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Young voters and budget deficits

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Abstract

This paper exploits a novel trial in Norwegian local elections in 2011 to provide empirical evidence on fiscal performance from lowering the minimum voting age from 18 to 16 years old. Using a difference in differences research strategy, we find that this voting age change reduced the net operating surplus by almost 600NOK (60euro) per capita. This finding is consistent with other micro evidence that young individuals have higher discount rates and are more likely to take risk than older ones.

1. Introduction

Understanding how people and institutions make intertemporal tradeoffs, i.e. trading off the use of resources today against potential benefits from future spending, is important to understand the political-economic determination of fiscal performance in terms of budget deficits and public debt. A large literature has dealt with the general development of fiscal deficits and its causes within a political-economic framework at the country and local levels. Key references include Persson and Tabellini (2000, ch 13.2) and Alesina and Tabellini (1990). Martin-Rodriguez and Ogawa (2017) review the local government oriented literature. Empirical research dealing with fiscal performance at the national and subnational level has been at the forefront in the literature. Key issues have been the role of soft budget constraints in countries with a large amount of transfers between the central and local level and their effects on fiscal performance of subnational governments¹.

In recent years, the potential intergenerational conflicts in the provision of public services to different age groups associated with the rapid aging of the population have been in an important issue in the public debate. Applied studies of fiscal adjustment usually include age composition and other demographic variables to account for variation in spending needs and costs of providing public services across jurisdictions. However, whether the demographic changes in the composition of the *voters* affect fiscal performance in terms of budget deficits and public debt has been given less attention. To our knowledge, the only paper providing evidence on the effect of exogeneous changes in the electorate's composition on budget deficits is Krogstrup and Walti (2011). Exploiting staggered introduction of female suffrage in Swiss cantons to identify causal effects, they find that female enfranchisement decreased budget deficits in the cantons.

Similar exogeneous changes in the *age composition* of the electorate is hard to find. As discussed, the aging of the population is an interesting issue, but imply smooth and long run changes in the age composition of the electorate and hence leaves little scope for obtaining credible estimates of causal effects. At the same time, broadening democracy in terms of lowering the voting age is on the political agenda in many countries, see Bergh (2013) and Wagner et al, (2012). This paper adds to the literature by investigating the relationship between

¹ An early contribution on the empirical relationship between fiscal performance and budgetary institutions and politics in US states is Poterba (1994). The paper by Rodden (2003) deals with fiscal performance in a multi-country framework. Petterson-Lidbom (2010) and Borge and Hopland (2020) consider fiscal adjustments in Sweden and Norway, respectively.

the age composition of the electorate and fiscal performance in local governments by exploiting a novel change in the voting rules in local elections in 2011 in Norway. The minimum voting age was reduced from 18 to 16 in selected Norwegian local governments, and we use this event to provide quasi-experimental evidence of the effect of the extension of the youth voting franchise on net operating surplus in local governments in Norway.

While related to the empirical literature on fiscal performance in local governments, the paper also contributes to the growing empirical literature on the relationship between the composition of the electorate and government spending decisions. Studies from the US find that extending the voting franchise by reducing the voting cost for poor people increased the size of government (Husted and Kenny, 1997) and that introduction of female suffrage increased total state spending (Lott and Kenny, 1999), spending on female-related items like health care (Lee, 2012) and schooling (Carruther and Wannamaker, 2015).² A small recent literature has also considered the effect of the age composition of the electorate on education spending. Bertocchi et al. (2020) find that reduced cost of voting for the young in US states increased turnout and subsequently increased spending on higher education. In contrast, Nyhus and Strøm (2021) find a negative impact on current compulsory school spending from reducing the minimum voting age from 18 to 16 using the same quasi-experimental research design as used in this paper.

Our quasi-experimental research strategy to assess the impact of voting age on fiscal performance exploits that the Norwegian parliament (“Stortinget”) in 2008 introduced a trial in the upcoming local election in September 2011 where the minimum voting age would be lowered from 18 to 16 in selected local governments. All local governments were invited to apply for the trial, and in October 2009 20 local governments were appointed trial governments by the Ministry of Local Governments.

Using panel data from 2006 to 2017 for all local governments in Norway, we estimate that extending the voting franchise to youth aged 16-17 reduces net operating surplus per capita by about 600 Norwegian kroner (60 Euro).

² Studies from Europe find less clear evidence on the spending effect of voting franchise extensions. Aidt et al. (2016) present cross-country evidence from Europe supporting the hypothesis that voting franchise extensions increase central government spending. Falch et al (2021) find that extending voting rights to poor females in Norway did not increase educational spending in Norwegian municipalities. Aidt, Daunton, and Dutta (2010) find that voting franchise extension in local governments (Boroughs) in the second part of nineteenth century England and Wales increased spending only when franchise exceeded a threshold. Chapman (2018) find that a democratic reform in 1894 in Britain led to lower levels of town council spending on public goods.

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 empirical results. Section 6 concludes.

2. Theoretical considerations

The modern literature on fiscal performance has focused on the role of budgetary and political institutions. In particular the interest has been on the deficit bias originating from an underlying common pool problem as emphasized in Persson and Tabellini (2000) ch. 13.2. As spending is directed to different interest groups for instance defined by age, while taxes are paid by everyone, the resulting political equilibrium will be characterized by spending too much and too fast and a deficit bias. The main question in the literature has been to what extent political and budgetary institutions may reduce the common pool problem. Several studies have emphasized the role of political leadership to overcome the common pool problem. Inman and Fitts (1990) find that strong presidents can internalize the externalities implied by the common pool problem in the US context. Borge (2005) finds robust evidence that lower party fragmentation reduces deficits in Norwegian local governments which is consistent with the hypothesis that strong political leadership can reduce the budgetary common pool problem.

While common pool problems and their relationship with political institutions is clearly an important issue, the literature on public deficits has to a very limited extent considered the role of the size and composition of the voting franchise per se. This is surprising since there has been several studies focusing on the relationship between the voting franchise and government size and scope. A common view dating back to Tocqueville (1835) is that democratization in terms of expansion of the voting franchise will increase government size and redistribution, The arguments were formally demonstrated in the pure redistribution case in Meltzer and Richard (1981) and modified to also include the production of public goods in the subsequent empirical literature following the contribution in Husted and Kenny (1997).

A necessary requirement for the extension of the voting franchise to affect the size of government as well as fiscal adjustments in terms of deficit and debt is that changes in the age composition of the voting franchise affects the preferences of the voting population. The sufficient requirement is that the policy of local governments react to changed preferences of the voters. Since our focus in this paper is on fiscal performance in terms of public deficits rather than the size of government, we concentrate on whether voters in different age groups want a given level of public spending to be paid for today or tomorrow through higher debt (or

reduced future wealth). In principle, extending the right to vote to new groups (women, youth, ethnic minorities) may affect the budget deficit through several different channels. By changing preferences for intergenerational redistribution, by changing the preferences for intertemporal smoothing of public consumption and precautionary savings or by changing the ability to handle the common pool problem as discussed above.

In line with Krogstrup and Walti (2011), we take the view that extension of voting rights to new groups mainly affects deficits through the two former channels. While our local government level data does not allow for a distinction between these different motives for running deficits, we nevertheless present an informal discussion of how these motives may differ between different voter groups (age groups). Krogstrup and Walti (2009) present a simple two-period model with two different groups of voters (men and women) and argue that enfranchisement of women will change the public saving rate (or the budget deficit) if women and men have different time preferences or different attitudes toward risk. In particular they show that if women are more prudent towards risk or if women discount time less than men, male enfranchisement will lead to lower budget deficits all else equal. This model's results can easily be extended to consider the preferences of old and young voters. If young voters are less risk averse and discount time more than old voters, extending the voting franchise to younger people will lead to higher budget deficits. Given this focus, a natural starting point is whether intertemporal preferences and attitudes towards risk vary systematically with age.

Standard economic theory assumes that an individual trades off benefits in different time periods according to an exponentially declining discount factor which implies time consistent preferences. Within this discounted utility model variation in the discount factor contains information about time preferences and intertemporal choices. In the last decades, quasi-hyperbolic utility functions have been shown to explain many anomalies in intertemporal choices that cannot be explained within the standard model with time consistent preferences. Cohen et al (2020) reviews the large and still growing literature on time preferences in general.

Harrison et al. (2002) provide experimental evidence from Denmark that discount rates vary significantly with several demographic variables and in particular they find that discount rates appear to decline with age, at least after middle age. Read and Read (2004) provide experimental evidence from UK on whether and how time preference changes from young adulthood through to old age and find that older people discount more than younger ones and that middle-aged people discount less than either group. Moreover, a tendency to downplay or

ignore future consequences (as considered by Laibson, 1997; O'Donoghue and Rabin, 1999) may make adolescents to differ from adults in their time preferences of public consumption.

As to attitude towards risk, Dohmen et al. (2011) use a survey measure asking people to assess their willingness to take risks and find it to depend significantly on gender, age and parental background. In particular, they show that willingness to take risk declines with age, and that the effect is particularly strong for young and old ages. They also find that willingness to take risk increase with parental education and that females are less willing to take risk than men.

Summing up, there is some micro evidence in support of the hypothesis that young people are more impatient and more willing to take risk than older ones. To what extent this would turn out to affect fiscal performance of local governments is however an empirical issue we want to answer exploiting the reduction in minimum voting age in Norwegian local elections described in detail in the next section of the paper.

3. The trial, institutional setting, and empirical strategy

3.1. The trial³

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 a minimum of two years have the right to vote in local elections in Norway. 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, 2009.⁴ 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 towards 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

³ The description of the trial builds on Bergh (2013) as well as official information from the Ministry at the website "regjeringen.no".

⁴ 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.

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.⁵ 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).⁶ The general effects on outcomes from the terrorist attack are captured by the year fixed effects in the empirical model.

3.2. Institutional setup

We now describe local government financing and budgeting issues relevant for the understanding of the intertemporal adjustments in Norwegian local governments. The description builds heavily on Borge and Hopland (2020). Norway consisted of more than 420 local governments located in 18 different counties in the period covered by the empirical analyzes. They range 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.

While the local governments have large degree of discretion on the determination of current expenditure, the revenue side is heavily regulated. Income taxes and block grants are the primary revenue sources, and all local governments use the maximum allowed income tax rate. The opportunity to affect current revenues are limited to determination of user fees and property taxes under some specific constraints. The empirical analysis treats the sum of regulated income taxes and block grants as local government revenue (*"Frie inntekter"*) as exogeneous. 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)).

⁵ 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 treated governments in 2011 election was 9,406, while the number of voters 18 years or older was 275,894 in these governments.

⁶ Bharadwaj et al. (2021) describes the terrorist attack and analyze the short and long run consequences for the survivors, their families and peers.

Prior to each fiscal year, 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 makes decisions on current expenditure, revenue, investment activity and borrowing for the next fiscal year.

As in most other countries, local governments in Norway are subject to budget balance requirements (BBR). The main requirement is operational budget balance. In the budget (ex ante), the current revenue must be sufficient to cover current expenditure (wages and materials), and debt servicing costs (net interest payments and net installment on debt). The BBR requires a nonnegative net operating surplus and implies that borrowing can be used for investment purposes only. The BBR is imposed ex ante, meaning that actual deficits can be carried over or financed by rainy day funds. A deficit that is carried over must be repaid within 2 years, i.e. the surpluses in the following two years must be sufficient to cover the accrued deficits. Before 2001, local governments that violated the BBR were not subject to formal sanctions, but all local governments budgets had to have their budgets and borrowing approved by the county governor, the central government's representative in the county. A reform in 2001 abolished the system of administrative control with the local governments that complied with the BBR, while local governments that have violated the BBR are subject to budget and borrowing approval until the accrued deficit is covered. In addition, the new control system introduced a register to keep track of local governments that needed borrowing approval. The register named Register for Government Approval of Financial Obligations (abbreviated ROBEK in Norwegian) and informs financial institutions whether local governments need approval to raise new loans. The register is administered by the Ministry of Local Government and the list is publicly available on a Web-site and has received a lot of media attention, see Borge and Hopland (2020) for further discussion.

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 executive board chairman, which consists of senior council members and has 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 budgets for calendar years 2010 and 2011 were prepared and

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 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.

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 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.

3.3. Empirical strategy

We want to investigate the relationship between the age composition of the electorate and local government fiscal performance measured by the size of the net operating surplus. Evaluation of the introduction of voting rights to the younger age group described above fits naturally into a difference in differences research design. The difference between net operating surplus before and after the 2011 election in treated governments is compared with the same surplus difference in control governments not participating in the treatment. Under the assumption that the change in surplus in the control governments is a valid estimate of the counterfactual change in surplus in the treated 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 the real net operating surplus per capita, $TREAT_i$ is dummy equal 1 if the local government is among the treated governments, $POST_t$ is a dummy equal 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 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 with respect to fiscal performance 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 fiscal policy 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 fiscal policy platform after receiving information about participation in the trial to increase the probability of reelection. In that case, we would observe changed fiscal policy 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 treatment group dummy interact 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 treated local governments before actual 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

$$(3) \beta_{2007} = \beta_{2008} = \beta_{2009} = \beta_{2010} = \beta_{2011} = 0$$

$$(4) \beta_{2012} = \beta_{2013} = \beta_{2014} = \beta_{2015} = \beta$$

Tests of these and other restrictions are presented in the result section of the paper.

4. Data

In order to investigate the effect of youth enfranchisement and local government fiscal performance measured by net operating surplus within the research design 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. The capital city, Oslo, is excluded from the data set since it is both a local government and a county. We first describe the net operating surplus variable 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 operating surplus

Detailed data available from the local government accounts collected by Statistics Norway is the main data source for our analysis of net operating surplus in Norwegian local governments.

In the main part of our empirical analysis, we will use real operating surplus as the dependent variable.

Figure 1. Net operating surplus per capital development

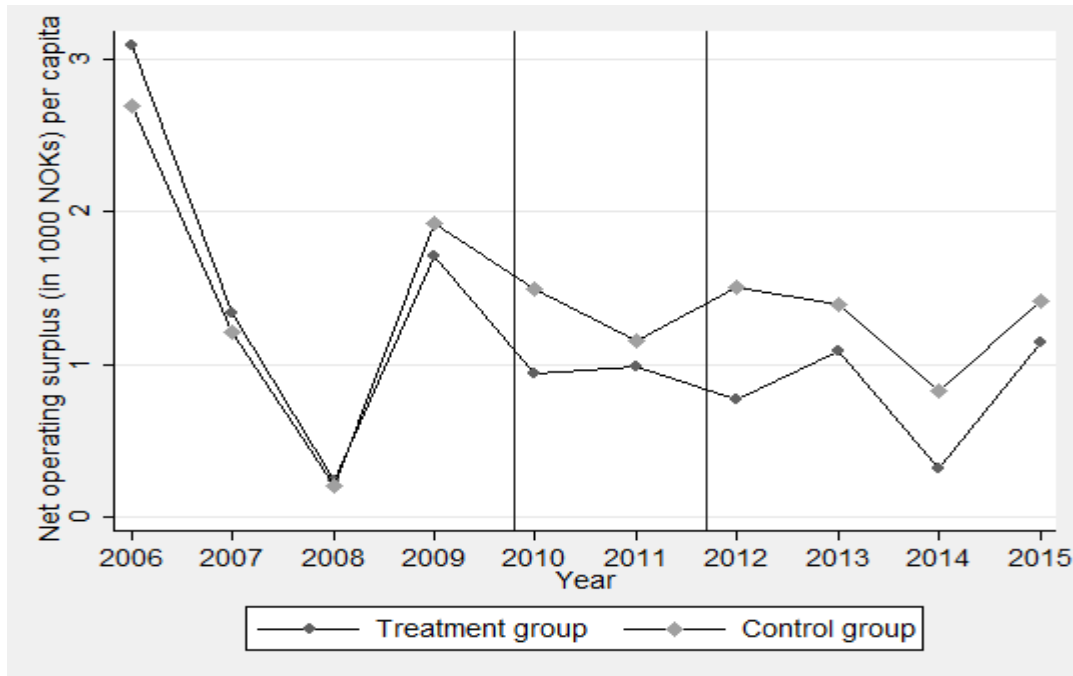


Figure 1 gives a visual picture of the development real net operating surplus per capita in 1000 NOK per capita in 2000 prices in the treated and control governments for 2006-2015. While the surplus development is very similar in the two groups in the pre-2010 period, development diverges substantially between the treatment and control groups from 2010 on, with a substantially lower surplus in the treatment group. This gives the first indication that giving youth aged 16-17 the right to vote decreased local government net operating surplus. However, empirical results in section 5 present more credible evidence on this issue.

4.2 Local government control variables.

To account for possible systematic differences across local governments in the development of net operational surplus we include a number of control variables, represented by X_{it} in equations (1) and (2). The variables are: Real regulated revenue per capita (in 2000 prices), population size, the shares of pree school children (age 0-5), school aged children (5-15) and the share of elderly (80+). Real regulated revenue is included to take into account the development in fiscal capacity in the local governments. Increased fiscal capacity is expected to increase the net operating surplus, as also found in Borge and Hopland (2021) among others. The demographic variables are included to account for differences in costs and spending needs

in the main services provided by the local governments and have been used for this purpose in earlier studies of fiscal performance in Norwegian local governments.

Earlier studies of net operating surplus in Norway and other countries has also included several political variables to account for differences across governments in the possibility to deal with fiscal adjustments like political fragmentation in the local council represented by a Herfindahl index, the ideological composition represented by a left-right party dummy among others⁸. We do not include such political variables in the set of controls as they may be possible outcomes of the key treatment variable in our setting and will thus introduce a “bad control problem” in the language of Angrist and Pischke (2009) ch. 3.2.3.

5. Empirical results

5.1. Baseline model results

Table 2 report the results fom different versions of the baseline model for the local operational surplus as formulated in equations (1) and (2). Column (1) shows the results from the most basic difference in differences formulation, without control variables, while column (2) adds fiscal capacity represented by the real local government revenue variable and demographic controls. Columns (3) and (4) show similar specifications when the treatment group variable is replaced by local government fixed effects. In all specifications, the point estimate for the impact of extending the franchise to age 16-18 voters is negative, while imprecisely estimated. However, as argued above, it is an open question to what extent the incumbents in the selected governments acted on the information available on their treatment status already before the 2011 election.

To further safeguard against possible bias due to the actual timing of treatment and to check for parallel trends across treatment and control groups, Table 3 reports several extended versions of the baseline model. Columns (1) and (2) in Table 3 extend the model by including separate treatment effects for the years 2010 and 2011, i.e. the years after the announcement of selected governments, but before the local council elected in 2011 could have influenced the budgetary process. Column (3) reports an event study version including treatment interaction terms for all years. Column (4), (5) and (6) exclude observations for the years 2010 and 2011.

⁸ See Borge (2005), Hopland (2013) and Borge and Hopland (2020) for Norwegian studies and Petterson-Lidbom (2010) for a Swedish study.

In the most general specification in column (3) using all observations in the sample, we cannot reject restriction (3) above that the interaction effects are zero before 2012 (F-value=1.21, P-value=0.303). 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=1.25, P-value=0.292). Also, we cannot reject the joint hypothesis that (3) and (4) holds at the same time (F-value=1.2, P-value=0.296).

Table 2. Baseline results.

	(1)	(2)	(3)	(4)
TREAT×POST2011	-0.395 (0.367)	-0.389 (0.344)	-0.384 (0.367)	-0.397 (0.299)
TREAT	-0.0598 (0.262)	0.0438 (0.308)		
Real local gov. revenue per capita		0.000193*** (4.14e-05)		0.000584*** (0.000152)
Population (in 10000)		0.0266 (0.0214)		0.0449 (0.247)
Share children 0-5 (%)		0.268** (0.119)		0.126 (0.226)
Share children 6-15 (%)		0.000948 (0.0960)		-0.252** (0.125)
Share elderly 80+ (%)		-0.158 (0.162)		0.0760 (0.171)
Year FE	Yes	Yes	Yes	Yes
Local gov. FE	No	No	Yes	Yes
Observations	4,279	4,279	4,279	4,279
R-squared	0.035	0.144	0.421	0.458

Notes: Dependent variable is net operating surplus per capita. Standard errors clustered at the local government level in parentheses. ***, **, * denotes significant at 1%, 5% and 10% level, respectively.

Looking at the estimation results when 2010 and 2011 are excluded from the sample, the effect of the treatment variable is -0.54 and -0.56, in columns (4) and (5). The effects is significant at 10% level in column (5) with a full set of local government fixed effects included.

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=0.70, P-value=0.550) nor restriction (4) that the treatment effects are homogenous in the period 2012-2015 (F-value=1.26, P-value=0.289). Also in this case, we cannot 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=1.20, P-value=0.307).

Table 3. Including separate treatment effects for 2010 and 2011, event study formulations, and excluding 2010 and 2011 from sample

	(1)	(2)	(3)	(4)	(5)	(6)
TREAT×POST2011	-0.535 (0.375)	-0.566* (0.299)		-0.535 (0.375)	-0.561* (0.299)	
TREAT×YEAR2012			-1.381** (0.546)			-1.387** (0.544)
TREAT×YEAR2013			-0.896* (0.512)			-0.900* (0.513)
TREAT×YEAR2014			-1.024** (0.435)			-1.024** (0.438)
TREAT×YEAR2015			-0.865* (0.494)			-0.862* (0.499)
TREAT×YEAR2010	-0.646 (0.410)	-0.570 (0.387)	-1.047** (0.487)			
TREAT×YEAR2011	-0.255 (0.490)	-0.438 (0.491)	-0.915 (0.708)			
TREAT×YEAR2007			-0.520 (0.410)			-0.525 (0.406)
TREAT×YEAR2008			-0.614 (0.608)			-0.619 (0.610)
TREAT×YEAR2009			-0.772 (0.678)			-0.788 (0.684)
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
2010 and 2011 excluded	No	No	No	Yes	Yes	Yes
Observations	4,279	4,279	4,279	3,423	3,423	3,423
R-squared	0.421	0.458	0.458	0.446	0.482	0.482

Notes: Dependent variable is net operating surplus per capita. Standard errors clustered at the local government level in parentheses. Local government controls: population (in 10000), local government revenue per capita, population share 0-5, population share 6-15, population share 80+. ***, **, * denotes significant at 1%, 5% and 10% level, respectively.

While the numerical results and statistical precision of the estimated effects differs somewhat between specifications, the broad conclusion so far is that the extension of the voting franchise to include age group 16-18 contributed to a reduction in operational surplus. To safeguard against confounding effects due to possible pre-election adjustments among the units in the treatment group, we take the estimated model in column (6) in Table 3 as our preferred specification. According to this model the treated governments experienced a reduction in net

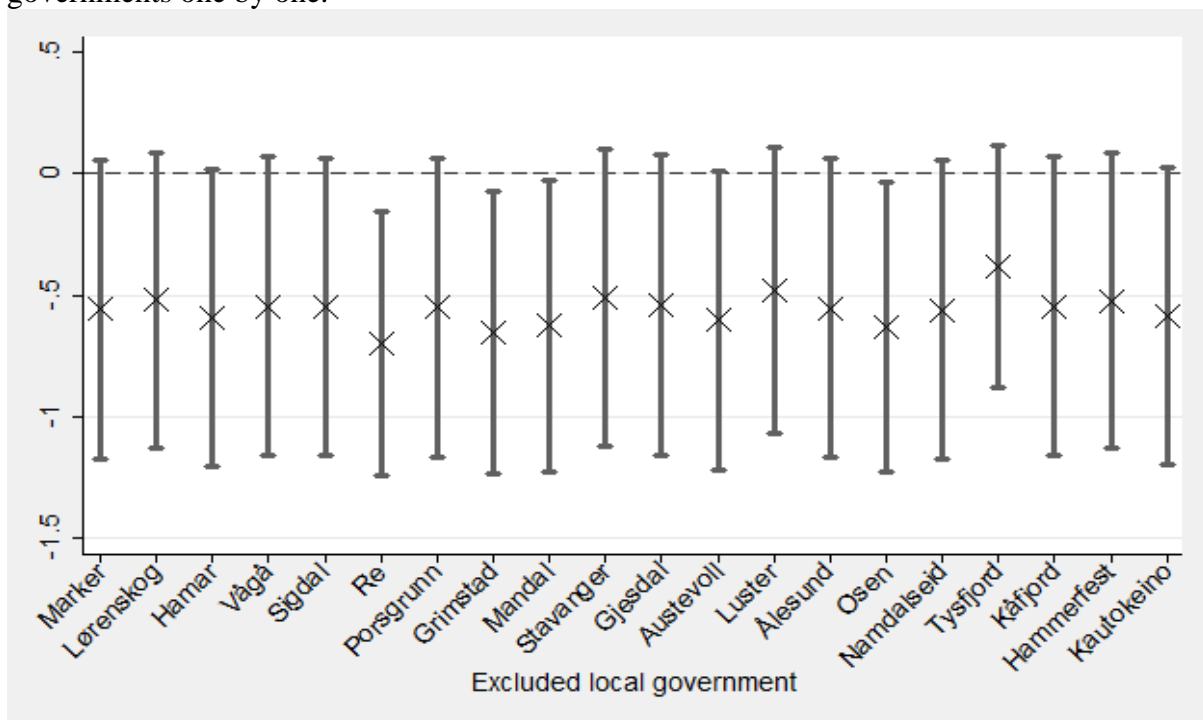
operational surplus of approximately NOK 560 per capita⁹. We will use this specification as point of departure in subsequent robustness checks.

5.2. Robustness checks

Composition of the treatment group

Are the results driven by specific units in the treatment group? To check this, we estimate our preferred model in Table 3 excluding one by one of the treated local governments. Figure 2 shows the estimated treatment effects using this procedure. While there is some differences across the left-out units, the broad picture is that the point estimate is quite stable around 0.56.

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



Allowing for linear local government trends

The results in section 5.1 provide evidence supporting the parallel trend assumption. Nevertheless, we are still concerned that unobservables may affect the treatment and control groups differently. To account for the effect on operational deficits from unobservables that develops smoothly within units we include two types of linear trends in the models. In Table 4, column (1) includes linear trends interacted with population level as of 2008, while column (2) includes linear trends interacted with age shares in 2008. These specifications may to some

⁹ To put in context, 1 NOK approximately equals 0.1 Euro

extent account for possible Tiebout sorting, i.e. endogeneous sorting of the population in the aftermath of the announcement of the extension of the voting franchise. The estimated effects at -0.59 and -0.56 in column (2) and (3) seems mainly unaffected by inclusion of these trends with point estimates close to those obtained in Table 3. Column (3) includes linear local government specific trends. In this specification the point estimate is still negative, but drops substantially to -0.29 and is no longer statistically significant. It should be pointed out that this is a demanding specification in terms of variation in the time dimension in our case with only a 10 year period.

Table 4. Specifications including local government linear trends.

	(1)	(2)	(3)
TREAT×POST	-0.585* (0.301)	-0.558* (0.335)	-0.278 (1.003)
Year * population (in 10000) in 2008	Yes	No	No
Year * age shares in 2008	No	Yes	No
Linear local government trends	No	No	Yes
P-value parallel trend	0.528	0.668	0.269
Observations	3,412	3,412	3,423
R-squared	0.126	0.126	0.341

Notes: Dependent variable is net operating surplus per capita. Standard errors clustered at the local government level in parentheses. The models are extensions of the model in column (5) in Table 3, where 2010 and 2011 are excluded from the analysis. A constant term and year and local government fixed effects are included in all models. Local government controls: population (in 10000), local government revenue per capita, and population shares 0-5, 6-15, and 80+. Population 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. ***, **, * denotes significant at 1%, 5% and 10% level, respectively.

Alternative control groups

In our differences in differences research design, the control group provides an estimate of the development of net operational surplus in the treated local governments in the absence of treatment, i.e. the counterfactual. So far, we have included all local governments that did not participate in the trial in the control group. Although the robustness checks above support the assumption of parallel trends, it is nevertheless possible that the control group deviates from the treatment group in terms of unobservables that generate systematic different development in net surplus in control and treatment group in the period after the franchise extension.

In this section, we first define and use two alternative control groups. The applicants not selected into treatment by the Ministry, may at first sight appear as a natural control group. However, the applicants will themselves be a selected group of local governments as the

decision to apply is potentially determined by expected outcomes from the trial. Thus, it is not obvious that the applicants not receiving treatment represents a better estimate of the counterfactual than the total number of governments that did not receive treatment. Table 5 column (1) nevertheless show results when the applicants not selected for treatment are defined as the control group. Compared to the estimate in Table (3), the point estimate increase to 0.77 and is significant at the 5% level.

As a second control group, we exploit that before the local election in 2015, the Ministry of Local Government and Labor announced in 2014 that they wanted to continue the 2011 trial with 20 selected governments in 2015. The selected governments in the 2015 election consisted of 10 of the 2011 governments selected governments, while 10 new governments were selected into treatment. The 10 new governments selected into treatment in 2015 may have more in common with the 20 governments receiving treatment in 2011 in terms of unobservables. The drawback of this strategy is the low number of governments in the control group. Column (2) in Table 5 shows the estimated effect which is -0.72 and significant at the 10% level.

Table 5. Alternative control groups.

Control group consists of:	(1) Applicants not selected in 2011	(1) Units treated in 2015 and not in 2011	(3) Semiparametric DiD on full sample	(4) Semiparametric DiD utilizing the 2011 applicant sample
TREAT×POST	-0.773** (0.384)	-0.721* (0.412)	-0.557 (0.384)	-0.742** (0.371)
P-value parallel trend	0.151	0.217	-	-
Year FE	Yes	Yes	-	-
Local gov. controls	Yes	Yes	-	-
Local government FE	Yes	Yes	-	-
Polynomial order			1	1
Observations	1,133	238	400	128

Notes: The dependent variable is net operating surplus per capita in columns (1)-(2). The dependent variable in columns (3) and (4) is the difference of net operating surplus per capita 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: population (in 10000), local government revenue per capita, 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. ***, **, * denotes significant at 1%, 5% and 10% level, respectively.

As a final strategy to address the challenge to define a valid control group, we apply a semiparametric difference in differences estimator proposed by Abadie (2005).¹⁰ The estimator represents a generalization of the conventional difference in differences model in the case when observable characteristics explain differences in the trends of the dependent variable in the treatment and control groups. 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 5 shows the estimation results from this method using the total number of governments not selected into treatment as the pool of control governments. The point estimate is -0.56 and similar to the effects obtained in Table 3 above, but not statistically significant. Column (4) in Table 5 shows the results when the applicants not selected in the 2011 trial are used as the pool of control governments. The point estimate is -0.74 and very close to that in column (1) and significant at the 5% level.

5.3. Components of the net operational surplus, investments, and debt

TBW

Table 6. Allocation of the net operating surplus

	(1)	(2)	(3)	(4)	(5)
Dep.variable (per capita)	Net increase in disposable funds	Net coverage earlier deficits	Net increase restricted funds	Transfer to the investment accounts	Transfer to future uncovered expenditures
TREAT*POST	-0.184 (0.1960)	-0.152 (0.1324)	0.151* (0.0813)	-0.277 (0.2181)	-0.397 (0.2527)
P-value parallel trend	0.236	0.696	0.197	0.149	0.019
Year FE	Yes	Yes	Yes	Yes	Yes
Local gov. controls	Yes	Yes	Yes	Yes	Yes
Local government FE	Yes	Yes	Yes	Yes	Yes
Observations	3,423	3,423	3,423	3,423	3,423

¹⁰ The estimator is implemented in Stata and described in Hounghbedji (2016)

Table 7. Main financial aggregates

Dep.variable (per capita)	(1) Gross income	(2) Gross expenditures	(3) Net financial result	(4) Depreciation	(5) Net dept
TREAT*POST	-0.540 (0.4464)	-0.242 (0.4942)	-0.349* (0.2008)	0.087 (0.0943)	0.963 (1.6160)
P-value parallel trend	0.023	0.907	0.558	0.941	0.621
Year FE	Yes	Yes	Yes	Yes	Yes
Local gov. controls	Yes	Yes	Yes	Yes	Yes
Local government FE	Yes	Yes	Yes	Yes	Yes
Observations	3,423	3,423	3,423	3,423	3420

Table 8. Income components

	(1)	(2)	(3)	(4)	(5)
<i>Panel A</i> <i>Dep.variable</i> <i>(per capita)</i>	User charges	Sales	Quid pro quo	Block grant	Other central government revenues
TREAT*POST	0.068** (0.0296)	0.158 (0.1652)	0.128 (0.2972)	0.281 (0.2310)	-0.443** (0.2143)
P-value parallel trend	0.403	0.253	0.037	0.263	0.556
<i>Panel B:</i> <i>Dep.variable</i> <i>(per capita)</i>	Other	Taxes	Property tax	Other taxes	
TREAT*POST	-0.287 (0.1873)	-0.339 (0.2356)	-0.171 (0.1083)	0.067 (0.0435)	
P-value parallel trend	0.007	0.277	0.015	0.582	

Table 9. Investments

	(1)	(2)	(3)	(4)	(5)
<i>Sector</i>	Total	Admin.	Kinder- garten	School	Health, elderly care
TREAT×POST	1.211 (1.0363)	-0.077 (0.1082)	-0.146 (0.1268)	0.084 (0.3708)	1.109* (0.5926)
P-value parallel trend	0.949	0.045	0.039	0.172	0.134
<i>Sector</i>	Social benefits	Child custody / care	Water, drains, waste	Area developm., environm.	Culture
TREAT×POST	-0.014 (0.0139)	-0.001 (0.0028)	0.045 (0.0939)	-0.064 (0.0390)	0.203 (0.4313)
P-value parallel trend	0.376	0.142	0.357	0.029	0.900
<i>Sector</i>	Church	Transport- ation & commns	Housing	Business, industry	Fire
TREAT×POST	-0.029 (0.0365)	0.121 (0.1531)	0.047 (0.2152)	-0.051 (0.2662)	0.004 (0.0299)
P-value parallel trend	0.091	0.002	0.056	0.950	0.469

Notes: N=3,423. The dependent variable is real gross investments per capita. Standard errors clustered at the local government level are reported in parentheses. Controls variables are: population (in 10000), local government revenue per capita, population share 0-5, population share 6-15, and population share 80+. 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. ***, **, * denotes significant at 1%, 5% and 10% level, respectively.

6. Concluding comments

This paper has addressed the potential effect of extending the voting franchise to youths on local government fiscal performance. While several studies exist on the effect of franchise extension to new groups in society on government size and spending on different public services like health and education, there is little knowledge of the effect of franchise extensions on fiscal performance in terms of budget deficit and public debt. In particular, little is known about the fiscal performance effects of broadening democracy in terms of lowering the minimum voting age that has been proposed in several countries. This paper exploits a novel trial in Norwegian local elections in 2011 to provide empirical evidence on the effect on fiscal performance from lowering the minimum voting age from 18 to 16 years old. The trial implied a reduction in the minimum voting age in 20 selected local governments while the rest continued to have voting age at 18. Using a difference in differences research strategy, we find that this voting age change reduced net operating surplus by almost 600NOK (60euro) per capita.

Taken literally, the finding that lowering the voting age decrease budget surplus is consistent with some micro evidence that young individuals have higher discount rates and is more likely to take risk than older ones. However, it would be a clear overstatement to conclude on this issue based on a single study based on data aggregated to the local government level. Obviously, more research from different countries and other institutional settings and using individual and aggregate data is needed to confirm this interpretation.

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