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School Spending and Extension of the Youth Voting Franchise: Evidence from an Experiment in Norway

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School spending and extension of the youth voting franchise: Evidence from an experiment in Norway[*]

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Abstract

Changes in population age composition is challenging in modern welfare states. Intergenerational conflicts may have important consequences for the provision of services directed towards specific age groups as schooling and care for the elderly. A relevant question is to what extent the supply-side responds to changes in the age composition of the electorate in terms of actual spending policies. This paper exploits a novel experiment that took place in Norway in the 2011 local elections to estimate the causal relationship between local government school spending and the age composition of the electorate. We exploit that the voting age was reduced from 18 to 16 years in local elections in selected local governments (experimental governments), while voting age was kept at 18 in others. Using a difference in differences strategy, we find that compulsory school spending *decreased* by approximately 2% in the experimental governments. The results are robust across several econometric specifications and robustness checks. Since all the newly enfranchised voters had just finished compulsory school and receive no direct benefits from local government school spending, the spending result is consistent with selfish voter behavior. The extension of voting rights to younger individuals did not, however, affect the age composition of the local council.

JEL-codes: D72; H10; H70

Keywords: Youth voting franchise; Compulsory school spending; Local governments

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1. Introduction

Local governments all over the world provide services directed towards 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 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 local politicians elected by voters. Two key questions arise. The first is to what extent preferences of different age groups differ with respect to public spending policy. The second question is to what extent actual local policies determined by local politicians and ultimately by the voters, respond to changes in the age composition of the *electorate*.

Applied spending studies traditionally include age composition measures as explanatory variables. Most studies find that a higher share of elderly decrease educational spending, while a higher share of the population having children increase spending. These 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.[1]Disentangling the role of the supply and demand-side factors is challenging in such spending equations as the effect of age composition variables are challenging to interpret causally due to the possibility of Tiebout sorting, i.e., people sort themselves across local jurisdictions with respect to preferences for private and public goods. Many studies, therefore, use additional evidence from voter surveys to judge whether age composition effects are consistent with supply-side responses.[2] 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.

Another growing literature studies the effect of franchise extension on public spending. Most of the literature in this area study the effect of historical events as the introduction of universal suffrage to males and females and removal of socio-economic restrictions on voting as well as other changes in pecuniary and nonpecuniary costs on voting[3]. Few studies in this literature

[1] See Borge and Rattsø (1995), Harris et al (2001), Figlio and Fletcher (2012), Ladd and Murray (2001), and Poterba (1997).
[2] See Brunner and Balsdon (2004), Brunner and Johnson (2016), and Rattsø and Sørensen (2010).
[3] Aidt et al. (2006), Carruther and Wannamaker (2015), Falch et al. (2019), Husted and Kenny (1997), and Vernby (2013) study the effects of suffrage extensions on the size and composition of public spending. Hodler

analyze effects of exogenous changes in the age composition of the electorate. An exception is a recent paper by Bertocchi et al. (2017) studying the introduction of pre-registrations laws in US states which reduced voting costs, especially for young voters. They find 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.

This paper relates to the literature on age structure effects on spending size and composition as well as to the literature on the public spending effects of franchise extension. Exploiting a novel experiment that took place in Norway in the local election in 2011, we estimate the effect of a narrowly and well-defined exogenous change in the age composition of the *electorate* on public education spending. Given the experimental setup, this can be interpreted as an estimate of the causal effect of an extended number of youths in the electorate.

In 2008, the parliament (*Stortinget*) decided to introduce an experiment in the next local elections to take place in September 2011 in which the voting age was lowered from 18-year-olds to 16- and 17-year-olds in selected local governments. All local governments were invited to apply for the trial, and in October 2009, 20 local governments (experimental 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 voters in the experimental governments eligible to vote in local elections in September 2011 consisted of 16-17-year-olds that finished compulsory school in spring 2010 and spring 2011. As the new eligible voters in the experimental governments had all finished compulsory school when the election took place, they would not be directly affected by spending decisions on compulsory schools in the local government in the period following the 2011 election. Instead, most of the newly enfranchised voters in the experimental local governments were enrolled in upper secondary schools owned and governed by a different local authority (counties). Thus, we have a combination of an 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.

et al. (2015) and Hoffman et al. (2017) study the effect of compulsory voting on voter turnout and subsequent public spending.

Using panel data from 2006 to 2017 for all local governments, we estimate the effect of the experiment on compulsory school spending and find that compulsory school spending decreased by approximately 2% in experimental local governments relatively to control governments. The result is robust across several econometric specifications and robustness checks and alternative definitions of control groups. The results suggest that increasing the political influence of young people in the electorate does not necessarily increase the support for educational spending as would be predicted based on earlier evidence. Instead, the evidence in this paper suggests the opposite. The evidence also 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 politicians elected.

The rest of the paper is organized as follows: Section 2 presents the theoretical background. Section 3 presents the experiment and empirical strategy. Section 4 describes data, while section 5 presents empirical results. Section 6 concludes.

2. Theoretical background

The traditional point of departure when modeling local public school spending financed by local taxation is that the local political units (school districts) choose the level of school spending and tax rate consistent with the preferences and constraints faced by the median voter. Under these circumstances, extension of the voting franchise will have counteracting income and price effects on the political equilibrium as it shifts the location of the median voter down the income distribution, while the tax price faced by the median voter decrease, see Husted and Kenny (1997). 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, the political equilibrium is more challenging to handle. 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) seems more relevant. In this framework, the political equilibrium turns out to be a weighted average of the preferred allocation of different interest groups. While we do not go into details in 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 years old 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 increase or decrease school spending crucially depends on whether 16-17 old voters have stronger preferences towards compulsory school spending than the rest of the voting population or not. On the one hand, 16-17-year-olds do not have children and have just left compulsory school and may have weaker preferences for compulsory school spending than other voters. An extreme version of this pure selfish argument is that the 16-17-year-olds may believe that increasing educational opportunities for younger cohorts harm their future wellbeing as it can increase competition for skilled jobs in the future.

On the other hand, the 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 experiment, schooling system, and empirical strategy

3.1. The experiment[4]

The ordinary rule in Norway is that all inhabitants who are 18 years of age or older or being 18 years of age during the election year and living in the local government for minimum two years have the right to vote in local elections in Norway. In 2008, the Norwegian parliament (*Stortinget*) decided to introduce an experiment 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 experiment 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, and the decision was announced in a press release on October 15, 2009[5]. According to the Ministry, the selection was made to have a variety of governments in terms of size, geographical location, political composition of the governing council as well as age composition of the population. In addition to these objective criteria, the ministry actively looked for local governments that had an activist policy towards getting the youth involved in political issues. The extension of the voting franchise applied only for the election of local government councils and not to the election of county council held on the same date. According to the numbers given in Bergh and Ødegård (2013), the franchise extension to 16- and 17-year-olds represented an increase in the voting franchise

[4] The description of the experiment builds on Bergh (2013) as well as official information from the Ministry at the website “regjeringen.no”.

[5] 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.

by approximately 3.4 percent in the experimental governments[6]. The election for local and county councils were 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 experimental and control government as also argued in Bergh (2013). The inclusion of general effects on outcomes from the terrorist attack is captured by the year fixed effects in the empirical model.

3.2. Institutional setup

We first describe local government financing and budgeting issues and then proceed to a brief presentation of the schooling system in Norway.

Local government financing and budgeting

Norway currently has more than 420 local governments located in 18 different counties. They range in size from around 200 inhabitants (Utsira) to 680 000 inhabitants (Oslo). Norwegian local governments are multipurpose institutions, providing a large number of 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. The rest consists of user fees and regulated property taxes. All local governments use the maximum allowed income tax rate and in the empirical analysis, we treat the sum of regulated income taxes and block grants as local government revenue given from the central government *("Frie inntekter")*. The block grants are based on objective criteria meant to reflect the demographic and socio-economic situation in the local government, see also Rattsø and Sørensen (2010).

The local government budget is prepared for the following calendar year during the fall, and the final decision of 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*)[7]. The mayor is the chairman of the

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

[7] 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

executive board, which consists of senior council members and have considerable agenda-setting power. Typically, all political parties are represented in the executive board. The local government administration implements the policies prepared by the executive board. The institutional setup means that the 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 some infrastructure services 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 together with the election to local councils.

Schooling system

Compulsory education in Norway consists of primary schools and lower secondary schools, and ends by grade 10 the year the students turn 16 years of age. 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 for 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 to fail a class neither in primary nor in lower secondary education during the empirical period, which implies that almost everyone finishes compulsory education on-time.[8] The education is comprehensive with no tracking and a common curriculum for all students. The 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 experiment described above fits naturally into a difference in differences strategy where the spending difference before and after the 2011 election in experimental governments is compared with the same spending difference in control

government "byråd" led by a government chairman. Currently this is implemented in Oslo, Bergen and Tromsø. All other local governments use the executive board model.

[8] 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.

governments not participating in the experiment. Under the assumption that the change in spending in the control governments is a valid estimate of the counterfactual change in spending in the experimental governments, this strategy gives the causal effect of the franchise extension. Equation (1) formally represents the difference in differences strategy.

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

where i denotes local government, t is year, y_{it} is compulsory school spending, $TREAT_i$ is dummy equal 1 if the local government is among the experimental governments, $POST_i$ is a dummy equal 1 if the observation is from a year with budget decisions taken by the local council elected in the 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 as well as before implementation of the treatment.

A critical issue to consider is the fact that the experimental governments were informed about their selection into the experimental group nearly two years before the local election in 2011 took place. One possibility is that the incumbent council members in the experimental local governments stick to their initial political platforms throughout the election period, while the political parties selected 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 experimental local councils (elected in 2007) adjusted their political platform after receiving information about participation in the experiment to increase the probability to become reelected. 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 use two strategies. One approach is to estimate the model with the years 2010 and 2011 excluded from the sample. A second approach is to estimate a general version of the model as in equation (2).

$$(2)\ y_{it} = \sum_{t=2007}^{2015} \beta_t TREAT_i \times POST_t + X_{it}\alpha + \delta_t + \gamma_i + u_{it}$$

In this specification, we let the experimental group dummy be interacted with all year dummies. We can use this general model to conduct two useful empirical tests. First, we can perform a placebo test, i.e., test whether the change in voting age affected the experimental local governments before implementation. Second, we can test whether the treatment effect is constant over time, as is the assumption made in equation (1). Formally, (1) appears as a special case of (2) with restrictions

$$(3)\ \beta_{2007} = \beta_{2008} = \beta_{2009} = \beta_{2010} = \beta_{2011} = 0$$
$$(4)\ \beta_{2012} = \beta_{2013} = \beta_{2014} = \beta_{2015} = \beta$$

We test these and other restrictions in the result section of the paper.

4. Data

In order to investigate the relationship between youth enfranchisement and school spending policies in the experimental setting described above, we explore a rich yearly panel data set from the accounts of Norwegian local governments from 2006 on. 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, Oslo, is excluded from the data set since it is both a local government and a county. 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. (2017).

4.1. Local government spending

Detailed data on spending in different sectors available from the local government accounts collected by Statistics Norway is the source for spending in compulsory schools. In the empirical analysis, we will use real operating expenditure per student as our spending variable. In the descriptive statistics shown in Table 1, we present data using spending per capita as this measure allows comparison of spending across different sectors. Table 1 shows log spending per capita in the pre-experiment year 2008 for the experimental and the control (non-experimental) 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 experimental governments are very similar to the rest, in terms of allocation between different sectors in the pre-experiment period.

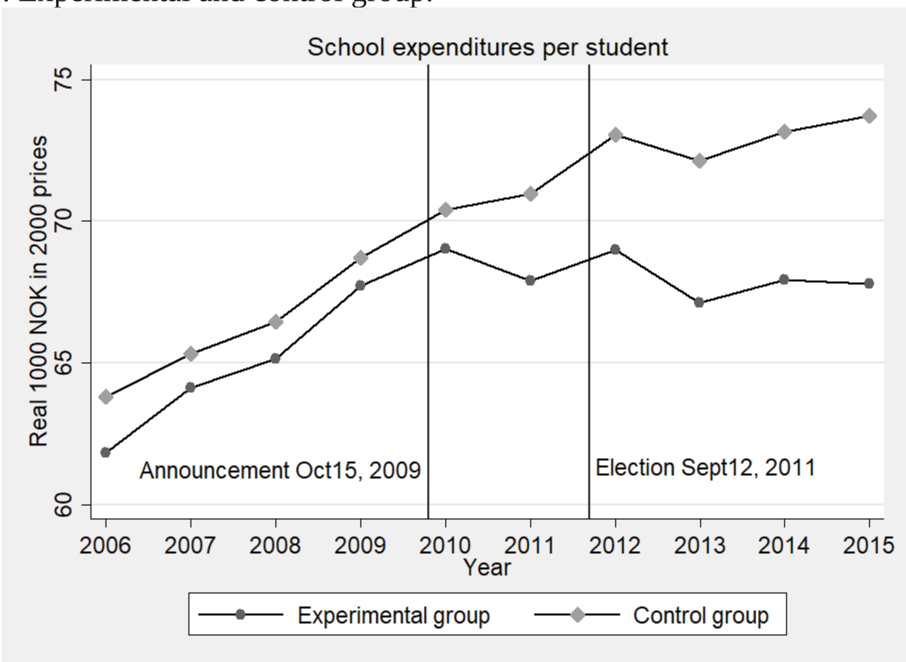
Table 1. Descriptive statistics before the announcement of the experiment (2008).

	Experimental local governments	All other local governments	Difference
(Log) spending per capita			
- Compulsory school	2.100 (0.204)	2.140 (0.192)	-0.0405 [0.3596]
- Kindergarten	1.351 (0.253)	1.309 (0.191)	0.0425 [0.3396]
- Elderly care	2.329 (0.266)	2.425 (0.303)	-0.0962 [0.1647]
- Primary health	0.529 (0.458)	0.598 (0.455)	-0.0694 [0.5056]
- Social benefits	0.159 (0.364)	-0.003 (0.555)	0.1620 [0.1974]
- Child welfare	-0.332 (0.393)	-0.300 (0.496)	-0.0313 [0.7815]
- Culture	0.321 (0.355)	0.237 (0.488)	0.0846 [0.4450]
(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	10.111 (0.192)	10.160 (0.226)	-0.0486 [0.3456]
(log) population	8.974 (1.272)	8.439 (1.120)	0.5351 [0.0388]
(log) # of students	6.924 (1.281)	6.406 (1.135)	0.5174 [0.0485]
Share children 0-5 (%)	7.175 (0.984)	6.719 (1.158)	0.4563 [0.0840]
Share children 6-15 (%)	13.075 (1.368)	13.367 (1.275)	-0.2917 [0.3200]
Share elderly 80+ (%)	5.253 (1.806)	5.615 (1.575)	-0.3618 [0.3197]

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

Figure 1 gives a visual picture of the development in compulsory school spending per student in the experimental and control governments for the period 2006-2015. While spending development is roughly similar in the pre-2011 period, spending development diverge substantially between the two groups after 2011 with a slowdown in experimental 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 presents more credible evidence on this issue.

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



4.2. Control variables

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

5. Empirical results

5.1. Baseline model results

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 to the model. While still significant at conventional levels, this reduces the estimated spending reduction to 3%. Column (3) is a version with fixed effects, while (4) includes both fixed effects and controls. The most demanding specification in column (4) gives an estimated decrease slightly above 2% and 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 0.08 while the elasticity with respect to the number of students is -0.4. The size of the revenue elasticity is similar to the estimates in education spending equations reported in Rattsø and Sørensen (2010). The negative elasticity concerning 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).

Table 2. Baseline results.

	(1)	(2)	(3)	(4)
TREAT×POST2011	-0.0404***	-0.0328**	-0.0419***	-0.0221*
	(0.0146)	(0.0137)	(0.0144)	(0.0125)
TREAT	-0.0268	0.0103		
	(0.0502)	(0.0241)		
(Log) real local gov.		0.579***		0.0759*
revenue per capita		(0.0456)		(0.0451)
(Log) # of students		-0.0509***		-0.407***
		(0.00735)		(0.0376)
Share children 0-5 (%)		-0.00232		-0.00436
		(0.00508)		(0.00445)
Share children 6-15 (%)		-0.0458***		-0.0253***
		(0.00416)		(0.00458)
Share elderly 80+ (%)		-0.0163***		-0.00123
		(0.00529)		(0.00481)
Year FE	Yes	Yes	Yes	Yes
Local gov. FE	No	No	Yes	Yes
Observations	4,276	4,276	4,276	4,276
R-squared	0.044	0.726	0.935	0.957

Notes: Dependent variable is real compulsory school spending per student. Standard errors clustered at the local government level in parentheses. ***, **, * denotes significant at 1%, 5% and 10% level, respectively.

5.2. Robustness checks

Estimating more general treatment models

So far, we have presented results under the assumption that the effect of the youth enfranchisement occurred after the 2011 election. As discussed in section 3.2, it is a possibility that the experimental governments adjusted their spending already in 2010 and 2011 since they were informed about selection into the experimental group in 2009. Table 3 column (1) and (2) reports effects from specifications with separate treatment effects for 2010 and 2011 added to the model. The 2011 treatment effect turns out to be statistically significant and negative in the simple difference in differences model in column (1), while the effect turns insignificant once control variables are added in column (2). It turns out that the effects of the original post-treatment variable are quite unaffected by the addition of the separate 2011 and 2010 effects. Column (3) reports results from an event study type specification, including interaction terms between the experimental group and the complete set of year dummies corresponding to equation (2) above. We cannot reject restriction (3) that the interaction effects are zero before 2012 (F-value=1.49, P-value=0.19). This supports the assumption of parallel trends in the pre-treatment period. Further, we cannot reject restriction (4) of homogeneous treatment effects in the post-treatment period after the 2011 election (F-value=1.53, P-value=0.21). However, we reject the joint hypothesis that (3) and (4) holds at the same time (F-value=2.08, P-value=0.036). This is not very surprising since columns (1) and (2) in Table 3 shows that the 2011 treatment effect is negative, although not statistically significant in column (2).

Table 3. Including separate treatment effects for 2010 and 2011, and general model formulations.

	(1)	(2)	(3)	(4)	(5)	(6)
TREAT×POST2011	-0.0452***	-0.0237*		-0.0450***	-0.0243*	
	(0.0155)	(0.0142)		(0.0155)	(0.0141)	
TREAT×YEAR2012			-0.0131			-0.0131
			(0.0174)			(0.0174)
TREAT×YEAR2013			-0.0231			-0.0234
			(0.0168)			(0.0168)
TREAT×YEAR2014			-0.0124			-0.0129
			(0.0179)			(0.0179)
TREAT×YEAR2015			-0.0192			-0.0199
			(0.0200)			(0.0200)
TREAT×YEAR2010	0.000369	0.00550	0.0123			
	(0.0114)	(0.0102)	(0.0131)			
TREAT×YEAR2011	-0.0203**	-0.0150	-0.00820			
	(0.0101)	(0.0107)	(0.0138)			
TREAT×YEAR2007			0.00626			0.00666
			(0.00658)			(0.00663)
TREAT×YEAR2008			0.00815			0.00842
			(0.00883)			(0.00889)
TREAT×YEAR2009			0.0127			0.0128
			(0.0114)			(0.0115)
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Local gov. controls	No	Yes	Yes	No	Yes	Yes
Local gov. FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	4,276	4,276	4,276	3,420	3,420	3,420
R-squared	0.935	0.957	0.957	0.931	0.955	0.955

Notes: Dependent variable is compulsory school spending per student. 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.

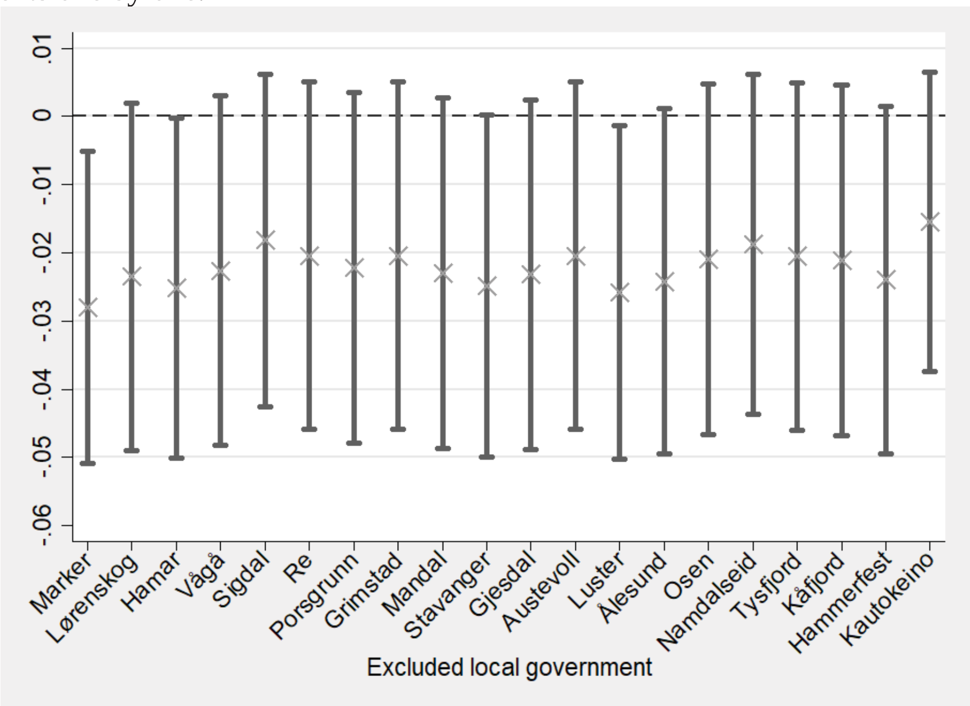
As a further check of the robustness of the baseline results, columns (4) and (5) in Table 3 reports the results when 2010 and 2011 are excluded from the sample. The effect of the original treatment variable corresponding to equation (1) is again significant (at 10% level in column (5)) and negative with point estimates that implies that the experimental governments experienced 4.5% and 2.4% reduction in school spending after the 2011 election, depending on whether local government controls are included or not. These results are very similar to those obtained in Table 2 using the whole sample from 2006-2015. Column (6) reports the results when estimating the event study specification in equation (2) with 2010 and 2011 excluded from the sample. Neither restriction (3) that the pre-treatment effects are zero cannot be rejected (F-value=0.47, P-value=0.70) nor restriction (4) that the treatment effects are homogenous in the period 2012-2015 (F-value=1.51, P-value=0.21). In this case, we cannot even reject that (3) and (4) holds jointly, i.e., that the specification in column (5) is a valid simplification of the specification in column (6) in Table 3 (F-value=0.88, P-value=0.51).

While the estimation results reported so far is not entirely clear on whether the extension of the voting franchise led to a reduction in school spending already in the election year 2011, it is nevertheless a fairly robust conclusion that a reduction occurred in the aftermath of the experiment.

Composition of the experimental group

We now check whether the results obtained above is driven by specific local governments included in the experimental group. To do this, we run the baseline model with control variables and fixed local government and year effects, excluding one by one of the experimental governments. Figure 2 shows the estimated treatment effects along with 95% confidence intervals using this procedure. The treatment effects seem quite stable around 2%, and we conclude that the negative effect of the franchise extension is not driven by specific experimental units.

Figure 2. Estimated treatment effects and 95% C.I. when excluding separate experimental governments one by one.



Alternative control group

According to Bergh (2013), the Ministry aimed for the highest possible variation in the experimental group in terms of 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 experimental group deviates from the other governments in unobservable ways.

Table 4. Alternative control group consisting of the ten new local governments that became experimental governments in the 2015 election.

	(1)	(2)	(3)
TREAT×POST	-0.0326	-0.0197	-0.0240
	(0.0199)	(0.0203)	(0.0162)
Year FE	Yes	Yes	Yes
Local gov. controls	No	Yes	Yes
Local government FE	No	No	Yes
Observations	300	300	300
R-squared	0.115	0.813	0.971

Notes: Dependent variable is compulsory school spending per student. Standard errors clustered at the local government level in parentheses. Columns (1) and (2) also includes the TREAT dummy variable. 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.

Before the local election in 2015, the Ministry of Local Government and Labor announced in 2014 that they wanted to continue the 2011 experiment with the extension of voting rights to 16 and 17-olds in selected local governments. Once again, a total of 20 local governments were selected into the experimental group. Ten of the 2011 experimental governments 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 experiment. We exploit this situation to construct an alternative control group consisting of the ten *new* local governments that was included in the 2015 experiment. The fact that we do not have clear criteria for the selection of governments into the experiment group, the governments selected into the experiment at different points in time may have more in common (for the purposes of evaluating the intervention) than all those governments that did not participate. The disadvantage is that this implies a small sample. However, it is nevertheless of interest to check whether the results obtained above still apply using this different control group. Table 4 reports the results from estimating the baseline model with this very different control group. As should be expected due to the small sample, the treatment effects are imprecisely estimated and not statistically significant at conventional levels. Nevertheless, the coefficients have the same sign (negative) as the baseline results in Table 2, and the point estimates are numerically in the same ballpark. According to the estimate in column (3) extending the electorate to include 16-17-year-olds reduce compulsory school spending by 2.4%.

Local government linear trends

As a final robustness test, we include local government trends in the model. Smooth changes in the preference for school spending in the local government electorates is potentially an omitted variable in the previous analyses. Including local government trends should absorb the influence of such changes and represents a further check of the credibility of the parallel trend assumption. However, a possible concern is that the inclusion of local government trends in a specification with only 20 treated units within a sample period of 10 years leaves too little variation to precisely identify the treatment effect.

Table 5. Specifications including local government linear trends.

	(1) See column (4) in Table 2	(2) See column (2) in Table 3	(3) See column (5) in Table 3
TREAT×POST	-0.0174	-0.0322*	-0.0316*
	(0.0123)	(0.0175)	(0.0179)
TREAT×YEAR10		0.00114	
		(0.00904)	
TREAT×YEAR11		-0.0213*	
		(0.0118)	
Observations	4,276	4,276	3,420
R-squared	0.793	0.794	0.838

Notes: Dependent variable is compulsory school spending per student. Standard errors clustered at the local government level in parentheses. Year and local government fixed effects, local government controls and linear local government trends are included in all models. 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.

Table 5 presents the main treatment effects when local government linear trends are included in the models. Column (1) is based on the specification in Table 2, column 4, which we consider as our baseline model. Inclusion of trends reduce the estimated effect in this specification from -2.2 to -1.7%. The standard error is the same as in the baseline model, resulting in a drop in the p-value to 0.16. As discussed earlier, it is not apparent that the years 2010 and 2011 should be included in the model, since the choice of experimental governments was announced as early as October 15th 2009. In columns (2) and (3), we therefore, respectively, *i*) control for potential treatment effects in 2010 and 2011 and *ii*) exclude these two years from the model. These models are based on columns (2) and (5), respectively, in Table 3. In both specifications, the treatment effect is estimated to -3.2%, and the effects are significant at the 10% level. The treatment effect is thus a bit larger in numerical terms compared to the findings in Table 3. We conclude that the main findings from the basic models are robust to the inclusion of local government trends.

Post-2015 effects

As noted above, the experiment continued in the 2015 local election with 10 of the original governments selected into the experimental group included in the experiment also 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 the original experimental governments. To do this, we estimate a model including 2016 and 2017 data and construct two new dummy variables interacted with a dummy variable POST2015 for the years 2016 and 2017. The dummy variable TREAT2011-NOT2015 takes the value 1 for the ten experimental governments in 2011 that returned to the ordinary voting age rules in the 2015 election. Finding a positive coefficient in front of TREAT2011-NOT2015×POST2015 suggest that governments returning to the ordinary voting rules in 2015 returned to their pretreatment path after 2015. The dummy variable TREAT2011&2015 takes the value 1 if the government remained in the experimental group also in the 2015 election. The coefficient in front of TREAT2011&2015×POST2015 indicates to what extent the treatment effect was different after 2015 for the experimental governments that remained in the experiment group also in 2015.

Table 6 reports the estimated treatment effects using this model formulation for the period 2006-2017. The estimated coefficient in front of TREAT×POST2011 is very similar to that obtained earlier, while the estimated coefficient in front of TREAT2011-NOT2015×POST2015 is of the same magnitude, but opposite in sign and statistically significant. On the other hand, the estimated coefficient in front of TREAT2011&2015×POST2015 is minimal and far from statistically significant. We cannot statistically reject the joint hypothesis that the coefficients in front of TREAT×POST2011 and TREAT2011&2015×POST2015 sum to zero and that the coefficient in front of TREAT2011×POST2015 is zero (F-value 0.08, P-value 0.92)[9]. These results indicate that

[9] The single hypothesis that the coefficients in front of TREAT×POST2011 and TREAT2011&2015×POST2015 cannot be rejected either (F-value 0.13, p-value 0.72).

school spending in the governments that reduced the voting age in 2011 returned to their pretreatment path after the experiment ended in 2015.

Table 6. Treatment effects when including a new experiment in the 2015 election.

	(1)	(2)
TREAT2011×POST2011	-0.0327**	-0.0231*
	(0.0136)	(0.0127)
TREAT2011-NOT2015×POST2015	0.0271	0.0279**
	(0.0268)	(0.0137)
TREAT2011&2015×POST2015	0.00212	0.00322
	(0.0151)	(0.0163)
TREAT2011	-0.0163	
	(0.0308)	
TREAT2011-NOT2015	0.0554	
	(0.0436)	
Year FE	Yes	Yes
Local gov. controls	Yes	Yes
Local government FE	No	Yes
Observations	5,007	5,007
R-squared	0.737	0.953

Notes: Dependent variable is compulsory school spending per student. Standard errors clustered at the local government level in parentheses. ***, **, * denotes significant at 1%, 5% and 10% level, respectively. Local government controls: log number of students, real log local government revenue, population share 0-5, population share 6-15, population share 80+.

5.3. Other outcomes

Budget shares

The Norwegian local governments are multipurpose governments providing a large number of services. While our previous results show that the extension of voting rights to individuals below 18 increased the level of compulsory school spending, we now ask to what extent the experiment affected budget shares for compulsory school. The analysis of budget shares also enables us to explore in a simple way to what extent the extension of the electorate into the younger voters 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.

Table 7 presents the results for spending shares in 14 categories as defined in the local government accounts. Similar to the school spending level equations presented above, we find that the intervention reduced the budget share of the compulsory school sector. According to the estimate in Table 7, the extension of the electorate to 16-17-year-olds reduced school budget share with 0.43 percentage points (significant at the 10% level). For the other spending shares, the effect is not statistically significant, although the point estimate is positive for health and elderly care. Evaluating the results at average compulsory spending share in the sample (20.6 percentage points) the share equation implies that the experiment reduced compulsory spending share by approximately 2%, which is relatively similar to the estimated effect on the spending level.

Table 7. Effects on budget shares in different local government sectors

	(1)	(2)	(3)	(4)	(5)
Sector	Administration	Kindergarten	School	Health, elderly care	Social benefits
TREAT×POST	0.0308	-0.00889	-0.438*	0.566	-0.246
	(0.357)	(0.167)	(0.241)	(0.415)	(0.156)
Observations	4,275	4,275	4,275	4,275	4,275
R-squared	0.884	0.950	0.924	0.912	0.839
Sector	Child custody/care	Water, drains, waste	Area development, environment	Culture	Church
TREAT×POST	0.114	0.214	7.64e-05	0.0539	0.00251
	(0.147)	(0.245)	(0.0750)	(0.130)	(0.0205)
Observations	4,275	4,275	4,275	4,275	4,275
R-squared	0.731	0.789	0.827	0.815	0.844
Sector	Transportation, communication	Housing	Business, industry	Fire	
TREAT×POST	0.175	-0.151	-0.00518	-0.0490	
	(0.117)	(0.0948)	(0.149)	(0.0896)	
Observations	4,275	4,275	4,275	4,275	
R-squared	0.670	0.694	0.825	0.563	

Notes: Dependent variables are the sectors share of total spending in percentage points. Standard errors clustered at the local government level in parentheses. All specifications include both a constant term, local government control variables, year fixed effects, and local government fixed effects. ***, **, * denotes significant at 1%, 5% and 10% level, respectively. Local government controls: log population, log local government revenue, population share 0-5, population share 6-15, population share 80+.

Composition and size of the local council

To what extent did the experiment change the composition of the local council? One possibility is that the political parties in the experimental governments nominated younger candidates in the 2011 local election, or that the younger voters systematically preferred younger candidates. Further, to some extent, the local governments can change the size of the local council, given a minimum number of representatives determined by population size, similar to the system in

Sweden described in Pettersson-Lidbom (2012).[10] Thus, another possibility is that the local governments responded to the experiment by changing the council size.

Table 8. The age composition in the local council and the number of total seats

	(1) Age share under 30	(2) Age share 30-39	(3) Age share 40-49	(4) Age share 50-59	(5) Age share above 60	(6) Total number of seats
TREAT×POST	0.0130	0.00136	0.0184	-0.0177	-0.0150	0.0599
	(0.0143)	(0.0226)	(0.0300)	(0.0207)	(0.0248)	(0.324)
Observations	4,254	4,254	4,254	4,254	4,254	4,262
R-squared	0.531	0.558	0.441	0.477	0.655	0.987

Notes: Standard errors clustered at the local government level in parentheses. All specifications include both a constant term, local government control variables, year fixed effects, and local government fixed effects. ***, **, * denotes significant at 1%, 5% and 10% level, respectively. Local government controls: log population, log local government revenue, population share 0-5, population share 6-15, population share 80+.

Table 8, columns (1)-(5) report the effect of participating in the experiment on the age shares in the council. The point estimates are negative for the share of representatives above 50 and positive for the share of representatives below 50. However, the estimates do not statistically differ from zero. Thus, we conclude that the extension of voting rights to younger individuals did not affect the age composition of the local council in any significant way. Column (6) report the effect on council size, measured by the total number of seats. The estimated effect is small and not significant, neither numerically nor statistically.

6. Conclusion

This paper exploits a novel experiment in local elections in Norway in 2011 to study the effect of youth political influence on educational spending. The experimental setup implied that 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 (experimental) 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 experimental and control governments. Using panel data from 2006 to 2015 for all Norwegian local governments, we estimate that compulsory school spending decreased by approximately 2% in experimental local

[10] According to the Local government act («Lov om Kommuner og fylkeskommuner»), the local council must determine the size of the local council within December 31 the year before the election.

governments relatively to control governments. The result is robust across several econometric specifications and robustness checks and alternative definitions of the control group. We also explore that a subset of the original experimental governments returned to the pre-experiment voting rules in the local election in 2015 while another subset continued to have voting age at 16. The result suggests that the governments returning to the pre-experiment voting rules also returned to the pre-experiment spending path. Since all the affected voters had just finished compulsory school and do not have children in school age, they do not receive direct benefits of local government school spending. The result is thus consistent with selfish voter behavior of the enfranchised 16-17-year-olds. The results suggest that increasing the political influence of young people in the electorate does not necessarily increase the support for educational spending as would be predicted based on earlier evidence with broader definition of age groups. The evidence also 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.

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