Statistisk sentralbyrå
Statistics Norway

Gender or class – What determines voting?

Lessons from expanding the suffrage in early 1900s Norway

TALL SOM FORTELLER
DISCUSSION PAPERS
988

Edda Torsdatter Solbakken

Discussion Papers No.988, September 2022
Statistics Norway, Research Department

Edda Torsdatter Solbakken

Gender or class – What determines voting?
Lessons from expanding the suffrage in early 1900s Norway

Abstract:
Previous literature has found that extending the suffrage to both females and poorer voters increases the supply of public goods. This paper investigates whether the difference in voting between men and women can be explained by differences in income alone, or if there exist gender specific differences in preferences. I exploit two key features of the expansion of suffrage in municipality elections in early 20th century Norway. First, the time at which people gained the right to vote depended on both their gender and their household income. Second, the income threshold for suffrage was set nationally, creating variation across municipalities in the share of new voters following each extension of the suffrage. This variation allows me to estimate separate effects for the change in supply of health personnel following the extension of suffrage to poor men, rich women, and poor women, respectively. I find that the enfranchisement of both poor men and rich women increases the supply of doctors relative to when only rich men had the right to vote. These results are consistent with gender specific preferences for health services to the community.

Keywords: Enfranchisement, women's suffrage, public goods, welfare state

JEL classification: P16, H42, D72, J16, N24, N44

Acknowledgements: This paper is a part of the research project "People and their incomes in Norway, 1859-2013" (231625) financed by the Norwegian Research Council. I thank Rolf Aaberge and Kalle Moene for help and advice, and Ingrid Huitfeldt, Hege Marie Gjefsen, Matteo Alpino, Andreas Kotsadam, Sturla Løkken, Jørgen Modalsli and Ola Vestad for helpful discussions, suggestions, and comments at various stages at this project. Comments from seminar participants at UIO, SSB and the The 41th Annual Meeting of the Norwegian Association of Economists are gratefully acknowledged.

Address: Edda T. Solbakken, Statistics Norway, Research Department. E-mail: ets@ssb.no

Discussion Papers	comprise research papers intended for international journals or books. A preprint of a Discussion Paper may be longer and more elaborate than a standard journal article, as it may include intermediate calculations and background material etc.

Abstracts with downloadable Discussion Papers
in PDF are available on the Internet:
http://www.ssb.no/en/forskning/discussion-papers
http://ideas.repec.org/s/ssb/dispap.html

ISSN 1892-753X (electronic)

Sammendrag

Hva folk stemmer i politiske valg avhenger både av deres preferanser og deres inntekt. Tidligere forskning har vist at tilbudet av offentlige tjenester økte både når kvinner og når fattigere velgere fikk stemmerett. Denne artikkelen bruker endringer i reglene for stemmerett i norske kommunevalg for å belyse om kvinner stemmer annerledes enn menn fordi de er fattigere enn menn eller fordi de har andre preferanser enn menn.

Både av kjønn og husholdningsinntekt avgjorde når folk fikk stemmerett ved norske kommunevalg. Dette gjør det mulig å skille mellom effekten av kjønn, og dermed kjønnsspesifikke preferanser, og inntekt. Rike kvinner og fattige menn fikk først stemme ved kommunevalget i 1901 og fattige kvinner først ved kommunevalget i 1910. Inntektsgrensen for hvem som fikk stemme ble satt nasjonalt og var på litt over medianinntekten. Som følge av den nasjonale inntektsgrensen varierte andelen velgere som tilhørte de nye velgergruppene mellom kommuner. Variasjonen i andelen nye velgere gjør det mulig å estimere den kausale effekten av nye velgergrupper på tilbudet av helsetjenester ved å sammenligne endringen i helsetjenester i kommuner med høy og lav andel nye velgere i et forskjeller-i-forskjeller-rammeverk.

Artikkelen ser på endringer i tilbudet av helsetjenester av to grunner. For det første var det høy barnedødelighet og begrenset tilbud av offentlig helsetjenester når de nye gruppene fikk stemmerett. For det andre var det private tilbydere av helsetjenester tilgjengelig for de som kunne betale for det. Tilstedeværelsen av private tilbydere gjør det mulig å skille mellom kjønnsspesifikke preferanser for helsetilbud til egen familie og til samfunnet generelt.

Resultatene viser en økning i antall leger når rike kvinner og fattige menn har stemmerett relativt til når bare rike menn har stemmerett. Siden både rike menn og rike kvinner har tilgang til private helsetjenester stemmer resultatene overens med hypotesen om at kvinner vektlegger tilbudet av helsetjenester til samfunnet generelt mer enn menn.

1 Introduction

Voting behavior may depend both on preferences and income. Women's suffrage and abolishing the economic constraint for suffrage have both been shown to increase the supply of public goods. Is this because women are less well-off than men or because they have stronger preferences for public goods? By using extensions of the suffrage in Norway in the early 20th century, I examine whether the difference in voting between men and women can be explained by differences in income alone, or if there exist gender specific differences in preference.

Understanding whether women have other preferences for policies than men is important both today and historically. The growing literature documenting that women have stronger preferences for children's wellbeing and social welfare in general has led many to think that female empowerment is not just a goal of equal treatment but a way to promote development in poor countries (Miller, 2008). If the differences in revealed preferences between men and women are due to differences in income, the focus on women instead of the poor can be misleading. From an historical perspective, it is interesting to see whether the change in policies coinciding with the extension of the suffrage to women is associated with the introduction of the female suffrage.

Separating social preferences by gender and economic status is difficult empirically, because demographic and socioeconomic characteristics may be overlapping. Preferences, gender, and economic status can be studied in surveys. However, participation in surveys is voluntary, and participation may be correlated with preferences. People might also answer dishonestly or differently than if they were forced to make real trade-offs (Brunner et al., 2011).

This paper separates gender and economic status by exploiting that the right to vote in Norwegian municipality elections depended on a combination of gender and household income. From just including rich men, the right to vote was granted to all men and women from rich households in 1901, and to all women in 1910. Three features make this an attractive context for drawing causal inference about the effect of voters' gender and economic status in a difference-in-differences framework. First, it is straightforward to identify when rich women, poor women, and poor men enter the pool of voters because the right to vote depended on household income and official statistics reports the number of eligible voters by gender. The threshold for suffrage was just above the national median household income, so what is referred to as rich and poor can be read as above and below the median. Second, there is not a concern that the timing of the treatment is correlated with potential confounders that may affect the outcomes. The income threshold for suffrage was set nationally and not by the discretion of the municipalities. The nationally set income threshold creates variation in the share of new voters across municipalities following each extension. Third, the setting and data availability

allow for studying real policy changes and does not rely on survey data. Together, these features allow for identifying the causal effect of new voters by comparing the change in outcomes in municipalities with a high share of new voters with the change in outcomes in municipalities with a low share of new voters.

Health services is chosen as the outcome of interest for two reasons. First, the combination of high mortality rates and low provision of public health services makes it likely that the demand for health services was a salient feature of the elections at the time. Second, private practitioners made it possible for the rich to pay for their own health services without having to contribute to others' access to health services by demanding it publicly provided. This allows for separating between gender specific preferences for health services to one's own family and preferences for health services to the community. I examine the change in supply of three types of health personnel; midwives, vaccinators, and doctors; and estimate effects on mortality.

The Meltzer and Richard (1981) model can be used to see how introducing poorer individuals to the suffrage may lead to more redistribution because the gap between the mean income and the income of the median voter increases. However, the empirical evidence is mixed. Acemoglu et al. (2015) describe the literature on democracy and redistribution as voluminous and full of contradictions. Looking at the abolishment of the economic restriction for suffrage in Europe and North America during the 19th and 20th century, several studies find a positive correlation between extension of the suffrage and governmental spending, social spending, and tax revenues; see Aidt et al. (2006), Aidt and Jensen (2009), Acemoglu and Robinson (2000) and Lindert (1994). However, there are econometric problems with much of this literature, so one must be careful with causal interpretations (Acemoglu et al., 2015). Comparing Brazilian states that introduce new election technology at different points in time, Fujiwara (2015) finds that facilitating voting for less educated voters shifts government spending toward health care that benefits the poor. Contrary to this, Mulligan et al. (2004) find no significant difference in social and economic policies between democracies and non-democracies in a cross-section of countries.

Using variation in the timing of the introduction of female suffrage across the United States, Lott and Kenny (1999) find that the enfranchisement of women coincided with an immediate increase in state government expenditures and revenue, and that the effect continued growing over time as more women took advantage of the franchise. Looking at the same reforms, Miller (2008) finds that female suffrage increased health spending and reduced infant mortality. Similarly, Aidt and Dallal (2008) and Aidt et al. (2006) find that female suffrage increased social spending in Western Europe, but had no effect on the total budget. The introduction of female suffrage in Switzerland in 1971 allows Abrams and Settle (1999) to compare changes in governmental spending in Switzerland to changes in neighboring countries where females already had the right to vote. Their

findings confirm that enfranchisement of women increased the social welfare spending also in more modern times.[1]

Gender differences in voting have been explained by women having less economic resources than men. Abrams and Settle (1999) claim that the increase in social spending coinciding with the enfranchisement of women is at least partly because the decisive voter becomes poorer. Edlund and Pande (2002) link the increasing gender gap in political preference in the United States to a decrease in the marriage rate. They argue that men transfer resources to women in marriage and thus men become richer and females poorer when the marriage rate goes down. Edlund et al. (2005) confirm that these results hold also for Western Europe. In contrast, Funk et al. (2015) find that the gender gap holds even when controlling for characteristics such as age, education and income.[2] They conclude that the gender gap is a result of gender specific preferences.

The empirical results presented in this paper are consistent with a hypothesis about gender specific preferences for health services to the community. The results suggest that rich women demand a higher public supply of doctors than rich men do. Since both rich women and rich men have access to private doctors, the increase in public supply driven by the enfranchisement of rich women indicates that the rich women care more than rich men about other people's access to health services. The enfranchisement of poor men alters the supply of doctors relative to when only rich men had the right to vote. This suggests that income, and thereby the accessibility to private alternatives, does impact the demand for public provision of health personnel. There are some indications of increased supply of vaccinators following the enfranchisement of poor women. This can be interpreted as gender specific preferences for health provision to one's family, as poor males already had the right to vote. However, the specification with yearly treatment dummies suggests that this effect is driven by pre-existing time trends that are dependent on the treatment intensity.

The remainder of this paper is structured as follows. Section 2 presents the institutional setting and provides a theoretical framework for how the expected changes in the supply of health personnel may depend on gender differences in voting. Section 3 introduces the data and discusses the empirical strategy. Section 4 presents the empirical results and section 5 concludes.

[1] See Doepke et al. (2012) for a review of the political and economic consequences of women's rights.

[2] Funk et al. (2015) use surveys containing both whether and what individuals voted under federal ballots and a broad range of socio-economic characteristics. The surveys are held shortly after the elections, and thus Funk et al. (2015) argue they can measure the survey accuracy perfectly by comparing the approval of policies in the survey to the actual approval from official ballot statistics.

2 Institutional setting and theoretical framework

2.1 Institutional setting

I consider two extensions of the suffrage in municipality elections: the 1901 election, when the suffrage was extended to poor men and rich women; and the 1910 election, when the suffrage was extended to poor women. Before the extension in 1901, only men with income above a certain threshold had the right to vote.[3]

Fear of increased taxes and public spending was at the center of the debate of extending the suffrage to poorer voters. Traditionally, the suffrage had been seen as a contract between voters and the society, where the qualification for suffrage was to contribute to society by paying taxes. The current elite was reluctant to extend the franchise as new voters potentially wanted the municipalities to provide more services. The municipalities' right to collect tax, and hence fund potential new services, was not constrained by the central government. The potential new voters had little or no tax contribution relative to the current elite, causing the elite to fear for future tax levels (Flo and Aars, 2010).

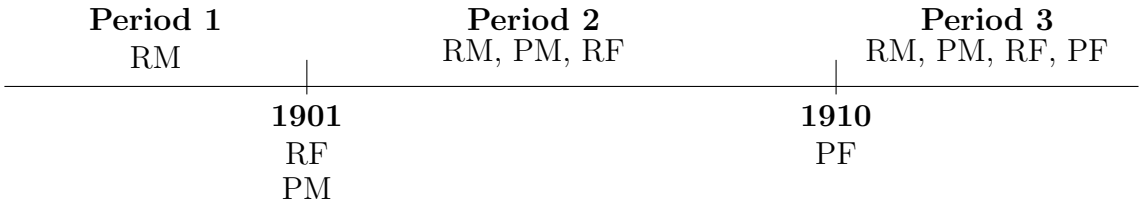
The traditional view on suffrage was challenged by increasing female participation in paid work and the growth of the labor movement. Already in 1888, The Liberal Party (Venstre) proposed that women who participated in paid work should be able to qualify for suffrage on equal terms as men. The growth of the labor movement and the formation of The Norwegian Labour Party (Det norske Arbeiderparti) in 1887 shifted the suffrage debate from female suffrage to universal suffrage for men (Larsen, 2013). When it became clear that extending the suffrage for males was inevitable, extending the suffrage to women in richer households gained new support. The conservatives saw granting women in rich household the right to vote as a way to keep the new poor male voters from gaining the majority of votes (Flo and Aars, 2010). The debates resulted in the enfranchisement of poor men and women with income above a certain threshold (or who were married to men with income above this threshold) in the 1901 election.[4]

Extending the suffrage to poor women would shift the power from rich voters to poor voters in many municipalities. Those in favor of extending the suffrage viewed it as easier to implement distributional and welfare policies in the municipalities than at the central government. Meanwhile, the existing elite was concerned that the municipalities would be easy victims for the new voters. A new tax law that allowed the central government to set an upper threshold on the amount of tax the municipalities could collect was introduced to dampen this concern (Flo and Aars, 2010). The income constraint on suffrage was removed for females in the 1910 election.

[3]This paper focuses on municipality elections. A brief summary of the suffrage reforms related to parliament elections that occurred in the same period is presented in Appendix Section A.

[4]The threshold was 400 Norwegian kroner per year in urban areas and 300 Norwegian kroner per year in rural areas.

This leaves us with three time periods, as illustrated in the diagram below: in period 1, only rich males (RM) have the right to vote; in period 2, rich males, poor males (PM) and rich females (RF) have the right to vote; and in period 3, rich males, poor males, rich females and poor females (PF) have the right to vote.



Although the income constraint was abolished in 1910, until 1919 the right to vote could be suspended due to conviction of crimes or dependence on poor relief (Søbye, 2017a). I constrain the analysis to the period between 1891 and 1918, to avoid picking up effects from the extension of suffrage to wage workers in 1884 and the inclusion of people on poor relief in 1919.[5]

I aim to infer the demand for public provision of health services by looking at how the supply of health personnel changed when the suffrage was extended. For a change in the demand for health personnel to translate into a change in supply, the voters need to express their demand through elections and the politicians need to be willing and able to respond to the change in demand. For the municipality politicians to be able to respond to the new voters' demands for health personnel, there needs to be available health personnel. This was the case, as it was educated more midwives and doctors than there were public positions at the end of the 19th century. The alternative for doctors was to start a private practice, but public employment was economically more attractive (Schiøtz, 2003). This indicates that educated personnel were available to increase the public supply of health personnel in response to increased demand.

2.2 Disentangling income from preferences

In this section I construct a simple model of demand for public health care in presence of private alternatives. The main purpose of this model is to show how the reforms can be used to identify whether gender differences in voting are driven by differences in income or by differences in preferences. The model demonstrates that the sets of predicted outcomes from the reforms differ depending on the assumptions about the underlying gender difference. These sets of predicted outcomes can be compared to the empirical results to help pin down the underlying mechanism behind the gender difference in voting.

The purpose of the arguments that follow is to show how extending the right to vote to

[5]The qualification for suffrage for men shifted from ownership of land and certain governmental official to income above a certain threshold in 1884.

new groups changes the composition of the voters. If the new voters have different political demands than the existing voters, the new voters may change the political balance and induce changes in the public provision of health services as political parties adjust their programs and promises to capture some of the new votes. Hence, the focus of the following model is to find the ideal policy for the different voting groups by constructing a generic set of preferences that allows for both differences in income and potential differences in preferences for healthcare.

Preferences and income

Let the health services that are provided by the municipality, k_m, and by the private market, k_p, be close substitutes. The voters can choose between demanding k_m through elections or buying k_p in the market. Publicly provided health services are available to everyone in the municipality and assumed to be financed by a flat marginal tax rate t on household income y. Privately provided health services are only available to those who buy them at market price p. The only differences between the public and private health services are the providers and the price.

One way to represent the electorate's demands is to assume that all voters have some basic preferences in common, but that they may differ in the weight they put on the different components in their overall utility and in their economic opportunities to satisfy their preferences. When k_m and k_p are close substitutes, a typical voter's utility of health services provided to the family members ca be presented by an increasing and concave utility function $H(k_p + k_m)$. For any level of public provision k_m, the voter can top up the provision by demanding k_p in addition. The voter's social utility of health services provided to everybody in the community can be represented by an increasing and concave function $A(k_m)$. Finally, let U be an increasing and concave utility function of private non-health consumption $C = y(1-t) - pk_p$, and $\bar{y}$ the mean income in the municipality. A balanced municipality budget implies $k_m = \bar{y}t$.

Now, the overall utility of a generic voter can be represented by

$$V = U(C) + \beta H(k_m + k_p) + \gamma A(k_m) \tag{1}$$

The voters can differ along three dimensions: their household income y, their evaluation of health services for their own family β, and their evaluation of health services for the community γ.

Optimal public provision

For a household with income y, the real price of the public provision k_m is $y/\bar{y}$. Hence, rich households pay more for public provision than poor households. Regardless of the

voters' utility of the community's access to health services, a voter always prefers public provision over private provision to their own family if the household income relative to the mean is below the price of private provision;

$$\frac{y}{\bar{y}} < p \tag{2}$$

The optimal amount of public provision for the voters with income below the corresponding income threshold $p\bar{y}$, can be found by assuming a balanced municipality budget and differentiating the overall utility of the voters (equation (1)) with respect to public provision. The resulting optimal amount is determined by

$$\frac{dU(C)}{dC}\frac{y}{\bar{y}} = \beta\frac{dH(k_m)}{dk_m} + \gamma\frac{dA(k_m)}{dk_m}, \tag{3}$$

saying that the private marginal cost of public provision (the left-hand side) should equal the marginal gain of higher public health services (the right-hand side).

In addition to their income, we can see from equation (3) that the optimal policy for the voters below the threshold $y < p\bar{y}$ depends on their evaluation of healthcare. The more these voters value access to health care both for their own family, β, and for the community, γ, the more public health provision they favor.

For the voters with income above the threshold $p\bar{y}$, the evaluation of health services for their own family does not affect their demand for public provision directly. For them, it is cheaper to buy health services for their own family in the private market. They may still demand public provision of health services if they care about the community's access to health services. Their ideal policy for public provision is determined by

$$\frac{dU(C)}{dC}\frac{y}{\bar{y}} = \frac{dU(C)}{dC}p + \gamma\frac{dA(k_m)}{dk_m}, \tag{4}$$

since they increase private provision k_p until $\beta dH(k_m + k_p)/dk_p = (dU/dC)p$. The more the voters care for health services to the community, γ, the higher price they are willing to pay for public provision. Hence, for a given income, voters who care more for health services to the community prefer more public provision of health services.

Predicted outcomes of the reforms

The first result from the simple model is that for voters with income above a certain threshold, choosing private health services for their own family is less costly than publicly provided health services. That the strong resistance against extending the suffrage among rich men was rooted in the fear of increased public spending indicates that most of the original voters had income above the threshold for preferring private provision of health

care to their own family.[6] Similarly, the fact that rich women was granted the right to vote to compensate for the inevitable extension of the suffrage to poor men suggests that rich women's access to the household resources was sufficient for them to prefer private provision of health care services for their own family.

Based on the assumptions that most rich households have income above the threshold $p\bar{y}$ and that rich women have sufficiently equal access to the household resources as their rich husbands, my simple model delivers the following predictions:

1. If the gender difference in voting is caused by women being poorer than men, extending the suffrage to both poor men and poor women increases public provision of health services, as both groups have ideal policies that include a higher k_m than what is ideal for rich men.

2. If the gender difference in voting is caused by women caring more for health provision to their own family than men, extending the suffrage to poor women increases the public provision of health care more than extending the suffrage to poor men.

3. If the gender difference in voting is caused by women caring more for health provision to the community, extending the suffrage to both rich and poor women increases the public provision of health care.

Note that it is only possible to identify whether women care more for health provision to the community if most rich households have income above the threshold $p\bar{y}$ and rich women have sufficiently equal access to the household resources as their rich husbands. If either of these assumptions fail, extending the suffrage to rich women will also increase the public provision of health services in the case where women care more for health provision to their own family.

An increase in the supply of public health services following the enfranchisement of poor women can either be due to poor women preferring a higher amount of public services than the already enfranchised groups, or that poor women increase the number of voters supporting an optimal amount they share with one of the already enfranchised groups. Both poor men and rich women already have the right to vote when poor women gain the right to vote. If the voting differences are caused by income differences, the inclusion of poor women will increase the support for the poor men's demand for higher supply of public health care. Similarly, if the differences in voting is caused by women caring more for health provision to the community, the inclusion of poor women will increase the support for rich women's demand for higher supply of public health care. Hence, it is not possible to identify empirically whether an increase in the supply of health services

[6] If the utility function U has a coefficient of relative risk aversion μ that is lower than unity, the predictions for male voters hold even if most existing male voters have income below the threshold for preferring private provision of health services. If $\mu < 1$, the private marginal cost in equation (3) is increasing in y, and higher incomes imply lower demand for k_m.

following the extension of the suffrage to poor women is due to the poor women caring more for the provision of health services to their own family then poor men.

3 Empirical strategy and data

3.1 Empirical strategy

I use a difference-in-differences (DiD) framework comparing the change in outcomes following the suffrage reforms in municipalities with high and low shares of new voters. The share of votes held by each voting group in a municipality is determined by a combination of election year and share of adults with household income above a certain threshold. The panel structure in the data allows me to eliminate time-invariant municipality characteristics and common time trends as potential sources of bias.

Since all municipalities are affected by both suffrage reforms, but to different degrees, I estimate effects of the two extensions separately. For the first reform, period 1 (1891-1900) is the pre-period and period 2 (1901-1909) is the treatment period. For the second reform, period 2 is the pre-period and period 3 (1910-1918) is the treatment period. For the first suffrage extension, and for each of the outcomes number of midwives, vaccinators, doctors, and mortality rates, I estimate the following specification:

$$k_{i,t+1} = \sum_{s=1891}^{1909} \tau_s (\text{PoorMale})_i \mathbf{1}[t = s] + \sum_{s=1891}^{1909} \lambda_s (\text{RichFemale})_i \mathbf{1}[t = s] + \delta_i + \delta_t + \epsilon_{it}, \quad (5)$$

where RichFemale_i and PoorMale_i measure the treatment intensity in terms of the shares of eligible voters in municipality i that are rich females and poor males, respectively, in the 1901 election; δ_i is a municipality fixed effect that captures time-invariant differences in outcomes across municipalities; and δ_t denotes year fixed effects. The error term ϵ_{it} includes unobserved transitory determinants of the outcomes. The regressions are weighted by population size to account for heteroscedasticity by population.[7] The two largest cities (Oslo and Bergen) would receive disproportionate weight and are hence excluded from the sample (Bailey and Goodman-Bacon, 2015).

The coefficients of interest τ_s and λ_s capture the impacts of the share of eligible voters that are rich females and poor males, respectively, in the 1901 election in year s. To summarize the estimates, I also construct single estimates of the impacts of the different voting groups by replacing the year-specific indicators $\mathbf{1}[t = s]$ and the corresponding coefficients τ_s and λ_s with a single post-treatment indicator $\mathbf{1}[t > 1900]$ and two single coefficients τ^{post} and λ^{post}.

[7] The size of the point estimates does not depend on the weights.

For the second reform, I estimate a corresponding DID specification with the share of poor female voters as the treatment variable, centering the data around the year of the second reform (1910):

$$k_{i,t+1} = \sum_{s=1901}^{1918} \gamma_s (\text{PoorFemale})_i \mathbf{1}[t = s] + \delta_i + \delta_t + \epsilon_{it}, \tag{6}$$

where PoorFemale_i states the shares of eligible voters that are poor females in the 1910 election in municipality i. To summarize the estimates, the year-specific indicators $\mathbf{1}[t = s]$ and the corresponding coefficients γ_s is replaced with a single post-treatment indicator $\mathbf{1}[t > 1909]$ and a single coefficient γ^{post}.[8]

In the main specifications, the treatment intensities RichFemale_i, PoorMale_i, and PoorFemale_i are simply defined as the share of eligible voters belonging to a specific group in the first election in which the group has the right to vote. However, it may be that effects are not linear in the share of votes that the group holds. In the robustness analysis, I therefore define the treatment intensity in two alternative ways, to verify that the results are robust to changes in the definitions of the treatment intensities.

3.1.1 Threats to identification

The year fixed effects capture unobserved differences across years, while the municipality fixed effects capture time-invariant unobserved differences between municipalities. The identifying assumption is that the growth rate in health personnel would have been the same across municipalities if they had not been differently impacted by the suffrage extensions.

A general problem when using suffrage extensions as the source of variation is that the countries/municipalities that choose to extend the suffrage differs from the countries/municipalities that do not extend the suffrage (Aidt and Dallal, 2008). This is not a concern in my case, as the municipalities could not impact the suffrage rules. Instead, it is a concern that municipalities with high and low shares of new voters may differ in a systematic and time-variant way in absence of the reform. Since the new voters are poorer than the old voters, municipalities with a high share of new voters may on average be poorer than municipalities with a low share of new voters. Hence, a potential concern is that absent the reform, the trends in the number of health personnel and health outcomes in poorer municipalities would be different from the trends in richer municipalities. Two measures are taken to address this concern. First, municipality-specific linear time trends are included to differentiate between the causal effect of expanding the suffrage

[8] The effect of extending the suffrage to poor women might be different in municipalities with different compositions of the three other voting groups. For example, poor women might have a different impact in a municipality where rich women have a large share of the votes than in a municipality where poor men have a large share of the votes. The estimates from equation (6) captures the effects for the average municipality.

and differences in secular trends. Including the period after the suffrage reform when estimating the set of municipality-specific time trends might pick up some of the treatment effect and lead to biased estimates (Wolfers, 2006). To avoid this bias, I follow Bhuller et al. (2013) in using only the pre-period to estimate the municipality time trends. The time trends are estimated separately for each outcome variable. When municipality specific time trends are included, the identifying assumption is that the deviation from the growth rate in health personnel does not vary systematically with the change in eligible voters.

Second, since income predicts the share of new voters, I follow Bütikofer et al. (2019) and include taxable income per capita as a control variable. Several changes, such as industrialization, may have affected the counterfactual trends differently in rich and poor municipalities without yielding a pre-trend. Including the yearly income alleviates this problem if the changes affect the mean income in the municipality. However, the extension of the suffrage might impact the income in a municipality. Thus, income might be endogenous. Results both with and without mean income as a control are therefore presented.

3.2 Data

I use a variety of historical sources recorded by Statistics Norway. These data sources include statistics related to municipality elections, population size, health personnel, mortality rates, and income. Tables 1 and 2 present descriptive statistics for the data, separately for the three time periods used in the analysis.

Share of eligible voters for each voting group

Vote shares are constructed using data for municipality elections. Since the municipality election in 1898, Statistics Norway has published statistics for the number of eligible voters and the number of voters in each municipality.[9] The election statistics differentiate the voters by gender, but not by income. I therefore use the difference between the number of eligible voters before and after the suffrage is extended to the poor to identify the number of poor voters.

For men, I take the difference between the number of eligible voters in 1898 and the number of eligible male voters in 1901 to identify the number of poor males. For women, I take the difference between the number of eligible female voters in 1907 and the number of eligible female voters in 1910 to identify the number of poor women. Once I have identified the number of poor voters, I calculate the share of eligible voters that are rich

[9]This data is digitalised and made available by Norsk samfunnsvitenskapelig datatjenestes (NSD). NSD is not responsible for any analyses or interpretations of the data.

women, poor women, rich men, and poor men, respectively.[10]

The method above only defines the share of votes for the first election where the poor have the right to vote. Any change in the vote shares of the different groups between the reforms is caused by changes in the income level and therefore possibly endogenous to the reform. Hence, I hold the share of votes belonging to each voting group constant for all elections between the reforms.

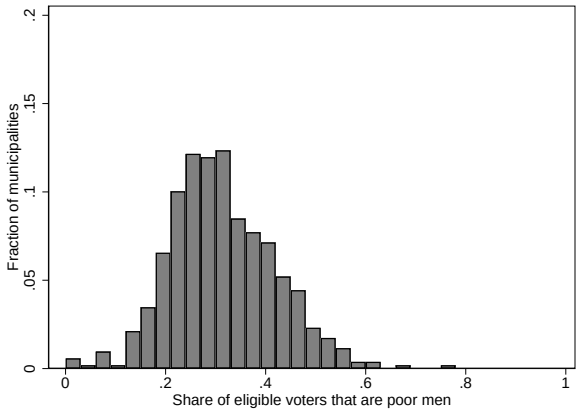
The first four rows in Table 1 show the vote shares held by each of the four voting groups in each of the three time-periods. The enfranchised voting groups were relatively equal in size at the national level. In the second time-period, rich men, rich women and poor men have about one third of the votes each and in the third period, the four voting groups had about one fourth of the votes each.

Figures 1a and 1b show the spread in the vote shares held by poor men and rich women in 1910, the first year they have the right to vote. We can see that the variation across municipalities in the share of eligible poor men is larger than the variation in the share of rich women. Figure 1c shows the variation in the share of eligible voters that are poor women in 1910, the first year they have the right to vote. The distribution is shifted to the left relative to the two previous distributions, since the voting groups are of similar size and all are now enfranchised.

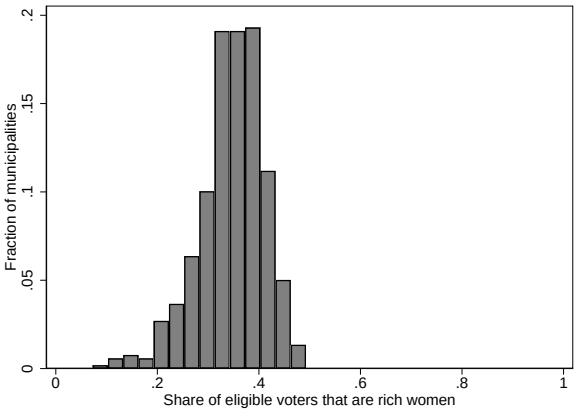
A potential problem with my identification strategy is the correlation between the voting groups. In the first reform, women can qualify for suffrage either by their own income or by the income of their husband. This creates a potential negative correlation between the share of rich women and poor men in a municipality. If the correlation is sufficiently high, I can not identify whether a potential effect from rich women is due to high representation of rich women or to low representation of poor men. In 1901, the correlation between the share of voters that are poor men and the share of voters that are rich women is -0.57. Figure 2 shows this graphically. It is evident that municipalities with a higher share of rich female voters have a lower share of poor male voters. However, for most levels of the share of voters who are rich women, there are municipalities with different shares of voters who are poor males.

The representation of a group is measured in terms of the share of eligible voters belonging to that group, not the share of votes casted by that group. The importance of this distinction depends on the difference in voter participation between the different groups within the same municipality. According to Larsen and Øksendal (2013), the

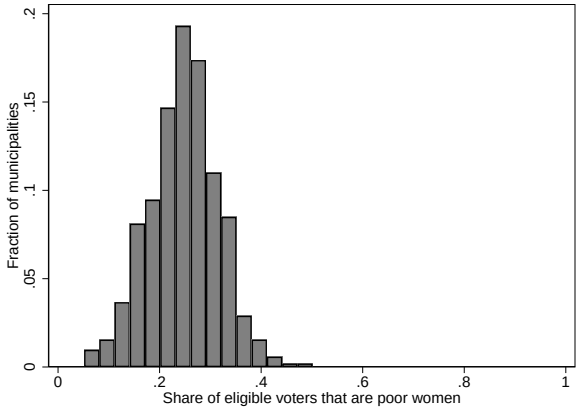
[10]The way the share of voters belonging to each group is identified introduces two potential measurement errors. A growth in the population above 25 years of age between the two elections and no change in the income level, leads to an overestimation of the number of poor men and women. Moreover, an income growth between the two elections overestimates the share of poor voters. I argue that the empirical strategy takes care of these concerns. First, any general population and income trends are accounted for by the year fixed effects. Second, any correlation between the treatment intensity and the pre-tend in outcomes, stemming from for example strategic migration, will be detectable in the specification with yearly treatment dummies.



(a) Poor men, 1901



(b) Rich women, 1901



(c) Poor women, 1910

Figure 1: Variation in shares of votes

Notes: This figure shows the spread across municipalities in the share of votes held by the new voting groups in the first election they have the right to vote. Figures 1a and 1b show the spread for poor men and rich women in the 1901 election, respectively. And Figure 1c shows spread for poor women in the 1910 election

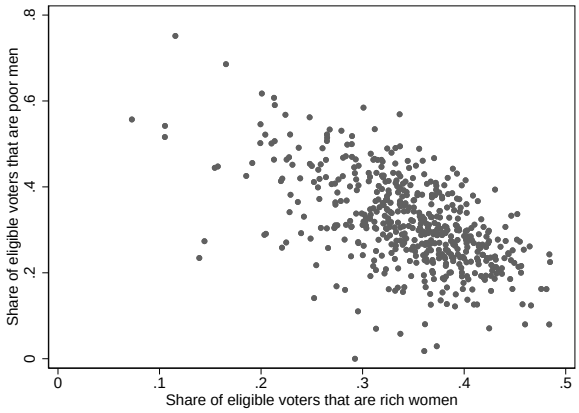


Figure 2: Correlation between poor men and rich women

Notes: This figure plots the share of voters that are poor men and the share of voters that are rich women in each municipality in the 1901 municipality election. The correlation between the share of voters that are poor men and the share of voters that are rich women is -0.57.

voter participation did not differ greatly across the voting groups within municipalities.

Health personnel

The second panel in Table 1 reports the average number of health personnel measured per 1000 inhabitants. The population size is linearly interpolated between the four censuses available in the time period; 1891, 1900, 1910, and 1920. The number of health personnel is reported in annual medical reports from district physicians.[11] The medical reports are standardized across space and fairly stable over time (Schønsby, 2005). Both private practitioners and publicly employed health personnel are recorded and it is not possible to separate between the two groups. The inability to separate public and private providers is only a problem for the identification if the number of private practitioners changes systematically with the share of new voters.

From Table 1 we can see that there was a large increase in both the density of midwives and the density of doctors from the time period when only rich men had the right to vote to the time period when also rich women and poor men had the right to vote. Whether this large increase is driven by the extension of the suffrage will be investigated in the next section. We do not see the same increase to the third period. For the density of vaccinators, we see a modest increase between each period.

The last row in Table 1 shows income per capita in the municipalities. The total taxable income in the municipalities is taken from annual statistics of income and wealth tax published by Statistics Norway (Kvile, 2017). Table 1 shows that there was a large income growth in the period I am analyzing.

[11] The data is digitalized by Kotsadam et al. (2002).

Table 1: Vote shares, health personnel, and income

	Municipality level means		
	1891-1900	1901-1909	1910-1918
Percent of eligible voters			
Rich men	100	32.7	24.9
	(0)	(7.95)	(6.36)
Poor men	0	31.1	23.1
	(0)	(10.3)	(6.98)
Rich women	0	36.0	28.4
	(0)	(6.46)	(6.64)
Poor women	0	0	23.4
	(0)	(0)	(6.61)
Health personnel measured per 1000 inhabitants			
Midwives	0.48	0.57	0.58
	(0.28)	(0.30)	(0.33)
Doctors	0.32	0.36	0.35
	(0.33)	(0.33)	(0.32)
Vaccinators	0.36	0.37	0.38
	(0.32)	(0.32)	(0.34)
Income			
Income per capita	134.7	190.0	440.8
	(138.9)	(95.9)	(978.1)
Observations	5189	4671	4671

Notes: Standard deviations in parentheses. Income is measured in nominal Norwegian Kroner (NOK).

Mortality rates

The development of mortality rates is reported separately in Table 2, as the data for mortality rates are available at the medical district level only. A medical district contained one or more municipalities. The data stem from the same annual medical publications as the data for health personnel. Infant and maternal mortality are measured per 1000 newborn. Table 1 shows a strong decline in the infant and maternal mortality rates throughout the period.

To study effects on mortality, I must aggregate the municipality level electoral data to medical districts. The electoral data is aggregated by taking the weighted average of the municipalities in a medical district, where the weights are the share of total number of eligible voters in the medical district belonging to each municipality.

Both the borders of the medical districts and the municipalities change over time. Following Kotsadam et al. (2002), I collapse the municipalities into units that are stable over time and use the time stable medical districts that they have constructed.

Table 2: Mortality rates

	Medical District level means		
	1891-1900	1901-1909	1910-1918
Infant mortality	86.2	68.3	60.0
per 1000 newborn	(25.3)	(24.9)	(19.7)
Maternal mortality	3.87	3.09	2.52
per 1000 newborn	(3.70)	(3.45)	(3.10)
Observations	1030	927	927

Notes: Standard deviations in parentheses.

4 Empirical results

Panel A in Table 3 presents the DiD estimates τ^{post} and λ^{post} from the specification in equation (5), and Panel B presents γ^{post} from equation (6). The vote shares are measured in percent of the total number of eligible voters. The estimated treatment coefficients can therefore be interpreted as the increase in number of health personnel per 1000 inhabitants when the representation of a group increases with one percentage point. Figure 3 plots the yearly DiD estimates τ_s, λ_s, and γ_s from equations (5) and (6) for the voting groups and outcomes for which the single treatment coefficients presented in Table 3 suggest a significant effect. For both reforms the estimates are normalized to be relative to the year before the reform.

Figures 3a and 3b show that higher representation of rich women and poor men leads to higher supply of doctors. Reassuringly, I find no evidence of significant differences in pre-trends neither between municipalities with high and low shares of rich females, nor between municipalities with high and low shares of poor males. In the years before the reform the estimates are relatively small and not statistically different from zero at conventional levels. The similarities in pre-trends support the identification assumption, and indicate that the growth in the number of doctors in municipalities with a low share of rich female voters may provide a valid counterfactual for the growth in the number of doctors in municipalities with a high share of rich female voters, and similarly for municipalities with different shares of poor males. As a result, the post-treatment estimates have a causal interpretation as the impacts of rich female and poor male voters.

On average, a ten percentage points increase in the share of votes held by poor men increases the number of doctors with close to .02 doctors per 1000 inhabitants. This corresponds to about 5 percent of the average physician density in the time period. The increase following more representation to rich women is larger in magnitude but not significantly different from the effect of more representation of poor men.

Table 3 suggests that the increased supply of doctors in municipalities with a high share of rich female voters translates into lower mortality rates for infants. However, the

yearly estimates in Figure 3c are imprecise, and the estimates in the period after the reform are volatile. Together with the findings regarding the aggregation to medical districts in Section 4.1, this indicates that the results for mortality rates must be interpreted with caution.

Figures 3d and 3e show that the poor female voters' impact on the number of vaccinators and infant mortality presented in Table 3 is driven by differences in pre-trends. In the years before the reform the estimates are considerable in size and significantly different from zero at the 5 percent significance level for some years. The differences in pre-trends violates the identification assumption, and indicate that the growth in the number of vaccinators in municipalities with a low share of poor female voters do not provide a valid counterfactual for the growth in the number of vaccinators in municipalities with a high share of poor female voters, and similarly for the change in infant mortality.

Tables B.1 and B.2 in Appendix B show the specifications with municipality-specific time trends and municipality mean income as additional control variables. Both rich women's and poor men's effect on the supply of doctors are significant and stable across specifications.

4.1 Sensitivity analysis

Definition of treatment

It is plausible that the impact of a certain group is not linear in the share of votes held by the group. To investigate this, I test two other specifications. The results for these specifications are shown in Tables B.3 and B.4 in Appendix Section B. In both specifications, I order the municipalities according to the share of voters that belong to a certain group at the time of enfranchisement for the group. In the first specification, I separate the sample at the median, letting the upper half constitute the treated municipalities and the lower half the comparison municipalities. Redefining the treatment variable does not reveal different results and does not change the signs of the estimated effects. In the second specification, I restrict the sample to include only the municipalities above the 60th percentile in the treatment group and only those below the 40th percentile in the comparison group. The reduction in sample size reduces the precision of the estimates, and the effects of poor males and rich females on the supply of doctors are similar in magnitude but no longer significantly different from zero.

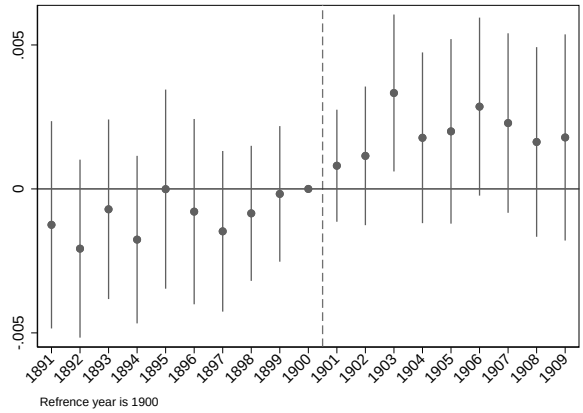
Aggregation to medical districts

The results for mortality rates must be interpreted with caution as the electoral data is aggregated to the medical district level due to data limitations. The electoral data is aggregated by taking the weighted average of the vote shares in the municipalities in a

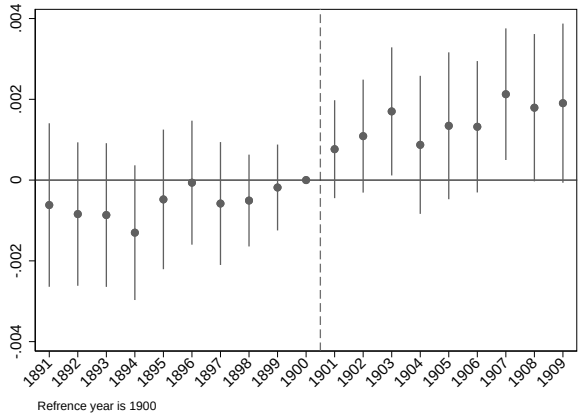
Table 3: The voting groups' effects on health personnel and mortality

	Midwives	Vaccinators	Doctors	Infant death	Maternal death
Panel A					
Poor men	.00128	.000889	.00194***	-.273	-.034
	(.00099)	(.00091)	(.0007)	(.18)	(.034)
Rich women	-.000185	-.0000923	.00284**	-.933**	.00918
	(.0019)	(.0015)	(.0013)	(.37)	(.056)
Mean dep. var	.521	.365	.339	76.1	3.37
No. of obs.	9850	9850	9850	1957	1957
Panel B					
Poor women	.00225*	.00357***	.000502	-.288**	-.00803
	(.0014)	(.00073)	(.00076)	(.13)	(.022)
Mean dep. var	.575	.374	.358	63.1	2.72
No. of obs.	9324	9324	9324	1853	1853

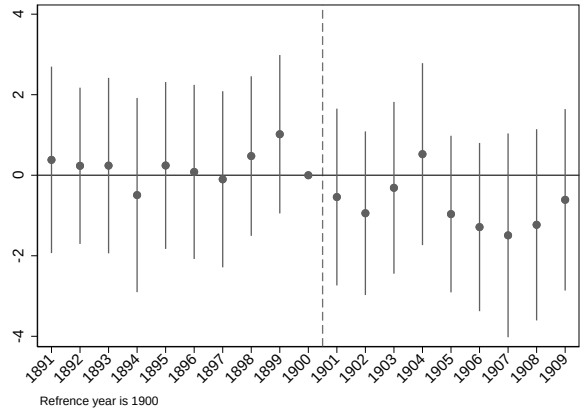
Notes: Panel A in this table reports estimated effects of rich women and poor men gaining the right to vote from a version of equation (5) where the year-specific indicators $\mathbf{1}[t = s]$ and the corresponding coefficients τ_s and λ_s are replaced with a single post-treatment indicator $\mathbf{1}[t > 1900]$ and two single coefficients τ^{post} and λ^{post}. Panel B reports estimated effects of poor women gaining the right to vote from a version of equation (6) where the year-specific indicators $\mathbf{1}[t = s]$ and the corresponding coefficients γ_s are replaced with a single post-treatment indicator $\mathbf{1}[t > 1909]$ and a single coefficient γ^{post}. The vote shares are measured in percent of total number of eligible voters, hence the coefficients can be interpreted as the increase in number of health personnel per 1000 inhabitants when the representation of a group increases with one percentage point. Standard errors are reported in parentheses and clustered at the municipality level. *p<0.1, **p<0.05, ***p<0.01.



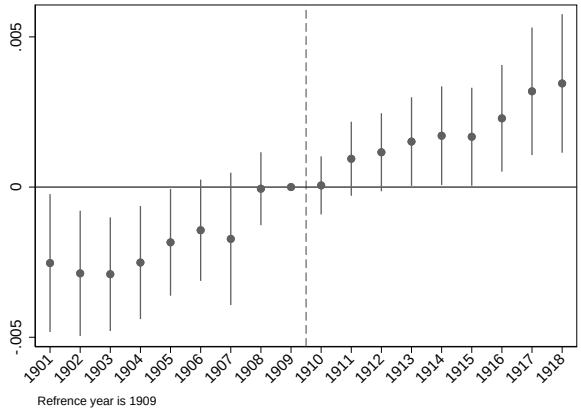
(a) Rich females' effect on doctors



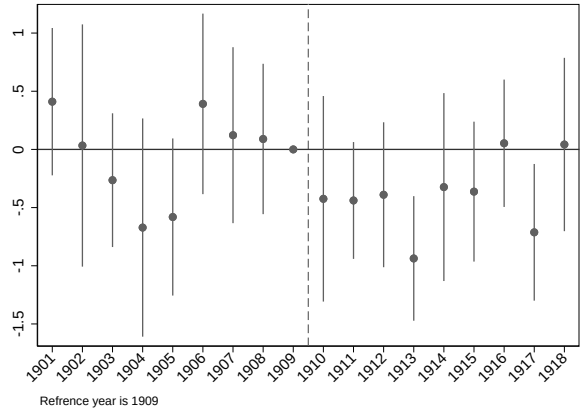
(b) Poor males' effect on doctors



(c) Rich females' effect on infant mortality



(d) Poor females' effect on vaccinators



(e) Poor males' effect on infant mortality

Figure 3: Yearly estimates of the voting groups' effects on health personnel and mortality

Notes: This figure plots the yearly DiD estimates from equations (5) and (6) for the voting groups and outcomes for which the single treatment coefficients presented in Table 3 suggest a significant effect. Figures 3a and 3b plot the estimated τ_s and λ_s coefficients (along with 95% confidence intervals) from the specification in equation (5) with number of doctors as outcome. Figure 3c plots the estimated τ_s coefficients from the specification in equation (5) with infant mortality as outcome. Figures 3d and 3e plot the estimated γ_s coefficients from the specification in equation (6) with number of vaccinators and infant mortality as outcomes, respectively.

medical district, using the share of total eligible voters belonging to each municipality as weights. To investigate whether the aggregation affects the results, I estimate effects on health personnel at the medical district level. Results are reported in Table B.5 in Appendix Section B. The aggregation changes both the size and the precision of the estimates compared to the estimates presented in Table 3. This implies that we might have seen other results if we had investigated the effect on mortality rates at the municipality level. The effects on mortality rates should therefore be interpreted with caution.

5 Conclusion

In this paper I have studied whether the difference in voting between men and women found in previous literature can be explained by income alone or if the difference in voting is an expression of gender specific preferences. To differentiate between the effect from gender and the effect from income, I exploit that the timing of enfranchisement in Norway depended on a combination of gender and household income. The income threshold for enfranchisement and the timing of each suffrage reform was equal for all municipalities in the country. Due to variation in income levels between municipalities, there is variation in the share of new voters after each reform. I use this variation to identify the difference in voting behavior between rich men, rich women, poor men, and poor women.

My results are consistent with a hypotheses about gender specific preferences for public goods provision. The results suggest that rich women demand a higher supply of doctors than rich men do. Since both rich women and rich men have access to private doctors, the increase in supply driven by the enfranchisement of rich women indicates that the rich women care more than rich men about other people's access to health services. The enfranchisement of poor men alters the supply of doctors relative to when only rich men had the right to vote in a similar way as the enfranchisement of rich women. This suggests that income, and accessibility to private alternatives, impacts the demand for public provision of health personnel. There are some indications of increased supply of vaccinators following the enfranchisement of poor women. This can be interpreted as gender specific preferences for health provision to one's family, as poor males already had the right to vote. However, the specification with yearly treatment dummies suggests that this effect is driven by pre-existing time trends that are dependent on the treatment intensity.

The results above are interpreted under the assumptions that rich women have sufficiently equal access to the household resources as their rich husbands, and that it is cheaper for most rich households to buy health services in the private market than to pay for public provision through taxes. If either of these assumptions fail, extending the suffrage to rich women will also increase the public provision of health services in the case where women care more for health provision to their own family. That the strong resis-

tance against extending the suffrage among rich men was rooted in the fear of increased public spending indicates that most of the rich men had income above the threshold for preferring private provision of health care to their own family. Similarly, the fact that rich women was granted the right to vote to compensate for the inevitable extension of the suffrage to poor men, suggests that rich women had sufficient access to the household resources to preferring private provision of health care services for their own family.

I have compared the effects of different voting groups on the provision of health services to infer the cause of gender differences in voting. It is only possible to detect all potential causes if the different voting groups have the same opportunity to affect the provision of health services when they gain the right to vote. Poor women were more restricted in their possibility to affect the provision of health care than rich women and poor men. First, they were the last to gain the right to vote. And since the four voting groups were of approximately the same size, the poor women had on average a smaller share of the votes when introduced to suffrage than rich women and poor men had. Second, when the suffrage was extended to poor women, the municipalities' right to collect taxes had been restricted. If poor women had not been more restricted than rich women and poor men, we might have seen an increase in other types of health personnel as well following the enfranchisement of poor women. This does not affect the results presented, but implies that we might not have seen the full effect of gender specific preferences for health services to one's own family.

I have not investigated the mechanisms that link the suffrage expansions and the increased supply of health services. A possible mechanism can be built around how the new voters vote for different parties or candidates. This could be explored by looking at changes in the party composition of the municipal councils. To my knowledge, data for such an analysis are not available.[12] However, I can account for changes in the gender composition in each municipal council. The qualification for being eligible for the municipal council coincided with the qualification for suffrage. Hence, the 1901 election is the first election where a woman can be elected as a representative. In municipalities where women makes up a larger share of the eligible voters, women also captured a larger share of seats in the municipal councils. However, this does not explain the pattern I have demonstrated.

In line with Miller (2008), I find a reduction in infant mortality following the introduction of female suffrage. However, these results must be interpreted with caution. Information on mortality is only available for medical districts, an administrative level that includes multiple municipalities. Sensitivity analyses show that aggregating the data to medical districts significantly changes the results for the supply of health personnel. Hence, to conclude regarding the effect on mortality, municipality specific mortality data

[12]Data on party composition are available for most municipalities for the 1901 election. For later elections in the analysed time period, data on party composition is only available for the larger cities.

are required.

References

Abrams, B. A. and R. F. Settle (1999, Sep). Women's suffrage and the growth of the welfare state. *Public Choice 100*(3), 289–300.

Acemoglu, D., S. Naidu, P. Restrepo, and J. A. Robinson (2015). *Handbook of Income Distribution, Volume 2*, Chapter Democracy, Redistribution, and Inequality, pp. pp. 1885–1966. Elsevier B.V.

Acemoglu, D. and J. A. Robinson (2000). Why did the west extend the franchise? Democracy, inequality, and growth in historical perspective. *The Quarterly Journal of Economics 115*(4), 1167–1199.

Aidt, T., J. Dutta, and E. Loukoianova (2006). Democracy comes to Europe: Franchise extension and fiscal outcomes 1830-1938. *European Economic Review 50*(2), 249 – 283.

Aidt, T. and P. Jensen (2009). Tax structure, size of government, and the extension of the voting franchise in Western Europe, 1860-1938. *International Tax and Public Finance 16*(3), 362–394.

Aidt, T. S. and B. Dallal (2008). Female voting power: The contribution of women's suffrage to the growth of social spending in Western Europe (1869-1960). *Public Choice*.

Bailey, M. J. and A. Goodman-Bacon (2015). The war on poverty's experiment in public medicine: Community health centers and the mortality of older Americans. *American Economic Review 105*(3), 1067–1104.

Bhuller, M., T. Havnes, E. Leuven, and M. Mogstad (2013). Broadband internet: An information superhighway to sex crime? *The Review of Economic Studies 80*(4), 1237–1266.

Brunner, E., S. L. Ross, and E. Washington (2011). Economics and policy preferences: Causal evidence of the impact of economic conditions on support for redistribution and other ballot proposals. *The Review of Economics and Statistics 93*(3), 888–906.

Bütikofer, A., K. V. Løken, and K. G. Salvanes (2019, 05). Infant Health Care and Long-Term Outcomes. *The Review of Economics and Statistics 101*(2), 341–354.

Doepke, M., M. Tertilt, and A. Voena (2012). The economics and politics of women's rights. *Annual Review of Economics 4*(1), 339–372.

Edlund, L., L. Haider, and R. Pande (2005). Unmarried parenthood and redistributive politics. *Journal of the European Economic Association 3*(1), 95–119.

Edlund, L. and R. Pande (2002). Why have women become left-wing? The political gender gap and the decline in marriage. *The Quarterly Journal of Economics*.

Flo, Y. and J. Aars (2010). Frå politiske rettar til politisk makt.

Fujiwara, T. (2015). Voting technology, political responsiveness, and infant health: Evidence from Brazil. *Econometrica 83*(2), 423–464.

Funk, P., C. Gathmann, C. Fumagalli, and J. Pijoan-Mas (2015). Gender gaps in policy making: evidence from direct democracy in switzerland. *Economic Policy 30*(81), 141–181.

Kotsadam, A., J. Modalsli, and J. T. Lind (2002). Call the midwife. health personnel and mortality in Norway 18871920. *Cliometrica 16*, 243276.

Kvile, J. (2017). The evolution of regional income inequality and social welfare in Norway: 1875-2015. *Master thesis, Department of Economics, University of Oslo*.

Larsen, E. (2013). *Stemmerett til alle!*, Chapter Norsk likestillingshistorie 1814-2013, pp. 159–218.

Larsen, E. and L. F. M. Øksendal (2013). De glemte kvinnevalgene. *Historisk Tidsskrift. 92(4)*, 563– 590.

Lindert, P. (1994). The rise of social spending, 1880-1930. *Explorations in Economic History 31*(1), 1–37.

Lønnå, E. (2017, 04). Stemmerett for kvinner i Norge. Store Norske Leksikon.

Lott, J. J. and L. Kenny (1999). Did women's suffrage change the size and scope of government? *Journal of Political Economy 107*(6), 1163–1198.

Meltzer, A. H. and S. F. Richard (1981). A rational theory of the size of government. *Journal of Political Economy 89*(5), 914–927.

Miller, G. (2008). Women's Suffrage, Political Responsiveness, and Child Survival in American History. *The Quarterly Journal of Economics 123*(3), 1287–1327.

Mulligan, C. B., R. Gil, and X. S. i Martin (2004). Do democracies have different public policies than nondemocracies? *The Journal of Economic Perspectives 18*(1), 51–74.

Schiøtz, A. (2003). *Folkets helse - landets styrke*, Volume vol. 2 of *Det offentlige helsevesen i Norge 1603-3003*. Universitetsforlaget.

Schønsby, H. P. (2005). The medical reports from the district physicians - an indispensable source to the Norwegian medical past. *Michael*.

Søbye, E. (2017a, 02). Stemmerettens historie i Norge. *Store Norske Leksikon* *https://snl.no/Stemmerettens_historie_i_Norge*.

Søbye, E. (2017b, 03). Stortingsvalgenes historie. *Store Norske Leksikon* *https://snl.no/Stortingsvalgenes_historie*.

Wolfers, J. (2006). Did unilateral divorce laws raise divorce rates? A reconciliation and new results. *American Economic Review 96*(5), 1802–1820.

A Expansions of the right to vote in parliament elections

The expansion of the right to vote in parliament elections followed the same pattern as for municipality elections. For men, the right to vote was expanded in 1885 to men who paid taxes on income over 800 NOK in urban areas and 500 NOK in rural areas. In 1900, the general suffrage was introduced for men (Søbye, 2017a). The first women got the right to vote in parliament elections in 1907 and the general right to vote was introduced in 1913 (Lønnå, 2017).[13] The same rules for suspension of the right to vote applied for parliament elections as for municipality elections. Before 1905, the election was a two-stage indirect system. First, the public voted on electors, and then these electors voted on parliament representatives. In 1905, a direct election system was introduced for parliament elections (Søbye, 2017b).

[13]The first expansion was in action from the parliament election in 1909 and the the second from the election in 1915 (Søbye, 2017a).

B Robustness analysis

Table B.1: The voting groups' effect on health personnel and mortality

	Midwives			Vaccinators			Doctors		
	(i)	(ii)	(iii)	(i)	(ii)	(iii)	(i)	(ii)	(iii)
Panel A									
Poor men	.00128	.00206**	.00206**	.000889	.00107	.00108	.00194***	.00158**	.00157**
	(.00099)	(.001)	(.00099)	(.00091)	(.00093)	(.00093)	(.0007)	(.00066)	(.00066)
Rich women	-.000185	.00121	.00122	-.0000923	.000489	.000545	.00284**	.00256**	.00252*
	(.0019)	(.0019)	(.0019)	(.0015)	(.0014)	(.0014)	(.0013)	(.0013)	(.0013)
Mean dep. var	.521	.521	.521	.365	.365	.365	.339	.339	.339
No. of obs.	9850	9850	9849	9850	9850	9849	9850	9850	9849
Panel B									
Poor women	.00225*	.000452	.000398	.00357***	.0023***	.00222***	.000502	.000501	.000424
	(.0014)	(.0012)	(.0012)	(.00073)	(.00064)	(.00064)	(.00076)	(.00081)	(.00084)
Mean dep. var	.575	.575	.575	.374	.374	.374	.358	.358	.358
No. of obs.	9324	9324	9324	9324	9324	9324	9324	9324	9324
Income	NO	NO	YES	NO	NO	YES	NO	NO	YES
Mun. trend	NO	YES	YES	NO	YES	YES	NO	YES	YES

Notes: Panel A in this table reports estimated effects of rich women and poor men gaining the right to vote from a version of equation (5) where the year-specific indicators $\mathbf{1}[t = s]$ and the corresponding coefficients τ_s and λ_s are replaced with a single post-treatment indicator $\mathbf{1}[t > 1900]$ and two single coefficients τ^{post} and λ^{post}. Panel B reports estimated effects of poor women gaining the right to vote from a version of equation (6) where the year-specific indicators $\mathbf{1}[t = s]$ and the corresponding coefficients γ_s are replaced with a single post-treatment indicator $\mathbf{1}[t > 1909]$ and a single coefficient γ^{post}. The columns labeled (i) is equivalent to the results shown in Table 3 and do neither include taxable income per capita nor municipality-specific linear time trends as control variables. The columns labeled (ii) include taxable income per capita as a control variable and the columns labeled (iii) include both taxable income per capita and municipality-specific linear time trends as control variables. The vote shares are measured in percent of total number of eligible voters, hence the coefficients can be interpreted as the increase in number of health personnel per 1000 inhabitants when the representation of a group increases with one percentage point. Standard errors are reported in parentheses and clustered at the municipality level. *p<0.1, **p<0.05, ***p<0.01.

Table B.2: The voting groups' effect on health personnel and mortality

	Infant mortality			Maternal mortality		
	(i)	(ii)	(iii)	(i)	(ii)	(iii)
Panel A						
Poor men	-.273	-.295	-.342*	-.034	-.00729	-.00359
	(.18)	(.18)	(.19)	(.034)	(.037)	(.038)
Rich women	-.933**	-.909**	-.963**	.00918	.055	.0584
	(.37)	(.38)	(.39)	(.056)	(.065)	(.066)
Mean dep. var	76.1	76.1	76.2	3.37	3.37	3.37
No. of obs.	1957	1957	1919	1957	1957	1919
Panel B						
Poor women	-.288**	-.238	-.323**	-.00803	.000696	-.000623
	(.13)	(.16)	(.15)	(.022)	(.022)	(.022)
Mean dep. var	63.1	63.1	63.2	2.72	2.72	2.73
No. of obs.	1853	1853	1817	1853	1853	1817
Income	NO	NO	YES	NO	NO	YES
Mun. trend	NO	YES	YES	NO	YES	YES

Notes: Panel A in this table reports estimated effects of rich women and poor men gaining the right to vote from a version of equation (5) where the year-specific indicators $\mathbf{1}[t = s]$ and the corresponding coefficients τ_s and λ_s are replaced with a single post-treatment indicator $\mathbf{1}[t > 1900]$ and two single coefficients τ^{post} and λ^{post}. Panel B reports estimated effects of poor women gaining the right to vote from a version of equation (6) where the year-specific indicators $\mathbf{1}[t = s]$ and the corresponding coefficients γ_s are replaced with a single post-treatment indicator $\mathbf{1}[t > 1909]$ and a single coefficient γ^{post}. The columns labeled (i) is equivalent to the results shown in Table 3 and do neither include taxable income per capita nor municipality-specific linear time trends as control variables. The columns labeled (ii) include taxable income per capita as a control variable and the columns labeled (iii) include both taxable income per capita and municipality-specific linear time trends as control variables. The vote shares are measured in percent of total number of eligible voters, hence the coefficients can be interpreted as the increase in number of health personnel per 1000 inhabitants when the representation of a group increases with one percentage point. Standard errors are reported in parentheses and clustered at the municipality level. *p<0.1, **p<0.05, ***p<0.01.

Table B.3: Main results - dummy 50 -50

	Midwives		Vaccinators		Doctors		Infant Mortality		Maternal Mortality	
	(i)	(ii)	(i)	(ii)	(i)	(ii)	(i)	(ii)	(i)	(ii)
Panel A										
Poor men	.0251	.0262	.00705	.00827	.0301**	.0227*	-1.25	-2.38	-.141	.279
	(.021)	(.02)	(.014)	(.014)	(.013)	(.013)	(2.9)	(3.1)	(.52)	(.56)
Rich women	-.0145	-.0126	-.0191	-.0134	.0256**	.0212*	-1.97	-2.53	.485	1.13*
	(.021)	(.019)	(.015)	(.015)	(.013)	(.013)	(2.9)	(3.2)	(.52)	(.59)
Mean dep. var	.521	.521	.365	.365	.339	.339	78.4	78.8	3.94	3.95
No. of obs.	9850	9849	9850	9849	9850	9849	1957	1919	1957	1919
Panel B										
Poor women	.0405*	.0147	.0449***	.0267***	.0148	.0101	-.911	-2.98	-.112	.267
	(.021)	(.019)	(.012)	(.0097)	(.012)	(.012)	(2.8)	(2.5)	(.43)	(.42)
Mean dep. var	.575	.575	.374	.374	.358	.358	64.8	64.9	3.4	3.43
No. of obs.	9324	9324	9324	9324	9324	9324	1853	1817	1853	1817
Income	NO	YES	NO	YES	NO	YES	NO	YES	NO	YES
Mun. trend	NO	YES	NO	YES	NO	YES	NO	YES	NO	YES

Notes: Panel A in this table reports estimated effects of rich women and poor men gaining the right to vote from a version of equation (5) where the year-specific indicators $\mathbf{1}[t = s]$ and the corresponding coefficients τ_s and λ_s are replaced with a single post-treatment indicator $\mathbf{1}[t > 1900]$ and two single coefficients τ^{post} and λ^{post}. Panel B reports estimated effects of poor women gaining the right to vote from a version of equation (6) where the year-specific indicators $\mathbf{1}[t = s]$ and the corresponding coefficients γ_s are replaced with a single post-treatment indicator $\mathbf{1}[t > 1909]$ and a single coefficient γ^{post}. The columns labeled (i) is equivalent to the results shown in Table 3 and do neither include taxable income per capita nor municipality-specific linear time trends as control variables. The columns labeled (ii) include taxable income per capita as a control variable and the columns labeled (iii) include both taxable income per capita and municipality-specific linear time trends as control variables. The treatment variables are constructed by ordering the municipalities according to the share of voters that belong to a certain group at the time of enfranchisement for the group and separating the sample at the median. The half of the municipalities with the highest shares of voters that belong to a certain group constitute the treated municipalities and the half of the municipalities with the lowest shares constitute the comparison municipalities. Standard errors are reported in parentheses and clustered at the municipality level. *p<0.1, **p<0.05, ***p<0.01.

Table B.4: Main results - dummy 40 -60

	Midwives		Vaccinators		Doctors		Infant Mortality		Maternal Mortality	
	(i)	(ii)	(i)	(ii)	(i)	(ii)	(i)	(ii)	(i)	(ii)
Panel A										
Poor men	.0322	.0393	.00106	.00236	.0314	.0275	-3.19	-4.91	.443	1.23
	(.03)	(.029)	(.021)	(.022)	(.019)	(.019)	(5.8)	(6.2)	(.93)	(.89)
Rich women	.00293	.0107	-.0189	-.0143	.0165	.0159	-3.6	-4.92	.683	1.81**
	(.03)	(.029)	(.022)	(.022)	(.019)	(.019)	(5.7)	(6.1)	(.85)	(.86)
Mean dep. var	.526	.526	.345	.345	.364	.364	81.2	81.7	4.05	4.07
No. of obs.	6392	6391	6392	6391	6392	6391	1273	1254	1273	1254
Panel B										
Poor women	.0444*	.0192	.0565***	.0315***	.0138	.00665	1.37	-2.06	-.39	.0456
	(.023)	(.022)	(.013)	(.011)	(.013)	(.014)	(2.8)	(2.6)	(.48)	(.47)
Mean dep. var	.564	.564	.374	.374	.361	.361	65	64.9	3.44	3.45
No. of obs.	7452	7452	7452	7452	7452	7452	1494	1476	1494	1476
Income	NO	YES	NO	YES	NO	YES	NO	YES	NO	YES
Mun. trend	NO	YES	NO	YES	NO	YES	NO	YES	NO	YES

Notes: Panel A in this table reports estimated effects of rich women and poor men gaining the right to vote from a version of equation (5) where the year-specific indicators $\mathbf{1}[t = s]$ and the corresponding coefficients τ_s and λ_s are replaced with a single post-treatment indicator $\mathbf{1}[t > 1900]$ and two single coefficients τ^{post} and λ^{post}. Panel B reports estimated effects of poor women gaining the right to vote from a version of equation (6) where the year-specific indicators $\mathbf{1}[t = s]$ and the corresponding coefficients γ_s are replaced with a single post-treatment indicator $\mathbf{1}[t > 1909]$ and a single coefficient γ^{post}. The columns labeled (i) is equivalent to the results shown in Table 3 and do neither include taxable income per capita nor municipality-specific linear time trends as control variables. The columns labeled (ii) include taxable income per capita as a control variable and the columns labeled (iii) include both taxable income per capita and municipality-specific linear time trends as control variables. The treatment variables are constructed by ordering the municipalities according to the share of voters that belong to a certain group at the time of enfranchisement for the group and defining the municipalities above the 60th percentile as the treated municipalities and the municipalities below the 40th percentile as the comparison municipalities. Standard errors are reported in parentheses and clustered at the municipality level. *p<0.1, **p<0.05, ***p<0.01.

Table B.5: The voting groups effects on health personnel and mortality measured at the medical district level

	Midwives		Vaccinators		Doctors	
	(i)	(ii)	(i)	(ii)	(i)	(ii)
Panel A						
Poor men	.00125	.00325*	.00556*	.00712***	.00149*	.00111
	(.0016)	(.0019)	(.0029)	(.0024)	(.00086)	(.00092)
Rich women	-.002	.000817	.00684	.00939**	.00454***	.00341*
	(.0035)	(.0041)	(.0054)	(.0041)	(.0016)	(.0019)
Mean dep. var	.582	.579	.484	.48	.326	.325
No. of obs.	1957	1919	1957	1919	1957	1919
Panel B						
Poor women	.00607**	.00431	.00635***	.00427**	.00236	.00207
	(.0027)	(.0034)	(.0019)	(.002)	(.0024)	(.0025)
Mean dep. var	.651	.647	.498	.492	.339	.339
No. of obs.	1854	1818	1854	1818	1854	1818
Income	NO	YES	NO	YES	NO	YES
Mun. trend	NO	YES	NO	YES	NO	YES

Notes: Panel A in this table reports estimated effects of rich women and poor men gaining the right to vote from a version of equation (5) where the year-specific indicators $\mathbf{1}[t = s]$ and the corresponding coefficients τ_s and λ_s are replaced with a single post-treatment indicator $\mathbf{1}[t > 1900]$ and two single coefficients τ^{post} and λ^{post}. Panel B reports estimated effects of poor women gaining the right to vote from a version of equation (6) where the year-specific indicators $\mathbf{1}[t = s]$ and the corresponding coefficients γ_s are replaced with a single post-treatment indicator $\mathbf{1}[t > 1909]$ and a single coefficient γ^{post}. The columns labeled (i) include neither taxable income per capita nor municipality-specific linear time trends as control variables. The columns labeled (ii) include both taxable income per capita and municipality-specific linear time trends as control variables. The vote shares are measured in percent of total number of eligible voters, hence the coefficients can be interpreted as the increase in number of health personnel per 1000 inhabitants when the representation of a group increases with one percentage point. The electoral data is aggregated by taking the weighted average of the vote shares in the municipalities in a medical district, using the share of total eligible voters belonging to each municipality as weights. Standard errors are reported in parentheses and clustered at the municipality level. *p<0.1, **p<0.05, ***p<0.01.