

# Democracy comes to Europe: Franchise extension and fiscal outcomes 1830–1938<sup>☆</sup>

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## Abstract

This paper examines the relation between public spending and the spread of democracy in Western Europe during the period 1830–1938. Our data set includes measures of the size of the electorate, the election rule, and electoral participation, as well as measures of the size and composition of central government expenditures for 12 countries. We estimate panel regressions, and find that (1) the gradual lifting of socio-economic restrictions on the voting franchise contributed to growth in government spending mainly by increasing spending on infrastructure and internal security; (2) the female suffrage had a weak positive effect, through spending on health, education and welfare; (3) the change from majority to proportional rule, which took place in 10 of the countries, did not contribute to growth in government spending, and held back spending on health, education and welfare; (4) there exists (weak) complementarity between economic development and the spread of democracy.

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## 1. Introduction

Democracy, today, means that all adult citizens have the right to vote in elections that determine the composition of their government. It was not always so; universal suffrage is less than a century old. Historically, relatively few European countries were democratic. Those that were often restricted the right to vote; to men only till the twentieth century, and before that, to men who owned property, or paid taxes, or possessed specific educational qualifications. As this suggests, the extension of the franchise was not an “all or nothing” decision, nor was it extended at the same time by a number of similar countries in Western Europe.

The purpose of this paper is to evaluate, and quantify, the relation between fiscal outcomes and the spread of democracy in Western Europe during the period 1830–1938. Our main interest lies in quantifying the impact of the franchise extension and the election rule (majority versus proportional rule) on the size and composition of central government spending. Economic theory suggests a number of different channels through which these changes in restrictions on political participation (Meltzer and Richard, 1981; Acemoglu and Robinson, 2000; Lizzeri and Persico, 2004; Boix, 2001) and in the election rule (Austen-Smith, 2000; Persson and Tabellini, 2000, Chapters 8 and 9; Lizzeri and Persico, 2001) came about and how they might have affected fiscal outcomes. To guide the empirical analysis we develop, in Section 3, four specific hypotheses regarding the link between the voting franchise and the election rule and fiscal outcomes.

Our sample covers 12 countries: Austria, Belgium, Denmark, Finland, France, Germany, Italy, the Netherlands, Norway, Sweden, Switzerland, and the United Kingdom. In these countries, the extension of the franchise happened in two stages. The first extension—the *economic franchise*—gradually lifted over 90 years property and income qualifications for male voters, and was completed in most countries by 1920. The second extension—the *female franchise*—allowed women to vote. This roughly doubled the number of citizens eligible to vote in one go, and took place in two waves; the first after World War I and the second after World War II. A lot of experimentation with different voting rules took place before 1920, and 10 of the countries in the sample changed their election rule from majority rule to proportional representation during the period. The period 1830–1938 saw substantial changes in government spending levels and patterns. While total spending remained fairly low for much of the period and did not start to increase as a fraction of GDP until after World War I, substantial changes in the composition of the government budget took place throughout. Crudely speaking, spending as a proportion of the budget shifted out of spending on defence, administration, the judiciary and policing into spending on public services, such as transport, communication and construction, and later on into spending on public provision of private goods, such as education and health, and, to a lesser extent and only at the end of the period, on welfare programs.

We have four main findings. First, the economic franchise contributed to growth in government spending during the nineteenth and early twentieth century mainly by increasing spending on roads, transportation and communication and internal

security and administration. Second, the female franchise had an impact on public finances mainly through an increase in spending on health, education, housing, redistribution and social insurance, but the effects are statistically weak. Third, we find that the election rule matters for fiscal outcomes, but in surprising ways. Countries with proportional representation as opposed to a majority rule system had lower overall spending, mainly because spending on health, education, housing, redistribution and social insurance. Taken together these results imply that the different reforms often had off-setting influences on fiscal outcome and that the net impact of these reforms on spending levels and patterns was modest, both in the short- and in the long-run. Fourth, we find evidence of complementarity between economic development and the voting franchise. In particular, the forces of economic development (rising incomes, urbanization and aging populations) have a larger positive impact on spending on roads, transportation, and communication and on health, education, housing, redistribution and social insurance in societies with a more extended voting franchise.

The rest of the paper is organized as follows. In Section 2, we present the main facts about public spending, the franchise extension, and other major political reforms in the 12 Western European countries. In Section 3, we develop four specific hypotheses regarding the impact of the extension of the voting franchise and the choice of election rule on fiscal outcomes. These hypotheses guide our statistical analysis. In Section 4, we report the results of the statistical analysis and relate our findings to previous studies. In Section 5, we provide some concluding remarks.

## 2. Elections and public spending: Some facts

Our data tracks the evolution of political institutions and fiscal outcomes in 12 Western European countries covering the period from about 1830 to 1938.<sup>1</sup> The time period covered for each country varies depending on when democratic institutions were adopted and, more often, on data availability. For Germany and Italy, we have excluded the periods with National Socialist (1933-) and Fascist regimes (1923-), respectively.<sup>2</sup> The data has been compiled from secondary sources. Data on fiscal outcomes is from Flora et al. (1983), Mitchell (1998), and Maddison (1991). Data on political institutions is from Flora et al. (1983), Cook and Paxton (1998), Carstairs (1980), and Marshall and Jagers (2000). The data and its sources are described in more detail in the data appendix and Table 13 reports the raw data for each country broken down by decade. We emphasize that historical data, in particular for the nineteenth century, is less reliable and less consistent over time and space than what

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<sup>1</sup>The 12 countries are Austria, Belgium, Denmark, Finland, France, Germany, Italy, the Netherlands, Norway, Sweden, Switzerland, and the United Kingdom.

<sup>2</sup>Norway was formally part of a union with Sweden until 1905, and Finland was a largely autonomous Grand Duchy of the Russian Empire until it gained full independence in 1917 (Carstairs, 1980, Chapters 9 and 11). Both countries had, however, wide-ranging fiscal autonomy in the period leading up to independence, and the two countries are treated as fiscally independent units throughout the whole period, which for Finland means 1901–1938 and for Norway means 1844–1938.

we have learned to expect from contemporaneous data. This should be kept in mind when interpreting the results that emerge from our analysis.

### 2.1. *The franchise extension and election procedures*

In 1831, Belgium had its first election, where 4.5% of its adult, male population had the right to vote, and 62% of these electors exercised that right. In 1971, Switzerland was the last Western European country to allow women to vote in national elections. In the intervening 140 years, Western European countries first imposed, and then abolished, a number of requirements that adult citizens needed to satisfy to earn the right to vote for their governments. At the same time, countries experimented with different voting procedures; making voting compulsory or not; direct versus indirect elections for the parliament; secret versus open ballots, and majority versus proportional rule. The purpose of this section is to describe some of these trends.

In the early days of democracy in Western Europe, the franchise was restricted to a subset of the population.<sup>3</sup> Some citizens were excluded from voting because they did not satisfy certain economic criteria (such as minimum income or minimum tax payment requirements, property holding requirements, or occupational requirements), because of their gender or ethnic or religions affiliation, or because of their age.<sup>4</sup> The movement towards universal suffrage took place in two stages, broadly speaking. During the second half of the nineteenth century, economic restrictions were gradually lifted and by 1920 all countries in our sample had universal male suffrage. The second stage gave women the right to vote. This happened in two waves, one after World War I and another after World War II. All these facts are summarized in Table 1.<sup>5</sup>

The journey to universal franchise can be quantified by the proportion of the population in each country that was allowed to vote in parliamentary elections. Flora et al. (1983) provide two useful measures of this. The first measure reports the percentage of the population of 20 and older that could cast their vote in parliamentary elections. This measure captures a combination of gender and socio-economic restrictions on the franchise, and we shall refer to it as the *total franchise*. The second measure reports the size of the electorate in percentage of its reference age group. Before the female suffrage, the reference group is all men of voting age and after, it is all citizens of voting age. Thus, it can be interpreted as a measure of the economic and social restrictions imposed on the franchise, and, for this reason, we shall refer to it as the *economic franchise*.

<sup>3</sup>Throughout the paper, we use the term franchise to refer to the right of an adult citizen to vote in elections to the national parliament, noting that it was not uncommon that some citizens were allowed to vote in local elections, but not in national ones.

<sup>4</sup>In contrast to America, literacy requirements were not imposed in Western Europe (Engerman et al., 1998).

<sup>5</sup>Carstairs (1980) contains a short and concise historical account of the evolution of electoral systems in each country.

Table 1  
The extension of the voting franchise in Western Europe

Country	Economic restrictions	Universal male franchise	Universal franchise
Finland	1869–1904	1907	1907
Denmark	1849–1915	1918	1918
Netherlands	1849–1897 <sup>a</sup>	1918 <sup>b</sup>	1922
Austria	1883–1901	1907	1919
Norway	1815–1900	1900 <sup>a</sup>	1915
Sweden	1866–1908	1919 <sup>a</sup>	1921
UK	1831–1910	1918 <sup>a</sup>	1928 <sup>a</sup>
France	1815–1848	1848	1945
Germany	Never	1848	1919
Italy	1861–1919	1919	1946
Belgium	1831–1892	1919 <sup>b</sup>	1948
Switzerland	Never	1848	1971

Source: Flora et al. (1983); Carstairs (1980).

Notes: <sup>a</sup> = No significant restrictions beyond this date, <sup>b</sup> = Compulsory voting, in Belgium 1919–1946, and in the Netherlands till 1970. Economic restrictions refer to minimum property or income, tax payment, occupation or other requirements associated with gaining the vote; Universal male franchise means that all males of voting ages were allowed to vote in parliamentary elections; Universal franchise means that all males and females of voting ages were allowed to vote in parliamentary elections. The years recorded refer to the first election in which universal or universal male franchise applied, not to the year in which the relevant legislation was introduced.

In Table 2, we report the average values for the total franchise ( $\bar{e}1$ ) and for the economic franchise ( $\bar{e}2$ ) for each country for three time periods. The trend towards universal franchise is clearly illustrated by the data: The jump in the total franchise in countries such as Denmark, Norway, Sweden and Germany after World War I captures womens' suffrage, while the more gradual change in the economic franchise observed in the period leading up to World War I illustrates that the economic restrictions imposed on the franchise were, in most countries, gradually lifted by a sequence of reforms (see Carstairs, 1980). In the interwar period, few socio-economic restrictions were left, reflecting the fact that universal male franchise had been reached by 1920. From a cross-country perspective, we notice the variation in both measures of democracy before 1914; a variation that was vastly reduced in the interwar period. This illustrates that the process of democratization in Western Europe by no means was a uniform process.

These two measures of democracy identify differences in restrictions on political participation across time and space, but provide little information about real political influence. In Germany before 1918, for example, the franchise was fairly wide, but the executive was largely unaccountable to the parliament and, thus, to voters, and in the country-side voting was to a large extent controlled by the landlords (Abrams, 1995). Similarly, in Denmark the executive branch of government was from 1870 onwards controlled by a small group of large landowners against the wishes of the majority of the parliament and against (the spirit of) the constitution. This arrangement basically blocked the voice of the enfranchised

Table 2

The size of the franchise, electoral turnout rates, and the polity index for the 12 countries, selected time periods

Country	$\bar{e}1$	$\bar{e}2$	$\bar{t}1$	$\bar{p}$	$\bar{e}1$	$\bar{e}2$	$\bar{t}1$	$\bar{p}$	$\bar{e}1$	$\bar{e}2$	$\bar{t}1$	$\bar{p}$
	Till 1880 (%)	(%)	(%)		1881–1914 (%)	(%)	(%)		1920–1938 (%)	(%)	(%)	
Finland	n.a.	n.a.	n.a.	n.a.	66.2 <sup>a</sup>	77.1 <sup>a</sup>	60.3 <sup>a</sup>	n.a.	74.4	85.6	61.2	7.1
Denmark	25.7	73.3	34.2	−0.1	29.2	84.8	63.4	−3	85.9	93.8	79.2	9.8
Netherlands	5.0	11.4	n.a.	−3	17.8	42.5	70.8	−2.2	82.5	97.1	91.9	10
Austria	n.a.	n.a.	n.a.	n.a.	38.0 <sup>a</sup>	94.5 <sup>a</sup>	n.a.	−4	90.6	90.6	87.8	3.2
Norway	8.8	21.8	46.2	−6.3	55.1	27.4	69.7	3.8	89.3	98.2	73.3	10
Sweden	10.2	22.3	19.6	−4	15.2	34.5	43.7	−4	79.5	93.6	62.8	10
UK	8.6	18.8	n.a.	2.5	26.4	57.2	60.9	7.4	80.4	94.9	68.6	10
France	19.1	39.8	75.3	−2.7	42.4	88.7	72.2	7.5	39.9	86.9	80.9	9.4
Germany	35.9	87.7	59.7	−4	37.6	92.6	73.1	−0.2	96.5 <sup>b</sup>	97.7 <sup>b</sup>	79.5 <sup>b</sup>	6 <sup>b</sup>
Italy	3.6	8.8	54.4	−4	35.0	16.4	59.8	−2.7	52.1 <sup>c</sup>	108.6 <sup>c</sup>	58.1 <sup>c</sup>	−1 <sup>c</sup>
Belgium	2.8	6.5	71.4	1.6	24.2	57.7	86.3	6	45.3	95.2	93.0	9.5
Switzerland	n.a.	n.a.	n.a.	n.a.	37.7	78.3	52.0	10	41.0	86.9	77.5	10
All countries	14.9	36.2	58.7	−1.5	64.2	30.1	64.9	2.1	93.4	74.0	77.3	7.3

Source: Flora et al. (1983).

Notes:  $\bar{e}1$  = average total franchise (electorate as % of population 20 and older);  $\bar{e}2$  = average economic franchise (electorate as % of enfranchised age and sex group);  $\bar{p}$  average score of the polity index (scale from −10 to 10);  $\bar{t}1$  = average turnout rate (fraction of those enfranchised turning out to vote); <sup>a</sup> means that data is available from 1907 onwards only; <sup>b</sup> indicates that data refers to the period before 1933; <sup>c</sup> indicates that the data refers to the period before 1924.

majority for more than 30 years. In contrast, in Belgium the franchise was fairly narrow till 1893, but the executive was accountable to the electorate. An attempt to quantify such differences has been made by Marshall and Jagers (2000) who provide a set of indicators of aspects of political competition and openness. Their main indicator, which we shall refer to as the *polity index*, measures on a scale from −10 to 10 the degree of democratic accountability by combining information on the degree of de facto political competition, the way the executive is recruited, the type of constraints imposed on the chief executive once in office and other similar aspects of the political environment. The average values of the polity index ( $\bar{p}$ ) are shown in Table 2. While the general trend toward democracy is clear and most countries score close to 10 in the interwar period, the data clearly demonstrates that having a wide franchise is not the same as having democratic accountability.

Extending the right to vote is no guarantee that this right will be used. The extent to which enfranchised citizens use their right or not can be measured by the proportion of the eligible voters that turns up to vote in each election. We report the average turnout rates ( $\bar{t}1$ ) in Table 2. We notice that the extension of the franchise was associated with an increase in participation rates in every Western European country. Yet, significant cross-country variation exists with very low turnouts in the

Scandinavian countries before 1880 and consistently high turnout rates in France and Belgium.

In many cases the franchise was extended at the same time as significant changes in voting procedures were introduced. In Table 3, we record some of these changes. In most countries, the open ballot was used throughout the nineteenth century, and voting took place by show of hands (e.g., in Denmark) or by oral declaration or acclamation.<sup>6</sup> The last country to introduce the secret ballot was Germany at the outset of the Weimar Republic. Whether voters elect their representatives directly or indirectly (by choosing a board of electors that appoints the representatives) is another important voting procedure that differed in nineteenth century Europe, but by 1909 all 12 countries had adopted direct elections for their parliaments. The most far-reaching institutional reform that took place during the period was the change from majority rule to proportional representation.<sup>7</sup> Initially, all 12 countries used some version of the majority rule, but, by the end of World War I, only two—the United Kingdom and France—were still using it. All the other Western European countries had changed to some version of proportional representation. The (time) variation in election rule during the nineteenth century stands in sharp contrast to the stability or institutional inertia observed in more mature democracies (Persson and Tabellini, 2003, Chapter 3).

## 2.2. *Public spending*

The universal, and explosive growth of government spending observed in industrial societies since World War II, and particularly since 1960, is a fact established beyond question, and can for a large part be attributed to growth in expenditures on transfers and subsidies (Tanzi and Schuknecht, 2000, Table II.4). The patterns of public spending in the period before 1938 is less well documented and the purpose of this section is to report some facts about public spending in Western Europe for this period.

We measure the size of government as central government spending out of GDP (or NNP). In countries with a federal structure (such as Switzerland and Germany), focussing on central rather than general government spending can significantly underestimate the size of government.<sup>8</sup> Unfortunately, data on general government spending and its components is hard to come by, and we have only been able to get data on (total) general government spending during the nineteenth century for six of

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<sup>6</sup>This is most likely one of the key factors behind the recorded cross-country differences in turnout rates in the nineteenth century.

<sup>7</sup>Majority rule in its purest form has two candidates running in each election district, and the candidate with the largest number of votes wins the seat. Proportional representation in its purest form treats the whole country as one district and each party gains representation according to its proportion of the total vote. In practice, there is a great deal of variation in the way the two systems are adopted.

<sup>8</sup>Even in countries without a federal structure, it was not uncommon that local government bodies provided a significant portion of total government spending. For example, in England and Wales during the period 1870–1914, Millward and Sheard (1995) show how *local* authorities undertook significant investments in infrastructure and social overhead accounting for as much as 90% of all public investments.

Table 3  
Voting procedures

Country	First election in <sup>a</sup>	Secret ballot from <sup>b</sup>	Direct elections from <sup>c</sup>	Election rule	
				MR till <sup>d</sup>	PR from <sup>e</sup>
Finland	1869	1907	1907	–1901	1907–
Denmark	1849	1901	1849	–1915	1919–
Netherlands	1849	1849	1849	–1917	1918–
Austria	1873	1907	1909	–1911	1919–
Norway	1815	1885	1906	–1918	1921–
Sweden	1866	1866	1909	–1917	1919–
UK	1831	1873	1813	–now	—
France	1815	1831	1824	–1936 <sup>f</sup>	1945 <sup>f</sup>
Germany	1848	1919	1871	–1918	1919–1933
Italy	1861	1861	1861	–1913	1919–1924
Belgium	1831	1877	1831	–1894	1899–
Switzerland	1848	1872	1848	–1918	1919–

Source: Flora et al. (1983); Carstairs (1980); Cook and Paxton (1998).

Notes: MR = majority rule; PR = proportional representation.

<sup>a</sup> indicates the year in which the first democratic election was held; <sup>b</sup> indicates the year in which the secret ballot was introduced, before that the ballot was open; <sup>c</sup> indicates the year in which direct elections of representatives to the parliament was introduced, before that voters elected representatives indirectly by appointing electors; <sup>d</sup> indicates the date of the last election at which MR was used; <sup>e</sup> indicates the date of the first election at which PR was used. <sup>f</sup> = France had a mixed MR and PR system 1919–1927; but MR only 1928–1945.

the countries (Denmark, Norway, France, Germany, United Kingdom, and Switzerland) in the sample.<sup>9</sup>

Table 4 shows average central ( $g/y$ ) and general ( $G/y$ ) government spending as percentage of GDP for selected time periods for each of the 12 countries. Among the big spenders of this era, we find Belgium, Italy, France and the United Kingdom, all with central government spending out of GDP well above the average, while the Scandinavian countries were small spenders throughout with central government spending below average. Switzerland and Germany had relatively low levels of central government spending, with most of the spending being decentralized to federal governmental bodies. The time series pattern is also clear: Modest growth in government spending or even, in some countries, a decline in central government spending during the period leading up to World War I, and, then, a large increase in spending during the interwar period. Central government spending out of GDP increased by 73 percent from an average around 8.3 percent to 14.4 percent, while general government spending out of GDP (in the six countries for which we got data) increased by as much as 83 percent, from an average around 12 percent to 21.6

<sup>9</sup>Comparable data on the composition of general government spending is not available, except for Germany (see Flora et al., 1983).



Table 4  
Central and general government spending as a proportion of GDP in the 12 countries before 1938

Country	Central government spending ( $g/y$ )			General government spending ( $G/y$ )		
	Till 1880 <sup>a</sup> (%)	1881–1913 (%)	1920–1938 (%)	Till 1880 (%)	1881–1913 (%)	1920–1938 (%)
Finland	n.a.	6.8 <sup>b</sup>	12.6	n.a.	n.a.	n.a.
Denmark	6.5	6.1	7.6	9.7	10.6	14.9
Netherlands	n.a.	8.6 <sup>c</sup>	15.9	n.a.	n.a.	n.a.
Austria	n.a.	n.a.	13.6	n.a.	n.a.	n.a.
Norway	3.9	6.8	7.9	6.2	8.9	17.8
Sweden	5.7	6.1	9.5	n.a.	n.a.	15.5
UK	8.5	8.1	19.9	10.3	12.1	25.3
France	12.2	11.6	19.6	13.5	14.3	29.1
Germany <sup>d</sup>	4.3	4.3	15.8	n.a.	13.8	28.9
Italy <sup>e</sup>	12.7	15.3	24.1	n.a.	n.a.	n.a.
Belgium	n.a.	13.9 <sup>f</sup>	23.2	n.a.	n.a.	n.a.
Switzerland	1.8	2.1	5.5	16.5	13.4	21.4
All countries	8.5	8.2	14.4	9.9	11.5	21.6

Source: Flora et al. (1983); Mitchell (1998).

Notes:  $g$  = total nominal central government spending;  $G$  is nominal general government spending;  $y$  = nominal GDP, where available (otherwise NNP); <sup>a</sup> the averages for central and general government, where applicable, refer to the same time period; <sup>b</sup> data available from 1901; <sup>c</sup> data available from 1900; <sup>d</sup> until 1933 only; <sup>e</sup> until 1923 only; <sup>f</sup> data available from 1912.

percent of GDP (Table 5, columns 2 and 3).<sup>10</sup> Thus, growth in government did not start happening until after World War I.

We measure the composition of central government spending—the public sector portfolio—by four categories of government expenditures:<sup>11</sup>

- The first category is called “security” ( $g_0$ ) and includes the basic services that governments provide, always and everywhere. This includes defence, and internal security, as well as the maintenance of law and order by the provision of a judicial system and public administration.
- The second category is called “long-term public services” ( $g_1$ ) and includes spending on roads, transportation, and communication. These services require long-term investments. Once provided, they are, in principle, available to all citizens. Yet, in practise, there might be an element of geographical redistribution, whereby certain geographical locations benefit disproportionately.

<sup>10</sup>Excluding Austria, Finland and Belgium on the ground that we only have data for the twentieth century does not significantly affect these calculations.

<sup>11</sup>In some cases, the four categories do not add up to total central government spending. The difference is due to interest payments, mainly on war-time debt, and a residual containing all the expenditures that could not be allocated to one of the four categories. See the data appendix.

Table 5

Central and general government spending as a proportion of GDP and the components of central government spending as a proportion of total spending, selected periods

Years	All countries					Scandinavian <sup>b</sup>	
	$g/y$ %	$G/y^a$	$g_0/g$	$g_1/g$	$g_{23}/g$	$g_2/g$ %	$g_3/g$
1850–1870	9.0	9.9	59.7	11.6	5.4	7.9	<1
1871–1890	7.7	10.1	50.0	12.7	8.6	12.0	<1
1891–1913	8.3	11.8	45.6	17.5	14.8	18.7	3.3
1920–1938	14.4	21.6	30.5	13.1	26.1	21.6	8.7
Till 1938	10.1	16.7	42.6	13.9	17.5	16.6	5.1

Source: Flora et al. (1983) and Mitchell (1998).

Notes: <sup>a</sup> data refers to Norway, France, Germany, Sweden, Denmark and United Kingdom; <sup>b</sup> Denmark, Norway, Finland and Sweden.  $g$ ,  $G$  and  $y$  are defined in the notes to Table 4;  $g_0$  is central government spending on security;  $g_1$  is central government spending on long-term public services;  $g_2$  is central government spending on collective goods;  $g_3$  is central government spending on transfers; and  $g_{23} = g_2 + g_3$ .

- The third category is called “collective goods” ( $g_2$ ) and includes expenditures on health, education, and housing. These are private goods, though publicly provided. Public provision often coexists with private provision—the fact that the state pays for subsidized housing or hospitals does not make it compulsory for individuals to use them. In consequence, these services often turn out to be redistributive: The rich pay for private, and higher quality, education, health and housing and so consume relatively little of what is publicly provided (Boadway and Marchand, 1995).
- The fourth category is called “transfers” ( $g_3$ ) and includes expenditures on various government-sponsored insurance and redistribution programs, such as unemployment benefits, old age pensions, and transfers to families or individuals with special financial needs. In the historical data, these appear as “social security expenditures” but are not recorded separately for most of the countries and are too spotty in coverage to facilitate independent analysis. For most parts then, we are forced to add the spending on collective goods and transfers up to one category denoted ( $g_{23}$ ).

Table 5 (columns 4–8) shows the changes in the spending portfolio, measured by spending on a particular category ( $g_i$ ) as a fraction of total central government spending ( $g$ ). The portfolio changes significantly during the period. Spending on security ( $g_0$ ), which until 1890 constituted over 50 percent of total spending, declined in importance and gave gradually until 1913 way to more spending on long-term public services ( $g_1$ ), and to more spending on collective goods (health, education and housing) and transfers ( $g_{23}$ ). The increase of the combined share of collective goods and transfers was particularly strong in the interwar period and largely due to an increase in spending on collective goods. Most countries did not record any spending

on insurance and welfare programs until the 1930s and those countries that did, Denmark, Norway, Sweden and Finland, spent a small fraction of their budgets on such programs. This has been extensively documented by Lindert (1994), who demonstrates that it was not until the 1930s that state pensions, unemployment relief, housing allowances and other social programs began to play more than a marginal role in most countries. Taken together, this suggests that the growth in government as it happened in Western Europe before World War II can largely, if not wholly, be attributed to growth in government spending *net* of spending on transfers. This, of course, stands in sharp contrast to what happened after World War II where a significant portion of the growth in government can be attributed to growth in government-sponsored transfer and welfare programs (Tanzi and Schuknecht, 2000).

### 3. Main hypotheses

Basic economic principles suggest that the extension of the voting franchise should be associated with some expansion of the public sector, in particular in cases where democratically elected governments become accountable to poorer voters (Meltzer and Richard, 1981). This, however, begs the question why a powerful elite, with the right to tax others and the power to spend the revenues as they wish, would be willing to dilute that power. Acemoglu and Robinson (2000) argue that the extension of the franchise can be understood as a rational response by the elite to the threat of revolution.<sup>12</sup> The threat of revolution varies with circumstances, and, so the argument goes, the elite cannot make credible promises to redistribute (when the threat is weak). Granting the vote to the poor may therefore be the only credible way to redistribute and, by implication, to prevent a revolution.<sup>13</sup> This theory, thus, predicts that extending the franchise to the poor should be associated with more redistribution (from the rich minority to the poor majority).<sup>14</sup> In contrast to this, Lizzeri and Persico, 2004 argue that the extension of the franchise was driven by a conflict of interest among the members of the elite. As the franchise is extended to more individuals, the constituency of government becomes wider and politicians start spending more on public goods as opposed to transfers that only benefit a fraction of the enfranchised elite. Anticipating this, sub-groups within the elite, who do not receive transfers under the current franchise, may find it in their interest to give the vote to the poor with the view to trigger an expansion of spending on public

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<sup>12</sup>See also Conley and Temimi (2001).

<sup>13</sup>The fact that investments in public education, health and housing can be redistributive and require long-term investments suggests that incumbent governments may have had alternative ways to make credible redistribution promises for the future.

<sup>14</sup>To the extent that it takes time for the poor to successfully contest the power of the elite in elections (after they have been granted the franchise), the time lag between the extension of the franchise and the expected expansion of redistributive spending may be substantial. Formally, Justman and Gradstein (1999), for example, show how a process of gradual extension, triggered by increasing levels of alienation among the disenfranchised, can lead to a gradual increase in redistribution.

goods. We should, as a consequence, observe a positive correlation between the extension of the franchise (both the economic and the female franchise) and spending on (pure) public goods.

Insofar as males and females have the same preferences and face the same constraints, the extension of the voting franchise to females should have little impact on the level and pattern of public spending. However, in reality men and women face different constraints and opportunities. This is particularly true for married females who have specialized in household production. In case of break down of marriage or widowhood, this group of females may find it difficult to enter or reenter the labor market. Lott and Kenny (1999) argue that such factors would induce female voters to support spending on publicly provided private goods, such as health and education, and on social insurance, as a precautionary measure. If so, the female franchise should be associated with higher spending on these items.

In our data, rich-to-poor redistribution is best measured by spending on collective goods and transfers ( $g_{23}$ ). Spending on public goods, on the other hand, can be measured by spending on security ( $g_0$ ) and long-term public services ( $g_1$ ), although the latter category might involve elements of geographically targeted spending. We can summarize the predictions as follows:

1. The economic franchise increases the size of government by
  - (a) increasing spending on collective goods and transfers that redistribute from rich to poor (Acemoglu and Robinson, 2000),<sup>15</sup>
  - (b) increasing spending on security and long-term public services (Lizzeri and Persico, 2004).

The female franchise increases the size of government mainly by increasing

2. spending on collective goods and transfers.

The effective constituency of government increases with the extension of the franchise, but also with a change from majority rule to proportional representation. This is because single-member districts and (winner-takes-all) majority voting allow a party to win elections with only 25 percent of the national vote (half the votes in half the districts), while under proportional rule, it would need 50 percent of the national vote to win. Lizzeri and Persico (2001) argue that the introduction of proportional representation is associated with higher spending on public goods: It induces politicians to focus on broader spending programs that benefit a larger segment of the population. Persson and Tabellini (2000, Chapters 8 and 9) reach a similar conclusion, but focus on differences in district size. Majority rule is, typically, associated with smaller districts and political parties, therefore, target their policies

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<sup>15</sup>Strictly speaking, Acemoglu and Robinson's (2000) theory predicts that the driving force behind both the extension of the franchise and the level of redistributive spending is the threat of revolution. Redistributive spending is predicted to be high when the threat is permanent and when it is transitory, but only in the latter case a positive correlation between the voting franchise and spending should be expected. Thus, a proper test of Acemoglu and Robinson's theory would have to measure the threat of revolution explicitly. At best, the evidence presented in this paper can be seen as a weak test of this theory.

to small constituencies. Under proportional representation districts are larger, and parties need to reach a broader segment of the voter population to win and, thus, would find it advantageous to offer public goods and other broad-based spending programs instead of (geographically) targeted programs. While differences in district size under the two election rules need not lead to higher overall spending under proportional representation, Austen-Smith (2000) points out that more parties are, typically, represented in parliament under proportional representation than under majority rule and that may lead to higher overall spending.<sup>16</sup> In his model, redistributive taxation is decided by post-election bargaining in the legislature. Under majority rule, the winning party can decide for itself how much to tax, while under proportional representation a coalition of parties must form to win a majority. This difference, typically, leads to higher overall levels of taxation and spending under proportional representation.

With our data it is hard to make a clear-cut distinction between “broad” and “narrow” public spending programs. However, spending on security and, to some extent also, spending on collective goods and transfers represent broad-based spending programs that benefit relatively large groups of voters.<sup>17</sup> Spending on long-term public services, on the other hand, might contain spending items that can be targeted to particular election districts, but it is not a very precise measure of such spending programs. Thus, we operationalize the theoretical predictions as follows:

3. A change from majority rule to proportional representation increases spending on security and on collective goods and transfers. The associated change in the composition of spending may lead to an increase in overall spending.

Growth in the size of the public sector has traditionally been attributed to social progress and demographic changes (Wagner, 1883; Holsey and Borchering, 1997). However, as pointed out by Boix (2001), the impact of these forces depends on underlying political structures for a number of plausible reasons. Social progress, for example, is associated with urbanization and industrialization, both of which create a need for state intervention to provide infrastructure and education (to facilitate skill formation). Likewise, as life expectancy goes up and populations start aging pressures for intergenerational redistribution and public health programs are naturally created. In a society, then, with a fully extended franchise these demands can filter through the political process and, as a consequence, public expenditures on long-term public services and collective goods and transfers should start to increase. In societies with a more restricted franchise, on the other hand, the forces of development would not filter through to the same

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<sup>16</sup>See Milesi-Feretti et al. (2002) for a model with post-election legislative bargaining where larger districts are unambiguously associated with higher overall spending.

<sup>17</sup>Persson and Tabellini (2000, Chapter 6) use spending on welfare to measure broad-based spending programs.

extent and spending on these categories would, *ceteris paribus*, be lower. Thus, we have the final prediction:

4. Economic development (as measured by GDP per capita, urbanization or life expectancy) has a larger impact on spending on long-term public services and on collective goods and transfers in societies with a more extended voting franchise (Boix, 2001).

#### 4. Evidence from panel data for 12 European democracies

In this section, we provide evidence regarding the hypotheses developed in the previous section. Our data set is well-suited to do so. The time series variation in the political variables, including the election rule, within each country, is of particular importance as it allows us to separate the effect of political institutions from other (time-invariant) country-specific features simply by using conventional panel estimation techniques. As discussed above, the franchise extension is endogenous and to establish causal links with fiscal outcomes, we would need to take this into account. We have, however, not been able to identify suitable instrument variables and, for this reason, we stress that the regressions reported below allow us to identify correlations but not to establish causality.

##### 4.1. The econometric specification

The dependent variable in our regressions is central government spending out of GDP ( $g/y$ ) or one of the three components of central government spending—security ( $g_0/y$ ), long-term public services ( $g_1/y$ ), and collective goods and transfers ( $g_{23}/y$ )—out of GDP. To test hypothesis 1 and 2, we need quantitative measures of the two stages of the franchise extension. We use the variable *economic franchise*, which measures the electorate in percentage of the relevant reference group as discussed in Section 2, to quantify the gradual lifting of economic and social restrictions and the dummy variable *female franchise*, which is equal to 1 if women were allowed to vote and equal to 0 otherwise, to quantify female suffrage. To quantify the impact of the election rule and to test hypothesis 3, we introduce the dummy variable *proportional rule*, which is equal to 1 if proportional representation is used and 0 otherwise.<sup>18</sup> Finally, to test hypothesis 4 regarding the interplay between democracy and economic development, we interact the *economic franchise* with indicators of economic development such as real GDP per capita, the degree of urbanization and the age composition of the population (see below for precise definitions of these variables).

<sup>18</sup>The partial correlations between *economic franchise*, *female franchise* and *proportional rule* range from 0.43 to 0.72. This suggests that multi-colinearity can be a problem for inference regarding the parameters of interest. However, the fact that we are able to estimate the parameters fairly precisely in many cases and the estimates do not change much when observations either in the time dimension or in the country dimension are excluded suggests that this is not in actual fact a major problem.

Changes in the level and composition of government spending are affected by many other socio-economic factors beyond those related to the political reforms of the era. Some of these are country-specific and stable over time and can be captured by fixed effects. Others are common to all countries at a given point in time and can be captured by time effects. To control for observable factors that vary over time, we include some control variables in all the regressions. First, the impact of the franchise is likely to be different in societies with open and competitive political institutions than in societies with more closed and less competitive institutions. To control for this effect, we include in the regressions the *polity index*. As discussed above, the *polity index* is coded on a scale from  $-10$  to  $10$ . Clearly, this cannot be interpreted as a cardinal scale and using the *polity index* directly in the regressions can be misleading. We have experimented with different specifications where we break the index down into a set of dummy variables. It makes little difference for the results and in the reported regressions, we use a simple dummy variable (*polity dummy*) that is equal to 0 if the *polity index* is negative (political competition is low) and 1 if it is positive (political competition is high). Second, the degree of urbanization is likely to affect spending levels and patterns, simply because of new needs for infrastructure, education facilities and other publicly provided goods. We include the variable *urbanization rate*—the proportion of the population that lives in towns with more than 10,000 inhabitants—to control for this effect. Third, as argued by Lindert (1994) and others, the age structure of the population is likely to affect government spending patterns, simply by changing the composition of the electorate and by giving rise to demands for intergenerational redistribution and publicly provided health services. We include the variable *old*, which measures the proportion of the total population that is aged above 65, to capture this. Fourth, the size of the population might, to the extent that there is scale economy in the provision of public services, affect government spending. We include the variable *population*, which is the size of the total population, to allow for this possibility. Finally, we include a measure of PPP adjusted GDP per capita, *gdp per capita*, to control for the general level of development. All these variables and their sources are described in the data appendix.

It is clear that wars have exceptional impacts on government spending and bias spending patterns towards spending on defense. To control for this effect, we have constructed a dummy variable that is equal to 1 if a country is at war and 0 if not. With a single dummy variable, however, our regressions cannot capture what happened during World War I, and the residuals from the regressions show large outliers for these years. To deal with this problem, we have, in all regressions, introduced four “war dummies”. One that measures if a particular country was at war *excluding* World War I and three dummy variables for those countries that participated in World War I. These dummies cover the periods 1913–1915, 1915–1917 and 1917–1919 separately. This improves the fit significantly and eliminates most of the outliers. Modelling World War I is important not only for statistical reasons, but also because it was during the period 1913–1920 that many of the key political reforms took place. We believe that our approach with three separate dummy variables for World War I makes it less likely that we attribute changes in spending caused by war anomalies to these reforms.

We estimate two models for each of the fiscal outcomes. The first model, which we call the equilibrium model, can be formulated as follows:

$$\ln x_{i,t} = \eta_\tau + \alpha_i + \mathbf{X}_{i,t}^{\text{pol}} \boldsymbol{\gamma} + \mathbf{X}_{i,t}^{\text{econ}} \boldsymbol{\delta} + \varepsilon_{i,t}. \quad (1)$$

where  $x_{i,t}$  denotes a particular fiscal outcome in country  $i$  in year  $t$ ;  $\mathbf{X}_{i,t}^{\text{econ}}$  is the vector of socio-economic control variables; and  $\mathbf{X}_{i,t}^{\text{pol}}$  is the vector of political variables. The vector of unknown parameters  $\{\boldsymbol{\gamma}, \boldsymbol{\delta}\}$  is to be estimated, and  $\varepsilon_{i,t}$  is an error term. In all regressions, we allow for country-specific fixed effects ( $\alpha_i$ ) and common time effects  $\eta_\tau$ . The second model, which we call the partial adjustment model, can be formulated as follows:

$$\ln x_{i,t} = \eta'_\tau + \alpha'_i + \beta \ln x_{i,t-1} + \mathbf{X}_{i,t}^{\text{pol}} \boldsymbol{\gamma}' + \mathbf{X}_{i,t}^{\text{econ}} \boldsymbol{\delta}' + \varepsilon'_{i,t}. \quad (2)$$

This formulation assumes a partial adjustment mechanism whereby the impact of the franchise (and the other variables) is spread out over time as captured by the estimated persistence parameter ( $\beta$ ). This formulation makes it possible to separate the short-run from the long-run impact of the political reforms and allows for the possibility that the reforms affect spending patterns only with a lag. This may be so for many reasons, most importantly because it takes time to build the institutional infrastructure that can deliver large-scale expansions in government spending.

Our data set contains information on spending patterns for 12 countries for varying time periods. For some countries we got data for the period after World War I only; for others, we got data stretching back into the nineteenth century. Thus, we face a trade-off between including as many countries as possible and excluding some countries to make the sample more homogenous in the time dimension. In resolving this trade-off, we focus on two samples. The first sample contains Italy, Denmark, Norway, Sweden, and United Kingdom. For these countries, we have data on government spending out of GDP and its components for a comparable period before and after World War I and thus before and after (if relevant) the female suffrage and the reform of the election rule took place. The second sample contains all the countries. Finally, we also report estimates for a balanced panel data set with Italy, Denmark, Norway, Sweden, and United Kingdom.

We estimate the two models using a fixed-effects estimator to control for country-specific effects.<sup>19</sup> We allow the error term to have different variance across the panels (panel heteroscedasticity), and the reported standard errors of the parameter estimates are panels corrected standard errors, as recommended by [Beck and Katz \(1995\)](#). For the balanced panel data set, we have tested, using an IPS test, the

<sup>19</sup>The presence of a lagged dependent variable among the regressors in the second model implies that both the within group fixed effect estimator and the random effects estimator are biased and inconsistent unless the number of time periods is large ([Baltagi, 1995](#), pp. 125–126). The random effects estimator has an additional bias when, as we would expect here, the country-specific effects are correlated with the other explanatory variables. The relatively long time period covered by our sample should, however, reduce the bias associated with the fixed effects estimator, and justifies our choice of estimation technique. Alternatively, we could avoid the potential bias associated with the fixed effects estimator by using estimators based on the generalized method of moments (GMM) but this would require many more countries than we have to produce consistent estimates ([Arellano and Bond, 1991](#)).



hypothesis of nonstationarity of the residuals and the null is strongly rejected in all cases.<sup>20</sup> This, thus, gives some support to the notion that there exists a long-run relationship between the variables in our model.

#### 4.2. The franchise effects and the election rule

Tables 6 and 7 report the results from the equilibrium and partial adjustment model for the sample with Norway, Sweden, Denmark, United Kingdom and Italy covering the period 1869–1938. Tables 8 and 9 report the results for the same countries but with a balanced panel covering the period 1872 (1889) to 1923. Table 10 reports the results for the larger sample with 11 countries.<sup>21</sup>

The impact of the economic franchise is, with one exception, clear and robust across specifications and samples: It correlates positively with spending on security and long-term public services, as well as with total government spending. The exception is the impact on spending on collective goods and transfers. In the unbalanced panel with Norway, Sweden, Denmark, United Kingdom and Italy, the estimated coefficient is positive and statistically significant in both specifications (Tables 6 and 7). Excluding most of the interwar period to make the panel balanced (Tables 8 and 9), however, suggests that the impact is insignificant or, if anything, *negative*. The same is true in the full sample (Table 10). Thus, these results suggest that the economic franchise increased total spending mainly by increasing spending on long-term public services ( $g_1/y$ ) and security ( $g_0/y$ ) as suggested by hypothesis 1b, whereas the evidence on the relationship between the economic franchise and spending on collective goods and transfers ( $g_{23}/y$ ) is ambiguous, and, in contrast to hypothesis 1a, the economic extension may in some countries have held back the expansion of spending on these items.

In contrast, the female franchise had little or, if anything, a *negative* impact on overall central government spending ( $g/y$ ), spending on security ( $g_0/y$ ) and spending on long-term public services ( $g_1/y$ ). The negative effects are only significant in the equilibrium model. Importantly, the impact on spending on collective goods and transfers ( $g_{23}/y$ ) seems to have been positive, although the coefficient on the female franchise dummy is not statistically significant in all regressions. This provides partial support for hypothesis 2, and suggests that the two stages of the franchise extension had very different impacts: The economic franchise contributed to the general growth in government mainly through an expansion of spending on security and long-term public services, while the female franchise had an impact mainly through spending on collective goods and transfers.

We find no evidence that countries with proportional rule have higher overall government spending than countries with majority rule. On the contrary, the change from majority rule to proportional representation had, if anything, a negative impact on overall spending and most of the spending components. Statistically, the

<sup>20</sup>The power of the test is likely to be low due to the small number of countries in our sample (see Im et al., 2003).

<sup>21</sup>We have no data for the components of central government spending in Switzerland.

Table 6

Regression results for central government spending and its components, unbalanced panel with Norway, Sweden, Denmark, United Kingdom and Italy, 1869–1938

Equilibrium model	(1)	(2)	(3)	(4)
Dependent variable	$\ln(g/y)$	$\ln(g_0/y)$	$\ln(g_1/y)$	$\ln(g_{23}/y)$
Economic franchise	0.0029* (0.0008)	0.0061* (0.0008)	0.0050* (0.0016)	0.0035* (0.0014)
Female franchise	- 0.1294+ (0.0543)	- 0.3177* (0.0710)	- 0.6600* (0.1307)	0.0309 (0.0840)
Proportional rule	- 0.1020* (0.0407)	- 0.0201 (0.0465)	- 0.2104+ (0.0961)	- 0.9289* (0.0815)
Polity dummy	- 0.0719# (0.0418)	- 0.1056+ (0.0470)	- 0.1615# (0.0915)	- 0.5163* (0.0777)
Old	0.0484* (0.0110)	0.0283+ (0.0133)	0.1394* (0.0258)	- 0.1510* (0.0193)
Urbanization rate	0.0239* (0.0060)	- 0.0013 (0.0067)	0.0483* (0.0115)	0.0622* (0.0103)
$\ln(\text{gdp per capital})$	- 0.8676* (0.1149)	- 0.5036* (0.1498)	- 0.2335 (0.2707)	- 0.6180* (0.1947)
$\ln(\text{population})$	0.0897 (0.2883)	- 0.4079 (0.3182)	0.3895 (0.6296)	3.782* (0.5729)
<i>vfrN</i>	329	310	310	310
$T_{\min}$	55	50	50	50
$T_{\max}$	69	69	69	69
$T_{\text{average}}$	65.8	62	62	62
Wald $\chi^2$	3994	3032	3725	6090

Notes: Panels corrected standard errors in brackets (PCSEs). \* = significant at the 1% level; + = significant at the 5% level; # = significant at the 10% level (two-tailed tests).  $N$  is the total number of observations,  $T_{\min}$ ,  $T_{\max}$  and  $T_{\text{average}}$  are the minimum, maximum and average number of observations in the panels. All regressions include country-specific fixed effects, time dummies and a set of dummies for participation in wars.

strongest and most robust result is the negative correlation between *proportional rule* and spending on collective goods and transfers ( $g_{23}/y$ ), while there might have been a small positive, but statistically insignificant, impact on spending on security ( $g_0/y$ ) (see, e.g., Table 10). Thus, the results do not support the hypothesis that proportional representation is associated with an expansion of broad-based spending programs ( $g_0/y$  and  $g_{23}/y$ ), and, overall, the historical evidence in favor of hypothesis 3 is weak. Finally, we notice that political competition, as measured by the polity dummy variable, contributed to holding government spending back: The estimated coefficient is negative in all regressions and significantly so in just over half.

An implication, then, is that the different reforms often had offsetting influences on fiscal outcomes. To judge the net impact and to gain a better understanding of the economic significance of the estimated effects, we report in Table 11 how much each of the four fiscal outcome variables would have changed, *ceteris paribus*, as a

Table 7

Regression results for central government spending and its components, unbalanced panel with Norway, Sweden, Denmark, United Kingdom and Italy, 1869–1938

Partial adjustment model	(5)	(6)	(7)	(8)
Dependent variable	$\ln(g/y)$	$\ln(g_0/y)$	$\ln(g_1/y)$	$\ln(g_{23}/y)$
Economic franchise	0.0011 <sup>+</sup> (0.0006)	0.0024* (0.0007)	0.0019# (0.0011)	0.0015 <sup>+</sup> (0.0007)
Female franchise	-0.0069 (0.0386)	0.0025 (0.0441)	-0.0975 (0.0813)	0.0158 (0.0469)
Proportional rule	-0.0292 (0.0280)	0.0145 (0.0308)	-0.0723 (0.0601)	-0.2394* (0.0500)
Polity dummy	-0.0330 (0.0286)	-0.0751 <sup>+</sup> (0.0323)	-0.0924 (0.0635)	-0.1531* (0.0414)
Old	0.0231* (0.0080)	0.0245* (0.0095)	0.0504* (0.0176)	-0.0308 <sup>+</sup> (0.0126)
Urbanization rate	0.0093 <sup>+</sup> (0.0041)	0.0005 (0.0046)	0.0040 (0.0079)	0.0081 (0.0052)
$\ln(\text{gdp per capita})$	-0.3911* (0.0954)	-0.1926# (0.1125)	-0.2925 (0.1944)	-0.3000 <sup>+</sup> (0.1341)
$\ln(\text{population})$	0.2101 (0.2073)	0.1149 (0.2380)	0.6776# (0.4091)	1.224* (0.3221)
Lagged endogenous	0.6625* (0.0405)	0.6645* (0.0428)	0.7665* (0.0483)	0.7876* (0.0311)
$N$	329	309	309	309
$T_{\min}$	55	49	49	49
$T_{\max}$	69	69	69	69
$T_{\text{average}}$	65.8	61.8	61.8	61.8
Wald $\chi^2$	9781	7636	7139	17870

Notes: Panels corrected standard errors in brackets (PCSEs). \* = significant at the 1% level; <sup>+</sup> = significant at the 5% level; # = significant at the 10% level (two-tailed tests).  $N$  is the total number of observations,  $T_{\min}$ ,  $T_{\max}$  and  $T_{\text{average}}$  are the minimum, maximum and average number of observations in the panels. All regressions include country-specific fixed effects, time dummies and a set of dummies for participation in wars.

consequence of each of the political reforms, relative to the sample mean. We focus on the reform effects that are statistically significant in the regressions reported in Tables 6 and 7 (restricted sample) and Table 10 (full sample). First, consider the results for the equilibrium model, reported in column 5. The economic franchise increased total government spending from 10 percent of GDP to 13.4 percent of GDP. This effect was offset by the female franchise and the introduction of the proportional rule system, both of which contributed to a reduction in total spending by about one percentage point. The economic franchise increased spending on security from around 4.5 percent to 8–10 percent of GDP and it almost doubled spending on long-term public services from 1.2–1.3 percent to 1.7–1.9 percent of GDP. Again, these effects were offset by the female franchise, which, *ceteris paribus*, reduced spending on security and on long-term public goods by 25–50 percent.

Table 8

Regression results for central government spending and its components, balanced panels with Norway, Sweden, Denmark, United Kingdom and Italy, 1872 (1889)–1923

Equilibrium model	(9)	(10)	(11)	(12)
Dependent variable	$\ln(g/y)$	$\ln(g_0/y)$	$\ln(g_1/y)$	$\ln(g_{23}/y)$
Economic franchise	0.0026* (0.0009)	0.0036* (0.0012)	0.0068* (0.0018)	−0.0029+ (0.0014)
Female franchise	−0.1083 (0.0695)	−0.1689+ (0.0801)	−0.2648+ (0.1228)	0.1857+ (0.0893)
Proportional rule	−0.0501 (0.0437)	−0.0551 (0.0563)	−0.1379 (0.1036)	−0.5300* (0.0734)
Polity dummy	−0.0303 (0.0526)	−0.0694 (0.0650)	−0.0957 (0.1017)	−0.0039 (0.0759)
Old	0.0594* (0.0146)	−0.0163 (0.0277)	0.2331* (0.0427)	−0.2113* (0.0272)
Urbanization rate	0.0389* (0.0105)	0.0457* (0.0150)	0.1809* (0.0217)	0.1584* (0.0183)
$\ln(\text{gdp per capita})$	−0.8052* (0.1556)	−0.6853* (0.2544)	−0.4427 (0.4063)	0.0200 (0.2516)
$\ln(\text{population})$	−0.2094 (0.4810)	−2.141* (0.7331)	−6.602* (1.001)	−3.427* (0.8017)
<i>N</i>	265	180	180	180
<i>T</i>	53	36	36	36
Wald $\chi^2$	2934	1837	2982	1193

Notes: Panels corrected standard errors in brackets (PCSEs). \* = significant at the 1% level; + = significant at the 5% level; # = significant at the 10% level (two-tailed tests). All regressions include country-specific fixed effects, time dummies and a set of dummies for participation in wars. *N* is the total number of observations and *T* is the number of observations per panel.

Spending on collective goods and transfers was reduced by about 50 percent by the introduction of the proportional rule. In the full sample with 11 countries, this effect is, however, partly offset by the female franchise, which, *ceteris paribus*, contributed to an increase in spending on collective goods and transfers by 16 percent.

The partial adjustment model allows us to separate the short- and long-run effects by taking into account that the adjustment to equilibrium is gradual, as captured by the parameter  $\beta$ . From Tables 6, 8 and 10, it is clear that some components of government spending are more persistent than others. In particular, spending on collective goods (health, education and housing) and transfers is more persistent than spending on security and long-term public services. To judge the economic significance of this, we report in Table 11 the short-run (column 6) and the long-run (column 7) impact of the franchise and the election rule (measured as  $\frac{\gamma_i}{1-\beta}$ ). The short-run effects are small. The long-run effects are somewhat larger and comparable to those found in the equilibrium model.

It is of interest to compare these findings with previous results in the literature and with results from other countries. The voting franchise was not extended at the same

Table 9

Regression results for central government spending and its components, balanced panels with Norway, Sweden, Denmark, United Kingdom and Italy, 1872 (1889)–1923

Partial adjustment model	(13)	(14)	(15)	(16)
Dependent variable	$\ln(g/y)$	$\ln(g_0/y)$	$\ln(g_1/y)$	$\ln(g_{23}/y)$
Economic franchise	0.0012 <sup>#</sup> (0.0007)	0.0015 <sup>#</sup> (0.0009)	0.0036 <sup>+</sup> (0.0016)	0.00003 (0.0009)
Female franchise	0.0052 (0.0545)	0.0255 (0.0524)	-0.0718 (0.0952)	0.1246 <sup>+</sup> (0.0578)
Proportional rule	-0.0106 (0.0318)	0.0026 (0.0435)	-0.1212 (0.0847)	-0.2533 <sup>*</sup> (0.0583)
Polity dummy	-0.0304 (0.0395)	-0.0761 (0.0517)	-0.1285 (0.0859)	-0.0758 (0.0515)
Old	0.0298 <sup>*</sup> (0.0117)	0.0027 (0.0213)	0.0954 <sup>*</sup> (0.0403)	-0.0722 <sup>+</sup> (0.0296)
Urbanization rate	0.0156 <sup>+</sup> (0.0082)	0.0081 (0.0121)	0.0639 <sup>*</sup> (0.0230)	0.0537 <sup>*</sup> (0.0138)
$\ln(\text{gdp per capita})$	-0.4258 <sup>*</sup> (0.1269)	-0.3573 <sup>#</sup> (0.2130)	-0.6143 <sup>#</sup> (0.3672)	-0.2420 (0.2209)
$\ln(\text{population})$	0.1556 (0.3791)	-0.2034 (0.6044)	-10.667 <sup>#</sup> (0.986)	-0.8979 (0.5738)
Lagged endogenous	0.6255 <sup>*</sup> (0.0477)	0.5833 <sup>*</sup> (0.0534)	0.5937 <sup>*</sup> (0.0899)	0.6578 <sup>*</sup> (0.0593)
<i>N</i>	260	175	175	175
<i>T</i>	52	35	35	35
Wald $\chi^2$	7192	4451	3081	3083

Notes: Panels corrected standard errors in brackets (PCSEs). \* = significant at the 1% level; + = significant at the 5% level; # = significant at the 10% level (two-tailed tests). All regression include country-specific fixed effects, time dummies and a set of dummies for participation in wars. *N* is the total number of observations and *T* is the number of observations per panel.

time across US states. Husted and Kenny (1997) exploit differences in tax and literacy requirements during the period 1950–1988 to evaluate the impact of these restrictions on public spending patterns. They find that the (economic) extension of the franchise had a significant and positive effect on welfare spending (or transfers) but little, or negative, impact on other spending categories (including spending on public goods). Quite remarkably, we find evidence of the opposite in Western Europe before World War II. Lott and Kenny (1999) evaluate US evidence from the period 1870–1940, and find that the observed pre-war expansion in the size of government can be attributed in large part to the female suffrage. US states did not extend the franchise uniformly; several states gave women the right to vote long before federal legislation made this compulsory in 1919–1920. Spending patterns, as well as congressional voting records, suggest that state governments or representatives elected after this extension voted for more government spending and higher taxes. In our analysis, the impact of the female suffrage is remarkably weak and, in contrast to the US evidence, often negative. In effect, the female franchise contributed to the

Table 10  
Regression results for the components of central government spending in Norway, Sweden, Denmark, United Kingdom, Italy, Germany, France, Netherlands, Belgium, Finland and Austria, 1869–1938

Dependent variable	(17)	(18)	(19)	(20)	(21)	(22)
	$\ln(g_0/y)$	$\ln(g_1/y)$	$\ln(g_{23}/y)$	$\ln(g_0/y)$	$\ln(g_1/y)$	$\ln(g_{23}/y)$
Economic franchise	0.0077* (0.0007)	0.0025+ (0.0011)	-0.0047* (0.0011)	0.0022* (0.0006)	0.0005 (0.0008)	-0.0006 (0.0006)
Female franchise	-30.092* (0.0806)	-0.4553* (0.1293)	0.1606# (0.0877)	0.0225 (0.0418)	-0.1294 (0.0869)	0.0523 (0.0423)
Proportional rule	0.0168 (0.0546)	-0.2000+ (0.0924)	-0.6375* (0.0987)	0.0349 (0.0310)	-0.0988# (0.0593)	-0.1631* (0.0439)
Polity dummy	-0.1492* (0.0505)	-0.0971+ (0.0827)	-0.1896* (0.0717)	-0.0666+ (0.0297)	-0.0263# (0.0511)	-0.0398* (0.0314)
Old	0.0180 (0.0125)	0.1391* (0.0244)	-0.2156* (0.0191)	0.0130 (0.0091)	0.0438* (0.0151)	-0.0449* (0.0131)
Urbanization rate	0.0072 (0.0074)	0.0235+ (0.0117)	0.0916* (0.0099)	0.0051 (0.0044)	0.0016 (0.0070)	0.0102# (0.0055)
$\ln(\text{gdp per capita})$	-0.4760* (0.1343)	-0.5957* (0.1953)	-0.0020 (0.1594)	-0.0676 (0.0934)	-0.1798 (0.1332)	-0.0911 (0.0911)
$\ln(\text{population})$	-0.0948 (0.0762)	-0.5808* (0.1188)	-0.8635* (0.1254)	-0.0066 (0.0496)	-0.0677 (0.0769)	-0.0840# (0.0520)
Lagged endogenous				0.7120* (0.0409)	0.7466* (0.0432)	0.8231* (0.0258)
$N$	399	399	385	392	392	377
$T_{\min}$	5	5	5	4	4	4
$T_{\max}$	69	69	69	69	69	69
$T_{\text{average}}$	36.3	36.3	35	35.6	35.6	34.3
Wald $\chi^2$	6619	8815	8633	26038	10969	137689

Notes: Panels corrected standard errors in brackets (PCSEs). \* = significant at the 1% level; + = significant at the 5% level; # = significant at the 10% level (two-tailed tests).  $N$  is the total number of observations,  $T_{\min}$ ,  $T_{\max}$  and  $T_{\text{average}}$  are the minimum, maximum and average number of observations in the panels. All regressions include country-specific fixed effects, time dummies and a set of dummies for participation in wars.

Table 11

The economic significance of the franchise and election rule effects reported in Tables 6 and 7 (Norway, Sweden, Denmark, United Kingdom and Italy) and 10 (full sample)

Variable	Table	Franchise effect	Sample mean (% of gdp)	Equilibrium model (% of gdp)	Partial adjustment model	
					Short run (% of gdp)	Long run (% of gdp)
$g/y$	6/7	Economic franchise	10	13.4	11.2	13.8
$g/y$	6/7	Female franchise	10	8.7	—	—
$g/y$	6/7	Proportional rule	10	9.0	—	—
$g_0/y$	6/7	Economic franchise	4.4	8.1	5.6	8.9
$g_0/y$	6/7	Female franchise	4.4	3.2	—	—
$g_0/y$	10	Economic franchise	4.7	10.2	5.8	9.0
$g_0/y$	10	Female franchise	4.7	3.5	—	—
$g_1/y$	6/7	Economic franchise	1.2	1.9	1.4	2.7
$g_1/y$	6/7	Female franchise	1.2	0.6	—	—
$g_1/y$	6/7	Proportional rule	1.2	1.0	—	—
$g_1/y$	10	Economic franchise	1.3	1.7	—	—
$g_1/y$	10	Female franchise	1.3	0.8	—	—
$g_1/y$	10	Proportional rule	1.3	1.1	1.2	0.8
$g_{23}/y$	6/7	Economic franchise	1.4	2.0	1.6	2.9
$g_{23}/y$	6/7	Proportional rule	1.4	0.6	1.1	0.5
$g_{23}/y$	10	Economic franchise	1.9	1.2	—	—
$g_{23}/y$	10	Female franchise	1.9	2.2	—	—
$g_{23}/y$	10	Proportional rule	1.9	1.0	1.6	0.9

Note: The economic extension effect is calculated for an increase in the economic franchise from 0 to 100. The other effects correspond to a shift in the relevant dummy variable from 0 to 1.

growth of government in Western Europe before World War II only through a small positive, but statistically weak, effect on spending on collective goods and transfers. We interpret this as saying that the impact of the female franchise was not so much on the scale of social spending. Its main impact, as argued by Lindert (1994), may have been to induce countries to start developing new social programs, that is, to broaden the scope of social spending to new areas. This, of course, eventually would lead to higher levels of spending.

We are not aware of any studies that investigate the role of the election rule in explaining fiscal outcomes before World War II. For the Post War period, however, recent research has identified a relationship between the election rule and the size of government in cross-sections of countries. Persson and Tabellini (2003, Chapter 6) report evidence from a large cross-section of countries in the 1990s that countries with the majority rule have small governments and spend comparably little on welfare programs (unemployment benefits and pensions).<sup>22</sup> In a comparable study, Milesi-Feretti et al. (2002) develop a measure of the degree of proportionality of election system in OECD and Latin American countries that takes into account that

<sup>22</sup>See also Persson and Tabellini (2002).

most electoral systems lie somewhere between the logical extremes of pure proportional representation and pure majority rule. Their findings show that countries with proportional representation spend more on transfers and welfare, but less on public services. We find for the period before World War II that countries using the proportional rule tended to spend less, in particular on collective goods and transfers, but also on long-term public services. Thus, the negative effect on spending on public services identified by Milesi-Feretti et al. (2002) receives support from history, while the result that countries with majority rule have lower overall government spending does not. To reconcile the negative “election reform effect” on spending on collective goods and transfers with the post-war evidence reported by Persson and Tabellini (2003, Chapter 6) and others, we emphasize that central government spending on welfare and transfers was almost non-existent before World War II. Thus, our finding can best be interpreted as saying that countries with proportional rule tended to spend less on education, health and public housing. This, however, is in itself puzzling as health and education services are hard to target to specific groups of voters and thus should appeal more to politicians under proportional rule than under majority rule.

#### 4.3. *Economic development and the economic franchise*

Table 12 reports evidence related to hypothesis 4—that economic development has a larger impact on spending on long-term public goods and on collective goods and transfers in societies with a more extended voting franchise. Economic development can be proxied by various variables. In regressions 23–26, we focus on PPP adjusted real GDP per capita (*gdp per capita*) as the main measure of economic development and have included an interaction term between  $\ln(\textit{gdp per capita})$  and *economic franchise*. We notice that economic development measured this way in itself has a negative impact on government spending. Importantly, however, when we take the interaction between economic development and the economic franchise into account, we note a striking difference between societies that impose different restrictions on the franchise. In societies with a restricted franchise, economic development reduces spending on long-term public services and collective goods and transfers, while in societies with a wide franchise the opposite is true. The turning point for spending on long-term public services is somewhat lower (at about 70 percent) than that for spending on collective goods and transfers (at about 80 percent). We note, however, that the interaction terms are only significant in the equilibrium model.

Another consequence of economic development is that life expectancy increases and that the fraction of older citizens increases. Lindert (1994) reports that aging populations contributed to the rise of social spending in a broader sample of countries than our’s during the period 1880–1939. Quite remarkably, we find the opposite: The fraction of citizens aged above 65 (*old*) has, consistently, a negative impact on spending on collective goods and transfers (see Tables 6–10). The size of this effect, however, depends on the extension of the economic franchise. As shown by regressions 27 and 28 in Table 12, the interaction term between *economic franchise* and *old* has a positive impact on spending on collective goods and transfers. Yet,



Table 12  
Testing for interactions between economic development and the voting franchise in Norway, Sweden, Denmark, United Kingdom and Italy 1869–1938

Dependent variable	(23)	(24)	(25)	(26)	(27)	(28)
	$\ln(g_1/y)$	$\ln(g_1/y)$	$\ln(g_{23}/y)$	$\ln(g_{23}/y)$	$\ln(g_{23}/y)$	$\ln(g_{23}/y)$
Economic franchise	-0.1647* (0.0292)	-0.0332 (0.0230)	-0.0747* (0.0239)	-0.0069 (0.0144)	0.0011 (0.0038)	-0.0016 (0.0021)
$\ln(\text{gdp per capita})^*$ (economic franchise)	0.0227* (0.0039)	0.0047 (0.0031)	0.0150* (0.0032)	0.0011 (0.0019)		
(old)* (economic franchise)					0.0003 (0.0005)	0.0005# (0.0003)
Female franchise	-0.7562* (0.1269)	-0.1328 (0.0839)	0.0135 (0.0828)	0.0111 (0.0475)	0.0149 (0.0889)	-0.0048 (0.0505)
Proportional rule	-0.0935 (0.0886)	-0.0521 (0.0590)	-0.8750* (0.0808)	-0.2366* (0.0490)	-0.9274* (0.0806)	-0.2373* (0.0493)
Polity dummy	-0.0201 (0.0894)	-0.0654 (0.0636)	-0.4511* (0.0774)	-0.1478* (0.0416)	-0.5295* (0.0812)	-0.1704* (0.0413)
Old	0.1307* (0.0243)	0.0510* (0.0175)	-0.1550* (0.0190)	-0.0317* (0.0126)	-0.1837* (0.0532)	-0.0731* (0.0261)
Urbanization rate	0.0261+ (0.0119)	0.0006 (0.0084)	0.0519* (0.0106)	0.0072 (0.0056)	0.0634* (0.0106)	0.0096# (0.0051)
$\ln(\text{gdp per capita})$	-1.555* (0.3395)	-0.5684+ (0.2769)	-1.227* (0.2700)	-0.3680+ (0.1845)	-0.6201* (0.1949)	-0.3052+ (0.1338)
$\ln(\text{population})$	-0.2310 (0.6052)	0.5411 (0.4063)	3.495* (0.5627)	1.204* (0.3177)	3.732 (0.5681)	1.158* (0.3195)
Lagged endogenous		0.7455* (0.0517)		0.7843* (0.0326)		0.7880* (0.0309)
$N$	310	309	310	309	310	309
$T_{\min}$	50	49	50	49	50	49
$T_{\max}$	69	69	69	69	69	69
$T_{\text{average}}$	62	61.8	62	61.8	62	61.8
Wald $\chi^2$	3711	7386	5985	18923	6027	19003

Notes: Panels corrected standard errors in brackets (PCSEs). \* = significant at the 1% level; + = significant at the 5% level; # = significant at the 10% level (two-tailed tests).  $N$  is the total number of observations,  $T_{\min}$ ,  $T_{\max}$  and  $T_{\text{average}}$  are the minimum, maximum and average number of observations in the panels. All regressions include country-specific fixed effects, time dummies and a set of dummies for participation in wars.

even in a society with a fully extended franchise, aging populations did not contribute to the expansion of spending on collective goods and transfer. Finally, the process of urbanization contributed to an increase in total spending mainly by having a positive impact on spending on long-term public services and collective goods and transfers (see Tables 6–10). Except for spending on collective goods and transfers, which seems to be more responsive to the forces of urbanization in societies with a widely extended economic franchise, this impact does not depend on the extension of the economic franchise.<sup>23</sup> On balance, these results provide some support for hypothesis 4, but much depend on the precise specification. The relatively weak interaction between the forces of development and democratization in the historical sample stands in contrast to the much stronger effects found in contemporaneous data by Boix (2001).

## 5. Conclusions

We examine the relationship between public spending and the spread of democracy in Western Europe, as it happened from 1830 to 1938. Many of our results point in the direction predicted by positive theories of public finance. There are others, however, that come as a surprise. This raises many interesting questions that require further analysis, quantitative or qualitative. Below we discuss two of these. First, we find that the spread of democracy provides some explanation for the growth of government but it cannot explain the explosive growth of government, measured by government expenditures or tax revenues as percentage of GDP, that we observe after World War II—the long-run effects reported in Table 11 are (except for spending on security) far too small. It is possible, of course, that political effects are slow, and that the true effects of democratization are only perceptively 25 or 50 years later. Investigating the sources of gradualism, persistence, and *status quo* biases in fiscal policy is likely to be an interesting avenue of further research. An important issue concerns differential persistence. We find that spending on social investments (such as education and health) is more persistent than other spending categories. Combined with the fact that the outcomes of these investments are themselves durable, this suggests that spending on health and education should be the more enduring outcome of the franchise extension. In actual fact, we observe, after World War II, an increase in spending on transfer programs. Democratic governments want to redistribute, but seem to have changed their preferred method of redistribution at some point after World War II.

Second, there was some growth in the size of government net of transfers and welfare spending in the period leading up to World War II, but the explosive growth in government spending did not happen until the 1960s. This could be due to limits on tax raising capacities. Democracy channels the “demand” for public spending by granting citizens voice. Yet the “supply” can be constrained by lack of government finance, and by lack of administrative capacity more generally. It is possible that

<sup>23</sup>These results are not reported.

governments invested in tax raising capacities early on, that this investment took time to yield results, and that by 1960, the technology of tax raising could begin to deliver on the demands of the electorate. Before World War I governments responded to their electorates by reallocating expenditures from an essentially fixed budget. The hypothesis is consistent with our data, but so are many others. Further exploration of this question requires study of the evolution of the tax system.

## Appendix A. Data Appendix

This appendix describes the data material underlying our study in more detail. The data set contains information from 12 Western European countries (Austria, Belgium, Denmark, Finland, France, Germany, Italy, the Netherlands, Norway, Sweden, Switzerland and the United Kingdom). For some countries, the data dates back to the mid 19th century, but for others and for specific series, in particular for the composition of government spending, the sample period is much shorter. Our sample runs through to the outset of World War II, except for Germany (to 1933) and Italy (to 1923). For each country, the fiscal and political data are summarized in Table 13 by decade.

Table 13  
Government spending and the development of democracy in 12 countries 1831–1938

	$g/y$	$G/y$	$g_0/y$	$g_1/y$	$g_{23}/y$	$g_0/g$	$g_1/g$	$g_{23}/g$	Total franchise	Economic franchise	Polity index
Norway											
First year	1865	1865	1865	1865	1865	1850	1850	1850	1844	1844	1844
Gaps <sup>a</sup>	0	58	0	0	0	0	0	0			
<1850						76.3	7.3	7.2	23.1	9.4	-7
1851–1860						70.3	12.5	5.7	21.9	8.9	-7
1861–1870	3.8	5.8	2.4	0.3	0.3	68.9	10.9	7.2	21.4	8.7	-7
1871–1880	3.9	6.8	2.2	0.4	0.4	62.8	10.4	10.2	20.7	8.3	-4.6
1881–1891	5.8	7.4	2.5	0.5	0.8	53.6	12.1	17.2	27.5	10.9	-2.6
1891–1900	6.5	9.9	2.7	0.9	1.1	50.2	15.7	19.9	51.5	20.1	1.6
1901–1910	7.9	9.3	2.7	0.9	1.3	45.2	13.9	21.3	82.3	42.7	10
1911–1920	6.5	12.8	2.6	0.7	1.2	38.7	10.14	17.8	85.3	72.6	10
1921–1930	8.4	17.4	1.7	1.1	2.1	22.7	13.9	27.2	97.1	88.1	10
1931–1938	7.3	18.5	2.0	1.9	2.5	27.3	24.9	33.8	100.4	91.8	10
Austria											
First year	1924	n.a.	1924	1924	1924	1924	1924	1924	1907	1907	1907
Gaps <sup>a</sup>	0		0	0	0	0	0	0			
1901–1910									94.5	37.9	-4
1911–1920									90.2	71.3	-1.6
1921–1930	11.6		2.9	0.92	2.9	27.2	9.2	25.8	90.8	90.8	8
1931–1938	15.5		4.5	1.6	3.6	30.6	10.8	24.6			-4.1

Table 13 (continued)

	<i>g/y</i>	<i>G/y</i>	<i>g<sub>0</sub>/y</i>	<i>g<sub>1</sub>/y</i>	<i>g<sub>23</sub>/y</i>	<i>g<sub>0</sub>/g</i>	<i>g<sub>1</sub>/g</i>	<i>g<sub>23</sub>/g</i>	Total franchise	Economic franchise	Polity index
<b>Belgium</b>											
First year	1912	n.a.	1934	1934	1934	1833	1833	1833	1833	1833	1833
Gaps <sup>a</sup>	20		0	0	0	89	89	89			
<1850						60.4	7.1	1.8	5.2	2.2	−4
1851–1860									7.8	3.3	4
1861–1870						51.4	9.3	4.3	8.5	3.6	6
1871–1880											6
1881–1891						36.6	14.2	8.4	8.2	3.9	6
1891–1900						33.7	15.9	7.3	62.9	26.3	6
1901–1910											6
1911–1920	13.9					27.6	16.1	7.9	92.15	41	7.2
1921–1930	22.9								95.6	45.3	9.1
1931–1938	23.3		4.2	3.4	4.5	19.3	14.4	9.2	94.7	45.3	10
<b>Finland</b>											
First year	1901	n.a.	1926	1926	1926	1901	1901	1901	1907	1907	1917
Gaps <sup>a</sup>	5		0	0	0	5	5	5			
1901–1910	6.46					27.3	25.5	30.8	72.9	62.1	
1911–1920	7.23					33.8	23.4	25.5	86.0	75.5	9
1921–1930	10.4		3.6	3.1	3.3	28.6	22.9	24.8	85.4	74.7	9.4
1931–1938	15.8		3.9	2.5	3.5	22.7	14.6	20.1	86.2	73.5	4
<b>France</b>											
First year	1820	1872	1920	1920	1920	1825	1825	1825	1815	1815	1815
Gaps <sup>a</sup>	4	31	0	0	0	82	82	82			
<1850	9.2					57.6	9.0	3.3	14.2	6.7	−1.7
1851–1860	10.7					49.5	16.2	5.2	87.3	41.4	−7.8
1861–1870	9.9					50.4	13.8	4.8	86.2	41.6	
−5.41											
1871–1880	12.3	13.5				50.6	6.4	4.4	88.1	42.5	7
1881–1891	12.8	15.0				47.2	10.7	6.7	86.3	41.5	7
1891–1900	11.7	15.2				52.0	10.3	9.4	87.4	41.9	7.3
1901–1910	10.8	14.5				50.3	10.6	11.3	91.3	43.4	8
1911–1920	13.1	23.1	15.9	3.6	2.8	54.3	11.7	11.9	92.9	43.1	8.2
1921–1930	17.9	24.4	7.9	2.0	3.8	38.5	9.1	19.5	87.5	39.9	9.1
1931–1938	21.3	32.0	9.6	2.3	6.8	41.7	10.2	29.4	86.3	39.8	10
<b>Germany</b>											
First year	1872	1881	1913	1913	1913	1913	1913	1913	1871	1871	1871
Gaps <sup>a</sup>	11	39	11	11	11	11	11	11			
1871–1880	4.3								87.7	35.8	−4
1881–1891	3.2	9.9							90.0	36.9	−3.5
1891–1900	4.2	12.9							93.5	37.8	1.3
1901–1910	5.0	14.7							94.1	38.3	1.2
1911–1920	5.3	17.0	3.5	0.1	0.2	82.9	2.5	5.3	95.7	77.2	2.9
1921–1930	12.6	25.7	1.8	0.4	3.3	23.0	4.6	37.2	98.1	98.1	6
1931–1938	18.1	31.2	2.4	0.7	4.7	25.5	6.6	47.5	97.9	97.9	−5.2

Table 13 (continued)

	$g/y$	$G/y$	$g_0/y$	$g_1/y$	$g_{23}/y$	$g_0/g$	$g_1/g$	$g_{23}/g$	Total franchise	Economic franchise	Polity index
<b>Italy</b>											
First year	1862	n.a.	1862	1862	1862	1862	1862	1862	1861	1861	1861
Gaps <sup>a</sup>	0		0	0	0	0	0	0			
1861–1870	13.7		6.13	1.7	0.3	41.6	13.1	2.4	8.5	3.5	–4
1871–1880	11.7		4.6	1.9	0.3	35.8	15.4	2.1	9.0	3.7	–4
1881–1891	15.8		5.9	3.4	0.4	36.1	20.7	2.4	28.9	13.8	–4
1891–1900	14.7		5.9	3.2	0.5	38.5	20.5	3.3	28.0	13.1	–3.7
1901–1910	14.1		5.5	3.7	0.6	38.3	25.9	4.7	30.6	14.3	–1
1911–1920	34.8		19.3	3.6	1.7	58.1	15.2	6.2	96.4	45.4	–1
1921–1923	19.2		7.2	3.8	2.5	35.6	18.2	12.9	108.5	52.1	–7
<b>Netherlands</b>											
First year	1900	n.a.	1901	1901	1917	1853	1870	1917	1853	1853	1853
Gaps <sup>a</sup>	0		8	8	0	52	36	0			
1851–1860						42.6			10.7	4.6	–3
1861–1870						55.0	12.8		11.3	5	–3
1871–1880						55.9	17.6		12.2	5.4	–3
1881–1891									19.7	8.7	–2.7
1891–1900	8.4					41.2	37.2		34.1	14.6	–2
1901–1910	8.3		5	1.7		58.4	19.6		57.8	23.8	–2
1911–1920	14.4		8.8	3.1	2.2	61.9	20.9	13.5	76.9	31.7	4
1921–1930	13.6		5.3	2.0	3.9	37.9	15.3	29.5	96.2	81.5	10
1931–1938	18.9		6.5	1.9	4.8	37.4	10.8	27.6	98.3	84.0	10
<b>Sweden</b>											
First year	1865	1913	1865	1865	1865	1865	1865	1865	1872	1872	1872
Gaps <sup>a</sup>	42	12	53	53	53	52	52	52			
1861–1870	6.2		4.3	0.5	1.0	69.1	8.3	15.7			–4.8
1871–1880	5.1		3.4	0.3	0.9	67.5	6.6	16.6	22.3	10.2	–4
1881–1891	5.9		3.6	0.4	1.2	61.5	6.9	19.5	23.6	10.5	–4
1891–1900	5.7		3.5	0.43	1.1	62.5	7.8	19.7	24.1	11.0	–4
1901–1910									30.8	14.2	–4
1911–1920	7.5	11.4	3.7	1.1	1.0	56.4	15.2	15.4	77.1	32.6	1.6
1921–1930	9.0	14.9	2.9	1.3	1.9	39.4	17.5	25.1	96.4	88.2	10
1931–1938	10.1	17.4	2.7	2.05	2.5	31.2	23.1	28.5	97.1	89.5	10
<b>Denmark</b>											
First year	1855	1855	1869	1869	1869	1869	1869	1869	1849	1849	1849
Gaps <sup>a</sup>	6	6	60	60	60	60	60	60			
< 1850									73.7	25.7	2
1851–1860	5.9	9.0							72.5	25.5	2
1861–1870	7.9	11.1	3.3	0.2	0.2	55.9	2.7	2.5	71.8	25.2	0.3
1871–1880	5.3	8.7	3.1	0.15	0.15	62.2	3.5	3.1	75.8	26.4	–3
1881–1891	6.2	10.1	3.4	0.4	0.5	62.3	6.7	7.6	80.9	28.1	–3
1891–1900	6.2	10.5	2.9	0.6	0.9	53.8	10.7	16.9	85.7	29.6	–3
1901–1910	5.8	10.9	2.3	0.6	1	48.8	12.2	21.4	86.7	29.5	–3
1911–1920	5.9	11.2							91.5	57.7	4.8
1921–1930	7.2	13.7	1.2	0.6	2.7	18.5	10.2	42.2	94.3	80.7	10
1931–1938	7.9	16.2	1.5	1.0	4.8	16.0	10.5	51.3	96.1	82.9	10

Table 13 (continued)

	$g/y$	$G/y$	$g_0/y$	$g_1/y$	$g_{23}/y$	$g_0/g$	$g_1/g$	$g_{23}/g$	Total franchise	Economic franchise	Polity index
United Kingdom											
First year	1831	1840	1889	1889	1889	1840	1840	1840	1831	1831	1831
Gaps <sup>a</sup>	0	66	22	22	22	70	70	70			
<1850	10.6	11.5				39	9.4	9.4	12.5	5.6	1.5
1851–1860	10.1	10.7									3
1861–1870	7.2	8.7							24.7	11.4	3
1871–1880	6.2	9.1							32.2	14.9	3.4
1881–1891	6.9	9.2	3.3	0.1	0.6	58.9	1.2	9.8	52.2	24.0	7
1891–1900	7.6	12.6	5.7	0.1	0.75	70.3	0.9	10.1	62.5	28.9	7
1901–1910	8.3	12.5	4.0	0.1	1.05	63.8	1.3	16.4	61.9	28.6	8
1911–1920	32.5	31.4	21.2	0.5	1.7	37.7	2.4	9.9	88.6	74.8	8
1921–1930	19.2	24.8	4.2	0.8	4.7	25.1	4.6	28.6	90.8	80.2	9.8
1931–1938	20.7	25.5	4.6	0.5	6.0	28.2	3.3	37.4	99.7	97.2	10
Switzerland											
First year	1880	1880	n.a.	n.a.	n.a.	n.a.	n.a.	n.a.	1882	1882	1882
Gaps <sup>a</sup>	41	49									
1881–1891	1.8	16.5									10
1891–1900	1.8	15.0							79.6	38.3	10
1901–1910	2.3	11.1							79.2	38.1	10
1911–1920	2.1								77.5	37.6	10
1921–1930		14							79.6	38.0	10
1931–1938	4.5	17.2							86.0	40.5	10

Notes: n.a. means not available;

<sup>a</sup>records the number of gaps in each of the spending series. As explained in the text, we have filled these by linear interpolation. The data reported in the table are the raw data and the averages do not include these interpolations.

### A.1. Notation and summary statistics

The following notation is used:

1.  $g$  is central government spending in current domestic prices.
2.  $y$  is nominal GDP (or NNP if GDP numbers were not available).
3.  $g_0$  is central government spending on defence, general administration, the judiciary and the police (“security”) in current domestic prices.
4.  $g_1$  is central government spending on economic services, transport and communication (“long-term public services”) in current domestic prices.
5.  $g_2$  is central government spending on health, public housing, and education (“collective goods”) in current domestic prices.

6.  $g_3$  is central government spending on social security (“transfers”) in current domestic prices.
7.  $g_{23}$  is the sum of  $g_2$  and  $g_3$ .
8. *gdp per capita* is real GDP at international 1985 US relative prices, adjusted to exclude the impact of border changes, per capita.
9. *old* is the fraction of the population above 65 years of age.
10. *urbanization rate* is the proportion of the population that lives in towns with more than 10,000 inhabitants.
11. *population* is the size of the population (often interpolated between census years) in 1000s.
12. *total franchise* is the electorate in percentage of the population of 20 and older (parliamentary elections).
13. *economic franchise* is the electorate in percentage of the enfranchised age and sex group, before female suffrage, male population only (parliamentarian elections).
14. *turnout rate* is the number of votes (or total valid votes) recorded in each parliamentary election as a percentage of the electorate.
15. *female franchise* is a dummy variable equal to 1 in each year after the female suffrage and equal to 0 in each year before that.
16. *proportional rule* is a dummy variable equal to 1 if proportional representation is used and equal to 0 if the majority rule is used.
17. *polity index* is an index of political competition, ranging from  $-10$  (extremely autocratic) to  $+10$  (fully functional democracy). The polity dummy is equal to 1 if the polity index is positive and equal to 0 otherwise.

## A.2. Data sources

The data regarding the extension of the franchise and election participation (turnout) is from three main sources: Flora et al. (1983), Marshall and Jagers (2000) and Cook and Paxton (1998). The economic data is from Flora et al. (1983), Mitchell (1998), and Maddison (1991). Government spending data from Flora et al. (1983) has been supplemented by nominal GDP data from Mitchell (1998). The data on real GDP at international 1985 US relative prices is constructed from Maddison (1991) by splicing the real GDP at national prices series onto the PPP adjusted GDP numbers for 1950. The demographic data is from Maddison (1991).

## A.3. Data comparability and consistency

With respect to the franchise data, a number of problems of international and intertemporal comparability should be pointed out (see Flora et al., 1983, p. 89). First, in some countries, enfranchised voters had to register to be allowed to vote. This produces a discrepancy between the number of registered and qualified voters. Second, in cases where the parliament was partially renewed each year, it is difficult to calculate the number of enfranchised citizens as well as voter turnout. Fourth, where plurality voting was common, voter turnout is calculated as a fraction of the

maximum number of potential votes, rather than as fraction of potential voters. Finally, where uncontested constituencies were common, the estimated turnout rates tend to underestimate the true turnout. All this makes the political data, and in particular the turnout data, less reliable for the beginning of the sample period than later on.

With respect to the data on fiscal outcomes, there are numerous problems with international and intertemporal comparisons that derive from variations in the concept of public expenditure and variations and incompleteness of the functional classification of total expenditure. Three particular problems should be pointed out. First, we focus on data on central government spending, because more data is available for the early periods than for general government spending. This poses a problem for the comparison between countries with different degrees of fiscal decentralization. Second, although an attempt has been made by [Flora et al. \(1983\)](#) to transform the national definitions of spending categories into a consistent functional-based system, it was not always possible to do so. Also, a big residual is present for most countries (this is also true today, as some expenditures are not allocated by function), and poses a problem for comparison, as the residual in addition to interest payments covers all expenditures that could not otherwise be allocated.

#### *A.4. Construction of the data sets used in the regression analysis*

The original data set contains many gaps. For the fiscal outcomes and for the demographic data, they are simply due to lack of data. For the franchise variables, they arise because the franchise is only recorded in election years. In off-election years, we have no observations of the two key variables. We have dealt with the gaps in the fiscal outcome series and in the demographic data by interpolating linearly the missing observations. For the franchise data, we have made the assumption that the franchise was constant between elections. [Table 14](#) provides summary statistics for the variables included in our regression analysis.

Table 14  
Summary statistics of variables used in the regressions for all countries, till 1938

Variable	Obs.	Countries	Mean	Std. dev.	Min	Max
$g/y$	761	12	10.1	7.1	1.6	63.7
$g_0/y$	429	11	4.7	4.6	1.0	39.4
$g_1/y$	429	11	1.3	1.2	0.1	8.1
$g_{23}/y$	413	11	1.9	1.6	0.1	8.1
Economic franchise	252	12	61.5	35.2	1	110.1
Polity index	916	12	2.6	6.0	-9	10
Gdp per capita	703	12	2435	923	723	4938
Urbanization rate	788	12	30.7	14.8	5.3	71.2
Population	746	12	17620	18923	1392	67883
Old	864	12	6.5	1.4	1.9	20.2



#### A.5. Country notes

Below we record some specific features of the data for each of the 12 countries and highlight some important historical events.

1. Norway. Social security expenditures are included in ( $g_{23}/y$ ). For the period, 1815–1897, registration was required to vote, and only about 65% of the qualified voters was registered. Norway had far-reaching home-rule under the Swedish king from 1814 and a democratic constitution. In 1905, the union with Sweden was abolished. Neutral during World War I.
2. Austria. Government spending data excludes social security spending. Regional differences in voting procedures 1873–1901. Involved in wars in 1859 (France, Sardinia); 1866 (Prussia); 1878 (occupation of Bosnia-Hercegovina); 1914–1919 (World War I).
3. Belgium. NNP used instead of GDP. Government spending data excludes social security. For 1894–1914, it was not possible to adjust for the fact that plural voting was common. Likewise, partial renewal of parliament over the period 1831–1914 makes it hard to quantify the extension of the franchise on a national basis. Adopted independent constitution in 1830 (no longer part of the Netherlands). Not involved in wars until World War I.
4. Finland. Government spending data excludes social security. Franchise data before 1901 not available. Under Russian dominance until 1906 where a democratic system was introduced. The independence recognized by USSR after the revolution in 1917. Civil war in Finland (1917–1919); the Winter War with USSR (1939–1940).
5. France. Government spending data includes social security, but it is combined with health spending. For the period 1815–1846, the franchise data are estimates with an error margin of 0.1–0.2 percentage points. The Napoleon wars (1799–1815); February revolution (1848); no opposition allowed (1852–1859); Krim wars (1853–1856); War with Prussia/Germany (1870–1871); World War I (1914–1919).
6. Germany. Central government data excludes social institutions. For the period 1871–1912, the franchise data refers to the territory of the German Empire and from 1919–1933 to that of the Weimar Republic. The period 1933–1939 is not covered by our sample. The German unification under Bismarck and the leadership of Prussia (1871); war with Denmark (1864); war with Austria (1866); war with France (1870–1871); World War I (1914–1918).
7. Italy. Government spending data excludes social security. Unification of Italy in 1866. World War I (1914–1919). Mussolini's march to Rome (1923). The period 1924–1939 is not covered by our sample.
8. Netherlands. Government spending data excludes social security. Valid votes are used instead of total votes to estimate turnout rates. Neutral during World War I. Luxemburg counted as part of the Netherlands.
9. Sweden. Government spending data includes social security. Napoleon wars (1805–1815), Norway part of the Swedish Kingdom until 1905. First democratic constitution in 1866. Neutral during World War I.

10. Denmark. Government spending data includes social security. Until 1898, the open ballot was used in some but not all districts. The estimated turnout rate assumes that the participation rate in districts with open ballots was as high as in districts with secret ballot. The Three Years War (1848–1850); War with Germany/Prussia (1964); Natural during World War I.
11. United Kingdom (understood as England, Wales and Scotland, but not Ireland). The data for the functional allocation of central government spending includes social security expenditures. During the 19th century, the combination of plural voting and uncontested constituencies makes it hard to calculate turnout rates accurately. Napoleon wars (1803–1815); Krim wars (1853–1856); Cypress war (1878); Suez crisis and war in Egypt (1882); World War I (1914–1919).
12. Switzerland. NNP is used instead of GDP. Government spending data includes social security. During the period 1848–1863, the legal framework of elections differed from region to region. Valid votes are used instead of total votes to calculate turnout rates. Switzerland's neutrality guaranteed in 1815.

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