the absence of anticipation effects. The third hypothesis is that the treatment effect after 2011 is constant, as is the assumption implicitly imposed in equation (1). Finally, we can test if all three hypotheses (i), (ii), and (iii) can be jointly imposed.

$$\beta_{2006} = \beta_{2007} = \beta_{2008} = 0 \quad (i)$$

$$\beta_{2010} = \beta_{2011} = 0 \quad (ii)$$

$$\beta_{2012} = \beta_{2013} = \beta_{2014} = \beta_{2015} = \beta \quad (iii)$$

4. Data

To investigate the relationship between youth enfranchisement and school spending policies within the research design described above, we explore a rich yearly panel data set from the accounts of Norwegian local governments from 2006. In the main empirical analysis, we include data up to 2015. In an extended version of the model shown below, we also include data for 2016 and 2017. The capital city, Oslo, is excluded from the dataset since it is both a local government and a county.¹² We first describe the spending variables before we proceed to a description of the control variables. Some of the data are collected from Fiva et al. (2020).

4.1. Local government spending

Detailed data on spending in different sectors are available from the local government accounts collected by Statistics Norway. The empirical analysis will use real operating expenditure per student as our spending variable.¹³ In the descriptive statistics shown in Table 1, we present data using spending per capita, as this measure allows for a comparison of spending across different sectors.

Table 1 shows log spending per capita in the pre-treatment year 2008 for the treated and the control (non-treated) local governments. As we can see from the table, spending on compulsory school and elderly care are the two main spending items. It is also clear from Table 1 that the treated governments are very similar to the rest in terms of allocation between different sectors in the pre-treatment period.

Fig. 1 gives a visual picture of the development in compulsory school spending per student in the treated and control governments from 2006 to 2015. While spending development is roughly similar in the pre-2010 period, spending development diverges substantially between the two groups after 2011, with a slowdown in treatment group spending. This gives the first indication that giving youth aged 16-17 the right to vote decreased school spending. However, empirical results in section 5 present more credible evidence on this issue.

4.2. Control variables

To account for possible systematic differences in the development of important spending determinants in treated and control local governments, we include a number of time-varying control variables: The population size (log), the number of compulsory school students (log), the level of real regulated revenue per capita (log) and the shares of children (0-5), youths (6-15) and elderly (80+). Table 1 shows the average level of these control variables in the pre-treatment year 2008 for the treated and the control (non-treated) local governments. The treated governments have a somewhat higher population and number of students than the control governments. However, Table 1 shows that the treated and control governments are relatively equal in terms of spending and other observable characteristics in the pre-treatment period.

¹² We have run the baseline models in the next chapter also including Oslo and find that treatment effects change by less than 0.02% points if Oslo is included.

¹³ These are expenditures before depreciation and payroll tax. This adjustment is a standard approach also when the Norwegian central government analyzes local government expenditures because it is known that depreciation is based on sometimes faulty valuation of assets in the balance sheet at the time of construction in 2000 and that the payroll tax rate varies between municipalities.

Table 1
Descriptive statistics before the announcement of the trial (2008).

	Treated local governments	All other local governments	Difference
(Log) spending per capita			
- Compulsory school	2.106 (0.201)	2.144 (0.188)	-0.0378 [0.3821]
- Kindergarten	1.358 (0.255)	1.313 (0.195)	0.0452 [0.3193]
- Elderly care	2.335 (0.260)	2.429 (0.296)	-0.0934 [0.1667]
- Primary health	0.535 (0.454)	0.602 (0.449)	-0.0667 [0.5170]
- Social benefits	0.166 (0.364)	0.001 (0.556)	0.1647 [0.1904]
- Child welfare	-0.325 (0.392)	-0.296 (0.497)	-0.0286 [0.8003]
- Culture	0.328 (0.355)	0.240 (0.486)	0.0873 [0.4288]
(Log) real compulsory school spending per student	4.151 (0.228)	4.173 (0.209)	-0.0225 [0.6391]
(Log) real local gov. revenue ("Frie inntekter") per capita	10.118 (0.187)	10.164 (0.220)	-0.0459 [0.3587]
(log) population	8.968 (1.266)	8.435 (1.114)	0.5324 [0.0388]
(log) # of students	6.924 (1.281)	6.406 (1.135)	0.5174 [0.0485]
Share children 0-5 (%)	7.087 (0.952)	6.734 (1.143)	0.3536 [0.1747]
Share children 6-15 (%)	13.260 (1.461)	13.602 (1.303)	-0.3416 [0.2557]
Share elderly 80+ (%)	5.275 (1.813)	5.630 (1.584)	-0.3546 [0.3321]

Note: Standard deviations in parentheses and p-values in brackets.

5. Main empirical results

5.1. Difference in differences estimation

Table 2 reports the results from different versions of the baseline model in equation (1). Column (1) is the simplest specification without

any control variables included. The difference in differences estimate is -0.04 and statistically significant at conventional levels. The point estimate means that the increase in youth enfranchisement reduced compulsory school spending by 4%. Column (2) adds control variables described in the data section above, but this does not affect the estimated treatment effect. Column (3) is a version with year and local government fixed effects, while (4) adds control variables to this specification. The most demanding specification in column (4) gives an estimated decrease of 2.3% and is significant at the 10 percent level.

As to the effect of the control variables, we find an elasticity of school spending per student with respect to revenue at 0.07, although it is not

Table 2
Baseline difference in differences results.

	(1)	(2)	(3)	(4)
TREAT×POST2011	-0.0408*** (0.0146)	-0.0402*** (0.0144)	-0.0420*** (0.0144)	-0.0230* (0.0135)
TREAT	-0.0267 (0.0502)	0.0190 (0.0257)		
(Log) real local gov. revenue per capita		0.565*** (0.0463)		0.0672 (0.0468)
(Log) # of students		-0.0519*** (0.00764)		-0.560*** (0.0308)
Share children 0-5 (%)		-0.0172*** (0.00518)		-0.00424 (0.00478)
Share children 6-15 (%)		-0.0359*** (0.00415)		-0.00142 (0.00366)
Share elderly 80+ (%)		-0.0160*** (0.00528)		-0.000353 (0.00528)
Year FE	Yes	Yes	Yes	Yes
Local gov. FE	No	No	Yes	Yes
Observations	4,279	4,279	4,279	4,279
R-squared	0.044	0.714	0.935	0.956

Notes: Dependent variable is the logarithm of real compulsory school spending per student. The samples include all Norwegian local governments from 2006 to 2015, excluding the capital city, Oslo. Descriptive statistics on the included variables are presented in Section 4. Standard errors clustered at the local government level in parentheses. ***, **, * denotes significant at 1%, 5% and 10% level, respectively.

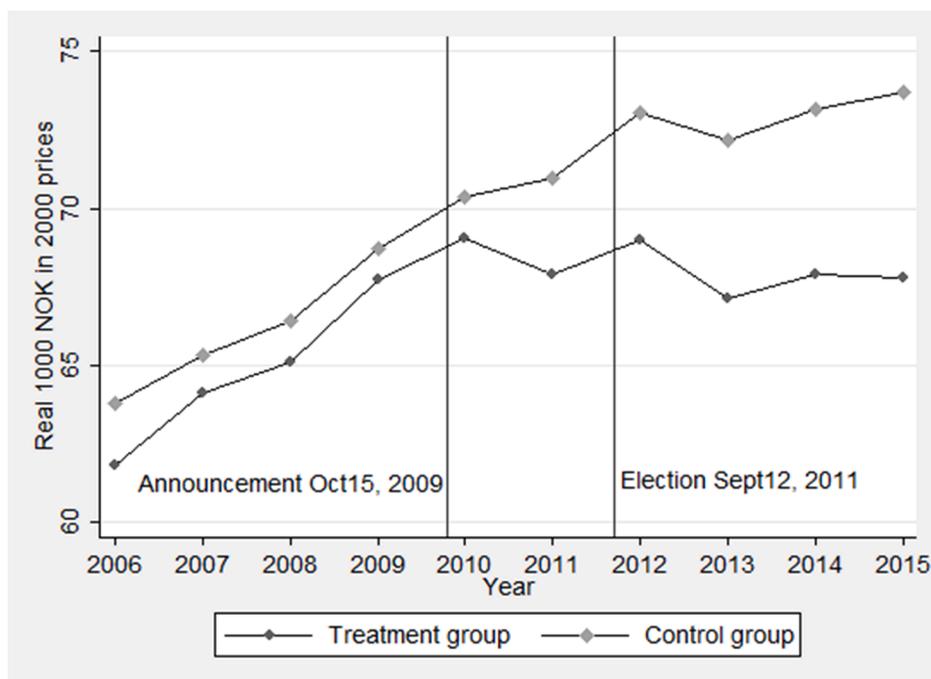


Fig. 1. Real compulsory school spending per student, 2006-2015 measured in 1000 NOK in 2000 prices. Treatment and control group.

statistically significant, while the elasticity with respect to the number of students is -0.56 . The size of the revenue elasticity is similar to the estimates in education spending equations reported in Rattsø and Sørensen (2010). The negative elasticity with respect to the number of students suggests that school spending increase when enrollment increases but not sufficiently to avoid a reduction in spending per student. The estimated elasticity is again quite similar to that found in the literature. The interpretation that being part of a large cohort is a disadvantage is consistent with the findings in Harris et al. (2001), Ladd and Murray (2001), Borge and Rattsø (2008), and Rattsø and Sørensen (2010). The estimates for the age composition variables are close to 0 in the fixed effects model and not statistically significant.

The Norwegian local governments are multipurpose governments providing a large number of services. While our baseline results show that the extension of voting rights to individuals below 18 decreased the level of compulsory school spending, we have analyzed to what extent the trial affected budget shares for compulsory schools. The analysis of budget shares also enables us to explore in a simple way to what extent the inclusion of younger voters in the electorate affected the allocation of resources across activities in general. The share equations are estimated using the same explanatory variables as in the previous regressions, except that the number of inhabitants replaces the number of students.

Appendix Table A1 presents the results for spending shares in 14 categories as defined in the local government accounts. As for the school spending level equations presented above, we find that the intervention reduced the budget share of the compulsory school sector. According to the point estimate, the extension of the electorate to 16-17-year-olds reduced the school budget share by 0.35% points, although this effect is somewhat imprecisely estimated (p -value = 0.19). Evaluating the point estimate at average compulsory school spending share in the sample (20.5%), the share equation implies that the treatment reduced school spending share by approximately 1.7%, which is relatively similar to the estimated effect on the spending level.

The effect is not statistically significant for the other spending shares, except for the positive effect on transportation and communication and the negative effect on social benefits.¹⁴ These sectors constitute relatively small shares of the total budget. In addition to compulsory school, the largest sector is health and elderly care (sample average at 32.7% of the total budget). For this sector, the point estimate is 0.67, but somewhat imprecisely estimated (p -value 0.172), which translates into an increase in the budget share of 2% evaluated at the sample average.

5.2. Event study estimation

So far, we have presented results assuming that the effect of the youth enfranchisement occurred after the 2011 election. As discussed in section 3.3, it is possible that treated governments adjusted their spending already in 2010 and 2011 since they were informed about selection into the treatment group late in 2009. To allow coefficients to vary by each treatment year, possible anticipation effects, and to assess the common trend assumption, Fig. 2 presents effects from the generalized event study model where 2009 acts as the reference year. The figure contains point estimates with 95% confidence intervals, including

¹⁴ The test result indicates that the assumption of parallel trends seems to be violated in some of the share equations, for instance the equation for social benefits. While we are not able to figure out exactly the reason for this, it can be noted that social benefits are an expenditure item that also seems to differ quite substantially in size between the treatment and control group in the pretreatment period according to the numbers in Table 1. Although this item is quite small in numerical terms in all local governments, the difference in social benefit expenditure between the treated and other governments in Table 1 might be related to the fact that the treated governments generally have larger populations as also noted in section 4.2 in the paper.

and excluding years 2010 and 2011, respectively, to illustrate the robustness of the results. The full model results are reported in Appendix Table A2.

Section 3.3 raised three hypotheses concerning the validity of the simplifications imposed on the baseline DiD effects presented in Table 2. The first hypothesis (i) refers to the parallel trend assumption. As reported in Appendix Table A2, we cannot statistically reject hypothesis (i) that the pre-2009 effects are zero (p -value = 0.548). As to the anticipation effects (2010 and 2011), the 2011 treatment effect turns out to be statistically significant and negative, whereas the 2010 treatment effect is close to 0 and far from statistically significant. The formal test of hypothesis (ii) is less clear as the joint restriction of zero treatment effect in 2010 and 2011 has a p -value of 0.075. Third, we cannot reject hypothesis (iii) that the post-2011 effects are homogeneous (p -value = 0.227). Finally, the joint test of hypotheses (i), (ii), and (iii) has a p -value of 0.014, indicating that imposing all three restrictions jointly must be rejected.

Note: The graph shows estimated treatment effects and the 95% confidence intervals from the event study formulation with and without 2010 and 2011 included in the sample. Numerical results from full models are presented in Appendix Table A2, column (1) and column (3).

To further illustrate the robustness of the baseline results, columns (3) and (4) in Appendix Table A2 report the results when 2010 and 2011 are excluded from the sample. The point estimates without 2010 and 2011 in Fig. 2 correspond to the results in column (3) in Appendix Table A2. Neither restriction (i) that the pre-treatment effects are zero cannot be rejected (p -value = 0.548) nor restriction (iii) that the treatment effects are homogenous in the period 2012-2015 (p -value = 0.237). In this case, we cannot even reject that (i) and (iii) hold jointly, i. e., that the specification in column (4) in Appendix Table A2 is a valid simplification of the specification in column (3) (p -value = 0.462). From Fig. 2, we observe that the different year-specific effects are quite unaffected by including or excluding 2010 and 2011 in the sample.

The effect of the original treatment variable corresponding to equation (1) is negative and almost significant (at the 10.4% level) in column (4) in Appendix Table A2. While somewhat imprecise, the point estimates imply that the treated governments experienced a 2.4% reduction in school spending after the 2011 election.¹⁵ These results are similar to those obtained in Table 2 using the whole sample period 2006-2015.

While the estimation results reported so far are not entirely clear on whether the extension of the voting franchise led to a reduction in school spending already in 2011, it is nevertheless a fairly robust conclusion that a spending reduction occurred in the aftermath of the reduction in voting age. The similarity of the post-2011 estimates and the evidence supporting the parallel trend assumption illustrates the robustness. Based on these findings, we will use the specification excluding 2010 and 2011 in robustness checks to safeguard against possible bias due to the timing of the treatment and anticipation effects.

6. Robustness

While the specifications estimated above give some supportive evidence for the parallel trend assumption, we are nevertheless concerned about the internal validity of the results. This section provides further robustness checks on the preferred model, with 2010 and 2011 excluded from the sample. We analyze to what extent the results are driven by specific units included in the treatment group, the definition of the control group, and whether the results are robust to the inclusion of local government trends. Finally, we estimate models with the semi-parametric difference in differences method proposed by Abadie (2005) to account for the possibility that spending trends depend on the characteristics of the treated and nontreated units.

¹⁵ Without local government control variables, the effect is 4.5%.

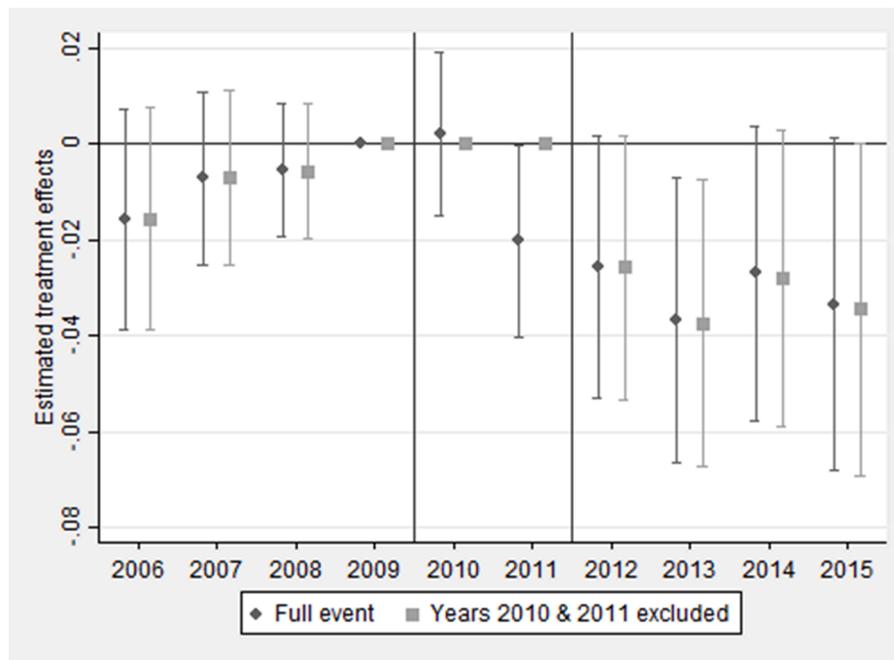


Fig. 2. Event study approach.

6.1. Composition of the treatment group

We now check whether the results obtained above are driven by specific local governments included in the treatment group. To do this, we run the preferred model with control variables and fixed local government and year effects, excluding one by one of the treated governments (see the model in column (4) in Appendix Table A2). Fig. 3 shows the estimated treatment effects and 95% confidence intervals using this procedure. The treatment effects seem quite stable at around 2.5%, and we conclude that specific treated units do not drive the negative effect of the franchise extension.

6.2. Local government trends and Tiebout sorting

A main concern in the literature on the relationship between age composition and school spending is Tiebout sorting. While Tiebout sorting should be less of a problem in our quasi-experimental research design, there is a possibility that the treatment systematically affects the attractiveness of the different local governments. One way to account for this possibility is to allow for smooth changes in school spending interacted with pre-treatment control variables. However, a possible concern is that including local government trends in a specification with 20 treated units within a sample period of only ten years may leave little variation to precisely identify the treatment effect.

Table 3 presents the main treatment effects when local government linear trends are included in the models. Column (1) adds a linear unit-specific trend interacted with (log) number of students in the pre-treatment year 2008. The point estimate increases somewhat from earlier estimated models, with an effect of -3.3%. Column (2) adds a linear unit-specific trend interacted with the age shares in 2008, which reduces the estimated effect from -2.3 to -2% and a corresponding reduction in precision. Column (3) shows a specification with unit-specific trends not interacted with any pre-treatment variables. This increases the point estimate to -3.4%, significant at the 10% level. While the point estimates and precision vary somewhat between the specifications in Table 3, we conclude that the main findings from the preferred model are robust to potential Tiebout sorting and the inclusion of local government trends.

6.3. Alternative control groups

The role of the control group in a difference in differences design is to provide an estimate of the counterfactual, i.e., the development of education spending in the absence of treatment. So far, we have used all local governments not included in the trial as the control group. According to Bergh (2013), the Ministry aimed for the highest possible variation in the treatment group regarding size, geographical location, age composition, and political composition of the local council. He also notices that the Ministry looked for local governments that had actively tried to get its youth involved in local society in various ways. Although the robustness checks above support the parallel trend assumption, it is thus possible that the treatment group deviates from the other governments in unobservable ways. Thus, we define and use two alternative control groups. The first is to let the control group consist of the local governments applying for the trial in 2008 but not selected into treatment by the Ministry. In total, 143 governments applied in 2008, with 20 being selected.

At first sight, the applicants not selected into treatment by the Ministry may appear as a natural control group. However, the applicants will themselves be a selected group of local governments since the decision to apply is potentially determined by expected outcomes from the trial. Thus, it is not obvious that the applicants not receiving treatment constitute a better control group than the total population of governments not participating in the trial. Column (1) in Table 4 nevertheless shows the results when the applicants not selected for treatment in 2011 are used as the control group. Compared to the findings in Table 2 and Appendix Table A2, the point estimate decreases from around -2.3 to -1.5%. The estimate is also no longer statistically significant but still points towards a negative impact of youth enfranchisement.

As a second control group, we exploit that before the local election in 2015, the Ministry of Local Governments announced in 2014 that they wanted to continue the 2011 trial with the extension of voting rights to 16 and 17-olds in selected local governments in 2015. Once again, 20 local governments were selected into the treatment group by the Ministry. Ten of the initially treated governments in 2011 were selected to continue with extended youth voting, while ten returned to the ordinary rules. Ten new local governments were included as participants in the trial in the 2015 election. We exploit this situation to construct an

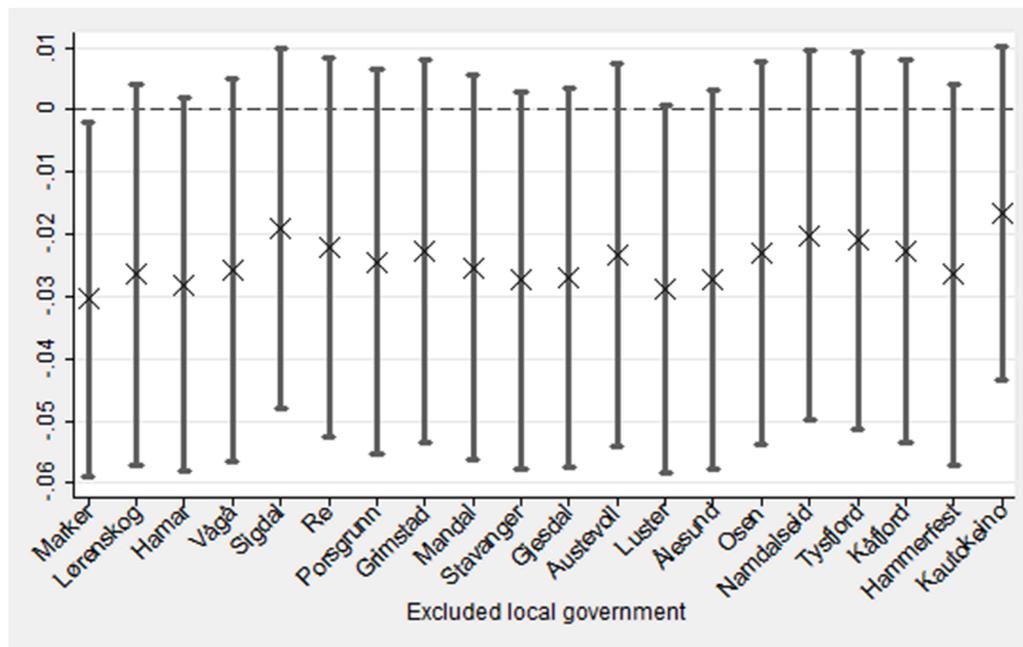


Fig. 3. Estimated treatment effects and 95% C.I. when excluding separate treated governments one by one.

Table 3
Specifications including local government linear trends.

	(1)	(2)	(3)
TREAT×POST2011	-0.0328* (0.0168)	-0.0197 (0.0150)	-0.0338* (0.0181)
Year * log # students in 2008	Yes	No	No
Year * age shares in 2008	No	Yes	No
Linear local government trends	No	No	Yes
P-value parallel trend	0.638	0.380	0.963
Observations	3,412	3,412	3,423
R-squared	0.531	0.626	0.838

Notes: Dependent variable is the logarithm of real compulsory school spending per student. Standard errors clustered at the local government level in parentheses. The models are extensions of the model in column (4) in Appendix Table A2. The samples include all Norwegian local governments from 2006 to 2015, excluding the capital city, Oslo. In addition, 2010 and 2011 are excluded from the analyses. A constant term and year and local government fixed effects are included in all models. Local government controls: log number of students, log local government revenue, and population shares 0-5, 6-15, and 80+. Log # of students is excluded from model (1), whereas the population age shares are excluded from model (2). The reported p-value is an F-test on the parallel trend assumption for a similar model, including year-specific treatment effects in the pre-treatment period (2006-2009). ***, **, * denotes significant at 1%, 5% and 10% level, respectively.

alternative control group consisting of the ten *new* local governments included in the 2015 trial. Because we do not have clear criteria for selecting governments into the control group, the governments selected into the trial at different points in time may have more in common (for the purposes of evaluating the intervention) than all governments that did not participate or the subsample of applicants not selected into treatment. The disadvantage is that this implies a small sample. However, it is nevertheless interesting to check whether the results obtained above still apply using this different control group.

Column 2 in Table 4 reports the results from estimating the model with the ten local governments that became treated in the subsequent 2015 election as the control group. The coefficient of interest has the same sign (negative) as the baseline results in Table 2 and Appendix Table A2, and the point estimates are numerically in the same ballpark. According to the estimate in column (2), extending the electorate to

include 16-17-year-olds reduces compulsory school spending by 3.5%. Reported p-values indicate that assumptions of parallel trends hold.

As a final strategy for addressing that treated units might deviate from a “valid” control group, we apply a semiparametric difference in differences estimator proposed by Abadie (2005).¹⁶ This estimator represents a generalization of the conventional difference in differences model in the case when observable characteristics potentially explain differences in the trends of the education spending variable. The estimator adjusts the distribution of the covariates between treated and nontreated units using propensity score matching, see Abadie and Cattaneo (2018). Column (3) in Table 4 shows the estimation results from this method using the total number of local governments not participating in the trial as the pool of control governments. The propensity score matching utilizes the first-order polynomial of pre-treatment values for the local government controls. The point estimate indicates that real spending per student in treated local governments decreased by 1.9% in the post-treatment period. The effect size increases to around -3.4% when applying second, third, and fourth-order polynomials in the matching procedure, also resulting in significant estimates at the 5% level. The 123 local governments applying for the trial but not selected into treatment in the 2011 election act as the pool of control governments in column (4). The effect size is smaller and less precisely estimated than the estimates obtained using the traditional DiD approach in column (1).

7. Heterogeneous effects

The results above indicate that reducing the minimum voting age from 18 to 16 reduced compulsory school spending in the treated governments. This section examines possible heterogeneous effects of the franchise extension. First, we exploit that half of the treated units returned to the ordinary minimum voting age of 18 in the 2015 election, while the other half continued with 16 to examine the spending path in these two groups in the post-2015 period. Second, motivated by the political-economy literature on decision-making in multiparty governments, we consider possible heterogeneity of treatment effects by the political composition of the local council in the pre-treatment period.

¹⁶ The estimator is implemented in Stata and described in Hounghbedji (2016).

Table 4
Alternative control groups.

Control group consists of:	(1) Applicants not selected in 2011	(2) Units treated in 2015 and not in 2011	(3) Semiparametric DiDon full sample	(4) Semiparametric DiD utilizing the 2011 applicant sample
TREAT×POST2011	-0.0145 (0.0158)	-0.0352* (0.0196)	-0.0194 (0.0152)	-0.00739 (0.0155)
P-value parallel trend	0.677	0.739	-	-
Year FE	Yes	Yes	-	-
Local gov. controls	Yes	Yes	-	-
Local government FE	Yes	Yes	-	-
Polynomial order			1	1
Observations	1133	238	398	130

Notes: The dependent variable is the logarithm of real compulsory school spending per student in columns (1)-(2). The dependent variable in columns (3) and (4) is the difference of log mean real spending per student in the periods 2012-2015 (post-treatment) and 2006-2009 (pre-treatment), respectively, and is estimated utilizing the *absdid* command in Stata. Standard errors are reported in parentheses. In columns (1)-(2), the standard errors are clustered at the local government level. Local government controls: log number of students, log local government revenue, population share 0-5, population share 6-15, population share 80+. In columns (3) and (4), the matching procedure exploits the status of local government controls in 2008. The reported p-value is an F-test on the parallel trend assumption for a similar model, including year-specific treatment effects in the pre-treatment period (2006-2009). ***, **, * denotes significant at 1%, 5% and 10% level, respectively.

7.1. Heterogeneity after the 2015 election

As noted above, the trial continued in the 2015 local election. Ten of the original governments selected into the trial group in the 2011 election continued with the trial in 2015, while ten returned to the ordinary voting age rule (18+) in the 2015 election. We now investigate to what extent the change in the treatment status in 2015 changed the school spending path in these two groups of the initially treated governments.

To do this, we estimate a model including 2016 and 2017 data and include a new set of dummy variables. The dummy variable TREAT2011-NOT2015 takes the value 1 for the ten treated governments in 2011 that returned to the ordinary voting age rules (18+) in the 2015 election. The dummy variable TREAT2011&2015 takes the value 1 for the ten governments in the treatment group in both elections. Finally, we interact these two dummy variables with a dummy variable POST2015 indicating the years following the 2015 election. The ten governments treated only in the 2015 election are naturally excluded from the sample.

The estimate for TREAT2011-NOT2015×POST2015 indicates to what extent governments returning to the ordinary voting rules in 2015 returned to their pre-treatment spending path after the 2015 election. The estimate for TREAT2011&2015×POST2015 indicates if the spending effect changed after the 2015 election for governments treated in both elections.

Table 5 reports the estimated treatment effects using this model formulation for 2006-2017. The estimated coefficient for TREAT×POST2011 is very similar to that obtained earlier, while the estimated coefficient for TREAT2011-NOT2015×POST2015 is of the same magnitude but opposite in sign and statistically significant. This indicates that the ten units returning to the ordinary voting age rule in 2015 returned to their pre-treatment spending path after exiting the treatment group. On the other hand, the estimated coefficient in front of TREAT2011&2015×POST2015 is close to zero and far from statistically significant, implying that the ten units in the treatment group in both elections continued at their lower spending path also after the 2015 election.¹⁷

¹⁷ We cannot statistically reject the joint hypothesis that the coefficients for TREAT×POST2011 and TREAT2011-NOT2015×POST2015 sum to zero and that the coefficient in front of TREAT2011&2015×POST2015 is zero (F-value 0.01, P-value 0.986).

Table 5
Treatment effects when including a new trial in the 2015 election.

	(1)
TREAT2011×POST2011	-0.0248* (0.0150)
TREAT2011-NOT2015×POST2015	0.0263** (0.0120)
TREAT2011&2015×POST2015	-0.00178 (0.0163)
P-value parallel trend	0.477
Year FE	Yes
Local gov. controls	Yes
Local government FE	Yes
Observations	4,176
R-squared	0.951

Notes: Dependent variable is the logarithm of real compulsory school spending per student. The sample includes all Norwegian local governments from 2006 to 2015, excluding the capital city, Oslo. In addition, 2010 and 2011 are excluded from the analysis. Standard errors clustered at the local government level in parentheses. Local government controls: log number of students, real log local government revenue, population share 0-5, population share 6-15, population share 80+. The reported p-value is an F-test on the parallel trend assumption for a similar model that also includes year-specific treatment effects in the pre-treatment period (2006-2009). ***, **, * denotes significant at 1%, 5% and 10% level, respectively.

7.2. Heterogeneity due to political composition

The literature on public spending has emphasized the effect of institutional variables. One issue is the common pool problem facing decision-makers in multiparty governments. As the number of parties increases or the strength of an agenda-setting agent decreases, the argument goes, the higher the potential for interest groups (age groups) to affect the overall size of governments as well as spending priorities (Shepsle (1979) and Weingast et al. (1981)). This has motivated the inclusion of party fragmentation or the number of parties in the elected assembly as a measure of political strength in empirical spending equations in multiparty settings. Most research finds that government size is positively associated with the number of parties in elected assemblies, while the effect on spending priorities is less clear.¹⁸

Bertocchi et al. (2020) find that weaker political competition

¹⁸ See Freier and Odendahl (2015), Bäck et al. (2017), and Meriläinen (2019) for recent contributions and updated reviews of the literature.

increases the spending response to reduced voting costs for young voters in the US two-party setting. We address spending heterogeneity in the multiparty setting in Norway and estimate the effect of the treatment variable on treated municipalities with above and below the median number of effective parties in the local council (inverse Herfindahl index) in the pretreatment period. Applied studies of local governments have also emphasized political party affiliation measured by the share of elected politicians representing left-wing (“socialist”) parties in the local council.¹⁹ Following this tradition, we also estimate the treatment effect in treated municipalities with a left-wing share above and below the median left-wing share in the pretreatment period. Half of the 20 treated local governments are classified as local councils with high/low political fragmentation, whereas 9 of the 20 have a left-wing share above the median. We use pretreatment values since the political variables are potential outcomes of the treatment variable.

The results from these estimations are presented in Table 6. Columns (1) and (2) show the estimated treatment effect among local governments with high and low political fragmentation, respectively. The results suggest that the negative impact on school spending from the youth enfranchisement mainly appears among local governments with high political fragmentation. Columns (3) and (4) present the impact of youth enfranchisement across the political party dimension. We observe that the negative impact on school spending mainly appears among local councils with a low share of left-wing seats.

8. Political outcomes

8.1. Age composition of candidates and elected representatives

Did the trial change the age composition of the local council? One possibility is that the political parties in the treated governments nominated younger candidates in the 2011 local election (nomination margin). Another possibility is that the newly enfranchised young voters

Table 6
Political strength and party affiliation.

	(1) High fragmentation	(2) Low fragmentation	(3) High left- wing share	(4) Low left- wing share
TREAT×POST2011	-0.0366* (0.02124)	-0.0112 (0.02076)	-0.0023 (0.02087)	-0.0431** (0.02035)
# treated local governments	10	10	9	11
Year FE	Yes	Yes	Yes	Yes
Local gov. controls	Yes	Yes	Yes	Yes
Local gov. FE	Yes	Yes	Yes	Yes
2010 & 2011 are excluded	Yes	Yes	Yes	Yes
Observations	1,710	1,713	1,716	1,707
R-squared	0.949	0.954	0.955	0.953

Notes: Dependent variable is the logarithm of real compulsory school spending per student. The samples include all Norwegian local governments from 2006 to 2015, excluding the capital city, Oslo. In addition, 2010 and 2011 are excluded from the analyses. Standard errors clustered at the local government level in parentheses. Local government controls: log number of students, log local government revenue, population share 0-5, population share 6-15, population share 80+. ***, **, * denotes significant at 1%, 5% and 10% level, respectively.

systematically preferred younger candidates, implying a systematic change in the age composition of the elected representatives (election

margin). Saglie et al. (2015) examined the change in age composition in the treated governments and found that the share of young council members increased while party nominations were largely unaffected. The novel data set of nearly complete lists of candidates in the 2003, 2007, and 2011 local elections used in Fiva and Røhr (2018) and made available in Fiva et al. (2021) makes it possible to consider both the nomination margin and the election margin within the Norwegian open-list proportional representation system.

To empirically assess the effect of franchise extension on the nomination and election margin, we estimate equations of the following form based on the listed candidate information

$$y_{ijt} = \beta_1 TREAT_j \times YEAR2011 + \beta_2 TREAT_j \times YEAR2011 \times AGE_{ik} + \beta_3 AGE_{ik} + \beta_4 Female_i + \theta_p + \delta_t + \gamma_i + u_{it}, \tag{3}$$

where y_{ijt} is an outcome measure for candidate i in local government j in local election year t . AGE_k are age group dummy variables, while we also include a dummy for female candidates. θ_p , δ_t , and γ_i are party affiliation, election year, and local government fixed effects, respectively. $TREAT_j$ is a dummy taking the value 1 for treated governments, while $YEAR2011$ is a dummy taking the value 1 in the 2011 local election.

To examine the nomination margin, we exploit that parties can opt to give certain candidates an increased share of the poll (25% of the total number of votes received by the party). Candidates with such a *pre-advantage* are listed at the top of the ballot paper in boldface, see Fiva and Røhr (2018) p. 144 for a detailed description. We define an outcome variable equal to 1 for candidates appearing as pre-advantaged on the party list as the dependent variable and estimate to what extent the relationship between the probability of being pre-advantaged and candidates' age differs between treated and non-treated governments in the 2011 election. To allow for nonlinear age effects, we define six age group dummy variables (AGE_k) based on the candidate's age. The youngest group is candidates 20 years old or younger. The reference group is candidates with a minimum age of 50.

To examine the effect of the franchise extension on the election margin, we define a dummy equal to 1 for candidates on the party list that became elected representatives in the local council. We estimate to what extent the relationship between the probability of being elected and age (defined by age intervals) differs between treated and non-treated governments in the 2011 election.

Table 7 reports results from candidate-level regressions using the elections in 2003, 2007, and 2011. Column (1) shows the regression results for the nomination outcome variable. The results show that females and the younger age groups of 18-29-year-olds generally have a lower probability of being pre-advantaged on the party list. The coefficient for $TREAT \times YEAR2011$ shows that treatment significantly lowers the probability of being pre-advantaged for the reference group (50+) with around 1.3% points. In contrast, the effect on the younger age groups (below 30) are insignificant.

Column (2) shows the results of the election outcome. As for the nomination outcome, the probability of being elected is significantly lower for the younger age groups (18-25) and females. However, the coefficient in front of $TREAT \times YEAR2011$ shows that the treatment lowers the probability of being elected by 2.2% points for the reference group (50+), while treatment significantly *increases* the probability of being elected for the age groups 18-20 and 21-25 by 15 and 7% points, respectively. This result confirms the finding in Saglie et al. (2015) that there was a tendency that the reduction in the voting age increased the representation of young people in the local council.

8.2. Election outcomes

Above, we considered to what extent treatment effects varied according to pre-treatment political variables. We now study to what extent giving youths between 16-17 years of age the right to vote

¹⁹ Recent contributions include Pettersson-Lidbom (2008), Fiva et al. (2018), and Riedel et al. (2021).

Table 7
Candidate age and electoral outcomes.

	(1) Being pre-advantaged	(2) Elected representative
TREAT x YEAR2011	-0.0134** (0.00647)	-0.0215*** (0.00673)
interacted with age 18-20	0.0339 (0.03558)	0.1476*** (0.03961)
interacted with age 21-25	0.0072 (0.01860)	0.0721** (0.03241)
interacted with age 26-29	0.0090 (0.01834)	-0.0089 (0.02293)
interacted with age 30-39	0.0118 (0.01533)	0.0078 (0.01995)
interacted with age 40-49	0.0032 (0.01343)	0.0200 (0.01700)
Age 18-20	-0.0434*** (0.00463)	-0.0302*** (0.00582)
Age 21-25	-0.0252*** (0.00491)	-0.0233*** (0.00556)
Age 26-29	-0.0124*** (0.00432)	-0.0082 (0.00507)
Age 30-39	0.0038 (0.00271)	0.0141*** (0.00338)
Age 40-49	0.0208*** (0.00223)	0.0411*** (0.00251)
Female	-0.0022* (0.00132)	-0.0259*** (0.00208)
Local party FE	Yes	Yes
Year FE	Yes	Yes
Local gov. FE	Yes	Yes
Observations	187,462	187,493
R-squared	0.021	0.060

Notes: Age 50+ is the reference group for the age group dummies. Standard errors clustered at the local government level in parentheses. ***, **, * denotes significant at 1%, 5% and 10% level, respectively.

affected several election outcomes. A frequent argument against extending the right to vote to 16-17-year-olds is that they are less mature and less interested in political issues than older ones and, thus, less likely to vote. Therefore, considering the treatment effect on election participation in addition to party composition and party fragmentation of the local council is of interest.

As to the treatment effect on election participation, we exploit that the election of the local and county council is held at the same time and in the same place and that the 16-17-year-olds in the treated governments are eligible to vote in the election of local council only. Thus, we use the difference in election participation between local and county council elections as a dependent variable to assess the treatment effect on participation.²⁰ This specification accounts for any influence from common variables affecting participation in both elections. Column (1) in Table 8 reports the treatment effect on voter turnout difference. It turns out that the difference in voting participation is not significantly affected by the franchise extension.

Table 8, Column (2) reports the results from local government-level regression using the local council's share of seats allocated to right-wing parties as dependent variables. There is no evidence of an effect of the franchise extension on this political outcome. Column (3) shows the results from a regression using political fragmentation as measured by the inverse Herfindahl index as the outcome variable. The results indicate that the franchise extension did not affect the political fragmentation in treated local councils.

²⁰ Andersen et al. (2014) use a similar approach when examining the effect of the size of local stakes on voting participation in Norwegian local elections.

Table 8
Election outcomes.

	(1) Difference in voting participation (%) between local and county election	(2) Share of seats for right-wing parties	(3) Political fragmentation (inverse Herfindahl)
TREAT* YEAR 2011	-0.7464 (0.52957)	0.0026 (0.02020)	-0.0149 (0.13264)
Year FE	Yes	Yes	Yes
Local gov. controls	Yes	Yes	Yes
Local gov. FE	Yes	Yes	Yes
Observations	1,278	1,279	1,276
R-squared	0.811	0.886	0.827

Note: Included years/elections are 2003, 2007, and 2011. Standard errors clustered at the local government level in parentheses. Local government controls: log population, log local government revenue, population share 0-5, population share 6-15, population share 80+. ***, **, * denotes significant at 1%, 5% and 10% level, respectively. Age 50+ is the reference group for the age group dummies.

8.3. Preferences of young voters: Survey evidence

Having examined several election outcomes, we now turn to more direct evidence on young voters' preferences. Since 1989, mock elections ("skolevalg") have been arranged in upper secondary schools before all parliamentary and local elections in Norway. An election survey is conducted among the students in connection with the mock elections.²¹ The data makes it possible to assess students' attitudes on different political issues. About 20% of upper secondary students usually participate in the election survey.

In the survey undertaken in the mock election in 2011, it is possible to identify respondents residing in local governments participating in the franchise extension trial. This enables us to consider if the attitudes of 16-19-year-olds differ between students in treated and non-treated local governments in the 2011 election.

Appendix Fig. A1 shows the overall distributions of the students' self-reported political color (left-wing/right-wing) and assessment of some key political issues in the 2011 mock election survey. The left-wing/right-wing distribution is based on a 0-10 Likert scale, where 0 indicates far-left-wing preferences and 10 indicates far-right-wing preferences. Their attitude towards political issues is measured on a 1-5 Likert scale, where 1 indicates that they consider the issue very important.²² The figures show that students are quite evenly distributed along the left-wing/right-wing dimension. It should be emphasized that the political issues considered in the survey are quite broad. Most students consider education very important. However, it should be taken into account that the survey does not distinguish between issues related to compulsory education, upper secondary education, or higher education. This limits the ability of the survey results to give credible information on preferences for compulsory school spending, which is the main issue in this paper. The student assessments of most of the other issues are more evenly distributed.

Despite these limitations, we have examined more systematically to what extent political preferences and attitudes towards key political

²¹ Further information is given at the website of the Norwegian Centre for Research Data (NSD) <https://www.nsd.no/en/the-school-election-project>.

²² In the survey, the students were given the following question: «How do you consider the importance of the following issues for your choice of party», scale 1 (very important) to 5 (unimportant): i) Health and social policy (care for elderly, social welfare), ii) School and education policy, iii) Cultural policy, iv) Environment and climate issues, v) Economic policy (taxes, interest rates etc.), and vi) Immigration policy.

issues differed between students living in treated local governments in 2011 and those living in nontreated governments. Table 9 shows the results from regressions between students' subjective placement on the right-wing/left-wing dimension and attitudes to different political issues as dependent variables. In addition to controlling for several student characteristics, the treatment indicator is our key explanatory variable. Overall, the left/right dimension and attitudes of students in treated local governments do not seem to differ significantly from those of other students. The exception is culture, where students in treated governments seem to consider this issue somewhat more important than other students.

9. Conclusions

This paper exploits a novel institutional setup in local elections in Norway in 2011 to study youth political influence on educational spending. The central government introduced a trial where a group of young people 16-17 years of age that had just graduated from compulsory school became eligible to vote in local elections in selected (treated) local governments. Compulsory schooling is the responsibility of local governments. We estimate the effect of including these young people in the electorate on school spending in a difference in differences framework by comparing spending change in treated and control governments.

Using panel data from 2006 to 2015 for all Norwegian local governments, we do not find a positive relationship between compulsory school spending and the extension of the voting franchise to 16-17-year-olds. Instead, in our main specifications, we estimate that compulsory school spending decreased by approximately 2% in treated local governments relatively to control governments. The result is fairly robust across several econometric specifications, robustness checks, and alternative definitions of the control group. We also explore that a subset of the initially treated governments returned to the pre-treatment voting rules in the local election in 2015, while another subset continued to have a reduced voting age. The result suggests that the governments returning to the pre-treatment voting rules also returned to the pre-treatment spending path. Since all the affected voters had just finished compulsory school and do not have children of school age, they do not receive direct benefits from local government school spending. Taken literally, insignificant or negative point estimates on the relationship between franchise extension and spending found in all model

specifications may be interpreted as supportive of the hypothesis of selfish voter behavior of the enfranchised 16-17-year-olds. However, more research using other types of data is needed to confirm this interpretation.

Heterogeneity analyses demonstrate that the negative impact on school spending from the youth enfranchisement mainly appears among local governments with high political fragmentation and a low share of left-wing politicians in the local council in the pre-treatment period. We also find that reducing the minimum voting age led to a significant increase in the probability of young candidates being elected to the local council. However, it had no impact on political fragmentation and the share of left-wing politicians. Despite analyses of several outcome variables, our study does not give clear evidence of possible mechanisms that may explain the negative spending effect.

The evidence in this paper, together with the findings in Bertocchi et al. (2020), broadly suggests that predictions about supply-side responses to age composition changes need to be based on clear definitions of the stakes for the age groups involved and the actual responsibilities for the political units considered.

Author statement

This is a joint statement on author contributions regarding the article «School spending and extension of the youth voting franchise: Quasi-experimental evidence from Norway», ref. submission ECOEDU_D_21_00477.

CRedit authorship contribution statement

Ole Henning Nyhus: Conceptualization, Methodology, Software, Validation, Formal analysis, Investigation, Resources, Data curation, Writing – original draft, Writing – review & editing, Visualization, Supervision, Project administration, Funding acquisition. **Bjarne Strøm:** Conceptualization, Methodology, Software, Validation, Formal analysis, Investigation, Resources, Data curation, Writing – original draft, Writing – review & editing, Visualization, Supervision, Project administration, Funding acquisition.

Data availability

The dataset is made available as supplementary material.

Appendix

Table 9
Students' political preferences across treatment status, 2011.

	(1) Left-wing/ right-wing dimension	(2) Health & social	(3) Education	(4) Culture	(5) Climate & environment	(6) Economy	(7) Immigration
Student in treated local government	0.075 (0.1168)	-0.044 (0.0335)	-0.074 (0.0500)	-0.111** (0.0394)	-0.012 (0.0281)	0.038 (0.0320)	0.035 (0.0535)
Observations	5,892	7,725	7,785	7,764	7,768	7,720	7,714
R-squared	0.037	0.066	0.049	0.017	0.034	0.006	0.011

Note: The regressions include a constant term and categorical control variables for the number of years in upper secondary education, immigration status, parental education, and county. Students residing in Oslo are not included in the sample. Standard errors clustered at the county level in parentheses. ***, **, * denotes significant at 1%, 5% and 10% level, respectively.

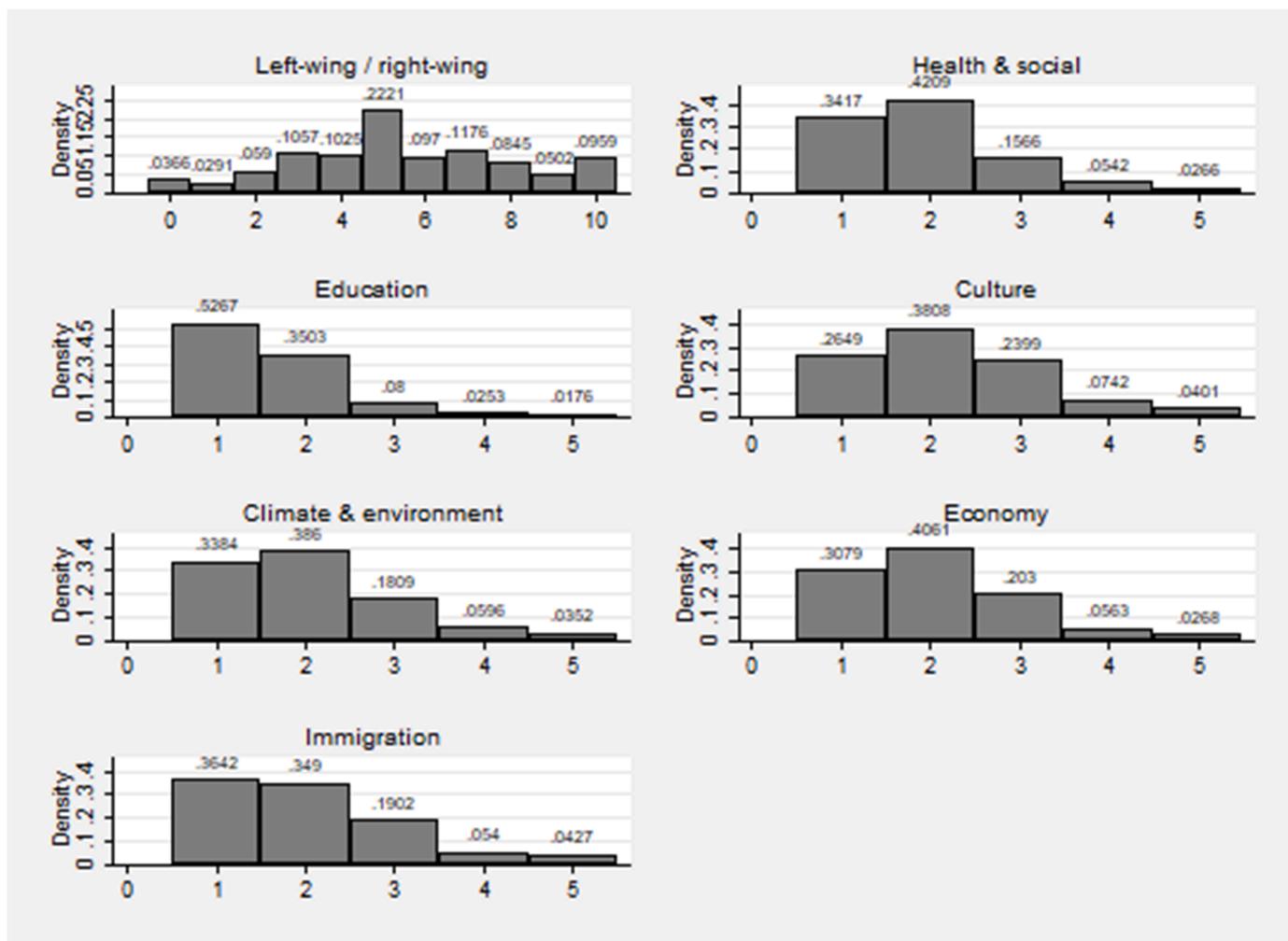


Fig. A1. Adolescents' political preferences, 2011.

Note: The graph shows descriptive statistics concerning upper secondary students... survey responses in the mock election ("skolevalg") just before the 2011 local election. The left-wing/right-wing distribution is based on a 0-10 Likert scale, where 0 indicates far-left-wing preferences and 10 indicates far-right-wing preferences. Their attitude towards political issues is measured on a 1-5 Likert scale, where 1 indicates that they consider the issue very important.

Table A1

Effects on budget shares in different local government sectors.

	(1)	(2)	(3)	(4)	(5)
<i>Sector</i>	Admin.	Kindergarten	School	Health, elderly care	Social benefits
TREAT×POST2011	0.152 (0.379)	-0.0832 (0.209)	-0.354 (0.270)	0.665 (0.486)	-0.377** (0.187)
P-value parallel trend	0.287	0.898	0.093	0.823	0.001
Observations	3,423	3,423	3,423	3,423	3,423
R-squared	0.879	0.952	0.924	0.906	0.830
<i>Sector</i>	Child custody / care	Water, drains, waste	Area developm., environm.	Culture	Church
TREAT×POST2011	0.115 (0.188)	0.167 (0.286)	0.0132 (0.0971)	0.0120 (0.141)	-0.00126 (0.0238)
P-value parallel trend	0.732	0.248	0.958	0.012	0.280
Observations	3,423	3,423	3,423	3,423	3,423
R-squared	0.709	0.787	0.817	0.810	0.839
<i>Sector</i>	Transportation & commns	Housing	Business, industry	Fire	
TREAT×POST2011	0.267** (0.134)	-0.114 (0.0897)	-0.0303 (0.171)	-0.0254 (0.115)	
P-value parallel trend	0.045	0.799	0.641	0.500	
Observations	3,423	3,423	3,423	3,423	
R-squared	0.683	0.692	0.818	0.539	

Notes: Dependent variables are the sectors' share of total spending in percentage points. The samples include all Norwegian local governments from 2006 to 2015, excluding the capital city, Oslo. The budget years potentially affected by anticipation effects between the trial announcement and a new local council (2010 and 2011) are excluded. Standard errors clustered at the local government level in parentheses. All specifications include a constant term, local government control variables,

year fixed effects, and local government fixed effects. Local government controls: log population, log local government revenue, and population shares 0-5, 6-15, and 80+. The reported p-value is an F-test on the parallel trend assumption for a similar model that also includes year-specific treatment effects in the pre-treatment period. ***, **, * denotes significant at 1%, 5% and 10% level, respectively.

Table A2
Event study formulations.

	(1)	(2)	(3)	(4)
TREAT×POST2011		-0.0230*		-0.0244 (0.0150)
TREAT×YEAR2006	-0.0158 (0.0117)		-0.0157 (0.0117)	
TREAT×YEAR2007	-0.0072 (0.0092)		-0.0070 (0.0093)	
TREAT×YEAR2008	-0.0054 (0.0071)		-0.0057 (0.0071)	
TREAT×YEAR2009	-		-	
TREAT×YEAR2010	0.0020 (0.0087)			
TREAT×YEAR2011	-0.0202** (0.0102)			
TREAT×YEAR2012	-0.0256* (0.0139)		-0.0258* (0.0140)	
TREAT×YEAR2013	-0.0368** (0.0152)		-0.0374** (0.0153)	
TREAT×YEAR2014	-0.0270* (0.0157)		-0.0282* (0.0158)	
TREAT×YEAR2015	-0.0335* (0.0176)		-0.0345* (0.0177)	
Test hypothesis i (p-value)	0.548		0.548	
Test hypothesis ii (p-value)	0.075			
Test hypothesis iii (p-value)	0.227		0.237	
Joint F-test (p-value)	0.014		0.462	
Year FE	Yes	Yes	Yes	Yes
Local gov. controls	Yes	Yes	Yes	Yes
Local gov. FE	Yes	Yes	Yes	Yes
2010 & 2011 are excluded	No	No	Yes	Yes
Observations	4,279	4,279	3,423	3,423
R-squared	0.956	0.956	0.954	0.954

Notes: Dependent variable is the logarithm of real compulsory school spending per student. The samples include all Norwegian local governments from 2006 to 2015, excluding the capital city, Oslo. Standard errors clustered at the local government level in parentheses. Local government controls: log number of students, log local government revenue, population share 0-5, population share 6-15, population share 80+. The hypotheses refer to (i) $\beta_{2006}=\beta_{2007}=\beta_{2008}=0$, (ii) $\beta_{2010}=\beta_{2011}=0$, and (iii) $\beta_{2012}=\beta_{2013}=\beta_{2014}=\beta_{2015}=\beta$ (see section 3.3 for details). ***, **, * denotes significant at 1%, 5% and 10% level, respectively.

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