

The effects of voting franchise extension on education policy¹

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Abstract

This paper studies the effects of female voting on public education policy in terms of spending and teacher-student ratio. To estimate causal effects, we exploit the introduction of a national voting reform in 1910 in Norwegian local elections that removed socioeconomic restrictions on female voting rights. The identification strategy utilizes heterogeneous changes in the number of eligible voters from the pre-reform (1907) to the post-reform election (1910) across local governments as a source of exogenous variation in the voting franchise. In contrast to studies on franchise extensions in the US, we find no systematic effects on educational spending and the teacher-student ratio, despite a significant increase in female voter turnout. Moreover, we find no significant effect on poverty relief either, which was an important redistributive spending category at that time.

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1. Introduction

The level of school resources in terms of spending per student or teacher-student ratios are important issues in the public debate on educational policy, both historically and today, although the impact of resource based policies is somewhat controversial (Hanushek, 2006). Both school enrolment and the resources allocated to education has increased steadily over time in developed countries around the world (Goldin, 2016). The traditional view is that private income and number of students are key determinants of school spending, both in terms of size of the school budget and number of teachers per student. This paper asks to what extent changes in political institutions affecting the composition of the electorate have contributed to the development in school spending.

At the general level, the link between democracy and public policy is an important issue. Dating back to Tocqueville (1835), a common view has been that democratization in terms of expansion of the voting franchise will increase government size and redistribution, although the theoretical literature suggests that effects are less clear-cut. On the one hand, extended voting franchise shifts the position of the decisive voter downwards in the income distribution and increases the demand for redistribution, as put forward in the seminal paper by Meltzer and Richard (1981). On the other hand, when the role of the government is to provide public services, extended voting franchise has price and income effects that work in opposite directions as argued in Kenny (1978) and Husted and Kenny (1998). In the end, the question whether expansion of the voting franchise increase public expenditure is an empirical one.

The empirical evidence is mixed, and results vary substantially across countries and studies. Papers exploiting changes in voting rules in the US in late 19'th and early 20'th century consistently find that removing restrictions on the right to vote substantially increases public spending. Husted and Kenny (1998) find that general franchise extensions increased several US state expenditure items and Lott and Kenny (1999) find positive effects of female suffrage on state government expenditures. Carruther and Wannamaker (2015) find that including women in the electorate had large positive effects on local spending on education in the southern US states in the 1920's. Miller (2008) finds large positive effects on local health spending.

On the other hand, European studies typically find much smaller effects from extensions of the voting franchise, if any effect at all. For example, the cross-country European evidence from

the 19'th and early 20'th century in Aidt et al. (2006) indicates that lifting socioeconomic restrictions on voting increased central government spending slightly, while extending voting rights to women had insignificant effects. Bertocchi (2011) finds positive effects on central government spending only in non-catholic European countries. Studies on local government expenditures even find negative effects of female suffrage or highly non-linear effects (Aidt et al. (2010)).

The different results across studies and especially across the Atlantic are puzzling and partly motivates our empirical study. We investigate the effect of removing socioeconomic restrictions on female voting on public spending on education and teacher-student ratios in Norwegian local governments. The paper makes several contributions to the literature.

First, we study the effect on local government spending in a European country, Norway, while most of previous European studies of spending effects from voting franchise extensions have used cross-country data that cover only central government spending.² Local governments provide a substantial part of public service production and redistribution, and obviously more so hundred years ago than today. Thus, cross-country studies using only central government spending might possibly misrepresent the overall impact of franchise extension on fiscal outcomes. Local governments within a single country are also more comparable units in terms of history, culture and institutions than different countries in Europe. By using Norwegian local government data from the beginning of the 20'th century, we analyze the behavioral responses for providers of the main public services before the introduction of the welfare state. While educational spending and the teacher-student ratio is our main outcome variables we also provide evidence for local spending on poverty relief.

Second, we exploit a national voting reform combined with data on pre-reform differences in the voting franchise across local governments as a source of exogenous variation in a difference-in-differences approach to identify causal effects. Since all local governments had to follow the national voting rules, this approach circumvents the potential problem of endogenous decisions of incumbent politicians, which is a concern in earlier studies. Most

² Some European studies also uses local government data. Stutzer and Kienast (2005) exploit intertemporal and cross-section variation in the female voting rights across Swiss cantons and find negative association between government spending and female suffrage. Funk and Gathman (2006) uses the same type of identification and find a *negative* immediate effect on spending, while there are positive effects on welfare and health spending after 20 years. Using similar Swiss data, Krogstrup and Wälti (2011) find that female suffrage reduces budget deficits.

previous evidence is based on combined temporal and cross-section variation in national or state level voting rules to estimate spending effects. Such evidence is difficult to interpret causally if incumbent politicians use voting rules as instruments to gain support for their policies or to reduce the probability of losing power as argued by Acemoglu and Robinson (2001), Lizzeri and Persico (2004), and Ticchi and Vindigni (2008). Moreover, Braun and Kvasnicka (2011) provide evidence that the timing of suffrage extensions to females in the US states is inversely related to the share of women in the population, while Aidt and Jensen (2014) find that perceived revolutionary threat and war triggered suffrage extension.

We exploit the introduction of universal suffrage for adult females in the Norwegian local elections held in the fall 1910. While adult males were granted universal suffrage already in the local elections in 1901, before 1910 females was eligible to vote only if they owned property, or earned taxable income above a certain amount, or were married to a husband fulfilling these criteria. These socioeconomic restrictions imply that the voting franchise varied across local governments in pre-reform elections. The change in franchise implied by the 1910 national reform gives the variation we use for identification of causal effects.

The paper is organized as follows: Section 2 reviews the literature, Section 3 describes the relevant institutional setting and voting reforms in Norway in the relevant period, while Section 4 presents the data and the empirical strategy. The empirical results are presented in Section 5, and Section 6 offers some concluding remarks.

2. Literature review

2.1. Theory

The median voter model as formulated in Downs (1957) is a natural point of departure for understanding the role of the voting franchise. Extending the franchise usually implies a median voter located further down the income distribution. As poorer people have stronger preferences for redistribution, the Meltzer and Richard (1981) pure redistributive government model predicts higher government spending. However, if the government mainly produces public services like education, police and infrastructure, extending the franchise have income and tax price effects that moves spending in different directions. If public services are normal goods, the income effect leads to lower spending on public services. If service production is financed by proportional income tax, poorer people face a lower tax price of public services which

partially increase spending. This is formally shown in Kenny (1978) and discussed in Husted and Kenny (1998). Thus, for public services, the net spending effect of extending the franchise is positive only if the tax price effect exceeds the income effect.

Taking these two approaches together, franchise extension is predicted to increase government spending on redistributive items, while the impact of spending on public services is generally ambiguous.

Recent papers have extended the early theoretical models by explicitly modeling behavior of politicians based on ideas with long traditions in political science, see Przeworski (2009) for an overview. Voting rules and the size and composition of the voting franchise is seen as policy tools of the incumbent political leadership rather than being exogenous. Acemoglu and Robinson (2001) argue that increasing franchise is a way to credibly commit to redistribution in order to prevent revolution in situations with a transient revolutionary threat. Aidt and Jensen (2014) find support for this prediction using data from European countries 1820-1938.

Lizzeri and Persico (2004) assume that the ruling party represents conflicting interest groups within the existing political elite, i.e., landowners and industrialists, and show that it may be optimal to extend the franchise in order to turn policies away from redistribution within the elite towards policies targeted towards provision of public goods. In particular, they argue that an exogenous increase in the demand for public goods such as sanitation and infrastructure, may lead the incumbent party to rationally increase the voting franchise. Spending does not increase because of increased franchise, but because of increased demand for public goods in the population. A similar argument is provided by Ticchi and Vindigni (2008) who formalize an argument originating from Weber (1961). Extension of the voting franchise is a mechanism whereby the incumbents can increase the people's willingness to participate as soldiers and exert effort in mass armies, which implies that voting franchise increases in war periods.

Female participation in elections may also affect policy. Economic constraints may differ between males and females. For instance, if females are systematically poorer than men, female suffrage will lead to changes in government expenditures similar to that generated by universal suffrage for men. In addition, women's preferences for public goods and redistribution might differ systematically from men.

Formal theoretical studies on the effect of extension of the voting franchise to females is limited. Bertocchi (2011) is an exception and considers a political-economic model where female suffrage emerges as a rational choice for incumbent males under certain conditions. Point of departure is a situation with females initially having stronger preferences for public goods than males and earning lower wages, while individual's preferred tax rate is decreasing in income. As the female relative wage increases, the exogenous societal cost of disenfranchisement at some point exceed the cost of the higher taxes resulting from female suffrage. Thus, the model predicts that the lower the male-female wage gap and the higher the societal cost of disenfranchisement, the sooner a society gets female suffrage. She finds empirical support for this prediction using data from 22 countries for the period 1870-1930.

2.2. Empirical evidence

A growing literature studies gender differences in political preferences and behaviour, see the recent review by Doepke et al. (2012). This has motivated several empirical studies on the impact of female suffrage on fiscal outcomes. Studies from the US tend to find large effects. Using state level data from 1870-1940 and a fixed effects framework, Lott and Kenny (1999) find that giving females the right to vote increases government spending. The effect on total spending is about 15% in the short run and 25% in the long run. Using similar, but more disaggregated data, Miller (2008) finds that within one year after the introduction of female suffrage, local health spending increased by approximately 35%, and subsequently led to a substantial reduction in child mortality.

Of particular interest in our setting are Carruthers and Wanamaker (2015), who study the impact of female suffrage on education expenditures. They use county level data for three US southern states where female suffrage were introduced in 1920 by the Nineteenth Amendment to the US constitution. To estimate causal effects, the paper exploits that the franchise extension varied with the cross-county level of female population shares. They conclude that up to one-third of the rise in education expenditures from 1920-1940 in the US south can be attributed to the implementation of universal female suffrage.

On the other hand, European studies tend to find small or no effect of the introduction of female voting rights. Using panel data for 12 European countries in a fixed-effects framework, Aidt et al. (2006) find insignificant effects of universal suffrage for females on central government spending. Aidt and Dulla (2008) use data from six European countries and find that suffrage

increases social spending as a share of GDP by about one percent in the short run. Although the long run effects are larger, the impact is substantially lower than in the US studies.

Funk and Gathman (2006) exploit that the introduction of female suffrage in Swiss Cantons took place at different points in time in the 1960s and 1970s. In a fixed effects framework, they find a *negative* immediate effect on spending, while there are positive effects on welfare and health spending after 20 years. Using similar Swiss data, Krogstrup and Wälti (2011) find that female suffrage reduces budget deficits. Hofer (2016) uses outcomes in Swiss referendum ballots concerning federal competency to levy taxes as proxy for political support for government spending. Comparing outcomes in two similar referendums held shortly before and after the extension of suffrage in federal elections in 1971, she finds that support for public spending is *higher* among males than females, contrary to expectations.

Related reforms to female franchise are abolishment of socioeconomic restrictions on voting rights for men. Lindert (2004) presents different cross-country data and argues that in general franchise extensions boost government spending. Pelzman (1980), on the other hand, finds no systematic effect on government spending on data for a sample of small countries with different timing of voting reforms. The cross-country analysis of Aidt et al. (2006) cited above includes an indicator for removing socioeconomic restrictions on voting rights for males in addition to female franchise in their empirical model. They find that the former type of reform had small positive effects on central government total spending and spending on infrastructure and security, but had negligible effects on other more redistributive spending items such as education, health and transfers.

Husted and Kenny (1997) exploit that the existence of literacy tests and poll taxes for voting varied, in the US system with voluntary registration in electoral registers. Using state panel data from 1950 to 1988, they show that increased voting franchise led to a sharp increase in welfare spending. In a study for Britain, a country with a similar party and election system as USA today, Justman and Gradstein (1999) find that expansion of the voting franchise in the 19th century was followed by a large increase in government redistribution programs. Aidt et al. (2010) use the increase in local voting franchise in local governmentes (boroughs) in England and Wales in 1860's and 70's to study the impact on sanitation and infrastructure expenditures. They find highly nonlinear effects. Expenditures decrease (increase) when franchise is below (above) a certain threshold. This nonlinear effect is consistent with results from a theoretical

political economy model with the voting population divided into certain groups with diverging preferences for the public goods provided.

Dincecco and Prado (2010) uses data on aggregate expenditures in eight European countries from 19th century until today. They find that warfare and not franchise extension is the key determinant of the long run increase in government size.

While most of the studies naturally explore changes in voting franchise many decades ago, a recent paper by Vernby (2013) uses an identification strategy similar to ours to identify the effect of giving immigrants the right to vote in local elections in Sweden in 1975. He exploits that the increase in voting franchise generated by the national reform varied substantially between Swedish local governments, and finds that increased voting franchise substantially increased local spending on education and social and family services.

A related literature studies explicitly the relationship between government expenditures, voter turnout and the direct costs of voting or abstention. The traditional view according to the Meltzer-Richard hypothesis is that lower voting cost will reduce government spending on redistributive items as the position of the decisive voter moves down the income distribution. However, recent empirical studies using credible identification strategies do not find support for this view. Hodler et al. (2015) use staggered introduction of postal voting in Swiss cantons to identify causal effects of lower voting costs and find that postal voting increases turnout, lowers average education of voters and subsequently *reduces* local government welfare expenditures in Swiss cantons and *reduces* business taxation. They argue that their finding is consistent with a model where the new voters are less informed and more vulnerable to campaigns from strong interest groups able to increase campaign contributions. Hoffman et al. (2017) studies the effect of within state changes in compulsory voting laws in Austria and find that removing compulsory voting reduces turnout, while there is no significant effect on government spending patterns.

One concern in many of the previous studies is that the effects of voting rules is difficult to interpret causally because changes in voting rules may coincide with changes in unobserved variables affecting both government expenditures decisions and voting franchise decisions. Recent theoretical work discussed above all emphasize that franchise extension is a policy tool

used by the incumbents to obtain specific goals, which implies that using timing of suffrage extensions might give biased estimates of the causal effects.

A concern with the European cross-country evidence is that central government spending constitutes a limited part of public sector service production and redistribution, which were mainly provided by local governments in the reform years. Thus, using only central government spending will not reveal the full impact on government distribution and service provision from extending the voting franchise.

3. Institutions and voting reforms

Governance systems

Norway was one of the first countries with an elected parliament (*Stortinget*). The constitution from 1814 is the oldest single-document constitution in Europe today. In 1837 the parliament approved a Local government act (*Formannskapsloven*) dividing the country into a large number of local governments governed by elected assemblies with discretion to set local taxes and expenditures.

Initially, the local tax revenues were divided between different purposes, i.e., one fund for education expenditures (*skolekasse*) and another fund for poverty relief expenditures (*fattigkasse*). These funds were administered by separate boards (*skolekommisjon*, *fattigkommisjon*, etc.), and financed by separate taxes with property as the tax base. The Tax act of 1882 represented a major change in the fiscal institutions. It instructed all local governments to have an overall local government budget and to have one single account system covering all activities. The act made income taxation compulsory for the local governments and introduced property tax rate caps. As a result, the local governments became the main economic and political unit at the local level, which they still are today.

According to Borge (2010), by 1900, the income tax was the most important local tax (about 60% of tax revenues) and the tax rate varied substantially across local governments. At the same time, central government grants constituted only 10% of local government income. The Tax act of 1911 represented a further change in the tax system. It introduced the duty to file tax returns, and it introduced a restriction that the income tax rate should not exceed 10%. Taken

³ The description in this section is to a large extent based on NOU (1997), Borge (2010) and Seip (1949).

literally, the latter restriction suggests that the local governments lost some of the discretion in fiscal policy, just at the same time as the voting franchise in local elections was extended. There is a concern that this change in fiscal rules could make it less likely to observe increased government spending following the extension of the voting franchise. Four arguments suggest that this is not likely to be the case. First, the tax cap set was higher than the initial tax rate in most local governments. Second, the local governments could deviate from the tax rule under certain conditions⁴ and data for tax rates from the city (urban) governments indicates that the cap was frequently exceeded. In the first (1912) and second year (1913) after the tax cap was introduced, 91 and 67 per cent of the cities exceeded the 10 per cent caps. Third, the local governments were free to decide the tax allowance, and could reduce it to increase tax revenues if the tax rate cap was binding. Fourth, the duty to file tax returns increased the tax base significantly, making a considerable revenue increase possible without increasing the tax rate (Gerdrup (1998)). In reality the Tax Act did not restrict tax revenues as discussed in NOU (1997, p.19) and the increase in the tax base might mean that the Tax Act increased the fiscal leeway of the local governments.

Voting reforms

While the elected local councils made decisions on local budgetary issues, the national parliament decided voting rules, and local government borders were formally determined by the central government. Up to 1884 the right to vote was restricted to males over 25 years of age owning property or having a civil servant position (*embetsmann*).

In 1884 a major reform took place adding males paying taxes from income above a given threshold to the electorate. Within the 30-years period from 1884 to 1915, the parliament made further substantial changes in the voting rules. Among the two largest political parties at the time, representatives from the liberal party (*Venstre*) were much more supportive of increasing the voting franchise than representatives from the conservative party (*Høyre*). In 1891, the liberal party included voting rights for all males in the party program. This was also the most important policy request for the initially small Social democratic party (*Det Norske*

⁴ The local governments could apply to the Ministry of Finance for exemption from the tax cap if more than $\frac{3}{4}$ of the members in the local government/local government council voted for such application. Moreover, the local governments were allowed to set a tax rate such that tax revenues from the previous year was maintained as long as that rate did not exceed 12%.

⁵ Data from Norges Officielle statistik v.80, Norges kommunale finanser 1912 and Norges officielle statistic v.100, Norges kommunale finanser 1913-14.

Arbeiderparti) established in 1887. Including females in the voting franchise was still a controversial question and several years with heated debates went before it was implemented. Only the Social democratic party supported full suffrage for females from the beginning.

Table 1 gives an overview of the voting rights in the local and parliamentary elections, respectively, in the relevant time period. Prior to the parliament election in 1900, voting rights were restricted to males owning property or having taxable income above a given threshold. The income threshold varied between urban and rural local governments. In 1898-1899, the parliament changed the voting rules in both the national and local elections. All males above 25 years of age got the right to vote.⁶

Table 1. Voting rights in local and parliamentary elections, Norway 1898-1920.

Local elections	
1886 – 1895	Males age 25+, owning property or paying taxes on income minimum NOK 800 (cities) or NOK 500 (rural areas)
1898	Males age 25+, owning property or paying taxes on income minimum NOK 400 (cities) or NOK 300 (rural areas)
1901, 1904 and 1907	All males age 25+. Females 25+ and either (i) owning property, (ii) paying taxes on income minimum NOK 400 (cities) or NOK 300 (rural areas), or (iii) married to a man with such income or property
1910 and afterwards	All males and females age 25+
National elections	
1884 – 1897	Males age 25+, owning property or paying taxes on income minimum NOK 800 (cities) or NOK 500 (rural areas)
1900, 1903 and 1906	All males age 25+
1909 and 1912	All males age 25+. For females, voting criteria equal to the local elections in 1901 – 1907.
1915 and afterwards	All males and females age 25+

The parliament decided to give some females the right to vote in the local election in 1901. The right was restricted to females who were above 25 years of age and either owned property, paid taxes of an income above the previous thresholds relevant for men, or were married to a man fulfilling the same criteria. The systematic collection of voting data from all local governments

⁶ Until 1919, people receiving poverty relief (*fattigunderstøttelse*) and people convicted for certain crimes were suspended from voting.

from 1898 and 1901 was initiated by the parliament to get some experience before eventually extending the voting franchise to parliamentary elections (Lindstøl, 1903).

The same voting rules applied in the local elections in 1904 and 1907. A proposal to extend the voting franchise to all adult females was discussed in the national parliament in May 1910. In the debate, prominent politicians from the conservative party as well as the liberal party warned against the effects of giving voting rights to all females. For instance, the minister of trade Sofus Arctander from the liberal party argued against full suffrage for females because “*among those many thousand women, the majority.....will vote for the social democrats and give them the majority which will be dearly bought for the other citizens in the town*”⁷. Representative Alfred P. Wright from the conservative party argued that “*is it safe for the society that also those not paying taxes, those not paying the burdens of the society’s expenditures should decide the society’s expenditures which is solely paid by others? I say no!*”⁸ Despite these arguments, and opposition from some prominent politicians, in May 1910 the parliament passed a law giving all women above 25 years of age the right to vote in local elections to be held the following fall. As a direct protest against this decision, Sofus Arctander resigned as minister of trade the same year.

Voting rights for females was implemented in parliamentary elections some years after the introduction in local elections as shown in Table 1. Females could vote conditional on wealth and income requirements in parliamentary elections 1909 and 1912. In 1913 the parliament finally decided unanimously to give females the right to vote in parliamentary elections on the same conditions as males, and this rule was first implemented in the 1915 parliamentary election.

The central government gave detailed instructions on how the local elections should be conducted, as well as the allocation formula used for translating votes into seats in the local government council (Ihlen, 1910). In rural (urban) areas, the local elections was to be held in October (December) every third year. Secret ballots were introduced in the local elections in 1898. The registration procedure for voters changed before the election in 1901. While voting in pre-1901 elections required voters to actively register themselves as voters as is currently the

⁷ Minutes from the debate in “Odelstinget”, May 13, 1910

⁸Minutes from the debate in “Lagtinget” , May 27, 1910. “Lagtinget” and “Odelstinget” was the smaller (larger) of the two houses in the Norwegian parliament (“Stortinget”). Authors’ translation.

case in the US, from 1901 on the Norwegian local governments were required to make publicly available lists containing all persons eligible for voting. Local elections have always been direct elections on candidates, partly based on lists for political parties, but also lists containing certain regions or interest groups within local governments. In the parliamentary elections, however, direct elections were introduced in 1906, using single-member election districts. In 1921, the single-member election districts for parliamentary elections were replaced by the proportional rule used today.

Education system

As was also the case in many other countries, the Norwegian central government introduced minimum standards for educational services across school districts. For example, according to the School Act of 1889 the local governments were obliged to provide compulsory education (folkeskole) to all children 7-14 years of age, free of charge and financed by the local government through local taxation. The local government provided schools and hired and paid teachers. The law distinguished between cities and rural areas where the minimum number of school days and the number of compulsory subjects was higher in cities than in rural areas⁹. Moreover, there was a minimum annual teacher wage that varied between cities and rural areas. The teachers were organized in a national union which covered almost all teachers. According to Falch (2001) the minimum wage level in both cities and rural areas increased in 1910 and 1920 due to pressure from the teacher union, although the union had no formal bargaining rights before WW2. While some national regulations and minimum standards existed, the level of education spending was clearly a local decision, given the minimum requirements,.

4. Data and empirical strategy.

4.1. Data

Voting franchise

Data for the local elections held in 1907 and 1910 were collected by Statistics Norway, published in NOS (1908) and NOS (1911) and digitalized in the Regional data base (Kommunedatabasen) provided by the Norwegian Centre for Research Data (NSD). During the period of study the number of local governments varied due to mergers or splitting of existing units. To construct a sample with comparable units over time, we exclude from the analyses all

⁹ As an example, foreign language (English) was taught only in city schools.

local governments that were involved in either mergers or splits during this time period as well as units with missing values of the dependent variables. This leaves us with a balanced panel data set of 524 local governments.¹⁰ By way of comparison, the number of local governments in 1908 was 644.

Since voluntary registration for voting was removed in Norway in 1898, the electorate is simply defined by the number of eligible persons living in the local government the two last years before the local election. Thus, the share of females in the voting franchise is fairly accurately described by the voting statistics provided by the reports from Statistics Norway in NOS (1908) and NOS (1911).

Figure 1 presents kernels densities for the share of females in the franchise for the elections in 1907 and 1910. Females made up 35% and 51% of the average franchise in the elections in 1907 and 1910, respectively. Before the reform in 1910, there were two sources of variation in the female franchise size; the gender distribution in the population and to what extent females fulfilled the socio-economic requirements for voting. The long left tail of the density curve for the 1907 election illustrates that in some local governments, very few females were eligible for voting. As expected, in the 1910-election the distribution of female franchise moves to the right, and the variation across local governments is considerably lower.

¹⁰ The information on the changes in the local government structure is based on the detailed historical list of local governments provided by Juvkam (1999).

Figure 1. Density of female voting franchise in the elections in 1907 and 1910

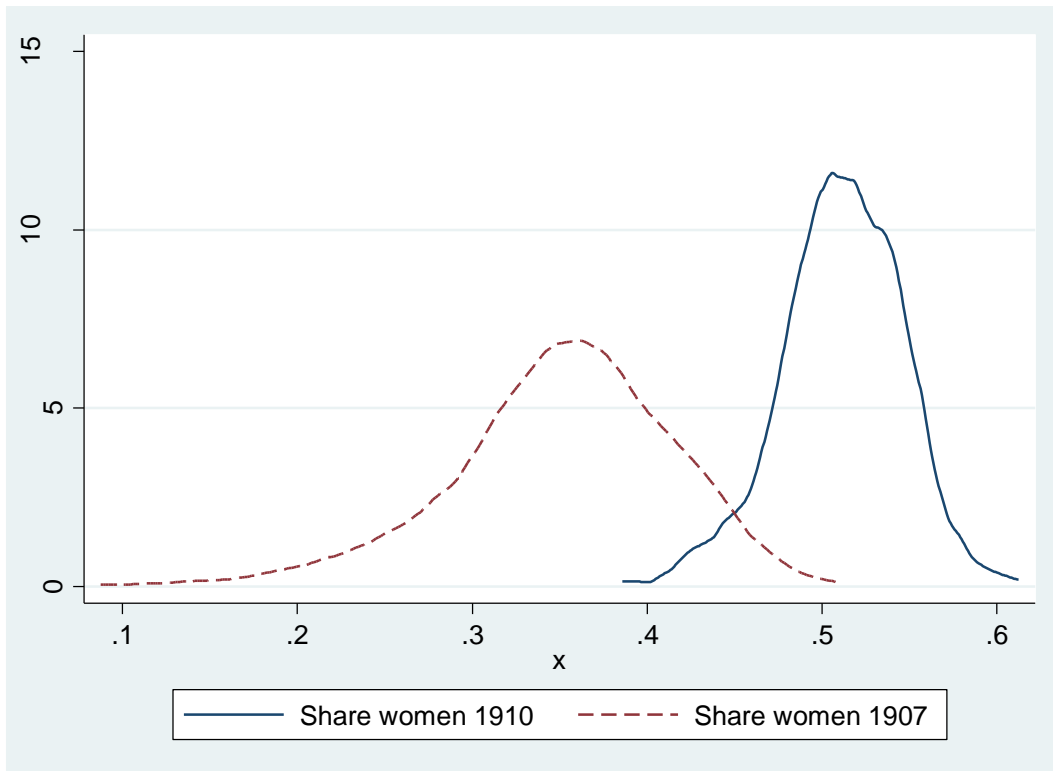
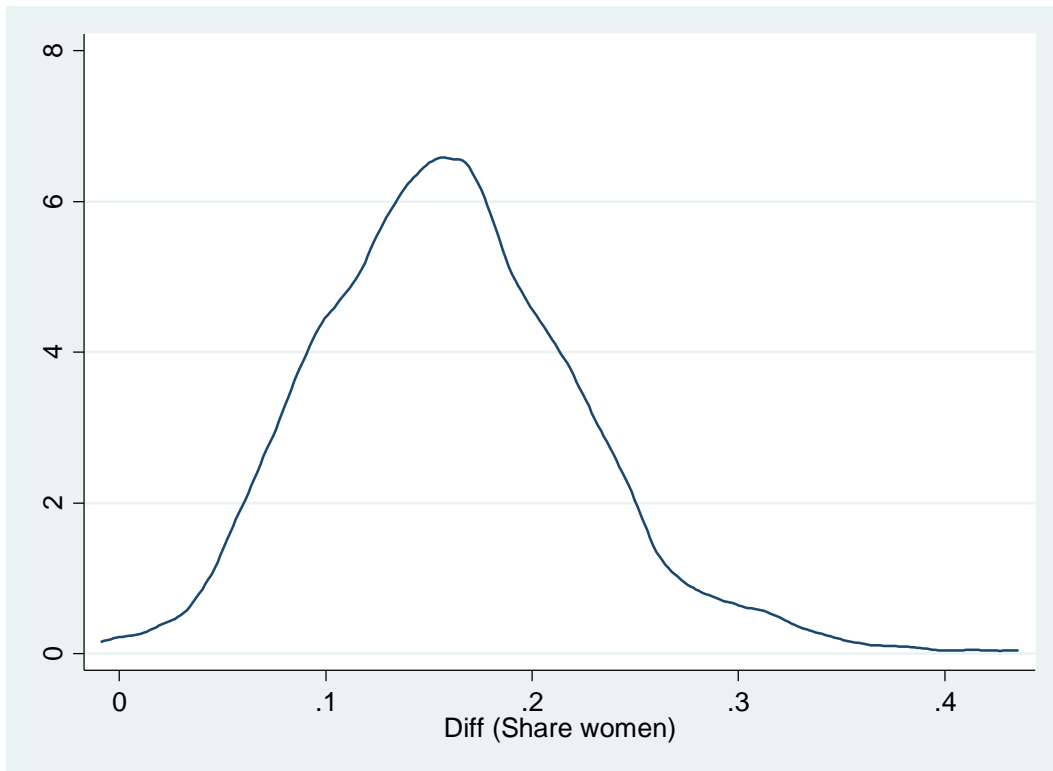


Figure 1 implies that there are substantial differences in the bite of the reform. The variation in treatment intensity is presented in Figure 2. The change in the female share of the franchise varies substantially across the local governments, up to a maximum increase of 40 percentage points. As displayed in Table 2 below, the average growth is 16 percentage points with a standard deviation of 6 percentage points. The low share of females in the franchise in some local governments in the 1907-election shows up as a long right tail of the density curve in Figure 2.

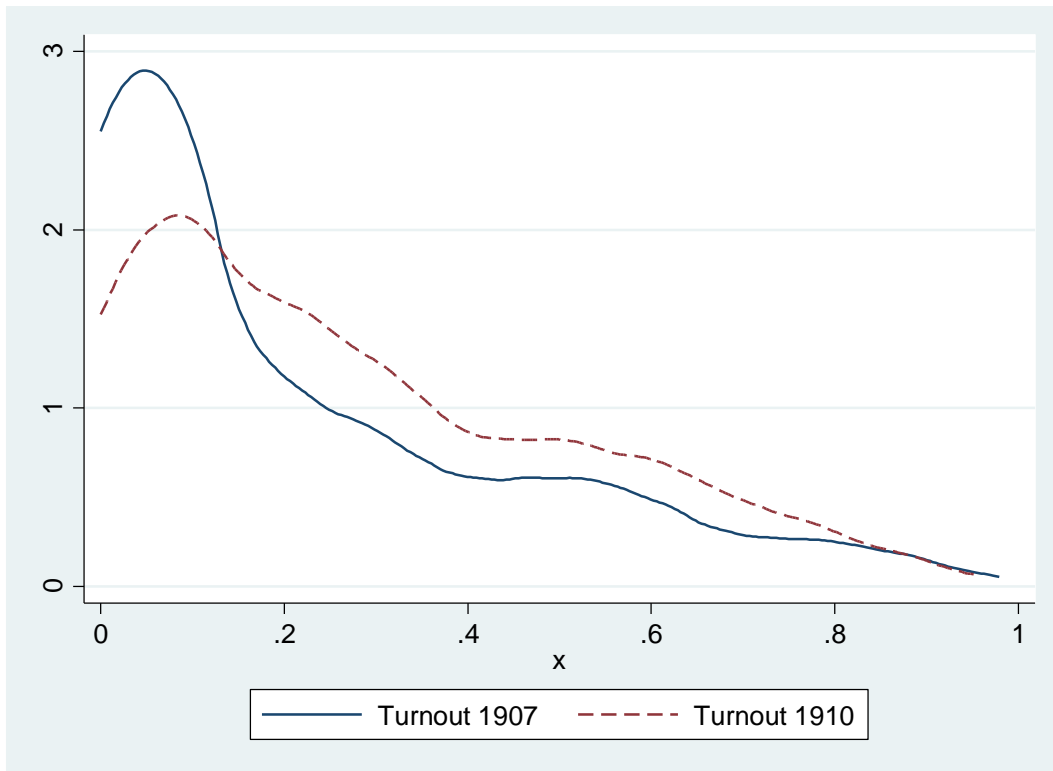
Figure 2. Density in the change in the share of female franchise from 1907 to 1910



Did the newly enfranchised females actually vote? The extension of the voting franchise is arguably more likely to affect fiscal outcomes if the females actually voted. There is no information available about the turnout among the newly enfranchised females, but Figure 3 presents the kernel densities of the total turnout of females in the elections. The majority of local governments have low female turnout in both elections, but the variation is large. Fewer units had female voter turnout below 15% in 1910 than in 1907,¹¹ while more units had turnout in the range 15% to 80%. The average turnout was 21% and 28% in the 1907- and 1910-elections, respectively, which is low compared to the corresponding male turnouts of 51% and 57%. Nevertheless, the combination of growth in both enfranchised females and turnout led to nearly doubling the females' share of cast votes.

¹¹ The number of local governments with zero female turnout decreased from 11% to 2.4%.

Figure 3. Density of female turnout in the elections in 1907 and 1910



Fiscal outcomes

Variables describing the fiscal situation and spending on different items in the local governments are collected from various editions of official publications (NOS) from Statistics Norway and digitalized by the authors. We use two measures of resources allocated to compulsory education. The first measure is total expenditures per student. Expenditures include teachers' salaries, books and other teaching material as well as maintenance and construction of school buildings. It is not possible to separate out investments in the accounts, which might give some spurious fluctuations across election periods. The second measure is the number of teachers per student. This is a measure close to the concept of "real" school resources. The correlation with expenditures per student is 0.24.

Table 2 presents descriptive statistics. Since we use a difference-in-differences approach, we present changes in the variables in the table in addition to statistics at level for the two periods.

Table 2. Descriptive statistics

	Average	Standard deviation	1 st decile	9 th decile
The franchise				
Share of women in the franchise, 1907	0.35	0.06	0.27	0.43
Share of women in the franchise, 1910	0.51	0.03	0.47	0.55
Change of women in the franchise	0.16	0.06	0.08	0.24
Average values educational outcomes, 1908-1910				
Nominal expenditures per student (NOK)	25.72	14.75	13.76	47.16
Number of teachers per 100 student	2.29	0.70	1.59	3.05
Average values educational outcomes, 1911-1913				
Nominal expenditures per student (NOK)	33.94	23.52	16.93	62.30
Number of teachers per 100 student	2.41	0.72	1.65	3.28
Change in log values of educational outcomes				
$\Delta \log(\text{Expenditures per student})$	0.25	0.22	0.01	0.51
$\Delta \log(\text{Teachers per 100 student})$	0.05	0.10	-0.07	0.17
Expenditures on poverty relief				
Nominal expenditures on poverty relief per capita (NOK)*, 1910	2.30	1.33	0.94	4.13
Nominal expenditures on poverty relief per capita (NOK)*, 1915	2.74	1.67	1.06	4.98
$\Delta \log(\text{Expenditures on poverty relief (NOK)})$	0.16	0.40	-0.21	0.63
Average values control variables, 1908-1910				
Nominal taxable private income per capita (NOK)*	174	73	106	273
Number of student	530	1,422	147	881
Average values control variables, 1911-1913				
Nominal taxable private income per capita (NOK)*	208	100	118	327
Number of student	537	1,423	147	884
Change in log values of control variables				
Log of private taxable income, nominal NOK	0.16	0.15	0.04	0.30
Log of Number of students	0.004	0.09	-0.08	0.10

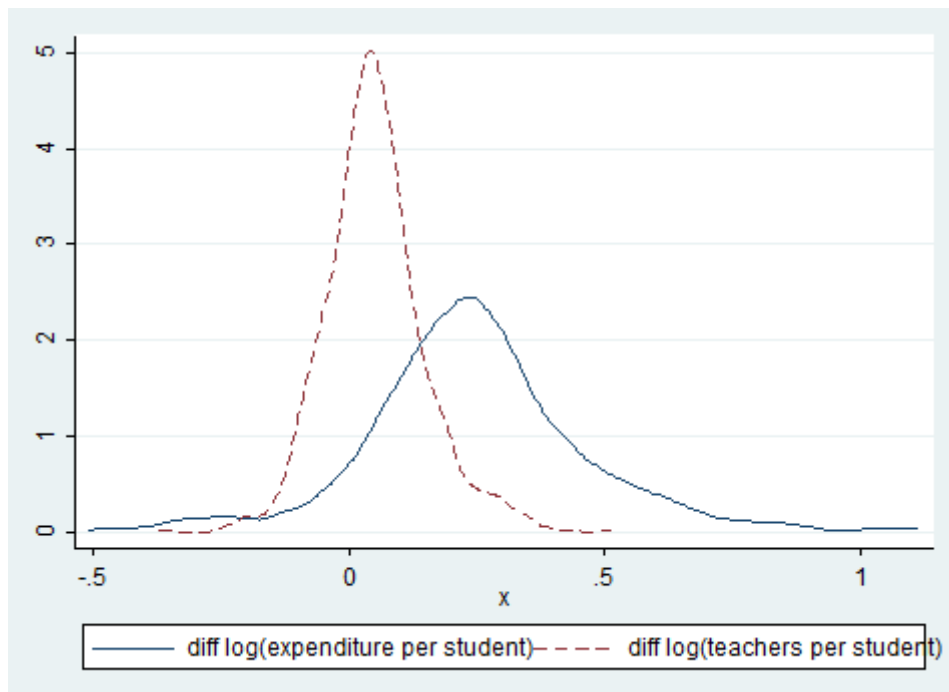
*Scaled by population size in 1910.

The fourth panel of table 2 displays an increase in spending on education between the election periods. On average, nominal expenditures per students increased by 25% between the election periods. Consumer prices grew by around 10% during the same period, suggesting a significant growth in real spending.¹² The number of teachers per students had an average growth of 5% during the same period. Thus, it seems to have been a real growth in education, including a growth in real teacher wages, teaching materials and investments. This is in accordance with the aggregate findings in Falch (2001). The variation across local governments is substantial for both spending measures. As expected, the variation is larger for expenditures since this measure also includes capital outlays. This variation is further illustrated in Figure 4 where the Kernel densities for the growth in spending are plotted. The figure use the logarithm of the

¹² Based on average values of the CPI for the two election periods. Source: Statistics Norway, <http://www.ssb.no/a/histstat/tabeller/12-3.html>

average values for the pre-reform election period (1908-1910) and the post-reform period (1911-1913).

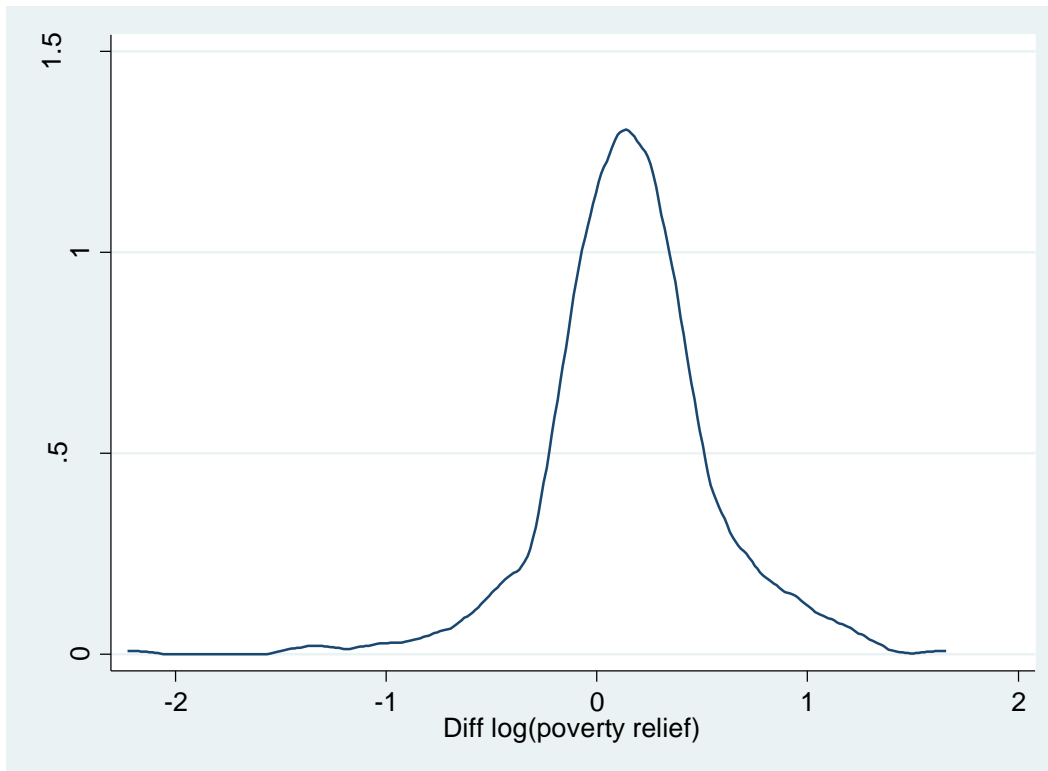
Figure 4. Growth in expenditures and teachers per student from 1908-1910 to 1911-1913



The empirical part also includes some analyses of spending on poverty relief, which was economic support for people unable to provide for themselves. The main reasons for receiving such support was old age and sickness.¹³ The support secured only the most basic needs such as shelter and food and was reserved for individuals without a family, or with a family lacking the necessary income, to provide for the person. The recipients of support were placed in poorhouses, mainly in the cities, or in households in a competitive bidding system where the household with the lowest bid got the responsibility for the indigent person. Investments in poorhouses or maintenance of such buildings is not included in the measure.

¹³ Fattigvesenet 1915, Norges Officielle Statistikk, Statistics Norway.

Figure 5. Growth in expenditures on poverty relief from 1910 to 1915



Data on spending on poverty relief are available for every fifth year and table 2 shows that the mean growth between 1910 and 1915 was 16 per cent. The consumer price index increased by 32.4 percent during the same period implying a 12 per cent average reduction in real spending. This reduction could be a consequence of the relative strong economic growth during the period.¹⁴ This explanation is corroborated by a 9 % decrease in the number of main persons defined as eligible for poor relief from 1910 to 1915¹⁵. A standard deviation of 0.4 indicates a substantial variation in the growth across the local governments. The distribution in growth is further illustrated with Kernel densities presented in figure 5. The growth varied between -20% to 70% for the large majority of local governments, but with some clear outliers.

¹⁴ According to historical statistics provided, real GDP per capita increased by 19% from 1910 to 1915, see Statistics Norway, <https://www.ssb.no/a/histstat/aarbok/ht-0901-bnp.html>

¹⁵ The number includes persons receiving free medical treatment and medicine as the only welfare benefit. The reduction in the number of persons receiving further support was 7 per cent. Source: Fattigvæsenet 1915, NOS, Statistics Norway.

4.2 Empirical strategy

In order to analyze the impact of the voting reform on fiscal outcomes, we formulate a two-period regression model similar in spirit to Vernby (2013). This framework is essentially a difference-in-differences strategy with different treatment intensities.¹⁶ We exploit that the 1910-reform increased the voting franchise to various degree across the local governments. Our starting point is election period averages where we compare the average of the outcome variable in the post-reform election period 1911-1913 to outcome in the pre-reform election period 1908-1910. Bertrand et al. (2004) show that such an averaging procedure improves the reliability of difference-in-differences-type models in the case with serially correlated dependent variables. As a robustness check, we also estimate the model on annual data.

We estimate the following model:

$$(1) \Delta \ln(y_{ijt}) = \alpha + \beta \Delta f_{ije} + \gamma \Delta x_{ijt} + \delta c_i + \varphi_j + \varepsilon_{ijt}$$

where y_{it} is the outcome variable in local government i in county j in election period t . f_{ie} is the share of females in the voting franchise in the local elections in year e , x is control variables, c is a dummy variable for cities, and φ is county specific effects. This amounts to a difference-in-differences model with local government fixed effects.

The control variables x are to some degree restricted by the availability of data. The model can be interpreted as a traditional demand equation augmented by the voting franchise term. Thus, we include private income as an explanatory variable. This is potentially important for the interpretation of the model since the extent of the franchise extension was related to initial taxable income. We expect a positive income effect on fiscal outcomes, but that the *change* in private income is not strongly related to the *change* in the female franchise f . Average private taxable income from the statistics on local government finances is the measure of private income. Clearly, some type of income was incompletely registered, in particular this is the case for income from farming and fishing. On the other hand, taxable income is the income measure, which defined voting rights prior to the introduction of universal suffrage.

¹⁶ The approach is similar in spirit to the use of geographical differences in the bite of a national minimum wage to identify the effect of minimum wages on employment as first introduced in Card (1992) and subsequently used in, e.g., Stewart (2002).

In addition, we include the number of students as a separate explanatory variable. Previous studies have typically found that education expenditures is positively associated with the number of students, but the relationship may be highly nonlinear as suggested by evidence in Poterba (1997), Borge and Rattsø (1995, 2007) and Harris et al. (2003). In our case, the change in the number of students also serves as a proxy for population growth since total population measures is only available from the censuses every 10th year.

The model also includes an indicator for city and county fixed effects. Thus, we allow the change in outcomes to differ along these dimensions, which essentially are county specific trends and a city trend. The indicator for city is included because cities and rural local governments faced different legal framework. County dummies will control for distinct cultural differences across Norwegian regions. For example, religious activity and the share of inhabitants connected to Christian organizations, which embraces traditional gender norm differences, has traditionally been strongest in the south and the along the west-coast of Norway¹⁷. Further, according to the law, the number of representatives in the local government councils located in rural areas (herredskommuner) in a county were decided by the county council (“amtstinget”), subject to a minimum of 12 and maximum of 48 representatives, see Ihlen (1910, p. 78).¹⁸ Evidence from Sweden and Finland in Petterson-Lidbom (2012) suggests that local spending is increasing in the council size. Inclusion of fixed county effects accounts for possible trends across counties in fiscal variables due to systematic variation in local government council size.

5. Empirical results

5.1 Main results

Table 3 presents the results for expenditures per student. Column I is without control variables and is simply the correlation between the change in expenditures and the change in franchise. Contrary to expected, the correlation is significantly negative. Taken at face value, increasing

¹⁷ The spatial distribution of gender norms is discussed in Haaland et al. (2013). They use the regional distribution of support for the Christian Democratic Party (“Kristelig Folkeparti”) in elections as a proxy for traditional gender norms. This is not a possible strategy in our study, since we use data from a period long before this party was established.

¹⁸ In the city local governments, the size of city councils was determined by a rule directly related to the number of inhabitants in the city as given by the last available census as described in Ihlen (1910), p. 109. This is similar to the current system in Finland as described in Petterson-Lidbom (2012).

the share of women in the franchise by 16 percentage points (the average growth), decreases expenditures per student by about 5%.

Table 3. Estimated effects on expenditures per student, election period averages

	I	II	III	IV	V
Dependent variable	$\Delta \text{Log}(\text{Expenditures per student})$				
Δ Share of females in the franchise	-0.329** (2.14)	-0.249 (1.57)	-0.055 (0.32)	-0.107 (0.61)	-0.132 (0.77)
$\Delta \ln(\text{Number of student})$	-	-	-	-0.239* (1.95)	-0.323*** (2.65)
$\Delta \ln(\text{Private income})$	-	-	-	-	0.245*** (3.31)
Indicator for city	No	Yes	Yes	Yes	Yes
County fixed effects	No	No	Yes	Yes	Yes
R ²	0.01	0.02	0.09	0.10	0.12
Observations	524	524	524	524	524

Note. Dependent variable is the change in the logarithm of total expenditures per student from the period 1908-1910 to 1911-1913. Estimated t-values in parenthesis. Heteroscedasticity robust standard errors. ***, ** and * denotes significance at 1, 5 and 10 percent level, respectively.

Columns II-III include indicators for cities and counties. Controlling for county fixed effects and a city indicator (column III), reduces the numerical value of the estimated coefficient somewhat. The estimated coefficient is close to zero and clearly insignificant.

The last columns in Table 3 include the number of students and average private income as explanatory variables. The effects of these variables are as expected, but the qualitative effect of enfranchisement does not change. The elasticity of spending per student with respect to the number of students is -0.32. More students imply less expenditures per student, which is in line with the international evidence. Using state level data for the US, Poterba (1997) finds an elasticity of per student spending with respect to the share of the population 5-17 years old close to -1, which implies that spending is unresponsive to cohort size. Harris et al. (2003), using data for US school districts, find elasticities of per student spending with respect to the share of 0-19 years old in the interval -0.3 to -0.9, while Ladd and Murray (2001) reports elasticities around -0.4. Using data from Norway, Borge and Rattsø (1995) find that local government spending in different sectors reacts slowly to demographic shocks.

The estimated income elasticity of 0.25 is broadly in line with findings in the education finance literature. There exist a number of estimates of the income elasticity of education expenditure, but mostly from the post-WWII-period. The novel paper by Bergström et al. (1982) reports elasticities around 0.4 based on microeconomic survey data from the 1970's, which are fairly similar to those obtained on real data. Of particular interest is the results in Hoxby (1998). She estimates elasticities of spending per student with respect to per capita income for several years for school districts in Massachusetts, US, and finds elasticities of 0.35, 0.29 and 0.3 for 1900, 1910 and 1920, respectively. Falch and Tovmo (2003) find an elasticity of 0.5 of private income on total local government spending in Norwegian local governments in the 1930s. It is reassuring that estimated effects of these key determinants of educational spending are in line with previous international and national evidence and suggest that the data we use are in general informative of the development in important education policy outcomes in local governments.

As discussed above, the expenditure data include infrastructure investments. Thus, Table 4 presents results for the teacher-student ratio. The share of females in the electorate is not associated with the teacher-student ratio in the most restrictive models (column I and II). When we condition on city and county effects only (column III) there is a positive, although statistically insignificant, effect. When the model includes the number of students and private income as additional explanatory variables, the effects are negative and very small. The qualitative impact of these two control variables is broadly consistent with the results in the expenditure equation. However, the number of students has a somewhat larger negative effect. This indicates that the local governments to a large degree respond to increased number of students by increasing the average class size. Finally, we note that the income elasticity is substantially lower in the teacher per student equation than in the spending per student equation. The result suggests that private income to a larger extent increases spending on other categories, notably teacher wages, teaching materials and school buildings and other capital outlays rather than the number of teachers.

Taken together, the results presented so far suggest that neither local spending on compulsory education nor the teacher student ratio are positively associated with the expansion of female franchise. This is very different from US evidence from the early 20'th century as demonstrated in Carruther and Wanamaker (2015), but more in line with the lack of robust evidence from European countries. In the next sections we provide further investigations of the robustness of the results as well as possible mechanisms to explain the results.

Table 4. Estimated effects on the change in log number of teachers per (100) student, election period averages.

	I	II	III	IV	V
	ΔLog (Teachers per student)	ΔLog (Teachers per student)	ΔLog (Teachers per student)	ΔLog (Teachers per student)	ΔLog (Teachers per student)
Δ Share of females in the franchise	-0.051 (0.74)	-0.009 (0.13)	0.103 (1.44)	-0.010 (0.15)	-0.017 (0.25)
$\Delta \log(\text{Number of students})$	-	-	-	-0.521*** (9.30)	-0.542*** (9.18)
$\Delta \log(\text{Private income})$	-	-	-	-	0.061** (2.52)
Indicator for city	No	Yes	Yes	Yes	Yes
County fixed effects	No	No	Yes	Yes	Yes
R ²	0.001	0.02	0.09	0.29	0.30
Observations	524	524	524	524	524

Estimated t-values in parenthesis. ***, ** and * denotes significance at 1, 5 and 10 percent level, respectively.

Heteroscedasticity robust standard errors.

5.2. Robustness analyses

Subsamples

The results in the previous section indicates that education spending was not systematically related to changes in the share of females eligible to vote. Still, the results reveals that estimated effects are somewhat sensitive to the inclusion of an indicator for cities and country fixed effects. Heterogeneities in the effect of female suffrage or outliers in the data may be one reason for differences across these specifications. We explore this issue further by estimating the model using different sub- samples, and results are reported in Table 5. The first subsample is constructed by dropping the observations with the largest increases or reductions in the dependent variable. In the second subsample we drop the smallest local governments (measured by population size in 1910) since small local governments are more likely to contribute to extreme observations. The results for expenditures per student are in the left panel of the table. To facilitate comparison, the results from the full sample are repeated in the first column and it appears that the sample changes do not qualitatively affect the estimated impact of enfranchisement on expenditures per student, which is still negative and insignificant.

The results for the teacher student ration are in the three last columns. Compared to the results for the full sample (column IV) the coefficients are still negative but numerically larger in absolute values. When the sample is trimmed according to the dependent variable, the coefficient is even statistically significant. To sum up, the results are quite robust across alternative specifications of the sample and do not indicate that the main results are affected by outliers.

Table 5. Estimation results for Subsamples, election period averages

	I	II	III	IV	V	VI
Dependent variable	$\Delta \text{Log}(\text{Expenditure per student})$			$\Delta \text{Log}(\text{Teachers per student})$		
Δ Share of females in the franchise	-0.132 (0.77)	-0.127 (0.85)	-0.104 (0.54)	-0.017 (0.25)	-0.116** (2.27)	-0.060 (0.92)
$\Delta \ln(\text{Number of students})$	-0.323*** (2.65)	-0.383*** (3.89)	-0.444*** (2.77)	-0.542*** (9.18)	-0.547*** (12.81)	-0.610*** (9.60)
$\Delta \ln(\text{Private income})$	0.245*** (3.31)	0.225*** (3.58)	0.309*** (4.10)	0.061** (2.52)	0.060** (2.46)	0.077*** (2.62)
Indicator for city	Yes	Yes	Yes	Yes	Yes	Yes
County fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
R ²	0.12	0.15	0.13	0.30	0.33	0.30
Sample	Full sample	Upper and lower 2.5% of observations defined by dependent variable excluded	Local governments below 1,000 inhabitants excluded	Full sample	Upper and lower 2.5% of observations defined by dependent variable excluded	Local governments below 1,000 inhabitants excluded
Observations	524	498	464	524	498	464

Estimated t-values in parenthesis. ***, ** and * denotes significance at 1, 5 and 10 percent level, respectively.

Spending on poverty relief

Poverty relief is the spending category that resembles the purest local redistributive good. Hence, using this outcome as the dependent variable is the most precise test of the hypothesis that franchise extension increases spending on redistribution due to the decisive voter having lower income and potentially stronger preferences for redistribution. Data for poverty relief in

Norway at the local government level are only available for every fifth year during this period. Thus, we can only use spending in the last pre-reform year available (1910) and the first post-reform year available (1915) in the analysis. The dependent variable is the growth in (log) poverty relief spending between 1910 and 1915. Because data on population size are available only from the censuses every 10th year we do not use a spending per capita measure for this variable. To avoid spurious effects due to population growth we include two variables that may work as proxies for population growth on the right hand side of the spending equation. These are the change in the number of students, and the change in the total number of eligible voters between 1910 and 1913. Since all adult males and females above 25 years of age with some minor exemptions were eligible to vote from 1910 on, we consider these variables as reasonable proxies for the change in number of inhabitants in the local governments.

Table 7. Estimated effects on poverty relief expenditures, change in log expenditure from 1910 to 1915.

	I	II	III	IV	V	VI
	$\Delta \log$ (Expenditure on poor relief)					
Δ Share of females in the franchise	-0.325 (1.15)	-0.139 (0.40)	0.008 (0.02)	-0.036 (0.10)	-0.252 (0.92)	0.005 (0.01)
$\Delta \ln$ (Number of students)	-	-	0.021 (0.13)	-0.038 (0.22)	0.132 (0.98)	0.104 (0.62)
$\Delta \ln$ (number of voters)	-	-	0.753** (2.32)	0.530 (1.46)	0.463* (1.72)	0.766** (2.05)
$\Delta \ln$ (Private income)	-	-		0.173* (1.81)	0.063 (0.75)	0.079 (0.75)
Indicator for city	No	Yes	Yes	Yes	Yes	Yes
County fixed effects	No	Yes	Yes	Yes	Yes	Yes
R ²	0.003	0.05	0.06	0.07	0.08	0.07
Sample	All	All	All	All	Upper and lower 2.5% of observations defined by dependent variable excluded	Local governments below 1,000 inhabitants excluded
Observations	524	524	524	524	498	463

Estimated t-values in parenthesis. ***, ** and * denotes significance at 1, 5 and 10 percent level, respectively.

The results for the poverty relief spending equation are reported in Table 7. Column I shows the results from the simplest model without controls. The impact of the share of females is negative and an average increase in the share (16 percentage points) reduces spending on poverty relief by around 5 percent. However, the coefficient is not significantly different from zero at conventional levels. Adding controls (column II-IV) does not change the qualitative impact of the franchise extension in any significant way, although the numerical size of the estimated effects varies somewhat. The proxies for population growth have the expected positive sign, but is statistically significant only when proxied by the number of voters. The estimated income elasticity is positive, although imprecisely estimated.

Dropping extreme observations (column V) and the smallest local governments (column VI) does not change the impact of the share of females in the franchise and the main message of no systematic relationship between the franchise increase and spending on poverty relief remains.

Summing up, the results so far does not reveal any systematic positive relationship between local government spending and enfranchisement of women in the early 20'th century Norway. The crucial question is then why the results are so different from those obtained on US data from roughly the same period. Below we investigate potential mechanisms behind our results.

5.3. Possible mechanisms

In this section we ask if the results obtained above can be explained by turnout and variables describing the translation of voter preferences into policy outcomes through the political process. While simple models in the Downsian tradition predicts that politicians will converge in platforms towards the position of the median voter, recent political economy literature allow for ideological differences between parties and more sophisticated models of the relationship between voters and politicians. The basic question is whether voters affect or elect policy. The evidence in Lee et al. (2004) using US congress data, Petterson-Lidbom (2008) using Swedish local government data and Fiva et al. (2016) using data from Norwegian local governments does not lend support to the median voter model¹⁹.

¹⁹ Lee et al.(2004) provide quasi-experimental evidence from representatives voting in the US House consistent with the view that exogenous changes in the relative popularity of the candidates has no impact on the candidates' positions. Petterson-Lidbom (2008) finds a strong causal positive effect of leftwing Swedish governments on fiscal outcomes as expenditures and taxes and strongly rejects the simple median voter model.

Ideally, we would like to study the impact of local voting reforms on the political composition of the local councils in a similar way as the outcome effects. Lack of data and the fact that the party system was in its infancy during the period analyzed, render such a study impossible. Instead, we analyze the role of turnout, female representation in local councils and local variation in election system.

Turnout

One potential explanation for the absence of policy effects is simply that the newly enfranchised females did not vote and consequently had no impact on the distribution of cast votes between males and females. To explore this we first investigate the effect of enfranchisement on female voter turnout, defined as the share of the total votes cast by females.

Table 8 presents the results. The first column is without control variables while the two last columns include controls. Independent of model specification, we find an effect of about 0.4 of increased share of females in the franchise. The result implies that when the share of females in the voting franchise increases by 16 percentage points (the average change in the sample), the share of votes cast by females increases by 6 percentage points. Thus, we reject the hypothesis that the lack of influence on political outcomes is due to unchanged composition of actual votes between men and females.

Using Norwegian data, and a quasiexperimental strategy, Fiva et al. (2016) find that small exogeneous shifts in representation can affect policy in proportional election systems.

Table 8. Estimated effects of female enfranchisement on change in female voter turnout, between local election in 1907 and 1910

	I	II	III
	Δ female turnout	Δ female turnout	Δ female turnout
Δ Share of females in the franchise	0.396*** (5.45)	0.387*** (4.64)	0.394*** (4.68)
Δ ln(Number of students)	-	-	0.038 (0.79)
Δ ln(Private income)	-	-	0.052* (1.93)
Indicator for city	No	Yes	Yes
County fixed effects	No	Yes	Yes
R ²	0.06	0.10	0.11
Sample	All	All	All
Observations	524	524	524

Estimated t-values in parenthesis. ***, ** and * denotes significance at 1, 5 and 10 percent level, respectively.

Female representation in politics

It is possible that female enfranchisement of the kind we consider here only affects policy if the preferences of the newly enfranchised females are mediated by female representatives in the local council. One way to investigate this is to include measures of female representation in the local council. We first extend the models with a dummy variable equal to 1 if at least one seat in the local council was held by a female in the pre-reform election period, as well as an interaction term between this variable and the change in the female voting share.

Columns I and IV in Table 9 shows estimation results from this model formulation for the spending per student and teacher-student ratio equations, respectively. The interaction terms are far from being statistically significant. Thus, female representation in local politics in the pre-reform period does not seem to explain the absence of relationship between franchise extension and policy outcomes.

A slightly different version of this model is to represent the preferences of newly enfranchised female voters by females elected to the post-reform council. However, it can be argued that elected females is an outcome variable and thus introduces a “bad control” problem using the language of Angrist and Pischke (2009). We nevertheless estimate models including a dummy variable equal to 1 if females were elected to the local government council in 1910 as well as

the interaction between this variable and the change in enfranchised females in the electorate. Columns II and V shows the results for the spending and teacher-student ratio equations, respectively. Again, the interaction term is clearly insignificant in the spending equation. In the teacher-student ratio equation, the interaction term is negative. Taken literally, this implies that the combination of increased female share in the electorate and female representation in the local council *reduced* the teacher-student ratio, which is a quite surprising result. In the specifications in columns III and VI where we add a dummy equal to 1 if the number of female representatives increased from 1907 to 1910 and a similar interaction term as above, the same result occurs.

Taken together, the results in Table 9 again suggest that enfranchisement of Norwegian women in local elections did not affect important education policy outcomes even when we allow the effect to differ according to female representation in the local council.

Table 9. Interaction with female representation in the local council, election period averages

	I	II	III	IV	V	VI
Dependent variable	$\Delta \log(\text{Expenditures per student})$			$\Delta \log(\text{Teachers per student})$		
Δ Share of females in the franchise	-0.157 (0.91)	-0.144 (0.82)	-0.133 (0.76)	-0.016 (0.24)	0.003 (0.04)	0.004 (0.06)
$\Delta \ln(\text{Number of students})$	-0.325*** (2.70)	-0.326*** (2.68)	-0.322*** (2.64)	-0.544*** (9.08)	-0.544*** (9.03)	-0.544*** (9.07)
$\Delta \ln(\text{Private income})$	0.247*** (3.43)	0.245*** (3.35)	0.240*** (3.24)	0.061** (2.52)	0.057** (2.28)	0.060** (2.45)
Females elected to local council 1907	-0.049 (0.48)	-	-	-0.031 (1.07)	-	-
(Females elected in local council 1907) * (Δ Share of females in the franchise)	0.773 (0.97)	-	-	-0.048 (0.23)	-	-
Females elected to local council 1910	-	-0.028 (0.34)	-	-	0.046 (1.47)	-
(Females elected to local council 1910) * (Δ Share of females in the franchise)	-	0.282 (0.47)	-	-	-0.427** (2.02)	-
Increase in females elected to local council 1907-1910	-	-	0.010 (0.14)	-	-	0.087*** (2.67)
(Increase in females elected to local council 1907-1910) * (Δ Share of females in the franchise)	-	-	-0.271 (0.50)	-	-	-0.675*** (2.80)
Indicator for city	Yes	Yes	Yes	Yes	Yes	Yes
County fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
R ²	0.12	0.12	0.12	0.30	0.30	0.30
Observations	524	524	524	524	524	524

Estimated t-values in parenthesis. ***, ** and * denotes significance at 1, 5 and 10 percent level, respectively.

Election system

In the period analyzed in this paper, the party system was in its infancy in Norway, and the role of political parties and other organized groups varied substantially across local governments. A considerable share of the local governments did not have a system based on political parties with seats allocated across the political parties in proportion to their votes (“forholdstallsvalg”), which we denote proportional elections. Instead, the voters in many small and rural local governments voted directly for the persons they wanted as representatives in the local council and the candidates with the largest number of votes formed the local council. We denote this system as majority elections (“flertallsvalg”) ²⁰.

²⁰ Unfortunately, data for the 1910 election is not available for this variable. As a proxy we constructed a variable based on data from the elections in 1901 and 1913 elections where the dummy for majority elections takes the value one if there were majority elections both in 1901 and 1913. Since most changes were from majority elections

As explained above, we cannot study the effect of franchise extension on political composition per se. However, information on election system (proportional or majority) can be included in the model. Precise and theoretically based hypothesis for the effect of this variable on policy outcomes is hard to formulate, but one possibility is that local governments without organized political parties or other organized groups are less likely to respond to the franchise extension. We extend the model with a dummy equal to 1 if majority elections and an interaction term that allows the impact of the franchise extension to differ across election systems. The results displayed in table 10 based on the models with the full set of controls indicates that the election system did not affect the fiscal outcomes.

Table 10. Interaction with type of election, election period averages

Dependent variable	$\Delta\log(\text{Expenditures per student})$	$\Delta\log(\text{Teachers per student})$
Δ Share of females in the franchise	0.061 (0.25)	-0.057 (0.48)
$\Delta \ln(\text{Number of students})$	-0.378*** (3.11)	-0.558*** (9.41)
$\Delta \ln(\text{Private income})$	0.222*** (3.10)	0.059** (2.38)
Majority elections	-0.030 (0.52)	-0.033 (1.48)
(Majority elections) * (Δ Share of females in the franchise)	-0.272 (0.83)	0.092 (0.68)
Indicator for city	Yes	Yes
County fixed effects	Yes	Yes
R ²	0.14	0.30
Observations	524	524

Estimated t-values in parenthesis. ***, ** and * denotes significance at 1, 5 and 10 percent level, respectively.

To summarize; this section has shown that the extension of voting rights to non-wealthy females did significantly increase the share of total votes cast by females. However, we find no systematic relationship between policy outcomes and the extension of the female voting franchise even when we allow for effects interacted with female representation in local council and election system.

to elections with political parties this proxy should be reliable and we find that 24 per cent of the local governments had majority elections in 1910.

6. Concluding remarks

A common argument dating back to Tocqueville (1835) is that extension of the voting franchise increase government size and redistribution. While empirical studies from US, exploiting time variation in voting reforms, consistently find that franchise extension increases public spending, similar cross-country and within country studies from Europe generally find small and even negative effects. Since voting rules can be regarded as a policy instrument for the ruling governments, it is not obvious that estimated effects based on time variation in voting rules can be interpreted causally.

Using national voting reforms and local fiscal outcomes, we circumvent the potential endogeneity problems and exploit local variations in the bite of reforms to estimate the effect of franchise extension. We find that removing socio-economic restrictions on female voting rights did not increase local government spending on education, the teacher-student ratio or spending on poverty relief. Rather, in a number of model specifications, the estimated effect on spending is actually negative, although not significantly different from zero. This absence of systematic positive effects of the franchise extension are robust to different sample definitions and model specifications, despite a positive and statistically significant effect on female turnout as measured by the share of total votes cast by females.

It is of interest to see these results in the historical context and the decisions made by the Norwegian parliament on female voting rights. To some extent, the gradual process towards universal female suffrage in local election was used by national politicians as a “laboratory” to get experience with the effects from possible similar reforms in subsequent parliamentary elections. While some prominent politicians strongly opposed universal suffrage in 1910, and argued that it would increase expenditures and taxes to unacceptable levels, on June 11, 1913 the parliament *unanimously* and *without any debate* granted full suffrage for women in parliamentary elections. It is possible that the politicians initially against universal suffrage just had experienced, as our evidence suggest, that spending did not raise significantly as a result of the reform, and thus, the expected political cost of universal suffrage in terms of votes for the incumbents was likely to be close to zero.

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Appendix

Variable definitions and sources

Poverty relief: Current local government expenditures on poverty relief. This includes payment to households which for an agreed sum of money took care of people not able to provide for themselves. This was a common way to take care of the poor where in local governments that lacked special homes or institutions for poor people. The measure also includes spending associated with such institutions. Support for persons with mental or physical handicaps and medical treatment was also included in expenditures for poor people but we have excluded it from our measure. Expenditures resembling investments (construction and maintenance of buildings and “other expenditures on properties and buildings”) are also excluded. Source: Fattigvæsenet 1910 and 1915, Norges Officielle Statistik (NOS), Statistics Norway.

Education: Current local government expenditures on compulsory education per student. The measure includes wages for teachers, expenditures on books and other teaching material and building and maintenance of schools. The latter means that we have not been able to separate out investments. Source: Skolevæsenets tilstand, several annual volumes, NOS, Statistics Norway.

Teachers: The number of teachers per student. Source: Skolevæsenets tilstand, Several annual volumes. NOS, Statistics Norway.

Private income: Total private disposable income. Source: Norges kommunale finanser, several annual volumes, NOS, Statistics Norway.

Population: The population size in 1910. Source: Folketellingen 1910, Statistics Norway.