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## Young Voters and Budget Deficits

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## Young Voters and Budget Deficits

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# Young Voters and Budget Deficits

## 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. Using a difference in differences research strategy, we find that this voting age change reduced the net operating surplus by around 600NOK (€60) per capita. This finding is consistent with micro evidence that young individuals have higher discount rates and are more likely to take risk than older ones, although other evidence is needed to confirm that interpretation. Further heterogeneity analysis demonstrates that increased deficits (reduced net operating surplus) due to the extension of the youth voting franchise mainly appear in governments with low party fragmentation and a large share of socialist politicians in the local council.

JEL-Codes: C230, D720, H720.

Keywords: local public finance, fiscal performance, minimum voting age.

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# 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 regarding 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 levels has been at the forefront of the literature. Key issues have been the role of soft budget constraints in countries with a large number of transfers between the central and local levels, balanced budget rules, and their effects on the fiscal performance of subnational governments.<sup>3</sup>

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 an important issue in the public debate. Applied studies of fiscal adjustment usually include age composition and other demographic variables to account for variations 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 exogenous changes in the electorate's composition on budget deficits is Krogstrup and Walti (2011). Exploiting a staggered introduction of female suffrage in Swiss cantons to identify causal effects, they find that female enfranchisement decreased budget deficits in the cantons.

Similar exogenous changes in the *age composition* of the electorate are hard to find. The aging of the population implies 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 by lowering the voting age is on the political agenda in many countries, see Bergh (2013) and Wagner et al. (2012). An important issue is to what extent a lower minimum voting age would affect the fiscal performance of governments in terms of

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<sup>3</sup> 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 (2001, 2010) studies fiscal adjustment in Swedish local governments while Borge (2005) and Borge and Hopland (2020) consider fiscal adjustments in local governments in Norway.

budget deficits and debt. This paper adds to the literature by investigating the relationship between *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. We use this event to provide quasi-experimental evidence of the extension of the youth voting franchise on the net operating surplus in local governments in Norway. The size of the net operating surplus plays an important role in the public debate on local fiscal performance in Norway. The central government has imposed some general guidance regarding a requirement that the net operating surplus should be positive to avoid a reduction in the local government's net wealth.<sup>4</sup> Local governments that violate the requirement are subject to administrative sanctions regarding budget and borrowing control and being put on a publicly available register on units that do not comply with the central government requirement. This list has received much media attention. It serves as a “list of shame”, as being on the list signals the local government's poor fiscal performance (see Borge and Hopland (2020)).

While related to the empirical literature on fiscal performance of 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 (Carruthers and Wannamaker, 2015).<sup>5</sup> 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 (2023)

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<sup>4</sup> Monitoring and constraints on government fiscal behavior in terms of fiscal rules has received a lot of attention in the literature. Halac and Yared (2014) provide a theoretical analysis of the optimal structure of fiscal rules when governments have time-inconsistent preferences. Rodden et al. (2003) contains examples of increased central government monitoring of local fiscal policies in several federations around the world. Bohn and Inman (1996) find that balanced budget requirements have significant positive effects on US states' fiscal performance. von Hagen and Harden (1995) demonstrate the role of the budget process itself as a commitment device for fiscal discipline.

<sup>5</sup> Studies from Europe find less clear evidence on the spending effect of voting franchise extensions. Aidt et al. (2006) 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 et al. (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.

find a negative impact on compulsory school spending from reducing the minimum voting age from 18 to 16 using the same quasi-experimental research design used in this paper. The negative impact on compulsory school spending is consistent with selfish voting behavior for the newly enfranchised group, as they had just finished compulsory education and thus had no direct benefit from compulsory school spending. They also find that reducing the minimum voting age led to a significant increase in the probability of young candidates being elected to the local council. However, it did not impact voting turnout, political fragmentation, or the share of left-wing politicians in the local government council.

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 the net operating surplus per capita by about 600 Norwegian kroner (60 euros).

The rest of the paper is organized as follows: Section 2 presents the theoretical background. Section 3 presents the trial and empirical strategy. Section 4 describes data, while Section 5 presents the main empirical results. Robustness analysis is presented in Section 6, while Section 7 allows for heterogeneity. Section 8 concludes.

## **2. Theoretical considerations**

Modern fiscal performance literature 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 (often defined by age), while everyone pays taxes, 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 in overcoming 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 several studies have focused 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 also modified to include the production of public goods in the subsequent empirical literature following the contribution of Husted and Kenny (1997). Krogstrup and Wälti (2011) consider the impact of including females in the voting franchise on local public budget deficits.

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 affect the preferences of the voting population. The sufficient requirement is that local governments' policy reacts to the voters' changed preferences. Since this paper focuses on fiscal performance regarding budget 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. Some examples are changing preferences for intergenerational redistribution, changing the preferences for intertemporal smoothing of public consumption and precautionary savings, or changing the ability to handle the common pool problem as discussed above.

In line with Krogstrup and Wälti (2011), we take the view that the 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 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 Wälti (2009) present a simple two-period model with two different groups of voters (men and women). They argue that the enfranchisement of women will change the public saving rate (or the budget deficit) if the two



groups (men and women) have different time preferences or attitudes toward risk. In particular, they show that if women are more prudent towards risk or discount time less than men, female 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 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, the literature on behavioral economics using quasi-hyperbolic utility functions has 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) review 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. In particular, discount rates appear to decline with age, at least after middle age. Read and Read (2004) provide experimental evidence from the 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, as considered by Laibson (1997) and O'Donoghue and Rabin (1999), a tendency to downplay or ignore future consequences may make adolescents differ from adults in their time preferences for public goods consumption.

Regarding attitude toward 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 risks 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.

To sum up, some micro evidence combined with insights from behavioral economics suggest that young people are more impatient and more willing to take risks than older ones and predicts that reducing the minimum voting age would increase budget deficits. On the other hand, young

voters have, by definition, a longer time horizon and hence stronger incentives to take future consequences of today's fiscal policies into account than older ones, as argued by Song et al. (2012) and Alesina et al. (2019). This argument partially suggests that reducing the minimum voting age would reduce budget deficits. The net effect of reducing the minimum voting age on budget deficits is thus theoretically ambiguous. The theoretical ambiguities described in this section highlight that the net effect of reduced voting age is largely an empirical question.

### 3. The trial, institutional setting, and empirical strategy

#### 3.1. The trial<sup>6</sup>

The ordinary rule in Norway is that all inhabitants from the election year they turn 18 years of age and have been living in the local government for at least 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.<sup>7</sup> According to the Ministry, the selection was made to have a variety of governments in terms of size, geographical location, the political composition of the governing council, and the population's age composition. In addition to these objective criteria, the Ministry actively looked for local governments with an activist policy toward getting the youth involved in political issues. The extension of the voting franchise applied only to the election of local government councils and not to the election of the county council held on the same date. According to Bergh and Ødegård (2013), the newly enfranchised 16- and 17-year-olds increased the voting franchise by approximately 3.4 percent in treated governments.<sup>8</sup> 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. The terrorist attack likely affected political participation and voting in the election. However, since the

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

<sup>7</sup> 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.

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

selection of participating governments was made more than one year earlier, there is no apparent reason why the terrorist attack should affect relevant patterns systematically differently in treatment and control governments, as argued in Bergh (2013).<sup>9</sup> 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 the Norwegian local government political context and the financing and budgeting issues relevant to understanding the intertemporal adjustments. The description builds heavily on Borge and Hopland (2020). Norway consisted of more than 420 local governments in 18 different counties in the period covered by the empirical analysis. 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.

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

While the local governments have a large degree of discretion on determining 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 is limited to determining user fees and property taxes under some specific constraints. The empirical analysis treats the sum of regulated income taxes and block grants as exogenous local government revenue (*“Frie inntekter”*). The

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

<sup>10</sup> 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 chairperson. Currently this is implemented in Oslo, Bergen, and Tromsø. All other local governments use the executive board model.

block grants are based on objective criteria meant to reflect the local government's demographic and socio-economic situation (see Rattsø and Sørensen (2010)).

Before 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 decides on current expenditure, revenue, investment activity, and borrowing for the next fiscal year.

Like most other countries, local governments in Norway are subject to budget balance requirements (BBR). The current system has been in place since 2001, and the main requirement is operational budget balance. In the budget (ex-ante), the current revenue must be sufficient to cover current expenditures (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 only be used for investment purposes. Given that the BBR is imposed ex-ante, actual deficits can be carried over or financed by rainy-day funds. A deficit that is carried over must be covered within the following two years. Before the reform in 2001, all local governments budgets had to have their budgets and borrowing approved by the county governor, the central government's representative in the county. The reform also introduced a new register to keep track of local governments that needed borrowing approval. The register is 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 website and has received much media attention; see Borge and Hopland (2020) for further discussion.

In recent years, the central government has also imposed some general guidance regarding the size of the net operating surplus and set requirements that all local governments must impose their own fiscal rules. The motivation for guidance regarding the level of the net operating surplus is based on the fact that this needs to be positive for a local government to maintain its wealth/capital since re-investment costs are assumed to exceed the depreciation of current wealth. This highlights the importance of our outcome variable in the public debate on fiscal performance and the central government policies towards the local governments in the Norwegian setting.

### 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. Evaluating the introduction of voting rights to the younger age group described above fits naturally into a difference in differences research design. The difference between the net operating surplus in treated governments before and after the 2011 election 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 standard difference in differences strategy.

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

where  $i$  denotes local government,  $t$  is year,  $y_{it}$  is the real net operating surplus per capita,  $TREAT_i$  is a dummy equal to 1 if the local government is among the treated governments,  $POST_t$  is a dummy equal to 1 if the observation is from a year with budget decisions taken by the local council elected in 2011.  $\gamma_i$  is local government fixed effects.  $\delta_t$  is year fixed effects, and  $X_{it}$  is a vector of local government control variables specified below. As usual,  $\beta_1$  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.

An important 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 policy (operating surplus) throughout the election period, while the political parties selected their candidates for the next election (2011) 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 (operating surplus) already in the election year or even when the selection was announced.

To assess the parallel trend assumption and allow for anticipation and year-specific treatment effects, we can estimate an event study version of the model as in equation (2), using 2009 as the reference year.

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

This specification lets the treatment group dummy interact with all year dummies. We can use this general model to assess the relevance of several types of restrictions. First, we can perform a placebo test, i.e., test the restriction that the change in voting age affected fiscal performance in treated local governments before the announcement of the participation in the trial in the fall of 2009 as formulated in (i). Second, we can impose and test the restriction that the treatment effect in the post-2011 period is constant over time, as formulated in (ii) while allowing for separate anticipation effects in 2010 and 2011. Third, we can impose and test the assumption that the treatment period includes anticipation effects in 2010 and 2011 as well as the post-2011 period and that the treatment effects are the same across these years. This restriction is formulated in (iii). Finally, we can impose and test joint restrictions of (i), (ii), and (iii).

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

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

$$(iii) \beta_{2010} = \beta_{2011} = \beta_{2012} = \beta_{2013} = \beta_{2014} = \beta_{2015} = \beta$$

The results from estimations of event study models with and without restrictions and corresponding tests are presented in the result section.

## 4. Data

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. 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. (2020).

### 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 the 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 capita, 2006-2015

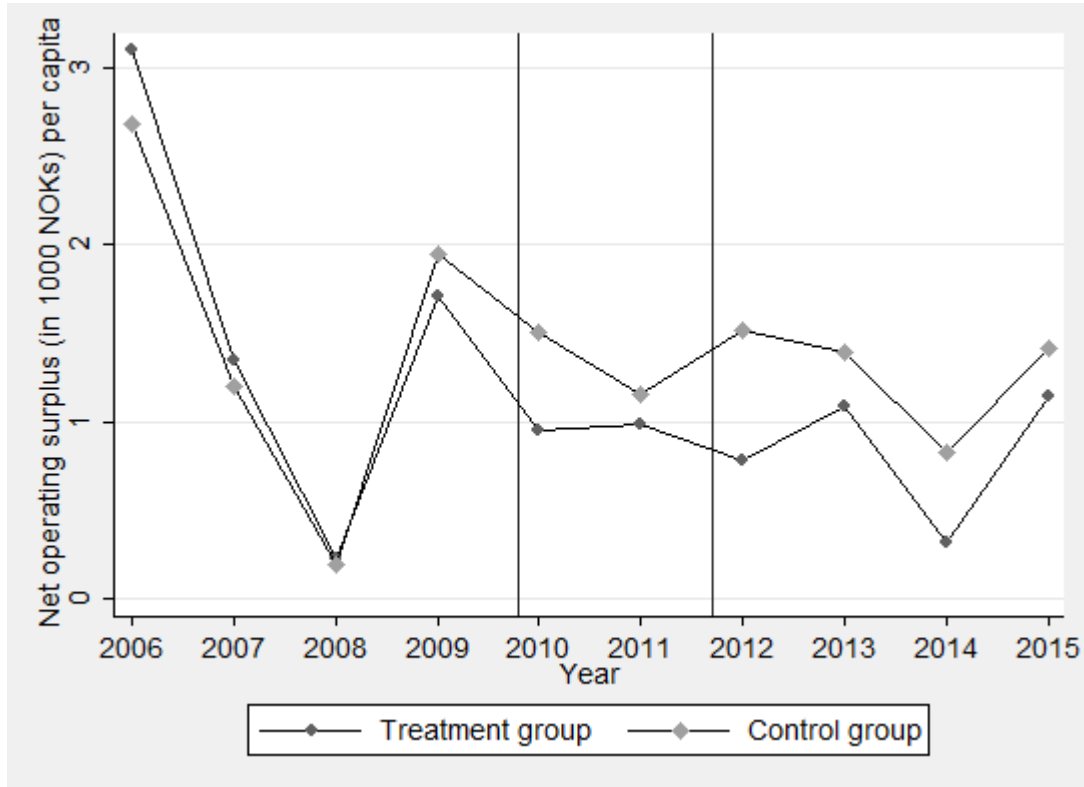


Figure 1 gives a visual picture of the real net operating surplus per capita in 1000 NOK per capita in 2000 prices in the treated and control governments for 2006-2015. While the surplus development is very similar in the two groups in the pre-2010 period, development diverges substantially between the 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 the local government's 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 several 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 percentage shares of preschool children (age 0-5), school-aged children (6-15), and the share of elderly (80+). Real regulated revenue is included to take into account the development in fiscal capacity in the local governments. An important feature of this variable is that it only includes central government transfers (block grants and revenue from income and wealth taxation) and no revenues where there is local discretion. Increased fiscal capacity is expected to increase the net operating surplus, as found in Borge and Hopland (2020) 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. Table 1 reports descriptive statistics for the control variables included in the empirical analysis before the announcement of the trial (2008). As shown in the table, the treated governments appear to be quite similar to the other local governments. The exception is population size, which is 8,000 higher in treated local governments.

Table 1. Descriptive statistics of control variables in 2008

	Treated local governments	All other local governments	Difference
Real local gov. revenue ("Frie inntekter") per capita	25.202 (4.2847)	26.621 (6.551)	-1.420 [0.3397]
Population (in 10000)	1.736 (2.709)	0.936 (1.858)	0.800 [0.0674]
Share children 0-5 (%)	7.087 (0.952)	6.734 (1.143)	0.3536 [0.1747]
Share children 6-15 (%)	13.260 (1.461)	13.602 (1.303)	-0.3416 [0.2557]
Share elderly 80+ (%)	5.275 (1.813)	5.630 (1.584)	-0.3546 [0.3321]

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

Earlier studies of net operating surplus in Norway and other countries have also included several political variables to account for differences across governments in the possibility of dealing 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 others.<sup>11</sup> 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. However, in the heterogeneity analysis below, we consider to what extent the treatment effect depends on the level of political variables in the pre-treatment period.

<sup>11</sup> See Borge (2005), Hopland (2013) and Borge and Hopland (2020) for Norwegian studies and Petterson-Lidbom (2010) for a Swedish study.



## 5. Main results

Table 2 reports the results from different versions of the baseline model for local government operational surplus as formulated in equations (1). Column (1) shows the results from the most general event study formulation corresponding to equation (2) above, where the treatment dummy is interacted with all years from 2006-2015, with 2009 as the reference year. Columns (2)-(4) report different restricted versions of the full event study model. The model in column (2) imposes parallel trends, i.e. restriction (i). Column (3) impose all treatment-year interactions before 2009 to be zero and the post-2011 treatment-year interactions to be equal, i.e. restrictions (i) and (ii). Column (4) shows results from a specification with the anticipation effects assumed to be equal to the post-2011 treatment effects as corresponding to restrictions (i) and (iii).

In the most general specification in column (1), the point estimates for the treatment-year interaction term are positive in all years before 2009 (the reference year) and highly insignificant. The test of restriction (i) reveals that the assumption of parallel trends cannot be rejected (p-value=0.511). For the post-2009 years, the treatment-year interaction terms are negative. Figure 2 illustrates the estimated effects in models with and without restriction (i) imposed. Further, restriction (ii) that the post-2011 treatment effects are equal across years cannot be rejected (p-value=0.289). Imposing these two restrictions together cannot be rejected (p-value=0.272). The corresponding estimate, as reported in column (3), implies that the extension of the voting franchise reduced local government surplus by approximately NOK 615 ( $\approx$ €62) in the post-2011 election period.<sup>12</sup> Finally, imposing restriction (iii), that the treatment period is extended to include potential anticipation effects in 2010 and 2011 together with the parallel trend assumption (i), cannot be rejected (p-value=0.447). The estimated treatment effect when these restrictions are imposed, as reported in column (4), implies a statistically significant reduction in local government surplus by NOK 583 ( $\approx$ €59).

As to the impact of fiscal capacity, the estimated coefficient in front of regulated revenue per capita reported in Table A1 in the Appendix is 0.681, which means that a revenue increase by NOK 1000 ( $\approx$ €100) leads to an increase in operating surplus by NOK 681 ( $\approx$ €68). The estimated revenue impact is in the same ballpark found in Borge and Hopland (2020). Thus,

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<sup>12</sup> 1 NOK approximately equals 0.1 Euro.

according to our estimates, the impact of reducing the minimum voting age to 16 years is fairly close to the impact of a 1000 NOK per capita reduction in regulated revenue.

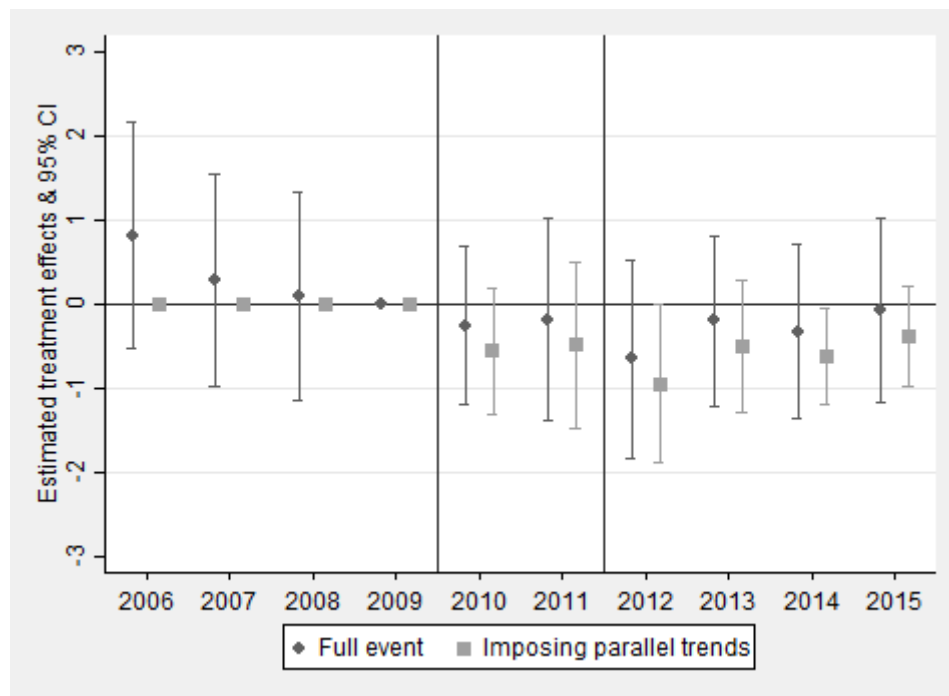
Table 2. Main empirical results

	(1)	(2)	(3)	(4)
<i>Treatment interacted with:</i>				
- Post2011			-0.615**	
- Post2009				-0.583**
- Year2006	0.815	-		
- Year2007	0.279	-		
- Year2008	0.095	-		
- Year2009	-	-		
- Year2010	-0.262	-0.558	-0.558	
- Year2011	-0.186	-0.482	-0.482	
- Year2012	-0.655	-0.951**		
- Year2013	-0.200	-0.496		
- Year2014	-0.332	-0.628**		
- Year2015	-0.085	-0.382		
<i>Statistical tests (p-values reported):</i>				
(i) Parallel pre-trend assumption	0.511			
(ii) y12=y13=y14=y15	0.289			
Joint F-test of (i) and (ii)	0.272			
(iii) y10=y11=y12=y13=y14=y15	0.537			
Joint F-test of (i) and (iii)	0.447			

Notes: Dependent variable is the net operating surplus per capita (2000 prices measured in 1000 NOKs). The sample includes all Norwegian local governments from 2006 to 2015, excluding the capital city, Oslo. All models include the control variables discussed in Section 4 (revenue, population, and age shares). See Table A1 for fully reported results. Estimated standard errors (reported in Table A1) are clustered at the local government level. \*\*\*, \*\*, \* denotes significant at 1%, 5% and 10% level, respectively.

While the estimated effects' numerical results and statistical precision differ somewhat between specifications, the broad conclusion from the restricted specifications is that the extension of the voting franchise to include the age group 16-17 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 (3) in Table 2 as our preferred specification. According to this model, the treated governments experienced a reduction in net operating surplus of NOK 615 per capita. In subsequent robustness checks, we will use this specification as the point of departure.

Figure 2. Event study formulations



Note: See Table A1, columns (1) and (2), for complete model results.

So far, we have considered the effect of extending the youth voting franchise on operating surplus holding revenue or fiscal capacity (“Frie inntekter”) constant, similar to what is done in other analyses of operating surplus in Norwegian local governments. While exogeneity is a realistic assumption regarding central government grants and income taxes, since all local governments use the maximum income tax rate allowed, this assumption does not apply to local property tax revenue. The local governments can choose to impose property taxation. They can also choose what type of property to tax (residential, cottages, businesses, hydroelectric power stations, and other works), whether to have a basic deduction, and the tax rate within certain limits, see Borge and Marcinko (2019) for a detailed description.

One possible mechanism behind the reduction in net operating surplus following the extension of the youth voting franchise is that the local council removed the property tax (extensive margin), reduced the tax rate (intensive margin), or both. In this section, we estimate the treatment effect on the total revenue per capita from property taxes representing potential effects on the combined extensive and intensive margin.

Table 3. Income from property taxation

	(1) Total property tax per capita	(2) Residential property tax per capita	(3) Commercial property tax per capita
TREAT×POST2011	-0.148 (0.151)	-0.084 (0.080)	-0.065 (0.121)
TREAT×YEAR2010	-0.091 (0.079)	-0.076** (0.029)	-0.015 (0.071)
TREAT×YEAR2011	-0.069 (0.132)	-0.050 (0.050)	-0.019 (0.121)
Observations	3,848	3,848	3,848
R-squared	0.983	0.707	0.988

Notes: The dependent variables are measured in 1000 NOKs in 2000 prices. Standard errors clustered at the local government level in parentheses. A constant term, year and local government fixed effects, and local government controls (population in 10000, local government revenue per capita, population share 0-5, population share 6-15, population share 80+) are included in all models. \*\*\*, \*\*, \* denotes significant at 1%, 5% and 10% level, respectively.

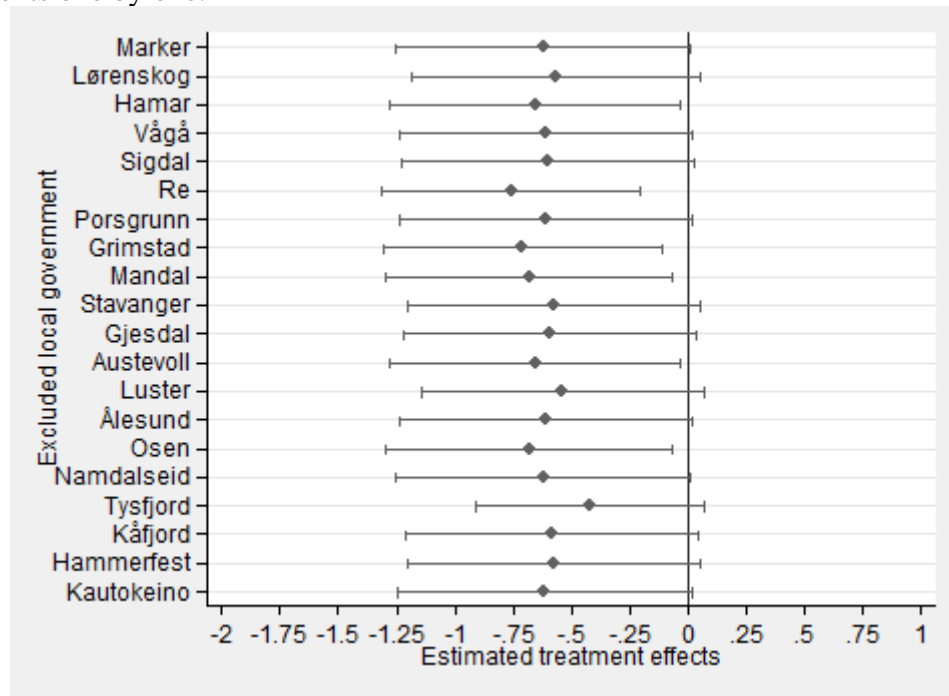
The results are presented in Table 3, where we distinguish between revenue from residential (houses and cottages) and commercial (businesses, hydroelectric power, and other works). While the treatment effect is negative, the estimates are imprecise and do not allow for strong conclusions regarding the impact of the extended youth voting franchise on property tax revenue.

## 6. Robustness checks

### 6.1. Treatment group composition

Do specific units drive the results in the treatment group? To check this, we estimate our preferred model in Table 2, excluding single-treated local governments one by one. Figure 3 shows the estimated treatment effects using this procedure. While some differences exist across the excluded units, the broad picture is that the point estimate is quite stable and suggests that outliers do not drive results.

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



## 6.2. Local government trends

The results in section 5 provide evidence supporting the parallel trend assumption. Nevertheless, we are still concerned that unobservables may affect the treatment and control groups differently. We include two linear trends in the models to account for the effect on operational deficits from unobservables that develop smoothly within units. 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 extent, account for possible Tiebout sorting, i.e. endogenous sorting of the population in the aftermath of the announcement of the extension of the voting franchise. The estimated effects at -0.64 and -0.55 in columns (1) and (2), respectively, are close to those in Table 2 in numerical terms but less precisely estimated in column (2). Column (3) includes linear local government specific trends. In this specification, the point estimate is still negative but drops somewhat to -0.38 and is not statistically significant. It should be pointed out that this is a demanding specification in terms of variation in the time dimension in our case.

Table 4. Specifications including local government linear trends.

	(1)	(2)	(3)
TREAT×POST	-0.643** (0.307)	-0.550 (0.334)	-0.383 (1.045)
TREAT×YEAR2010	-0.570 (0.383)	-0.474 (0.392)	-0.350 (0.631)
TREAT×YEAR2011	-0.498 (0.502)	-0.380 (0.501)	-0.353 (0.657)
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	0.487	0.653	0.331
Observations	4,268	4,268	4,279
R-squared	0.132	0.134	0.312

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 (3) in Table 2. 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+. 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.

### 6.3. Alternative control groups and estimation methods

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 different systematic development in net surplus in the 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 for treatment by the Ministry may, at first sight, appear as a natural control group. However, the applicants will 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 represent a better estimate of the counterfactual than the total number of governments that did not receive treatment. Table 5 column (1) nevertheless shows results when only the applicants not selected for treatment are defined as the control group. Compared to the estimate in Table 2, the point estimate increases to 0.716 and is significant at the 10% 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 comprised ten of the 2011 selected governments, while ten new governments were selected for treatment. The ten new governments selected for treatment in 2015 may have more in common with the 20 governments receiving treatment in 2011 regarding unobservables. The drawback of this strategy is the low number of governments in this control group. Column (2) in Table 5 shows the estimated effect, which is -0.769 and significant at the 10% level.

Table 5. Alternative control groups and estimation methods.

Control group	(1) Applicants not selected in 2011	(2) Units selected in 2015 and not in 2011	(3) All units not selected in 2011	(4) Applicants not selected in 2011
Estimation method	OLS/FE		Semiparametric DiD	
TREAT×POST2011	-0.716* (0.379)	-0.769* (0.416)	-0.552 (0.377)	-0.770** (0.372)
TREAT×YEAR2010	-0.824** (0.406)	-1.061** (0.453)		
TREAT×YEAR2011	-0.393 (0.496)	-0.960 (0.581)		
P-value parallel trend	0.130	0.282	-	-
Year FE	Yes	Yes	-	-
Local gov. controls	Yes	Yes	-	-
Local government FE	Yes	Yes	-	-
Polynomial order			1	1
Observations	1,419	298	409	134

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 of defining a valid control group, we apply a semiparametric difference in differences estimator proposed by Abadie (2005).<sup>13</sup> The estimator represents a generalization of the conventional difference in differences model 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

<sup>13</sup> The estimator is implemented in Stata and described in Hounghedji (2016)

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 for treatment as the pool of control governments. The point estimate is -0.55, similar to the effects obtained in Table 2 above, but not statistically significant by conventional levels. 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 -0.77, similar to that in columns (1) and (2) and significant at the 5% level.

## 7. Heterogeneous effects

The literature has emphasized the effect of institutional variables on fiscal performance. Among others, Alesina et al. (1993), Petterson-Lidbom (2001), and Borge (2005) allow fiscal policy (debt and deficits) to be affected by governmental political strength and political ideology. Applied studies in the Norwegian setting, Borge (2005) and Borge and Hopland (2020) use versions of a Herfindahl index of party fragmentation as an indicator of political strength and the share of elected politicians representing leftwing ("socialist parties") in the local council as an indicator of political ideology. Borge (2005) finds a significant negative effect of party fragmentation represented by the Herfindahl index, while the effect of party ideology varies across specifications. Motivated by these studies, we estimate the impact of treatment on treated municipalities with above and below the median number of effective parties (inverse Herfindahl index) in local councils in the pre-treatment period. Similarly, we estimate the treatment effect in treated municipalities with a left-wing share above and below the median left-wing share in the pre-treatment period. Half of the 20 treated local governments are classified as local councils with high/low political fragmentation, whereas 9 of the 20 are classified as local councils with a left-wing share above the median. We use pre-treatment values since the political variables are potential outcomes of the treatment.

The results from these estimations are presented in Table 6. Columns (1) and (2) show the estimated treatment effect among local governments with high and low political fragmentation, respectively. The numerical results suggest a negative treatment effect on operating surplus in both types of governments, although the point estimate is higher in absolute value and precisely estimated in the low fragmentation governments. Columns (3) and (4) present the impact of treatment across the political ideology dimension. We find that the negative impact on operating surplus from the youth enfranchisement appears among local governments with a high share of left-wing politicians in the pre-treatment period. The social democratic party



(“Arbeiderpartiet”) is the far largest party and the main party on the left-wing side of the political spectrum in most parts of the country. This implies that governments with low levels of party fragmentation also are likely to have a high share of left-wing politicians in the local council.

Table 6. Net operating surplus: political strength and party affiliation

	(1) High fragmentation	(2) Low fragmentation	(3) High left-wing share	(4) Low left-wing share
TREAT×POST2011	-0.576 (0.542)	-0.680*** (0.253)	-1.678*** (0.444)	0.211 (0.335)
TREAT×YEAR2010	-0.211 (0.563)	-0.889* (0.504)	-1.101* (0.656)	-0.083 (0.471)
TREAT×YEAR2011	0.218 (0.879)	-1.167** (0.464)	-0.942 (0.922)	-0.091 (0.494)
# treated local governments	10	10	9	11
Observations	2,136	2,143	2,144	2,135
R-squared	0.589	0.376	0.216	0.635

Notes: Dependent variable is net operating surplus (in 1000 NOKs) per capita (2000 prices). Standard errors clustered at the local government level in parentheses. A constant term, year and local government fixed effects, and local government controls (population in 10000, local government revenue per capita, population share 0-5, population share 6-15, population share 80+) are included in all models. \*\*\*, \*\*, \* denotes significant at 1%, 5% and 10% level, respectively.

These findings suggest that allowing younger individuals to vote would increase deficits (reduce operating surplus) in governments with an initially high share of left-wing politicians. However, more studies with other types of data are needed to confirm this interpretation.

For completeness, Appendix Table A2 shows the estimated effect on property tax revenue as the outcome variable allowing for heterogeneity in treatment effects across political variables. The results support the overall findings in section 5, as there are no clear differences in property taxation responses due to treatment across different political environments.

## 8. 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 public services like health and education, there is little knowledge of the effect of franchise extensions on fiscal performance regarding budget deficits and public debt. In particular, little is known about the

fiscal performance effects of broadening democracy in terms of lowering the minimum voting age 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 of 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 a voting age of 18. Using a difference in differences research strategy, we find that this voting age change reduced net operating surplus by around 600NOK (€60) per capita. We also find that the surplus reduction effect of increased youth voting franchise mostly appears among local governments with a high leftwing share of politicians in the pre-treatment period. A previous study (Nyhus and Strøm, 2023) demonstrates no impact of the reduction in minimum voting age on political variables such as voter turnout, party fragmentation, and the share of left-wing politicians.

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 at the local government level. Obviously, more research from different countries and other institutional settings using individual and aggregate data is needed to confirm this interpretation.

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## Appendix

Table A1. Detailed baseline results

	(1)	(2)	(3)	(4)
TREAT×POST2011			-0.615** (0.305)	
TREAT×POST2009				-0.583** (0.253)
TREAT×YEAR2006	0.815 (0.687)	-		
TREAT×YEAR2007	0.279 (0.637)	-		
TREAT×YEAR2008	0.095 (0.625)	-		
TREAT×YEAR2009	-	-		
TREAT×YEAR2010	-0.262 (0.479)	-0.558 (0.383)	-0.558 (0.383)	
TREAT×YEAR2011	-0.186 (0.607)	-0.482 (0.502)	-0.482 (0.502)	
TREAT×YEAR2012	-0.655 (0.602)	-0.951** (0.480)		
TREAT×YEAR2013	-0.200 (0.515)	-0.496 (0.397)		
TREAT×YEAR2014	-0.332 (0.524)	-0.628** (0.289)		
TREAT×YEAR2015	-0.085 (0.557)	-0.382 (0.304)		
Local gov. revenue per capita	0.682*** (0.170)	0.681*** (0.170)	0.681*** (0.170)	0.681*** (0.170)
Population (in 10000)	0.033 (0.235)	0.024 (0.236)	0.028 (0.236)	0.026 (0.237)
Share children 0-5 (%)	-0.029 (0.168)	-0.031 (0.168)	-0.032 (0.168)	-0.031 (0.168)
Share children 6-15 (%)	-0.379*** (0.132)	-0.376*** (0.131)	-0.375*** (0.131)	-0.375*** (0.131)
Share elderly 80+ (%)	0.002 (0.178)	0.000 (0.178)	0.000 (0.178)	-0.000 (0.178)
Constant	-14.036*** (4.486)	-14.008*** (4.489)	-14.009*** (4.487)	-14.008*** (4.483)
Observations	4,279	4,279	4,279	4,279
R-squared	0.470	0.469	0.469	0.469

Notes: Dependent variable is the net operating surplus per capita. The samples include all Norwegian local governments from 2006 to 2015, excluding the capital city, Oslo. Standard errors clustered at the local government level in parentheses. \*\*\*, \*\*, \* denotes significant at 1%, 5% and 10% level, respectively.

Table A2. Property taxes: political strength and party affiliation

	(1) High fragmentation	(2) Low fragmentation	(3) High left-wing share	(4) Low left-wing share
TREAT×POST2011	-0.191 (0.199)	-0.125 (0.188)	-0.300 (0.251)	-0.027 (0.125)
TREAT×YEAR2010	-0.185 (0.145)	0.013 (0.073)	-0.198 (0.129)	-0.003 (0.094)
TREAT×YEAR2011	-0.147 (0.195)	0.009 (0.157)	-0.267 (0.235)	0.090 (0.086)
# treated local governments	10	10	9	11
Observations	2,136	2,143	2,144	2,135
R-squared	0.969	0.983	0.969	0.978

Notes: Dependent variable is total revenues from property taxation in NOK 1000 per capita (2000-prices). Standard errors clustered at the local government level in parentheses. A constant term, year and local government fixed effects, and local government controls (population in 10000, local government revenue per capita, population share 0-5, population share 6-15, population share 80+) are included in all models. \*\*\*, \*\*, \* denotes significant at 1%, 5% and 10% level, respectively.