The causal effects of voting franchise extensions on fiscal outcomes

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Abstract
The paper studies the effects of removing socioeconomic restrictions on female voting rights on fiscal outcomes. We use a national voting reform in Norwegian local elections in the beginning of the 20th century and exploit that the bite of the reform varied across local governments to estimate causal effects on spending on poor relief and education. In contrast to studies on franchise extensions in the US, we find no systematic effects.

Preliminary version: August, 2015
Comments are welcome

JEL-codes: D72 H10 H70
Keywords: Voting franchise; Suffrage; Expenditure; Local governments

1 Thanks to Caroline Espegren for invaluable help with the data and to participants at the IIPF 2014 Congress in Lugano, the EEA 2014 congress in Toulouse, the 2014 meeting of the Norwegian association of Economics, 2015 meeting of the European Public Choice Society in Groningen and seminar participants at Norwegian University of Science and Technology for helpful comments on earlier drafts.
1. Introduction
The link between democracy and public policy is important for understanding development in government size and redistribution during the 20th century. Dating back to Tocqueville (1835), a common prediction has been that democratization in terms of expansion of the voting franchise will increase government size and redistribution. 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 services, extended voting franchise has price and income effects that work in opposite directions as argued in Kenny (1978) and Husted and Kenny (1998). Although many authors conclude that extended franchise increases public spending, more detailed inspection of the literature shows that the empirical evidence is rather mixed and varies substantially across countries and studies.

Papers exploiting changes in voting rules in late 19’th and early 20’th century both within and across US states consistently find that reducing restrictions on the right to vote substantially increases public spending. On the other hand, evidence from European studies varies to a large degree. Moreover, while US studies find robust evidence that woman suffrage generated sharp and often immediate increases in public spending, studies using data from Europe typically find very small effects.

The different results in European and US studies are puzzling and motivate our empirical study. One concern with the present literature is whether causal effects are estimated since changes in voting rules are decided by incumbent politicians. Incumbents may consider 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). 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.

Another concern with cross-country studies from Europe is that they only cover central government spending. A substantial part of public sector service production and redistribution is provided by local governments (municipalities and counties), and to a larger degree historically than today. Thus, by using only central government spending these studies might possibly misrepresent the real impact of franchise extension on fiscal outcomes. By using
Norwegian municipalities as the unit of observation, we analyze the behavioral responses for the most important providers of main government services such as education and welfare.

This paper contributes to the literature by exploiting variation in the local electorate induced by national voting reforms in Norway in early 20’th century. We estimate the causal impact of eliminating socioeconomic and gender restrictions on voting rights on local public spending on poverty relief and education.

By exploiting variation across municipalities induced by a national reform, we circumvent the potential endogeneity problem in the literature. Until 1898, the right to vote in Norwegian local and parliamentary elections was reserved for males and conditional on owning property or having a taxable income above a given threshold. The first local election with full suffrage for males was held in 1901. This election also introduced voting rights to females conditional on property or income similar to the rules for males in the pre-1901 period. Taking effect for the local election in 1910, women got full suffrage. The latter reform is exploited in our empirical analyses of the effect of female suffrage on local (municipal) expenditure on poor relief and education.

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

2.1. Theory

The median voter model for production of government services as formulated in Downs (1957) is a natural point of departure. Within this framework, franchise extension implies that the decisive voter moves down the income distribution and faces a lower tax price for the public services. As shown in Kenny (1978), this increases the provision of public services if the income effect exceeds the uncompensated price effect. Meltzer and Richard (1981) consider a pure redistributive government and show that government expenditure increases as the median voter is located further down the income distribution. Taking these two approaches together, franchise extension is expected to increase government expenditure on redistribution items, while the impact on governmental services is 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 power 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.

Lizzeri and Persico (2004) assumes 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. The expenditures do 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.

A related issue is female participation in elections. Economic constraints may differ between males and females. For instance, if females are systematically poorer than men, woman suffrage will lead to changes in government expenditure similar to that generated by universal suffrage
for men. In addition, women’s preferences for public goods and redistribution might differ systematically from men.

Extending the voting franchise to females has not been much explicitly considered in theoretical models. An exception is Bertocchi (2011) who build a political-economic model where female suffrage emerges as a rational choice for incumbent males in a setting where females initially have stronger preferences for public goods than males and earns lower wages. Under the assumption that individual’s preferred tax rate is decreasing in income, this model predicts that as the female relative wage increase, 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). Several papers also provide empirical evidence on the impact of female suffrage on fiscal outcomes, and studies from the US tend to find large effects. Using state level data from 1870-1940 in a fixed effects framework, Lott and Kenny (1999) find that giving females the right to vote led to increased government spending, and in particular welfare 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 woman suffrage, local health spending increased by approximately 35 percent, and subsequently led to a substantial reduction in child mortality.

Of particular interest in our setting is Carruthers and Wanamaker (2015), who study the impact of woman suffrage on school expenditures. They use county level data for three US southern states where female voter enfranchisement was 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 school expenditures from 1920-1940 in the US south can be attributed to the implementation of universal female suffrage.
European studies, on the other hand, find small or no effect 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. They find a negative immediate effect on spending, while there are small 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. Jaronicki (2013) 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.

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 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 vary, 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 municipal boroughs in England and Wales in 1860’s and 70’s to study the impact on sanitation and infrastructure expenditure in local governments (boroughs). They find that effects are highly nonlinear. Expenditure decreases (increases) when franchise is below (above) a certain threshold. This nonlinear effect is consistent with a theoretical political economy model where the voting population is divided into certain groups with diverging preferences for the public goods provided.

Dincecco and Prado (2010) uses data on aggregate expenditure in 8 European countries (Denmark, Finland, France, Italy, the Netherlands, Norway, Sweden, and the United Kingdom) from 19’th century until today. They find that warfare and not franchise extension is the key driver of increased government size in the countries in the long run.

While most of the studies naturally explore changes in voting franchise many decades ago, a recent paper by Vernby (2012) 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 municipalities. Using this variation to identify causal effects, he finds that increased voting franchise increased expenditure on education and social and family services substantially.

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 government expenditure and voting franchise decisions. The recent theoretical models 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 might not reveal the full impact on government distribution and service provision from extending the voting franchise.
3. Institutions and voting reforms

Institutions

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 municipalities governed by elected assemblies with discretion to set local taxes and expenditure. This paper estimates effects of the female suffrage in the local elections in 1910.

Initially, the local taxes were divided between different purposes and expenditure based on the utility from the services, i.e. one fund for general municipal expenditure, one fund for poor relief expenditures (“fattigkasse”) and one fund for school expenditure (“skolekasse”). These different funds were administered by separate boards denoted poor relief commission (“fattigkommisjon”) and school commission (“skolekommisjon”). The poor relief act of 1863, made a change toward general taxation and decided that at least half of the poor relief expenditure should be financed by taxes on income and property. The tax act of 1882 represented a major change for two reasons. Firstly, it made income taxation compulsory for the municipalities and introduced property tax rate caps. Secondly, it instructed all municipalities to have an overall municipal budget and to have an account system covering all its activities. This implied that the municipalities were established as the main economic and political unit at the local level, which they still are today.

The tax act from 1911 represented a further change in the tax system for two reasons. First it introduced the duty to file tax returns. Second, it stated that as general rule, the income tax rate should not be higher than 10%. However, the municipalities could deviate from this rule and this happened quite often. By some politicians, the tax act from 1911 was seen as an improper regulation of local responsibilities by the central government.

While fiscal decisions were almost exclusively made locally in the empirical period of the paper, the central government made some fundamental regulations on schooling. For example, the School Act of 1889 implied that municipalities were obliged to provide schools (“folkeskole”) for all children from 7-14 years of age. The law distinguished between cities and rural areas. For example, the compulsory minimum number of school weeks was higher in cities

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2 The description here is to a large extent based on NOU (1996) and Seip (1949).
than in rural areas and there were different subjects to be taught in rural areas and cities. However, the amount of education expenditure and its financing given these minimum requirements were still local decisions.

**Voting reforms**

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

In 1884 a major reform took place adding males paying taxes from income above a given threshold to the franchise to the electorate. Within the 30-year period from 1884 to 1915 the parliament made further substantial changes in the voting rules where franchise extensions were the most significant changes. Among the two largest political parties at that 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. Including females in the voting franchise were 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 at the time.

Panel A and B in Table 1 give an overview of the voting rights in the local and parliamentary elections, respectively, in the relevant period. Prior to the parliament election in 1900 only males owning property or paying taxes of an income above a given threshold which varied across urban and rural municipalities could vote. In 1898-1899, the parliament changed the voting rules at both the national and local elections. All males above 25 years of age got the right to vote.

After a heated debate, the parliament decided to give some females the right to vote in the local election in 1901. This included 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 with such income or property. The systematic collection of voting data from all municipalities from 1898 and 1901 was initiated by the parliament to get some experience
before eventually extending the voting franchise to parliamentary elections, see Lindstøl (1903).

The same voting rules applied in the local elections in 1904 and 1907. Prominent politicians from the conservative as well as the liberal party warned against the effects of giving voting rights to all females. For instance, in a heated debate in the parliament in May 1910, 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”. In the same debate, 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 expenditure should decide the society’s expenditures which is solely paid by others? I mean not”. 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 which was implemented in the elections taking place the same fall. As a direct protest against the law, 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 panel B in Table 1. While only males could vote in national elections until 1906, females could vote conditional on earning a certain amount of taxable income or owning property in the elections 1909 and 1912. In 1913 the parliament finally decided unanimously to give females the right to vote in parliamentary elections on same conditions as men, 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 municipal assembly, see 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 election in 1898. The registration procedure for voters changed before the election in 1901. While voting

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3Minutes from the debate in “ Lagtinget”, May 27, 1910. “Lagtinget” was the smaller part of the two houses in the Norwegian parliament (“Stortinget”). Authors’ translation.
4 Until 1919, people receiving welfare benefits (fattigunderstøttelse) and people convicted for certain crimes were suspended from voting.
in pre-1900 elections required voters to actively register themselves as voters, the municipalities were required to make lists of all persons eligible for voting from 1900. Local elections have always had direct elections on voting list, often lists for specific political parties. In the parliamentary elections, however, direct elections were introduced in 1906, using single-member election districts. In 1921, the single-member election districts were replaced by the proportional rule used today.

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

Panel A. Local elections

<table>
<thead>
<tr>
<th>Elections</th>
<th>Voting rights</th>
</tr>
</thead>
<tbody>
<tr>
<td>1895</td>
<td>Males age 25+, owning property or paying taxes on income minimum NOK 800 (cities) or NOK 500 (rural areas)</td>
</tr>
<tr>
<td>1898</td>
<td>Males age 25+, owning property or paying taxes on income minimum NOK 400 (cities) or NOK 300 (rural areas)</td>
</tr>
<tr>
<td>1901, 1904 and 1907</td>
<td>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</td>
</tr>
<tr>
<td>1910 and afterwards</td>
<td>All males and females age 25+</td>
</tr>
</tbody>
</table>

Panel B. National elections

<table>
<thead>
<tr>
<th>Elections</th>
<th>Voting rights</th>
</tr>
</thead>
<tbody>
<tr>
<td>1884 - 1900</td>
<td>Males age 25+, owning property or paying taxes on income minimum NOK 800 (cities) or NOK 500 (rural areas)</td>
</tr>
<tr>
<td>1900, 1903 and 1906</td>
<td>All males age 25+</td>
</tr>
<tr>
<td>1909 and 1912</td>
<td>All males age 25+. For females, voting criteria equal to the local elections in 1901 – 1907.</td>
</tr>
<tr>
<td>From 1915</td>
<td>All males and females age 25+</td>
</tr>
</tbody>
</table>
4. Empirical strategy and data

4.1 Empirical strategy

In order to analyze the impact of the voting reforms on fiscal outcomes, we formulate a two-period regression model similar in spirit to Vernby (2011). This is a simple framework easily interpreted as a difference-in-differences strategy with different treatment intensities where we exploit that the 1910 reform increased the voting franchise to various degree across the municipalities. Annual data are available for education allowing several tests of the impact of the franchise extension. The starting point is election period averages where we compare the average fiscal outcome in the post-reform election period 1911-1913 to the pre-reform outcome of the period 1908-1910. Bertrand et al. (2004) have shown that this procedure gives inference that is more reliable in difference in difference-type models with serially correlated dependent variables. Moreover, it should reduce the problem with measurement errors and a special concern is that we have not been able to separate out investments from school spending. An alternative test for immediate effects of the reform is to compare the fiscal outcomes in the last pre-reform year (1910) with the first post-reform year (1911).

We estimate the following model:

\[ \Delta \ln(y_{it}) = \alpha + \beta \Delta f_{ie} + \gamma x_{it} + \epsilon_{it} \]

where \( y_{it} \) is the fiscal outcome in municipality \( i \) in election period \( t \), \( f_{ie} \) is the share of females in the voting franchise in the local elections in year \( e \). Since we have a model that amounts to a difference-in-differences specification municipality fixed effects are taken care of. However, we still have to be concerned about changing characteristics of the municipalities that might affect fiscal outcome through the increased franchise and adding control variables (the vector \( X_{it} \)) is an attempt to address these concerns, although the availability of data from this period has obviously restricted the choice of control variables. The first control variables are county and city dummies which capture regional shocks in the diff-in-diff specification. For example, there are regional differences in female turnout and they might be correlated with regional

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5 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).
differences in fiscal outcomes. The dummy variable for the cities captures potential differences in time paths related to the fact that cities and rural areas faced different central government regulations on key services like education. For example, as discussed in section 3 above, there were significant differences between cities and rural municipalities in the minimum amount of school weeks during each school year.

It is reasonable to interpret the model as a traditional demand equation augmented by the voting franchise term. Consistent with this interpretation, and in order to facilitate comparison with the demand for local services literature, we include private income as an explanatory variable. The log of school spending (“Utgifter til folkeskolen”) is measured per pupil. Previous studies have generally found that education expenditure does not increase proportionally with the number of pupils, Poterba (1997) finds an elasticity of per pupil spending with respect to share of state population aged 5-17 years is nearly -1 which implies that spending is almost unresponsive to cohort size, Harris et al (2003) using data on US school district revenues and spending report elasticities of per pupil revenue/spending with respect to the share of 0-19 years old in the district varying between -0.3 and -0.9, Based on data from Norwegian municipalities in the 1980’s and 90’s, Borge and Rattsø (1995) find that municipality spending in different sectors reacts slowly to demographic shocks. Borge and Rattsø (2007) uses municipal panel data from Denmark from 1989-1996 and find an elasticity of educational spending per student with respect to the share of population aged 7-15 years old around -0.7. Based on this evidence, we also include the (log of) the number of pupils as a control variable in the education expenditure equations.

4.2. Data

Voting franchise

The data from the local elections are found in the regional statistics database provided by Norwegian Social Science Data Services (NSD). In this section, we present election data for the municipalities included in the fiscal outcome analysis. We have collected data for all the municipalities over the period of study but municipalities that were involved in either mergers or splits between 1905 and 1915 are left out of the sample, leaving us with a panel data set with 536 municipalities. The total number of municipalities was 627, 657 and 681 in 1905, 1910 and 1915, respectively. This work is based on the detailed list of historical municipality merges and splits in Norway provided by Juvkam (1999).
Voluntary registration for voting was removed in Norway in 1898, and the electorate is simply defined by the number of eligible persons living in the municipality the two last years before the local election. This means that the share of women in the voting franchise is fairly accurately described by the voting statistics provided by Statistics Norway. Figure 4.1 presents kernels densities for the share of women in the franchise for the elections in 1907 and 1910 when the reform was implemented.
Females made up 35 per cent of the average franchise in the 1907 election while the post-reform average was 51. Before the reform there were two sources of variation in the franchise size, the gender distribution in the population and to what extent females met the economic requirements for voting. The longer tail on the left hand side of the density curve for the 1907 election shows that very few females were eligible for voting in some municipalities, probably because they did not meet the economic requirement for voting. As expected, since the 1910 reform removed economic restrictions on female voting rights as a source of variation in the share of females in the electorate, the variation across municipalities is lower in 1910. This also implies that there were substantial differences in the bite of the reform and this picture gets clearer when the effect of the reform is illustrated with density curves for the change in the female share of the franchise in figure 4.2. The variation across municipalities is substantial from a reduction to more than 40 per cent growth. In the majority of municipalities the share increased between 7 and 25 with an average increase of 16 percentage points. The low share of women in the franchise in some municipalities in the 1907 election shows up as a long tail on the right hand side of this curve, i.e. a large positive change.

Figure 4.2 Change in the female share of the franchise
We use the change in the share of eligible women in the electorate as described in figure 4.2 as our explanatory variable of interest instead of actual turnout share since the latter is likely to be endogeneous. Still, it is of interest to ask whether the newly enfranchised females actually voted. One hypothesis is that the extension of the voting franchise is more likely to affect fiscal outcomes if the females actually voted and a concern for the empirical analysis is that the size of the franchise extension is systematically related to turnout. We have no information about the turnout among the newly enfranchised but data on the total turnout for females are available for both elections, Kernel densities are presented in figure 4.3. The majority of municipalities have low turnout in both elections but the variation is large. The average turnout was 21 and 28 per cent in the 1907 and 1910 elections, respectively. This is low compared to the male turnout of 51 and 57 per cent but the combination of growth in enfranchised females and turnout nearly doubled the women’s share of cast votes from 15 per cent in the 1907 election to 28 per cent in the 1910 election. The distribution is considerably right-skewed in both elections even though the number of municipalities with zero female turnout was reduced from 11 to 2.4 per cent between the elections. When we look at the correlation between the franchise extension and turnout there seems to be a negative connection in the sense that turnout increased less in municipalities whith large increases in the franchise\textsuperscript{6}. We do not know why and there is a concern

\textsuperscript{6} The partial correlation coefficient was -0.14 and statistically significant at the 5 per cent level.
that unobserved characteristics of the municipalities influence the change in turnout. While our first difference approach control for all time-invariant unobserved characteristics of the municipalities, we include in our empirical analysis below county dummies to control for possible county specific trends and private income growth. (Ten gjennom dette. Kommuner med stor endring I franchise hadde også lavere turnout før reformen Finner en partiell korreasjon på 0.24 som er signifikant på 1 % nivå mellom franchise og andelen damer som stemte som andel av totale avgitte stemmer)

Figure 4.3 Female turnout

Fiscal outcomes
The main fiscal outcome under study are spending on primary schools. Data sources and detailed descriptive statistics are in the appendix. We define spending on schools as expenditures per pupil. Expenditures include teachers’ salaries, books and other teaching material as well as construction and maintenance of school buildings. The latter means that we are not able to separate out investments, which may lead to some large and possibly spurious fluctuations in spending growth between the election periods. Fortunately, we also have an alternative measure, the number of teachers per pupil. Figure 4.4 and 4.5 show Kernel densities for the relative growth for the average value of expenditures and teachers per pupil from the
last pre-reform election period (1908-1910) to the first post-reform period (1911-1913), respectively.

Figure 4.4: Growth in expenditures per pupil

The average growth for expenditures is 25 per cent. Consumer prices grew by around 10 per cent during the same period\(^7\), suggesting a significant growth in real spending. This is consistent with the observed average growth of 5 per cent in the number of teachers per pupil. The variation across municipalities is substantial for both spending measures. The variation (measured by the coefficient of variation) is somewhat larger for teacher growth than for expenditure growth. Considering the fact that expenditures comprises more than teacher wage costs this is surprising but it indicates that year to year variation in investment expenditures is small.

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\(^7\) Based on average values of the CPI for the two election periods. See Statistics Norway, http://www.ssb.no/a/histstat/tabeller/12-3.html
Figure 4.6 Growth in the number of teachers per pupil
5. Empirical results

5.1 Education

Table 5.1 and 5.2 show the results for resource use in education. Column I in the tables displays the results for the simplest model where growth in the share of females in the franchise is the only explanatory variable besides the city and county dummies. The model is extended with control variables in column II before we check the sensitivity to extreme observations by dropping parts of the sample. In column III we trim the sample according to the dependent variable by dropping the observations with largest relative increases and reductions in spending, respectively. In the last column the smallest municipalities (measured by population size in 1910) are dropped from the sample since small municipalities are more likely to contribute to extreme observations.

Table 5.1 displays the results for expenditures per pupil. The two first columns show that the estimated coefficient for share of women in the franchise is negative but close to zero and statistically insignificant independent of whether controls are included or not. This results is consistent with the finding of negligible short run effects on education spending reported by Aidt et al. (2006). Funk and Gathman (2006) also find a small, but statistically significant, negative effect on education spending in the short run.

The estimated income elasticity of 0.24 is in line with findings in comparable studies. There exist a number of estimates of the income elasticity of education expenditure mostly from the post-WWII-period. The novel paper by Bergstrøm et al (1982) reports elasticities around 0.4 based on microeconometric survey data from the 1970’s which are fairly similar to those obtained in aggregate studies at that time. Of particular interest is Hoxby (1998)’s spending per student elasticities with respect to per capita income estimated for several years from 1900-1996 for school districts in Massachussets, US. Interestingly, her reported elasticity for 1900, 1910 and 1920 is 0.35, 0.29 and 0.3, respectively and are thus close to those found in our study using Norwegian data from the same period. We also notice that the estimated elasticity of spending per pupil with respect to the number of pupils is -0.33. A decrease in spending per pupil when the number of pupils increases is also in line with the results in other studies of school spending as discussed above.
Table 5.1: Estimated effects expenditures per pupil, election period averages

<table>
<thead>
<tr>
<th></th>
<th>I</th>
<th>II</th>
<th>III</th>
<th>IV</th>
</tr>
</thead>
<tbody>
<tr>
<td>Δ share of women in the</td>
<td>-0.059</td>
<td>-0.134</td>
<td>-0.126</td>
<td>-0.104</td>
</tr>
<tr>
<td>franchise</td>
<td>(0.34)</td>
<td>(0.78)</td>
<td>(0.86)</td>
<td>(0.54)</td>
</tr>
<tr>
<td>Δ ln(private income)</td>
<td>0.244***</td>
<td>0.225***</td>
<td>0.309***</td>
<td></td>
</tr>
<tr>
<td></td>
<td>(3.29)</td>
<td>(3.58)</td>
<td>(4.10)</td>
<td></td>
</tr>
<tr>
<td>ln(pupils)-ln(pupils)</td>
<td>-0.326***</td>
<td>-0.383***</td>
<td>-0.444***</td>
<td></td>
</tr>
<tr>
<td></td>
<td>(2.69)</td>
<td>(3.90)</td>
<td>(2.77)</td>
<td></td>
</tr>
<tr>
<td>Sample</td>
<td>Whole</td>
<td>Whole</td>
<td>Upper and lower 2.5% of observations defined by dependent variable excluded</td>
<td>Municipals with less than 1000 inhabitants excluded</td>
</tr>
<tr>
<td>R²</td>
<td>0.09</td>
<td>0.12</td>
<td>0.15</td>
<td>0.14</td>
</tr>
<tr>
<td># obs</td>
<td>527</td>
<td>527</td>
<td>501</td>
<td>465</td>
</tr>
</tbody>
</table>

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

The results displayed in the last two columns show that the results are very robust to the sample changes. The coefficient for the franchise extension is still negative and statistically insignificant and the impact of the control variables is also very stable with respect to the sample reductions.

As discussed above, the expenditure data used above have some problematic features. The availability of the teacher per pupil variable enables us to investigate the impact of the franchise extension on the use of real resources in education, results are presented in table 5.2.
### Table 5.2: Estimated effects on teachers per (100) pupil, election period averages

<table>
<thead>
<tr>
<th></th>
<th>I</th>
<th>II</th>
<th>III</th>
<th>IV</th>
</tr>
</thead>
<tbody>
<tr>
<td>Δ share of women in the franchise</td>
<td>0.135* (1.81)</td>
<td>0.021 (0.29)</td>
<td>-0.116** (2.28)</td>
<td>-0.057 (0.88)</td>
</tr>
<tr>
<td>Δ ln(private income)</td>
<td>0.057** (2.31)</td>
<td>0.063*** (2.57)</td>
<td>0.078*** (2.66)</td>
<td></td>
</tr>
<tr>
<td>ln(pupils)-ln(pupils)</td>
<td>-0.531*** (8.81)</td>
<td>-0.557*** (13.08)</td>
<td>-0.608*** (9.49)</td>
<td></td>
</tr>
</tbody>
</table>

**Sample**
- **Whole**
- **Upper and lower 2.5% of observations defined by dependent variable excluded**
- **Municipals with less than 1000 inhabitants excluded**

- **R²**: 0.09, 0.27, 0.35, 0.29
- **# obs**: 527, 527, 501, 465

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

In contrast to the results for expenditures, the share of women in the franchise has a positive and statistically significant impact. The estimate implies that an average increase in female suffrage (16 per cent) is followed by an increase in the teacher-student ratio by around 2 per cent. However, the coefficient is not significant when adding control variables (column II). The qualitative impact of the controls is consistent with the results for expenditures per pupil. The income elasticity is substantially lower than the corresponding elasticity for expenditures, suggesting that increased income to a larger extent increases spending on other categories than teachers (i.e. books and other school material and building and maintenance of school houses). The elasticity of pupil growth is larger in absolute value than the corresponding elasticity found.
for expenditures, meaning that spending on other categories than teacher salaries fluctuates more with the number of students.

When we trim the sample by dropping extreme observations of the dependent variable (column III) the coefficient changes sign and is even statistically significant. The estimate implies that an average growth in the share of females in the franchise lowers the number of teachers per pupil with around two per cent. This finding is line with the impact Funk and Gathman (2006) found for Switzerland. However, when the smallest municipalities are dropped from the sample in the last column the impact is again zero and the conclusion of no systematic impact of the franchise extension is kept.

Although estimation of the model based on election period averages is our preferred identification strategy we estimate the model on annual data as a robustness check where we compare spending on education in the last year before the reform (1910) with spending the first post-reform year (1911). Table A.2 and table A.3 in the appendix present the results. The results in column I-IV vary with respect to control variables included and the sample of municipalities in the same way as the results in table 5.1 and 5.2. The main coefficient of interest depends somewhat on model specification and sample but as expected, there are no systematic effects on fiscal outcome from the franchise extension.

5.2. Poor relief

Poor relief is the spending category that resembles a pure redistributional good the most which theory clearly predicts will increase when the franchise increases. Unfortunately, annual data are not available such that election period average outcomes can not be compared. However, data are available for every fifth year and we compare spending in the last pre-reform year available (1910) with the first post-reform year available (1915). The dependent variable is growth in total spending rather than spending per capita because data on population size are available only for every 10th year (1890, 1900, 1910, 1920 etc). the first column presents results for the benchmark model with the change in the female share of the franchise as the only explanatory variable in addition to the city and county dummies. A concern is that spending might be correlated with population growth. This is a problem if growth is correlated with the change in the female franchise, which in general not can be ruled out. As a proxy for population growth we include the change in the electorate between 1910
and 1913. The electorate includes all adult males and females and should be highly correlated with population growth. As in the analyses of education we include growth in private income as a control variable.

Table 5.3 displays the results for spending on poor relief. The results in the two first columns are based on the full sample while we check the robustness of the results by dropping some observations in the last two columns.

Table 5.3: Estimated effects on spending on poor relief

<table>
<thead>
<tr>
<th></th>
<th>I</th>
<th>II</th>
<th>III</th>
<th>IV</th>
</tr>
</thead>
<tbody>
<tr>
<td>Δ share of women in the</td>
<td>-0.129</td>
<td>-0.024</td>
<td>-0.249</td>
<td>-0.038</td>
</tr>
<tr>
<td>franchise</td>
<td>(0.38)</td>
<td>(0.07)</td>
<td>(0.93)</td>
<td>(0.11)</td>
</tr>
<tr>
<td>Δ ln(private income)</td>
<td>0.164*</td>
<td>0.089</td>
<td>0.096</td>
<td></td>
</tr>
<tr>
<td></td>
<td>(1.73)</td>
<td>(1.15)</td>
<td>(0.94)</td>
<td></td>
</tr>
<tr>
<td>ln(franchise13)-</td>
<td>0.496</td>
<td>0.477*</td>
<td>0.774**</td>
<td></td>
</tr>
<tr>
<td>ln(franchise10)</td>
<td>(1.45)</td>
<td>(1.79)</td>
<td>(2.19)</td>
<td></td>
</tr>
<tr>
<td>Sample</td>
<td>Whole</td>
<td>Whole</td>
<td>Upper and</td>
<td>Municipals</td>
</tr>
<tr>
<td></td>
<td></td>
<td></td>
<td>lower 2.5%</td>
<td>with less</td>
</tr>
<tr>
<td></td>
<td></td>
<td></td>
<td>of</td>
<td>than 1000</td>
</tr>
<tr>
<td></td>
<td></td>
<td></td>
<td>observations</td>
<td>inhabitants</td>
</tr>
<tr>
<td></td>
<td></td>
<td></td>
<td>defined by</td>
<td>excluded</td>
</tr>
<tr>
<td></td>
<td></td>
<td></td>
<td>dependent variable</td>
<td>excluded</td>
</tr>
<tr>
<td>R²</td>
<td>0.05</td>
<td>0.07</td>
<td>0.07</td>
<td>0.07</td>
</tr>
<tr>
<td># obs</td>
<td>536</td>
<td>536</td>
<td>508</td>
<td>473</td>
</tr>
</tbody>
</table>

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

Column I shows the results from the simplest model without controls other than city and county dummies. The impact of the share of women is negative and quite small, as an average increase
in the share of women (16 percentage points) reduces spending on poor relief by 2 percent. The coefficient is not significantly different from zero at conventional levels. Adding controls (column II) does not change the impact of the franchise extension in any significant way. Growth in the total franchises is included as a proxy for population growth and has as expected a positive impact but is not significant which is surprising since the dependent variable is growth in expenditures not scaled by population size. The estimated income elasticity is positive.

In the third column we trim the sample according to the endogenous variable by dropping the and in the last column we drop the smallest municipalities. The estimated coefficient for the share of women is still negative and insignificant and the main message of no connection between the franchise increase and spending on poor relief remains.
6. Concluding remarks

A common argument dating back to Tocqueville (1835) is that extension of the voting franchise increases 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 seen 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 affect local government spending on poverty relief and schools in any systematic way. Rather the estimated spending effect on poverty relief and education is actually negative, although not significantly different from zero.

It interesting to interpret 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 central politicians as a “laboratory” to get experience with the effects from possible similar reforms in subsequent parliamentary elections. Some prominent politicians strongly opposed universal suffrage in 1910, and argued that it would increase expenditure and taxes to unacceptable levels. However, 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 political cost of universal suffrage in terms of votes for the incumbents was likely to be zero.
References:


Appendix

Descriptive statistics and definitions

Table A1: Descriptive statistics

<table>
<thead>
<tr>
<th></th>
<th>Average</th>
<th>Standard deviation</th>
<th>Min</th>
<th>Max</th>
</tr>
</thead>
<tbody>
<tr>
<td><strong>1908-1910</strong></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Share of women in the franchise</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Expenditures per pupil</td>
<td>25.59</td>
<td>14.65</td>
<td>9.63</td>
<td>109.82</td>
</tr>
<tr>
<td>Teachers per 100 pupils</td>
<td>2.28</td>
<td>0.69</td>
<td>1.19</td>
<td>9.45</td>
</tr>
<tr>
<td>Private income</td>
<td>825</td>
<td>4953</td>
<td>41</td>
<td>112187</td>
</tr>
<tr>
<td>Children</td>
<td>526</td>
<td>1408</td>
<td>30</td>
<td>31311</td>
</tr>
</tbody>
</table>

| **1911-1913**          |         |                    |      |      |
| Share of women in the franchise |         |                    |      |      |
| Expenditures per pupil | 33.85   | 23.45              | 11.48| 208.79 |
| Teachers per 100 pupils | 2.41    | 0.72               | 1.01 | 7.85  |
| Private income         | 1021    | 6400               | 43   | 144915 |
| Children               | 534     | 1418               | 23   | 31090 |

Growth rates
Share of women in the franchise
Expenditures per pupil
Teachers per 100 pupils
Private income
Children

Variable definitions

Poor relief: Current municipal expenditures on poor 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 municipalities 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 1905, 1910 and 1915, Norges Officielle Statistik (NOS), Statistics Norway.
Education: Current municipal expenditures on education per pupil. The measure includes wages for teachers and schoolmistresses, 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 1905, 1910 and 1915. NOS, Statistics Norway.


Table A2: Estimated effects expenditures per pupil 1910-1911

<table>
<thead>
<tr>
<th></th>
<th>I</th>
<th>II</th>
<th>III</th>
<th>IV</th>
</tr>
</thead>
<tbody>
<tr>
<td>Δ share of women in the franchise</td>
<td>-0.140</td>
<td>-0.110</td>
<td>-0.064</td>
<td>0.075</td>
</tr>
<tr>
<td></td>
<td>(0.75)</td>
<td>(0.59)</td>
<td>(0.41)</td>
<td>(0.41)</td>
</tr>
<tr>
<td>Δ ln(private income)</td>
<td>0.341**</td>
<td>0.363**</td>
<td>0.396**</td>
<td></td>
</tr>
<tr>
<td></td>
<td>(2.29)</td>
<td>(2.41)</td>
<td>(2.53)</td>
<td></td>
</tr>
<tr>
<td>Δ ln (pupils)</td>
<td>-0.774***</td>
<td>-0.816***</td>
<td>-0.805***</td>
<td></td>
</tr>
<tr>
<td></td>
<td>(8.12)</td>
<td>(5.14)</td>
<td>(9.25)</td>
<td></td>
</tr>
</tbody>
</table>

Sample

<table>
<thead>
<tr>
<th></th>
<th>Whole</th>
<th>Whole</th>
<th>Upper and lower 2.5% of observations defined by dependent variable excluded</th>
<th>Municipals with less than 1000 inhabitants excluded</th>
</tr>
</thead>
<tbody>
<tr>
<td>R²</td>
<td>0.04</td>
<td>0.12</td>
<td>0.10</td>
<td>0.13</td>
</tr>
<tr>
<td># obs</td>
<td>529</td>
<td>529</td>
<td>500</td>
<td>466</td>
</tr>
</tbody>
</table>

Estimated t-values in parenthesis. ***, ** and * denotes significance at 1, 5 and 10 percent level, respectively. City and county dummies included.
Table A.3: Estimated effects on teachers per (100) pupil 1910-1911

<table>
<thead>
<tr>
<th></th>
<th>I</th>
<th>II</th>
<th>III</th>
<th>IV</th>
</tr>
</thead>
<tbody>
<tr>
<td>Δ share of women in the franchise</td>
<td>-0.016</td>
<td>-0.026</td>
<td>-0.060*</td>
<td>-0.039</td>
</tr>
<tr>
<td></td>
<td>(0.24)</td>
<td>(0.42)</td>
<td>(1.68)</td>
<td>(0.57)</td>
</tr>
<tr>
<td>Δ ln(private income)</td>
<td>-0.081</td>
<td>-0.012</td>
<td>-0.086</td>
<td></td>
</tr>
<tr>
<td></td>
<td>(1.46)</td>
<td>0.32()</td>
<td>(1.49)</td>
<td></td>
</tr>
<tr>
<td>Δ ln (pupils)</td>
<td>-0.586***</td>
<td>-0.781***</td>
<td>-0.536***</td>
<td></td>
</tr>
<tr>
<td></td>
<td>(3.63)</td>
<td>(16.17)</td>
<td>(3.42)</td>
<td></td>
</tr>
</tbody>
</table>

Sample

<table>
<thead>
<tr>
<th></th>
<th>Whole</th>
<th>Whole</th>
<th>Upper and lower 2.5% of observations defined by dependent variable excluded</th>
<th>Municipals with less than 1000 inhabitants excluded</th>
</tr>
</thead>
<tbody>
<tr>
<td>R²</td>
<td>0.04</td>
<td>0.29</td>
<td>0.43</td>
<td>0.27</td>
</tr>
<tr>
<td># obs</td>
<td>530</td>
<td>530</td>
<td>504</td>
<td>468</td>
</tr>
</tbody>
</table>

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