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The effects of voting franchise extension on education policy

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ABSTRACT

We study the effects of giving poor females the right to vote in local elections on education spending and teacher-student ratios. To estimate causal effects, we exploit a national voting reform in Norwegian local elections that removed socioeconomic restrictions on female voting rights. The identification strategy exploits heterogeneous changes in the share of females in the voting franchise from the pre-reform (1907) to the post-reform election (1910) across local governments. While US studies find positive effects on government spending of the introduction of female suffrage, we find no systematic effects on education spending, despite a significant increase in female turnout.

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1. Introduction

The removal of economic and social constraints on voting rights and inclusion of females in the voting franchise represent key events in the development of democracy in Western countries. However, an important research question is to what extent these changes in the gender and socio-economic composition of the electorate changed public policies. Schooling is both historically and today, one of the main services provided by the public sector. A common view is that females have particular interest and competence in family and child-related tasks such as schooling and social care and are thus likely to prefer public spending on these items.¹ This paper contributes to the research literature by studying the effect on school spending of a voting reform that included *poor* females in the voting franchise in early twentieth century Norway. We argue that voting reforms targeting poor females may have spending effects that differ from the effect of reforms that includes all females in the voting franchise.

Dating back to Tocqueville (1835), a common view is that democratization in terms of expansion of the voting franchise will increase government size and redistribution, a prediction formally demonstrated in the pure redistribution case in Meltzer and Richard (1981). However, when the government provide public services, such as public schooling, the net effect depends on two counter-acting effects since extending the voting franchise has price and income effects that work in opposite directions as argued in Kenny (1978) and Husted and Kenny (1997). Franchise extension is likely to move the position of the decisive voter down the income distribution as certainly happened in our case. If education spending is a normal good, the income effect generates lower

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demand for education. On the other hand, the poorer decisive voter faces a lower tax price on public services which tends to increase demand for education spending. These counteracting income and price effects add to the traditional hypothesis that inclusion of females in the electorate increase educational spending as females have stronger preferences for education than males. The net spending effect of including poor females in the voting franchise is thus ultimately an empirical question and depends on the size of these counteracting forces.

Lindert (2004), Engerman and Sokoloff (2005) and Goldin (2016) argue that broadening of the voting franchise played an important role in the expansion of public schooling for the masses in the US which in turn made US the leading country in terms of human capital accumulation and subsequent economic growth.²

As regards the extension of voting rights to females, the empirical evidence is mixed, and results vary substantially across studies. Lott and Kenny (1999) find that the introduction of female suffrage in the US immediately increased total state government spending and revenue and changed the states representatives voting patterns in the House and Senate in a more liberal direction. Carruther and Wanamaker (2015) find that including females in the electorate had no immediate effect, but a large long-run positive effect on educational spending in counties in the southern US states. Miller (2008) finds large positive immediate and long-run effects on local health spending. Husted and Kenny (1997) study the effect of allowing the poor to vote by removing requirements on poll taxes and literacy tests in the US states. They find that state expenditures on welfare increased, while expenditures on non-welfare items were largely unaffected.

On the other hand, results from European studies typically find much smaller effects from extensions of the voting franchise, if any effect at all. The results in Aidt, Dutta, and Loukoianova (2006), using data from 12 European countries, indicate that lifting socioeconomic restrictions on voting increased central government spending slightly, while extending voting rights to females had small and statistically insignificant effects. Using a smaller data set for 6 European countries, Aidt and Dallal (2008) find a small positive immediate effect and a larger long-run effect of female suffrage on social expenditures as share of GDP. Bertocchi (2011) finds positive effects of female enfranchisement on central government spending only in non-Catholic European countries.³ Some country studies find highly non-linear effects of franchise extensions. Aidt, Daunton, and Dutta (2010) find that voting franchise extension in local governments (Boroughs) in the second part of nineteenth century England and Wales increased spending only when franchise exceeded a threshold. Chapman (2018) finds that a democratic reform in the governance of English town councils in late nineteenth century led to lower government spending in towns controlled by the middle class in the pre-reform period. Similarly, Chapman (2016) finds that the effect of extension of the voting franchise on public goods spending in English towns has an inverted U-shape. While giving the middle class the right to vote in elections of town councils increases spending, a further extension of the voting franchise to the poor decreases spending. On the other hand, Vernby (2013) finds that giving immigrants the right to vote in local elections in Sweden in 1975 increased local spending on education and social and family services. The different results across studies and in particular across the Atlantic are puzzling and partly motivates our empirical study. We investigate the effect of removing socioeconomic restrictions on female voting on spending on education and teacher-student ratios in Norwegian local governments in the turn of the twentieth century and make several contributions to the literature.

First, the paper studies the effect of giving *poor* females the right to vote, while other studies of female voting have considered the change from a situation with no females in the electorate to a situation with universal suffrage. Second, we estimate the effect on *local* government spending within a European country in a causal framework, in contrast to most of the present European studies that use cross-country data on central government spending. Studies using only central government spending might possibly misrepresent the overall impact of franchise extension on policy outcomes since local governments provide a substantial part of public services, and clearly more so one hundred years ago than today. Moreover, local governments within a single homogenous

country in terms of language, religion, ethnicity and culture appear to be more comparable units than different countries in Europe. Our study of Norwegian local government school spending in the early twentieth investigates the response of decentralized and largely fiscally independent providers of a main public service before the introduction of the modern welfare state.

Third, 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 using 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 (2000), Lizzeri and Persico (2004), and Ticchi and Vindigni (2008).

Fourth, we investigate to what extent including poor females in the voting franchise affected the actual female share of cast votes and the role of female representation in elected assemblies. Recent evidence shows that ideological differences between parties and representatives are important and that the relationship between voter preferences and policy are more complicated than predicted by the median voter model.⁴ One particular strand of literature examine the role of female representation in elected assemblies on public spending. The evidence is rather mixed. While some studies find that female representation is necessary to shift policy in more 'female-friendly' direction, other studies find no relationship between female representation and spending policy.⁵ Motivated by this research, we analyze the relationship between school spending and female representation in the local council, and we ask to what extent the inclusion of poor females in the voting franchise changed female representation in local councils. Moreover, we study how spending effects might depend on whether partisan elements were present in the local election system.

Universal suffrage for adult Norwegian males and limited suffrage for females were introduced in the local elections in 1901. Females could vote and be elected into the local council, but only if they satisfied certain requirements in terms of property ownership and taxable income. In spring 1910, the national parliament ('Stortinget') decided to extend universal suffrage to include all adult females in the Norwegian local elections held in the fall of 1910. This election reform, combined with the substantial pre-reform variation in female voting franchise across local governments, defines our identification strategy. The change in franchise implied by the inclusion of poor female voters through the 1910 national reform is a source of exogenous variation across local governments.

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

2. Theoretical background

The median voter model, as formulated in Downs (1957), is a natural point of departure for understanding the role of the voting franchise for policy decisions. 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, in a model where the government produces public services such as education, police and infrastructure, extending the franchise have income and tax price effects with opposite signs. If public services are normal goods, the income effect partially leads to lower spending on public services of extending the franchise. If service production is financed by proportional income tax, poorer people face a lower tax price of public services than rich people, which works in the opposite direction. This is formally shown in Kenny (1978) and discussed

in Husted and Kenny (1997). Thus, for public services, the net spending effect of extending the franchise is positive only if the tax price effect is stronger than the income effect.

Recent papers have extended the early theoretical models by explicitly modeling the behavior of politicians based on ideas with long traditions in political science.⁶ Voting rules and the size and composition of the voting franchise are considered as policy tools of the incumbent political leadership rather than being exogenous. Acemoglu and Robinson (2000) argue that increasing the franchise is a credible way to commit to redistribution to prevent revolution in situations with a transient revolutionary threat.⁷

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 general 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 ([1927] 1961). Extension of the voting franchise is a mechanism whereby the incumbents can increase the people's willingness to enlist as soldiers and exert effort in mass armies, which implies that voting franchise increases in war periods.

Few theoretical studies exist on the extension of the voting franchise of females. 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. She considers a situation where females initially have stronger preferences for public goods than males and have lower wages, while the preferred tax rate decreases in income. As the female relative wage increases, the exogenous societal cost of disenfranchisement at some point exceeds 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 higher is the probability that the incumbents will introduce female suffrage. She finds empirical support for this prediction using data from 22 countries for the period 1870–1930.⁸

There are two main insights from this brief review. First, conventional political economy models suggest that the net effect on spending on public services is theoretically ambiguous due to conflicting price and income effects. This is of particular importance in our setting, since we consider a voting reform that extended the right to vote to poor females. Whether extension of the voting franchise to include poor females will increase spending on public schooling is an important empirical question since the conflicting price and income effects adds to the traditional hypothesis that females prefer higher spending on 'female-related' items as education and health than males.⁹ Second, voting reforms might be endogenously determined by the incumbent politicians in order to increase their probability to remain in power. Thus, obtaining credible evidence on the effect of changing the voting franchise requires a quasi-experimental empirical approach to address the endogeneity problem.

3. Institutions and voting reforms

3.1. Governance system¹⁰

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 multipurpose local governments governed by elected assemblies with discretion to set local taxes and allocate spending on different items, such as primary education, poverty relief, roads and other infrastructure.

Initially, the local funds were divided between different purposes, i.e. one fund for education expenditures ('skolekasse'), another fund for poverty relief expenditures ('fattigkasse'), etc. These funds were administered by separate boards ('skolekommisjon' and 'fattigkommisjon'), 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 multipurpose local governments became the main economic and political unit at the local level, which they still are today. According to Borge (2010), the income tax was the most important local tax in 1900 (about 60% of tax revenues), and the tax rate varied substantially across local governments. Central government grants amounted to 10% of local government revenues.

The Tax act of 1911 introduced the duty to file tax returns and a maximum tax rate of 10% on income. Taken literally, the latter restriction suggests that the local governments lost some of the discretion in fiscal policy, just at the same time as the extension of the voting franchise in local elections. There is a concern that this change in fiscal rules could make it less likely to observe increased educational spending following the extension of the voting franchise. Several arguments suggest that this is not likely to be the case. First, it is not clear that the tax rate limit was binding in any local government. The limit imposed only seems relevant for the cities because the tax rates in rural areas were initially far below the limit. In addition, according to the Tax Act of 1911, the local governments could set a tax rate that exceeded the tax rate limit under certain conditions.¹¹ Data for tax rates in the city local governments show that the 10 percent limit was frequently exceeded after 1911. In 1912 and 1913, the income tax rates in the cities exceeded the limit in 91% and 67%, of the cities, respectively.¹²

Second, the tax scheme had a progressive element since the tax payer had certain tax-free allowances dependent on family responsibilities, i.e. the number of dependants. The progressivity of the tax scheme was a local decision since the local council could choose between different schemes with different size of the tax-free allowances ('Reduksjonstabeller'). Thus, the council had some freedom to reduce the size of tax-free allowances if the upper tax rate limit was actually binding. Third, the duty to file tax returns introduced by the Tax Act broadened the tax base significantly. According to Gerdrup (1998), this implied that local governments could experience a considerable increase in tax revenue. NOU (1997, 19) also argue that the Tax Act did not restrict tax revenues. Thus, it might be argued that the increase in the tax base following the Tax Act increased the fiscal leeway of the local governments.

Finally, binding tax rate limits would only restrict *total* local spending. The multipurpose Norwegian local governments still had full discretion to allocate more of a given budget towards education at the sacrifice of other spending items. This is different from the situation in the US, where education is the sole activity of the school districts.

3.2. Voting reforms

While the elected local councils made decisions on local budgetary issues, the national parliament decided voting rules. 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, males with taxable income above a given threshold were included in the voting franchise. 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 Arbeiderparti') established in 1887.

Table 1. Voting rights in local and parliamentary elections, Norway 1884–1920.

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+, owning property or paying taxes on income minimum NOK 400 (cities) or NOK 300 (rural areas), or 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+

Table 1 gives an overview of the voting rights in the local and national elections, respectively, for the period 1884–1920. Throughout the period, voting rights were restricted to inhabitants above 25 years of age.

Prior to the national election in 1900 and the local election in 1901, 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, the national parliament changed the voting rules in both the national and local elections granting all males the right to vote.¹³

The parliament decided to give females who fulfilled the earlier requirements for men with respect to wealth and income the right to vote in the local election in 1901.¹⁴ Moreover, females fulfilling the requirements for voting could also be elected to the local council. Systematic collection of voting data from all local governments from 1898 and 1901, although somewhat incomplete, was initiated by the parliament to get some experience before eventually extending the voting franchise to national elections (Lindstøl 1903).

The same voting rules applied in the local elections in 1904 and 1907, but complete voting data were only collected from the 1907 election. In May 1910, the national parliament debated a proposal to extend the voting franchise to all females. In the debate, prominent politicians from the conservative party and 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 ('Venstre') argued against universal suffrage for females because 'among those many thousand females, 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'.¹⁵ Representative Alfred P. Wright from the conservative party (Høyre) 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!'.¹⁶ Despite the opposition from some prominent politicians, the parliament passed a law in May 1910 giving all women the right to vote in the local elections the following fall. As a direct protest against this decision, Sofus Arctander resigned as minister the same year.

Ideally, we would like to have more detailed information about the income and wealth as well as other socio-economic characteristics of the newly enfranchised females. Since information exists only on average income in each local government with no distinction between male and female income, it is not possible to assess the relative income level between the newly enfranchised females and the existing electorate. But as illustrated in the debate in the Parliament referred to above, there was a real concern among some politicians that giving the right to vote to females that did not pay taxes would lead to an increase in local expenditures much in line with the tax

price effect for the decisive voter in the theoretical models in Kenny (1978) and the arguments in Husted and Kenny (1997) and Chapman (2016, 2018). In reality, the new female voters were a mix of unmarried employed single females with income and wealth levels below the previous requirements, married females with low-income and low-wealth husbands as well as widows and females receiving support from other family members.

Females were included in the voting franchise in parliamentary elections some years after the introduction of universal suffrage 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 and without debate to include all females in the voting franchise for national elections.

The central government gave detailed instructions on how to hold the local elections, see Ihlen (1910). In rural (urban) areas, the local elections were 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, which is still the case in the US, from 1901 on the Norwegian local governments were required to make lists containing all persons eligible for voting publicly available. Political parties and other organized groups played a role in some, mostly urban, local governments where a *proportional election* system applied. In most of the rural local governments the representatives in the local council were elected directly through a *majority election* system described more detailed in Section 5.3 where we also study whether the effect of the franchise extension depended on election system.

3.3. Education system

The central government has set minimum standards for educational services since 1837. 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 governments were obliged to hire and pay the teachers and provide school buildings and school material. The law distinguished between cities and rural areas, and the minimum number of school days and the number of compulsory subjects was higher in cities than in rural areas.¹⁷ The local councils could freely choose the number of school days as long as the minimum requirements were met. Moreover, teacher pay was a local decision, given that pay exceeded a minimum annual level that varied between cities and rural areas. The teachers were organized in a national union covering nearly all teachers. According to Falch (2001), the minimum pay level in both cities and rural areas increased in 1910 and 1920 due to pressure from the teacher union. The inclusion of county and city government dummies account for these broad teacher wage increases. The teacher union had no formal bargaining rights before WW2, and teacher pay exceeding the minimum level was determined by the local councils.

Our conclusion is that while the legal framework required some national regulations, minimum service levels, and minimum teacher pay to be satisfied, the local units had substantial discretion in deciding the level of education spending and teacher-student ratios. Moreover, compared to the situation after the introduction of the modern welfare state especially after WW2, this local discretion to decide these issues was much larger.

4. Data and empirical strategy

4.1. Data

4.1.1. Voting franchise

Data for the local elections in 1907 and 1910 are 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 local governments over time, we exclude these local governments from the analyses. In addition, local governments with missing values of the included variables are excluded. This leaves us with a balanced panel data set of 524 local governments for the benchmark empirical period 1907–1913.¹⁸ 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 as the number of eligible individuals living in the local government the two last years before the local election. Voting statistics provided by the reports from Statistics Norway in NOS (1908) and NOS (1911) contain gender specific numbers of eligible voters as well as cast votes in each local government for the local elections 1907 and 1910, respectively. While some election data exists for the 1901 and 1904 local election as well, the data for these years are incomplete, see NOS (1908).

Table 2 presents descriptive statistics for the share of females in the voting franchise in 1907 and 1910. Females made up on average 35% and 51% of the franchise in the elections in 1907 and 1910, respectively. According to Table 2, the average growth in the female share of eligible voters between 1907 is 16 percentage points with a standard deviation of 6 percentage points.¹⁹

Figure 1 presents kernel densities for the share of females in the franchise for the elections in 1907 and 1910. 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. According to the voting rules that applied before 1910, variation in income and wealth across local governments generates variation in the number of eligible women allowed to vote in 1907. The long left tail of the density curve for the 1907 election illustrates that very few females were eligible for voting in some local governments. Unfortunately, due to lack of data on income levels for males and females separately, we are not able to tell to what extent these low numbers were the result of many females not satisfying the income and wealth requirements or if the number of women was particularly low in these governments. As expected, in the 1910-election the distribution of female franchise moves to the right, and the variation across local governments is considerably reduced.

Figure 1 implies that there is substantial heterogeneity in the bite of the 1910 reform. As a further illustration, Figure 2 presents the variation in the change in the female share of franchise between 1907 and 1910 across local governments. The maximum increase is around 40 percentage points, and there is substantial variation across the local governments. The right tail of the density curve in Figure 2 reflects the low share of females in the franchise in some local governments in the 1907-election.

Did the newly enfranchised females actually vote? The extension of the voting franchise is arguably more likely to affect educational spending if the females actually voted. While an empirical investigation of this issue, is delegated to Section 5 below, some preliminary descriptive evidence is presented here. There is no information available about the turnout among the *newly* enfranchised females, but Figure 3 presents the kernel densities of the turnout of females measured as the share of eligible females that actually voted in the elections in 1907 and 1910. In both elections, the majority of local governments have low female turnout, but the variation is large. Fewer local governments had female voter turnout below 15% in 1910 than in 1907.²⁰ The average female turnout was 21% and 28% in the elections in 1907 and 1910, respectively. Although this is low compared to the corresponding male turnouts of 51% and 57%, the combination of growth in enfranchised females and turnout led to nearly a doubling of the females' share of cast votes.

Table 2. Descriptive statistics on female voter franchise.

	Average	Standard deviation	1st decile	9th decile
Share of eligible voters who are female, 1907	0.35	0.06	0.27	0.43
Share of eligible voters who are female, 1910	0.51	0.03	0.47	0.55
Change in share of eligible voters who are female	0.16	0.06	0.08	0.24

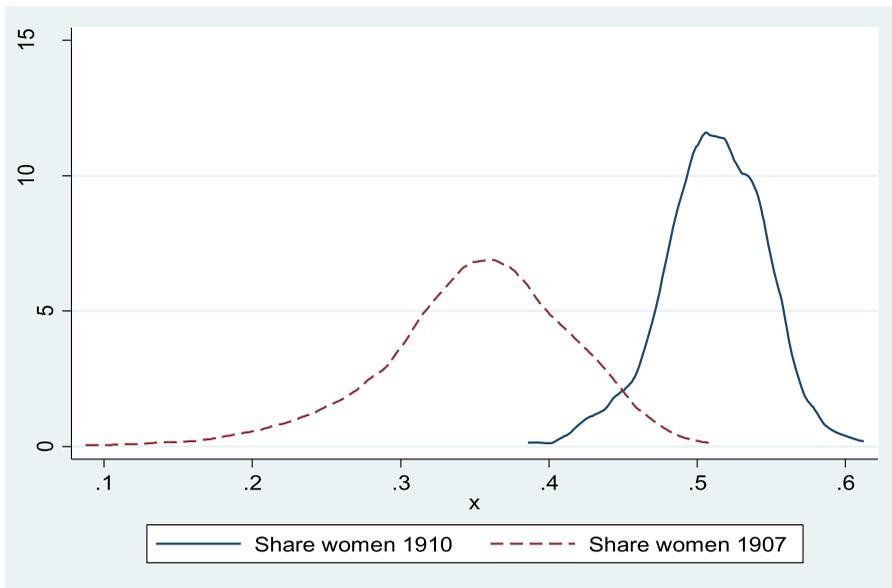


Figure 1. Density of female voting franchise in the elections in 1907 and 1910.

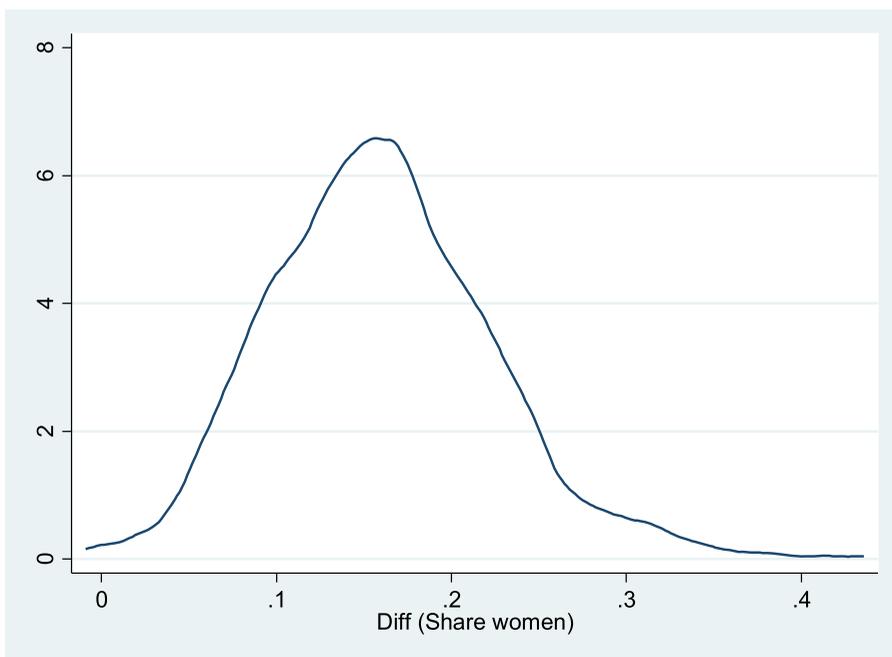


Figure 2. Density in the change in the share of female franchise from 1907 to 1910.

4.1.2. Outcome variables

We use two measures of resources allocated to compulsory education; total expenditures per student and the number of teachers per student. Data are collected from various editions of official publications (NOS) from Statistics Norway described in the online Appendix and digitalized and matched with election data by the authors. Expenditures on education include teachers' salaries,

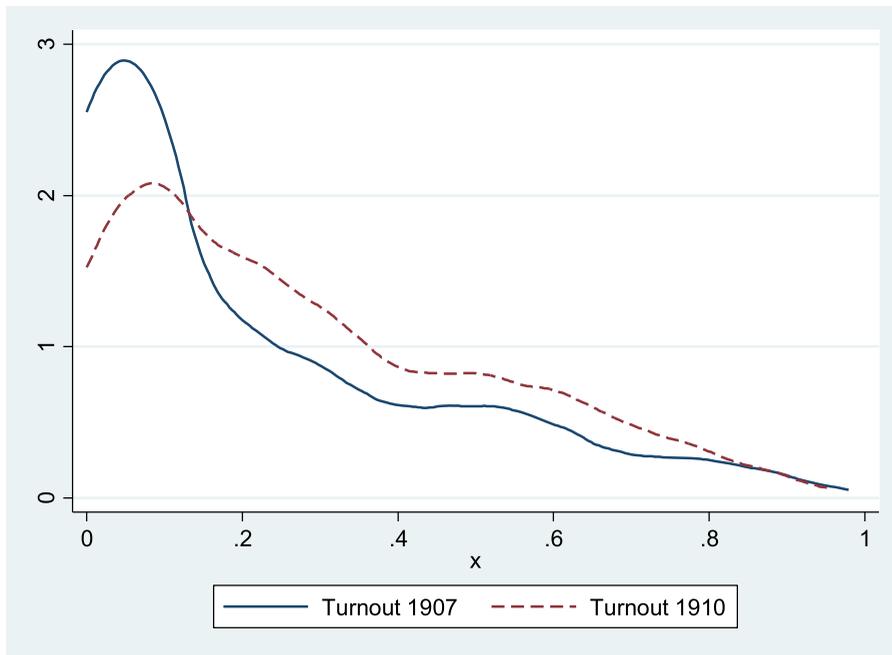


Figure 3. Density of female turnout measured as the share of eligible females voting in the elections in 1907 and 1910.

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 number of teachers per student is more closely related to actual resource use in schools. The correlation between teachers per student and expenditures per student is 0.24.

Table 3 presents descriptive statistics for the outcome variables as well as the controls. Since we use a difference-in-differences approach, the table contains information on changes in the variables between election periods, in addition to level information for each period.

Table 3. Descriptive statistics for outcome and control variables.

	Average	Standard deviation	1st decile	9th decile
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
Average values control variables, 1908–1910				
Nominal before tax private income per capita (NOK) ^a	174	73	106	273
Number of students	530	1,422	147	881
Average values control variables, 1911–1913				
Nominal before tax private income per capita (NOK) ^a	208	100	118	327
Number of students	537	1,423	147	884
Change in log values of control variables				
Log of before tax private income, nominal NOK	0.16	0.15	0.04	0.30
Log of Number of students	0.004	0.09	-0.08	0.10

^aComputed by dividing total income before tax with population size in 1910.

The average number of teachers per 100 students increases from 2.29 in the pre-reform period to 2.41 in the post-reform period. The table also presents the change between the election periods. On average, nominal expenditures per student increased by 25%. Consumer prices grew by around 10% during the same period, suggesting a significant growth in real expenditures.²¹ The average growth in the teacher-student ratio was 5%. The growth in educational resources seems to go beyond more teachers, which might include higher real teacher wages, teaching materials and investments.²² This is in accordance with the aggregate numbers presented in Falch (2001).

The variation across local governments is substantial. As expected, the variation is larger for expenditures than for the teacher-student ratio since this measure also includes capital outlays. The variation is further illustrated by Kernel densities for the growth in two outcome variables presented in Figure 4. To facilitate comparison, we use logarithm values both in the figure and the subsequent regression models.

4.1.3. Control variables

As described in the next section, we use a difference in differences strategy to identify the causal effect of the franchise extension. To obtain credible identification using this strategy, it is important to include some controls to account for potential confounding variables and make robustness checks. The empirical model includes two control variables; private income and the number of students. Private income ('antatt inntekt') is available in publications from Statistics Norway, see the online Appendix for definitions and sources. Clearly, some types of income were incompletely registered, in particular from farming and fishing. The number of students is available from the same sources as the other educational variables.

Data on population size is only available from the censuses each tenth year (1900, 1910, 1920). For illustration purposes, the descriptive statistics in Table 2 reports income per capita, using population size in 1910. Nominal income per capita increased on average by 16% in the empirical period, which indicate a real growth of 6%. The number of students was stable on average, but varies from -8% to 10% in the first to the ninth decile. A t-test shows that the average growth in the number of students

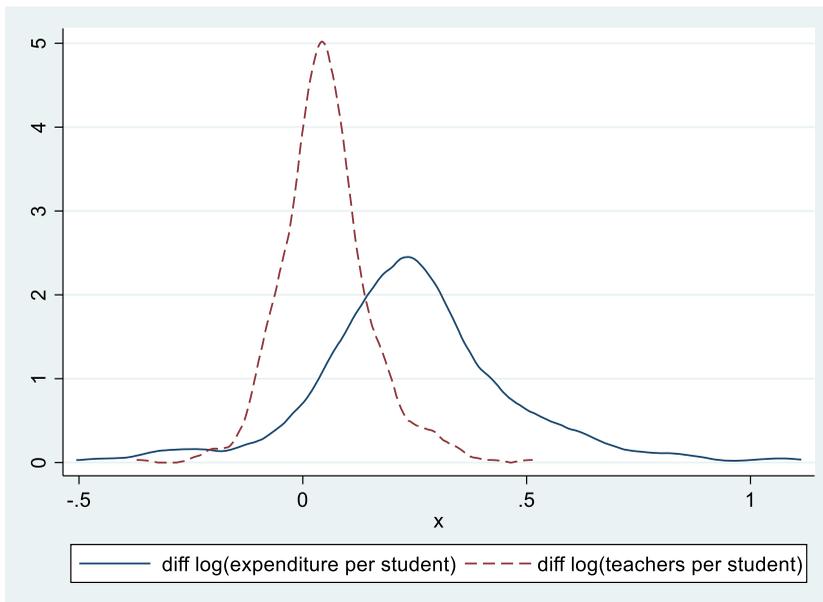


Figure 4. Growth in the log of expenditures and the log of the teacher-student ratio from 1908–1910 to 1911–1913.

is not statistically different from zero ($t = 1.1$), while the growth in per capita income is clearly positive ($t = 25.3$).

4.2. Empirical strategy

In order to analyze the impact of the voting reform, we formulate a two-period regression model similar to Vernby (2013), Berlinski and Dewan (2011) and Kroth, Larcinese, and Wehner (2016).²³ This framework is essentially a difference-in-differences strategy with different treatment intensities where we exploit the substantial variation across local governments in the change in the voting franchise due to the 1910-reform. We use election period averages for all variables. For instance, we compare for each local government the average of expenditures per student in the post-reform election period 1911–1913 with the pre-reform election period 1908–1910. Bertrand, Duflo, and Mullainathan (2004) show that such an averaging procedure improves the reliability of difference-in-differences-type models in the case with serially correlated dependent variables.

We estimate variants of the following model:

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

where y_{ijt} is the outcome variable in local government i in county j in election period t . f_{ije} is the share of females in the voting franchise in the local elections in year e , determining the local council in election period t . x the two control variables, c is a dummy variable for city governments, φ are county specific effects and ε is the idiosyncratic error term.

The model can be interpreted as a traditional demand equation augmented by the voting franchise term. Accounting for income level is important because income taxes was an important revenue source for the local governments in the period analyzed. Ideally, we would like to control for the income and wealth of females, since the variation in female franchise across local units were partly determined by these variables. But lack of data makes this impossible. Inclusion of average income in the models in combination with a first differenced regression equation will hopefully account for possible omitted variables. 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 . We do not use income per capita in the analyses because information on the population size is only available from the censuses each 10th year. The first differenced model takes account of time invariant differences in population size between local governments. In addition, we include the number of students in the model, which partly accounts for changes in population size within local governments. Previous studies have typically found that education expenditures are 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, 2008) and Harris, Evans, and Schwab (2001).

While the specification in first differences purges the regressions of omitted variables that are fixed over time, we remain concerned with the possibility that the variable of interest is correlated with the error term. Although the inclusion of control variables goes some way towards controlling for potential confounders in the relationship between education spending and female franchise extension, the model may still suffer from omitted variables problems. For example, it is possible that spending changes in local governments with large increase in female share of eligible voters is due to some underlying change in spending that would occur regardless of the change in voting rules. Therefore, specification (1) includes county ('fylke') dummies and a dummy indicating whether the local unit is a city government ('bykommune') or rural government ('herredskommune'). While private income is thought to be a strong determinant of school spending, government revenue sources in addition to income taxes, may vary systematically across local governments and may possibly affect school spending. According to Johansen (1965), the revenue from property taxes as share of total tax revenue was substantially higher in city governments ('bykommuner') than in rural governments ('herredskommuner') and the property tax share also evolved differently in city

and rural governments over time.²⁴ Thus, inclusion of city and county dummies may to some extent control for confounding changes in revenue composition between cities and rural governments and across regions in addition to increases in minimum teacher pay that differed between rural and city governments. In addition, cities and rural governments faced different legal frameworks. Further, distinct cultural differences exist across Norwegian regions today and these differences were even stronger 100 years ago. 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 Norway.²⁵ While the first difference specification effectively controls for cultural differences between local governments that are constant over time, the inclusion of county dummies controls for regional specific secular development in cultural norms. Further, according to the law, the number of representatives in the local councils ('herredsstyret') in rural governments ('herredskommuner') was decided by the county council ('amtstinget'), subject to a minimum of 12 and maximum of 48 representatives, see Ihlen (1910, 78).²⁶ Evidence from Sweden and Finland in Pettersson-Lidbom (2012) suggests that local spending is increasing in the council size. Thus, inclusion of fixed county effects also accounts for possible regional specific secular changes in council size.

5. Empirical results

5.1. Main results

A premise for any impact of female enfranchisement is that it is reflected in increased female turnout. Thus, before proceeding to the analyses of education outcomes, we investigate the connection between the increase in female voting franchise and female turnout, defined as the share of total votes cast by females. Table 4 presents the results. The first column shows regression results without control variables while the two last columns include the same controls as the analyses of education outcomes below. 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, from these reduced form effects, we conclude that female enfranchisement changed the composition of actual cast votes between males and females.

After having confirmed that female enfranchisement significantly affects female turnout, we proceed to the analysis of the effect on educational outcomes. Table 5 presents the results for expenditures per student. Column I shows the effects without the inclusion of control variables which

Table 4. Estimated effects on change in female voter turnout defined as the share of total votes casts by females. Local elections 1907 and 1910.

Dependent variable	I Δfemale turnout	II Δfemale turnout	III Δfemale turnout
Δ Share of females in the franchise	0.396*** (5.45)	0.398*** (5.39)	0.394*** (4.68)
Δ ln(Number of students)	–	–0.002 (0.04)	0.038 (0.79)
Δ ln(Private income)	–	0.059** (2.05)	0.052* (1.93)
Indicator for city	No	Yes	Yes
County fixed effects	No	No	Yes
R ²	0.06	0.06	0.11
Sample	All	All	All
Observations	524	524	524

Note: Dependent variable is measured as the change from 1907 to 1910. Estimated t-values in parenthesis based on heteroscedasticity robust standard errors. ***, ** and * denotes significance at 1, 5 and 10 percent level, respectively.

Table 5. Estimated effects on expenditures per student, election period averages.

Dependent variable	I	II	III	IV
	$\Delta \text{ Log (Expenditures per student)}$			
Δ Share of females in the franchise	-0.329** (2.14)	-0.249 (1.57)	-0.295* (1.90)	-0.132 (0.77)
$\Delta \log(\text{Number of students})$	-	-	-0.377*** (3.10)	-0.323*** (2.65)
$\Delta \log(\text{Private income})$	-	-	0.262*** (3.86)	0.245*** (3.31)
Indicator for city	No	Yes	Yes	Yes
County fixed effects	No	No	No	Yes
R ²	0.01	0.02	0.07	0.12
Observations	524	524	524	524

Note: Dependent variable is the change in the logarithm of the number of teachers per 100 students, from the period 1908–1910–1911–1913. Estimated t-values in parenthesis based on heteroscedasticity robust standard errors. ***, ** and * denotes significance at 1, 5 and 10 percent level, respectively.

simply represents the correlation between the change in expenditures and the change in franchise. Contrary to expectations, the association is significantly negative. Taken at face value, an increase in the share of females in the voting franchise by 16 percentage points (the average growth), is associated with a decrease in expenditure per student by about 5%.

Columns II–IV include different set of control variables. Adding a city indicator (column II), reduces the numerical value of the estimated coefficient and it is no longer statistically significant. The two last columns in Table 5 include the number of students and private income as explanatory variables. The effects of these variables are as expected, and the qualitative effect of enfranchisement is unaltered. The last column presents the results for our most preferred specification, including county fixed effects, and the estimated coefficient for enfranchisement is close to zero and clearly insignificant. The elasticity of expenditures 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, Evans, and Schwab (2001), using data for US school districts, find less extreme results with 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) report elasticities around -0.4 . Using post-WW2 data from Norway and Denmark, Borge and Rattsø (1995, 2008) find fairly similar results.

The estimated income elasticity is 0.25 and 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, Rubinfeld, and Shapiro (1982) reports elasticities around 0.4 based on micro-econometric survey data from the 1970s, which are fairly similar to those obtained based on actual spending 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) estimate an elasticity of total government spending with respect to private income at 0.5 in Norwegian local governments in the 1930s. It is reassuring that estimated effects of these key determinants of educational expenditures are in line with previous international and national evidence which indicates that the data we use are in general informative of the development in education policy outcomes.

Table 6 presents results for the teacher-student ratio. The share of females in the electorate has no significant impact on the teacher-student ratio in the most restrictive models (column I and II). Including the number of students and private income as additional explanatory variables (column III), results in a negative franchise effect, statistically significant at 10 per cent level. When including county dummies (column IV), the effect is close to zero and insignificant. The qualitative impact of

Table 6. Estimated effects on the change in log number of teachers per (100) student, election period averages.

Dependent variable	I	II	III	IV
	$\Delta \text{Log (Teachers per student)}$			
Δ Share of females in the franchise	-0.051 (0.74)	-0.009 (0.13)	-0.110* (1.67)	-0.017 (0.25)
$\Delta \text{ log(} \text{Number of students)}$	-	-	-0.553*** (8.96)	-0.542*** (9.18)
$\Delta \text{ log(} \text{Private income)}$	-	-	0.073*** (2.85)	0.061** (2.52)
Indicator for city	No	Yes	Yes	Yes
County fixed effects	No	No	No	Yes
R ²	0.001	0.02	0.25	0.30
Observations	524	524	524	524

Note: Dependent variable is the change in the logarithm of the number of teachers per 100 students, from the period 1908–1910–1911–1913. Estimated t-values in parenthesis based on heteroscedasticity robust standard errors. ***, ** and * denotes significance at 1, 5 and 10 percent level, respectively.

the two control variables is the same as for the expenditure equation. However, the number of students has a somewhat larger negative effect on the teacher-student ratio than expenditure per student. This suggest that the local governments to a large degree respond to increased number of students by increasing the average class size. The income elasticity is substantially lower than in the expenditure equation and suggests that the major effect of increased private income is to increase expenditures on other spending categories than the number of teachers, for example teacher wages, teaching materials and school buildings.

Taken together, the results presented so far indicate that local expenditures on compulsory education and the teacher-student ratio are unaffected by the expansion of female franchise. This is very different from US evidence from the early twentieth century, but more in line with the 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.

5.2. Robustness analyses

5.2.1. Long-run effects

If the newly enfranchised voters' demand for educational services is different from those that already has the right to vote, standard political economy models suggest that educational policy shifts immediately to reflect the preferences of the new electorate before actual voting patterns are observed, see, e.g. Miller (2008). However, franchise extension may take time to translate into actual policy changes, as politicians need time to obtain information about actual preferences of the newly enfranchised voters.²⁷ The evidence on long-run versus short-run effects differs between studies. Carruther and Wanamaker (2015) find no significant immediate effects on local education spending from the introduction of female suffrage, while the long-run effect (after 5 years) is positive and substantial. On the other hand, Miller (2008) and Lott and Kenny (1999) find large immediate effects on health spending and total state spending, respectively. To investigate whether the zero effect in the present paper is a result of using a too short time period, we present results when we extend the post-reform period to 1911–1920. Table 7 presents the results from using this longer observation window.²⁸

The results still show no statistically significant positive effects on expenditures or the teacher-student ratio. Moreover, the effects of private income and the number of students are quite similar to the estimates obtained when using the smaller window.²⁹

5.2.2. Placebo analysis

A potential problem with our empirical strategy is prevailing trends. As an example, if local governments having the largest increase in female share of the voting franchise were on a negative

Table 7. Estimated effects on expenditures and teachers per student, long-run effects.

Dependent variable	I Δ Log (Expenditure per student)			V Δ Log (Teachers per student)		
Δ Share of females in the franchise	0.022 (0.10)	-0.162 (0.71)	-0.173 (0.76)	0.017 (0.18)	-0.043 (0.49)	0.089 (0.97)
Δ ln(Number of students)		-0.420*** (3.68)	-0.332*** (2.71)		-0.452*** (7.76)	-0.447*** (7.09)
Δ ln(Private income)		0.302*** (5.37)	0.274*** (4.82)		0.051*** (2.79)	0.067*** (3.52)
Indicator for city	No	Yes	Yes	No	Yes	Yes
County fixed effects	No	No	Yes	No	No	Yes
R ²	0.001	0.11	0.22	0.001	0.24	0.30
Observations	487	487	487	488	488	488

Note: Dependent variable is the change in the log of total expenditures per student and log of teachers per student from the period 1908–1910 to 1911–1920. Estimated t-values in parenthesis based on heteroscedasticity robust standard errors. ***, ** and * denotes significance at 1, 5 and 10 percent level, respectively.

trajectory in terms of education expenditures before the 1910 voting reform, this would counteract any positive effects from the franchise extension. To investigate this issue, we extend the data back in time to 1905 and run a ‘placebo’ analysis, or a ‘Granger causality test’, as suggested by Angrist and Pischke (2009, 237). The idea is simply to test whether the change in voting franchise (the treatment variable) had an effect on educational spending before it actually happened. Thus, we run a regression with the change in the outcome variables between periods 1905–1907 and 1908–1910. The results are presented in Table 8. The effect of the franchise extension in 1910 is not significantly different from zero in the expenditure equations. In the teacher-student ratio equation there is a negative effect (column III) which might indicate a prevailing negative trend. However, in the most preferred specification with county dummies, the effect is zero, suggesting that the zero effect is not due to prevailing negative trends systematically related to the extension of the voting franchise.

5.2.3. Further robustness tests

We also conduct some additional robustness tests which supports the null effect results found in the previous section. First, we investigate whether a potential positive effect through females’ stronger preferences for educational spending is counteracted by negative income effect since many of the newly enfranchised females were poor. Lack of income data for the enfranchised females impede a direct test, but a crude alternative is to estimate a model with the franchise extension interacted with a dummy variable indicating whether the private income per capita in the local government was below or above mean (median) in 1910. The results are reported in Table A1 in online Appendix

Table 8. Estimated effects on expenditures and teachers per student, placebo regressions.

Dependent variable	I Δ Log (Expenditure per student)		IV Δ Log (Teachers per student)	
Δ Share of females in the franchise	-0.031 (0.15)	0.225 (1.03)	-0.122** (2.06)	-0.051 (0.81)
Δ ln(Number of students)	-0.508*** (4.03)	-0.466*** (3.61)	-0.673*** (11.13)	-0.655*** (10.69)
Δ ln(Private income)	0.520*** (4.53)	0.490*** (4.38)	0.100*** (2.72)	0.087** (2.33)
Indicator for city	Yes	Yes	Yes	Yes
County fixed effects	No	Yes	No	Yes
R ²	0.10	0.15	0.37	0.41
Observations	460	460	461	461

Note: Dependent variable is the change in the logarithm of total expenditure per student from the period 1905–1907–1908–1910. Estimated t-values in parenthesis based on heteroscedasticity robust standard errors. ***, ** and * denotes significance at 1, 5 and 10 percent level, respectively.

and do not indicate that systematic differences between low- and high-income local governments affect the results.

Further, we check if the results are sensitive to heterogeneities in the effect of female suffrage or outliers in the data by estimating the benchmark model using different subsamples.

The first subsample excludes the observations with the largest increases or reductions in the dependent variable. In the second subsample, the smallest local governments (measured by population size in 1910) are excluded since small local governments are more likely to contribute to extreme observations. The results are presented in Table A2 and overall, the results do not indicate that the results above are affected by heterogeneities or outliers.

Finally, we experimented with different measures of treatment to investigate potentially non-linearities. Results are reported in Table A3 in online Appendix. In column I and III treatment is simplified to 'large' and 'small' change in the female franchise with the average change as the threshold. In column II and IV, there are three categories of treatment where the 25 percent of municipalities with smallest change is defined as 'small' and, correspondingly, the 25 percent with largest change is defined as 'large'. Consequently, the reference group is the 50 per cent of the municipalities with franchise changes close to average. In both specifications the coefficient for the large change in franchise dummy is negative, although statistically insignificant, and there are no indications that the null effect found above is a result of misspecification due to omitted non-linear effects.

5.3. Possible mechanisms

In this section, we first investigate the link between public policy and female representation in the local council. Further, we investigate whether election system is important for the education outcomes and the effect of female voting franchise.

5.3.1. Female representation in politics

The previous section documents that including poor females in the voting franchise increased females' share in cast votes, without affecting public spending on education. As suggested by recent research presented in the Introduction, one hypothesis is that a requirement for female specific preferences to affect actual policy is that females are represented in elected assemblies. Inspired by this line of research, we first investigate the relationship between educational spending and female representation in the local council in our data before we proceed by looking at possible interactions between female enfranchisement and female representation.

We test the effect of female representation on spending directly by replacing the franchise extension with a measure of female representation in the benchmark model. The results are reported in Table 9. Female representation is measured with a dummy equal to unity if at least one seat was held by a female after the 1910 election. We also experimented with two alternative measures, the share of seats held by females after the election and a dummy equal to one if the 1910 election led to an increase in the share of seats held by females. The results were mainly independent of which measure applied and, consequently, we report only the results for the first measure.

When we estimate the models without city and county specific effects (column I and IV) we find that female representation increases spending. In column II and V, we include a dummy for cities with still positive impact of female representation, although statistically insignificant, as result. The insignificant effects remains when further extending the model with county specific effects (column III and VI). Insignificant effects in the richer model specifications might be a result of limited variation in the data. Females were represented in only 52 councils during the period of study, most of them cities. Thus, in line with previous studies of the impact of female representation, there is some weak support for a hypothesis that female representation increases school spending in the most parsimonious model specification but lack of statistical precision in the richer models due to limited variation in the data makes the results inconclusive. More research using other types of data not available to us are needed to give more conclusive evidence on this issue.

Table 9. Impact of female representation in the local council, election period averages.

Dependent variable	I	II	III	IV	V	VI
	$\Delta \log$ (Expenditures per student)			$\Delta \log$ (Teachers per student)		
$\Delta \ln(\text{Number of students})$	-0.355*** (2.96)	-0.352*** (2.90)	-0.313*** (2.58)	-0.545*** (8.91)	-0.543*** (8.90)	-0.542*** (9.22)
$\Delta \ln(\text{Private income})$	0.265*** (3.91)	0.260*** (3.84)	0.241*** (3.27)	0.075*** (3.49)	0.072*** (2.88)	0.060** (2.43)
Females elected to local council in 1910	0.077*** (2.72)	0.067 (1.51)	0.008 (0.22)	0.036*** (3.67)	0.013 (1.03)	-0.005 (0.36)
Indicator for city	No	Yes	Yes	No	Yes	Yes
County fixed effects	No	No	Yes	No	No	Yes
R ²	0.05	0.06	0.12	0.24	0.25	0.30
Observations	524	524	524	524	524	524

Note: Dependent variables are measured as the change from the period 1908–1910–1911–1913. Estimated t-values in parenthesis, based on heteroscedasticity robust standard errors. ***, ** and * denotes significance at 1, 5 and 10 percent level, respectively.

We next investigate the effect of enfranchisement on female representation in the local council. We use two of the dummy variables for female representation presented above; a dummy equal to unity if females held a least one seat after the 1910 election and one that equals unity if the number of seats held by females increased after the election. The results are presented in Table 10. Surprisingly, enfranchisement of women has a negative impact on female representation as females were less likely to hold seats in the council in municipalities where many females entered the franchise. While our data is not informative on individual voter behavior, one interpretation of the result is that women voted for men in the council, and even to a larger extent than men did. Extending the regression with a city dummy (columns II and IV) shows that females were more likely to hold seats in the cities, but the negative enfranchisement effect still holds. Including county fixed effects reduces the quantitative effect further (columns III and VI) but it is still clearly significant. To sum up, the franchise extension increases female turnout, but has a negative impact on female representation in the local councils. If, as suggested by earlier literature, representation is important for policy outcomes to reflect female specific preferences, the fact that increased enfranchisement did not increase female representation is a potential explanation for the absence of any positive association between female enfranchisement and educational spending and teacher-student ratios in our data.

5.3.2. Election system

In the period analyzed in this paper, the party system was in its infancy in Norway and played a minor role, especially in the rural areas. Data on the party distribution of votes exist only for the largest

Table 10. Determinants of female representation in the local council.

Dependent variable	I	II	III	IV	V	VI
	Female representation 1910, dummy			Growth in female representation, dummy		
Δ Share of females in the franchise	-1.299*** (6.18)	-0.763*** (4.22)	-0.380** (2.42)	-0.849*** (4.96)	-0.553*** (3.56)	-0.373*** (2.54)
$\Delta \ln(\text{Number of students})$		0.003 (0.02)	0.089 (0.69)		0.083 (0.63)	0.130 (1.04)
$\Delta \ln(\text{Private income})$		0.102 (1.43)	0.000 (0.01)		-0.065 (0.93)	-0.121 (1.73)
City dummy	-	0.620*** (9.48)	0.614*** (9.74)	-	0.345*** (4.98)	0.351*** (5.10)
County fixed effects	No	No	Yes	No	No	Yes
R ²	0.06	0.38	0.44	0.04	0.19	0.24
Observations	524	524	524	524	524	524

Note: Estimated t-values in parenthesis, based on heteroscedasticity robust standard errors. ***, ** and * denotes significance at 1, 5 and 10 percent level, respectively.

cities. In the traditional system (majority elections), each single voter simply wrote his own list of persons he/she wanted as representatives in the local council on the ballot and the persons with the largest number of votes were elected. However, under certain conditions, a local government could apply an alternative system (Proportional election).³⁰ In that system (which is used today), political parties or groups of people representing different geographical areas or interest groups within a local government nominated a list of names that appeared on the ballot. The voters could then choose among different ballots and the distribution of seats in the local council was determined by the number of votes for each 'party' based on an allocation formula described in Ihlen (1910, 63–66).³¹ The majority election system was common in the rural areas, while the proportional system was more common in the larger cities and densely populated areas.

Theoretically based hypotheses for the effect of these different systems on policy outcomes is hard to formulate, but one possibility is that the electoral system itself affected policy outcomes.³² Another possibility, and in particular relevant for this study, is that it affected female representation in the local councils. For instance, newly enfranchised females could vote for females in municipalities with majority elections in contrary to the proportional system where the 'parties' made the decision on council representation. To allow for possible differences between the two systems, we extend the regression models for education policy outcomes with the change in the share of females in the franchise interacted with a dummy equal to unity if the local government applied a majority election system.³³

The results displayed in Table 11 are based on the models with the full set of controls and indicates that the effect of the franchise extension did not depend on the election system in any statistical significant way.

6. Concluding remarks

A common argument dating back to Tocqueville (1835) is that extension of the voting franchise increases government size and redistribution. A particular hypothesis is that extending the voting franchise to include females would increase spending on female-related items like public education and health care. Empirical studies using historical data from the US consistently find that including females in the voting franchise increases public spending generally and especially on health care and educational items. On the other hand, cross-country and within country studies from Europe generally find small and even negative spending effects. However, voting rules can be policy instruments for the ruling governments. The size of the voting franchise is arguably endogenous, which makes it hard to obtain causal evidence especially using cross country data.

Table 11. 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	–0.137 (0.72)	–0.010 (0.13)
Δ ln(Number of students)	–0.333*** (2.75)	–0.546*** (8.98)
Δ ln(Private income)	0.241*** (3.27)	0.061** (2.42)
(Majority elections) * (Δ Share of females in the franchise)	0.148 (0.37)	–0.033 (0.21)
Majority elections	0.001 (0.02)	0.007 (0.25)
Indicator for city	Yes	Yes
County fixed effects	Yes	Yes
R ²	0.12	0.30
Observations	524	524

Note: Dependent variables are measured as the change from the period 1908–1910 to 1911–1913. Estimated t-values in parenthesis, based on heteroscedasticity robust standard errors. ***, ** and * denotes significance at 1, 5 and 10 percent level, respectively.

To provide credible evidence on education policy effects from franchise extension we exploit a voting reform in Norway in 1910 that extended the voting franchise in local elections to include poor females. Educational spending in Norway were at that time mainly determined at the local level by a number of small and largely fiscally independent local governments and the country had a very homogenous population in terms of religion, language and ethnicity. We circumvent the potential endogeneity problem by exploiting local variations in the bite of the reform to estimate the effect of franchise extension. We find that removing income and wealth restrictions on female voting rights did not increase local government expenditures on education or the teacher-student ratio. In fact, in a number of model specifications, the estimated effect on spending is actually negative. The absence of positive spending effects of franchise extension is robust to alternative sample definitions, length of period considered and model specifications, despite a positive and statistically significant effect on female turnout as measured by the share of total votes cast by females.

We present alternative explanations for why our results deviate from previous studies, especially those based on US data. One possible explanation is that we find a *negative* impact of the franchise extension on female representation in the local assemblies. Although the evidence on the effect of female representation on policies is mixed, reduced female representation might explain the absence of a positive association between female voting extension and educational spending in our data. Moreover, since the reform unambiguously shifted the decisive voter down the income distribution, the finding is consistent with conventional theory models predicting that increased voting franchise has ambiguous effects on government service expenditure due to conflicting price and income effects. Our finding is nevertheless different from the results in US studies. One possible reason is that we consider an extension of the voting rights to *poor* females, while the US studies consider a shift from a situation where no females could vote to a situation with universal female suffrage. Our result is consistent with the finding in Husted and Kenny (1997) that removal of poll taxes and literacy tests required for voting did not affect non-welfare spending in US states. Our result is also broadly consistent with recent evidence in Bursztyrn (2016), who finds that low-income voters in Brazil favor redistributive income programs that increase consumption possibilities in the short-run rather than educational spending. Further, our results are consistent with the inverted U-shaped relationship between local spending and franchise extension in English towns in late nineteenth century England found in Chapman (2016) and Chapman (2018).

Evidence in Aaberge, Atkinson, and Modalsli (2020) suggest that the overall income inequality in Norway decreased in Norway between late 1890s to around 1905 followed by stability between 1905 and 1914 and a sharp inequality increase during WW1. Unfortunately, lack of data on gender income gaps and income inequality at the local government level precludes more detailed investigation of preference differences between males and female and income groups in the period. Nevertheless, it is of interest to see our results in the historical context and the decisions made by the Norwegian parliament on female voting rights. To some extent, national politicians used the gradual process towards universal female suffrage in local elections 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. One possible interpretation of our results is that the politicians initially against universal suffrage just had experienced 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.

Notes

1. Some leading politicians actually used this as an argument when introducing restricted female suffrage in Norwegian local elections in 1901. This is illustrated by the following statement given by the leader of the conservative party ('Høyre'), Professor Francis Hagerup, in the debate in the parliament ('Stortinget') in May 1901 which

resulted in constrained suffrage for females in local elections the following fall: 'There are a number of tasks to be solved in the local governments where females have special advantages. First of all this is the case for the school system, then poverty relief and health issues and to some extent church affairs'. ('Stortingsforhandling 1900-1901', 8 p. 389-390). Author's translation.

2. The following statement in Engerman and Sokoloff (2005, 908-909) is illustrative: 'The movement for the establishment of public schools supported by local property taxes closely and successfully followed the expansion of the suffrage, which strongly suggests that the latter did indeed make a difference for policy'. Empirical evidence in Go and Lindert (2010) from the pre-1850 period lends support to this view and suggests that northern US states supplied more school resources to the county level relative to southern states because of a wider voting franchise.
3. Stutzer and Kienast (2005) exploit intertemporal and cross-section variation in the female voting rights across Swiss cantons and find a negative association between government spending and female suffrage. Two other papers use similar Swiss data. Funk and Gathmann (2006) find a *negative* immediate effect on total spending although positive effects on welfare and health spending appear after 20 years. Krogstrup and Wälti (2011) find that female suffrage led to reduced 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 the expectations. A related recent literature studies the effect of shifts in the direct costs of voting or abstention on voter turnout on government spending. Hodler, Luechinger, and Stutzer (2015) and Hoffman, León, and Lombardi (2017) use credible identification strategies and data from Switzerland and Austria, respectively, and find that reduced voting costs increases voter turnout but do not increase government spending.
4. The basic question is whether voters affect or elect policy. Lee, Moretti, and Butler (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 have no impact on the candidates' positions. Similarly, Petterson-Lidbom (2008) finds a strong causal positive effect of leftwing Swedish local governments on expenditures and taxes and strongly rejects the simple median voter model. Using Norwegian data, and a quasi-experimental strategy, Fiva, Folke, and Sørensen (2018) find that small exogenous shifts in representation can affect policy in proportional election systems. Ågren, Dahlberg, and Mörk (2007) show that preferences differ a lot between voters and representatives in Swedish local councils.
5. Besley and Case (2003) find that female representation increases spending on family related issues and child support in the US states. Chattopadhyay and Dufló (2004) found that increased female representation in politics in Indian villages increased investments in public goods more closely linked to women's concerns. Svaleryd (2009) finds that female representation in local councils increased education spending in Swedish local governments. On the other hand, Ferreira and Gyourko (2014) found no effect of mayoral gender on the size and composition of expenditures in US local governments. Exploiting a gender quota reform in 1992, Geys and Sørensen (2019) find no consistent evidence that more females in executive boards in Norwegian local governments affected public policies.
6. See Przeworski (2009) for an overview.
7. Aidt and Jensen (2014) find support for this prediction using data from European countries 1820-1938.
8. Braun and Kvasnicka (2013) provide evidence that the timing of suffrage extensions to females in the US states are inversely related to the share of females in the population.
9. The evidence in Funk and Gathmann (2015) suggests that preferences over total government resources do not differ between males and females, but females prefer different allocations than males. Females care more about environment, public health and social welfare and less about military and nuclear energy than males. Bursztyn (2016) provides evidence from expenditure in Brazilian municipalities as well as experimental evidence consistent with the hypothesis that low-income households prefer that government allocates resources to uses that yield immediate gains in consumption (cash transfers) rather than to education spending.
10. The description in this section is to a large extent based on Seip (1949), NOU (1997) and Borge (2010).
11. The local governments could apply to the Ministry of Finance for exemption from the tax limit if more than $\frac{3}{4}$ of the members in the local government council voted for the application. Moreover, according to the 1911 Tax Act, local governments were allowed to decide a tax rate such that tax revenues from the previous year was maintained as long as that rate did not exceed 12%.
12. Data from 'Norges Officielle statistik v.80', 'Norges kommunale finanser 1912', 'Norges Officielle Statistic v.100' and 'Norges kommunale finanser 1913-14'.
13. Until 1919, people receiving poverty relief ('fattigunderstøttelse') and people convicted for certain crimes were suspended from voting.
14. Also females married to a man satisfying these requirements were eligible to vote in local elections in 1901.
15. Minutes from the debate in 'Odelstinget', May 13, 1910, in Stortingsforhandling 1910, vol 8. Forhandling i Odelstinget 1910, p.1 80. Authors' translation.

16. Minutes from the debate in 'Lagtinget', May 27, 1910, referred in 'Stortingsforhandling 1910, vol 8, Forhandling i Lagtinget, p. 29. 'Lagtinget' and 'Odelstinget' was the smaller (larger) of the two houses in the Norwegian parliament ('Stortinget'). Authors' translation.
17. As an example, foreign language (English) was taught only in cities. Due to dispersed settlement, schools in rural local governments were also much smaller than schools in cities, and a single teacher was typically responsible for teaching at two or more schools in sparsely populated areas.
18. The information on the changes in the local government structure is based on the detailed historical list of local governments provided by Juvkam (1999).
19. A t-test of statistical significance has a t-value of 57.3, clearly confirming that growth is positive.
20. The number of local governments with zero female turnout decreased from 11% to 2.4%.
21. 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>.
22. The t-values for a test for statistical significance takes the values 25.5 (expenditures per student) and 11.8 (teacher student ratio) confirming that growth is positive.
23. Berlinski and Dewan analyze the impact on electoral outcomes of extending the vote to the unskilled urban population by the Second Reform Act, introduced in the United Kingdom in 1867. Kroth, Larcinese, and Wehner (2016) analyze the effect on electrification of blacks gaining the right to vote in South Africa in 1994.
24. According to Table 9, p. 209 and Table 10, p.210 in Johansen (1965), the contribution of the property tax in total tax revenue in city governments was 24.5%, 23.6% and 22,8% in 1900, 1910 and 1920, respectively. In rural governments, the corresponding numbers for 1900, 1910 and 1920 were 11.6%, 7.5% and 1.5%, respectively.
25. The spatial distribution of gender norms in Norway 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.
26. In the city local governments ('bykommuner'), the size of the city council ('bystyret') was determined by a rule directly related to the number of inhabitants in the city as given by the last available census, see Ihlen (1910, 109). This is similar to the current system in Finland as described in Petterson-Lidbom (2012).
27. According to Lott and Kenny (1999, 1166) it took several decades for turnout to fully adjust to the introduction of female suffrage in US states.
28. Since we exclude governments that merged or were split, the number of governments included in the analysis is reduced relatively to the benchmark 1907–1913 period when we use the longer observational window. Using the same restricted sample in the benchmark model does not affect the results presented in Tables 4 and 5.
29. We have also estimated a specification with the change in the outcome variables from the period 1905–1910 to 1911–1920. The estimated coefficients from this specification are very similar to those reported in Table 5 and there was no significant effect of the franchise extension.
30. In rural governments proportional elections had to be used if required by minimum 1000 voters (or minimum 1/5 of the voters if total number of voters were less than 5000). In city governments proportional elections had to be used if required by 1600 voters (or minimum 1/5 of the voters if total number of voters were less than 8000), see Ihlen (1910, 34) for details.
31. In the following we use the term 'party' to denote political parties or other organized groups participating in the local election and represented by a ballot.
32. Although not directly comparable to our case, Hinnerich and Petterson-Lidbom (2014) exploit that two different political regimes coexisted in Swedish local governments in the 1919–1938 period. They find that direct democracies (town-meetings) spent 40–60 percent less on public welfare than representative democracies (voting for political parties).
33. 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. Based on this method, we find that 24 per cent of the local governments had proportional elections in 1910.

Disclosure statement

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