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Toke S. Aidt  
Peter S. Jensen

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# Democratization and the Size of Government: Evidence from the Long 19<sup>th</sup> Century

## Abstract

We study the consequences of franchise extension and ballot reform for the size of government in Western Europe between 1820 and 1913. We find that franchise extension exhibits a U-shaped association with revenue per capita and a positive association with spending per capita. Instrumental variables estimates, however, suggest that the U-shaped relationship may be non-causal and our fixed effects estimates point to substantial cross-country heterogeneity. Further, we find that the secret ballot did not matter for tax revenues per capita but might have expanded the size of government relative to GDP.

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*Toke S. Aidt*  
*Faculty of Economics*  
*University of Cambridge*  
*United Kingdom – Cambridge CB3 9DD*  
*toke.aidt@econ.cam.ac.uk*

*Peter S. Jensen*  
*Department of Business and Economics*  
*University of Southern Denmark*  
*Campusvej 55*  
*Denmark - 5230 Odense M*  
*psj@sam.sdu.dk*

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# 1 Introduction

One of the most striking stylized facts in public finance is the enormous expansion of the size of the public sector that has taken place over the past two centuries. Central government real expenditure was less than 5% of GDP in most countries in Europe, North America and Oceania in 1870; today the average is just under 20% (Tanzi and Schuknecht 2000:Table II.1). This expansion is part of a complex transformation of economic, social, and political institutions – referred to by Gundlach and Paldam (2009) as the grand transition – which takes place as societies develop. One important challenge for public choice scholars interested in understanding this transformation is, therefore, to come to grips with the particularities of the driving forces behind this shift in the balance between the public and the private sectors. Many factors are undoubtedly at play. Some factors are political, some relate to warfare, and some are economic or ideological.

The purpose of this paper is to revisit the evidence on the determinants of the size of government. We are particularly interested in political factors related to changes in the fundamental institutions that govern elections. As pointed out by Peter Lindert in his seminal piece on the rise in social spending (Lindert 1994), the much-studied postwar experience of growth in government is a weak scale for weighing competing theories because many of the key variables exhibit very little variation. This is particularly true for the rules governing who could vote and how. Improving our understanding of the relationship between reform of electoral institutions and the size of government thus requires a long historical perspective. We believe that much can be learned from exploring data for the long 19th century (1820 to 1913) in Western Europe, which was a period of large-scale economic and political transformation. This, among other things, entailed a political transition from absolute to constitutional monarchy or republican governance. It is this deep historical coverage that allows us to contribute new evidence to an already substantial literature.

We test three political hypotheses: (a) the franchise extension hypothesis, which contends that reforms that enlarge the electorate lead to fiscal expansion; (b) the retrenchment hypothesis, which contends i) that the initial franchise extension by which the incumbent elites begin to share power with the emerging middle class led to fiscal contraction and ii) that the expansion predicted by the franchise extension hypothesis does not materialize until the emerging working class gets the vote; and (c) the ballot hypothesis, which contends that the introduction of the secret ballot by itself (i.e., for a given extension of the franchise) leads to fiscal expansion. Testing these hypotheses is complicated by the fact that suffrage (and ballot reform) may be endogenous to the evolution of the fiscal state or both may be driven by the same underlying, but unobserved, factors. All our tests explore within-country deviations from a common time trend (fixed country and fixed time effects). This helps reduce the risk of spurious results. In addition, inspired by the theories of franchise extension developed by Acemoglu and Robinson (2000, 2006), Boix (2003:Chap. 1), and others, we propose to use revolutionary events (suitably weighted) in other countries to instrument for suffrage reforms. This allows us to get a peek into the potentially causal nature of the relationship between suffrage and the size of government.

On top of these political hypotheses, we also test (1) the war-finance hypothesis, which links growth in government to the need to finance external wars, (2) the modernization hy-

pothesis, which links growth in government to the forces of economic development, and (3) the globalization hypothesis, which views growth in government as insurance against the risk of external shocks. We stress, however, that our data are less well-suited for those purposes and these tests suffer from obvious endogeneity problems.

We are certainly not the first to explore these issues – we review the existing literature in more detail in Section 6 – but two features of our study set it apart from many existing ones. First, it takes a long historical perspective. Previous work using historical data for Europe, the US states, the Americas, or combinations of these is at best able to track developments back to around 1870, thus missing a significant part of the 19th century. An important exception is the work by Dincecco (2009, 2011). He studies links between political transformations and public finance in Europe between 1650 and 1913 with an emphasis on the transition from absolutism to limited government where constraints are imposed on the way rulers (kings, mostly) could spend public funds. We focus on a shorter time span (1820 or 1870 to 1913) and on the details of the rules governing elections rather than on the division of authority between kings and parliaments over the annual budget, which is the main focus of Dincecco’s work.<sup>1</sup> In particular, we zoom in on two mechanisms through which limits were imposed on rulers: reallocation of voting rights and making the vote a private rather than a public act. This, in turn, allows us to separately test the three political hypotheses put forward above and to provide richer insights into the transformation from absolute to limited government. Second, we use IV techniques to dig deeper into the nature of the link between franchise extension and growth in government. This offers a unique opportunity for getting a better understanding of the causality issues involved in the Western European transition from absolutism to universal male suffrage. Most studies of the effect of suffrage extensions on the size of government do not apply IV techniques and those that do focus on the last part of the 19th century; see Kim (2007) and Aidt and Jensen (2009b).

These advantages are not without costs, however. While the long historical perspective makes it possible to study the absolute size of government measured by real central government revenue and spending per capita, it is impossible to study the size of government relative to GDP for the entire 19th century. The relevant data simply do not exist for a large enough set of countries for the period before 1870 to make econometric testing possible. Any quantitative study of the evolution of the relative size of government in Europe must necessarily be confined to the last part of the 19th century (or to the modern period). As a supplement to our main sample, we make use of a shorter sample of eight Western European countries between 1870 and 1913. By doing so we obviously lose the advantage of the long historical perspective, but we gain an opportunity to test our hypotheses using measures of the relative size of government.

We find that political institutions mattered for the evolution of the size of government over the long 19th century, but they did so in much more complex and intriguing ways than often is assumed. For the long 19th century (1820 to 1913), we find some interesting differences between the revenue and expenditure sides of the budget. With respect to real revenue

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<sup>1</sup>He defines limited government as being established in the year that parliament gains the constitutional right to control the national budget on an annual basis and had had that right for at least two consecutive decades (Dincecco 2011:28).

per capita, our fixed effects estimations are consistent with the retrenchment hypothesis. They suggest that a real expansion of government revenue per capita did not follow in the footsteps of franchise extension until roughly 40% of adult males already could vote. We emphasize, however, that one should be careful before attributing a causal interpretation to this correlation, as our instrumental variables estimates are statistically insignificant. We also stress that there is a high degree of country heterogeneity in the effects. With respect to the expenditure side of the budget, the evidence both from the fixed effects panel model and from the instrumental variables estimates support the franchise extension hypothesis and the evidence on retrenchment is weak. Our tests on the short sample from 1870 to 1913, moreover, show that government revenue relative to GDP is uncorrelated with suffrage. For this period, all of the action is on the expenditure side and we, again, find evidence consistent with the franchise extension hypothesis.

The European data for the long 19th century clearly reject the ballot hypothesis, i.e., the secret ballot was not associated with higher real tax revenue or spending per capita. Our tests on the shorter sample from 1870 to 1913, however, uncover a positive correlation between the ballot and the sizes of government revenue and expenditure relative to GDP. This suggests that the secret ballot might have mattered more for the relative than for the absolute size of government, an interpretation which is supported by the fact that the secret ballot is uncorrelated with real revenue and spending per capita also for the period 1870 to 1913. With respect to the secondary hypotheses, we find that war shocks generate persistent growth in government at least over the long 19th century, but that neither the modernization nor the globalization hypothesis is well-supported by any of our data.

The rest of the paper is organized as follows. In Section 2, we develop the hypotheses that govern our empirical work. In Section 3, we present our data. In Section 4, we discuss our empirical strategy and introduce our instrumental variable. In Section 5, we report the results. In Section 6, we review the existing empirical literature on suffrage reform and the size of government in the light of our findings. In the final section, we offer some concluding remarks.

## 2 Hypotheses

The increase in the size of government that occurred over the past centuries in what now constitutes the developed world can be attributed to a host of different factors.<sup>2</sup> At the risk of over-simplifying, we may identify three different classes of explanations: political, structural, and military. Our main concern in this paper is the political factors, but we also want to engage with two of the structural explanations and with the role played by warfare.

The political explanations view the size (and composition) of the fiscal state at any given point in time as an equilibrium outcome in a political market induced by the prevailing institutions. Although these explanations certainly acknowledge that changing economic conditions (income growth, rising inequality, international trade integration and so on) can shift the political equilibrium, the emphasis is on how changes in the political institutions them-

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<sup>2</sup>Lindert (2004a,b) and Holsey and Borchering (1997) provide comprehensive overviews of the literature.

selves induce observable shifts in the political equilibrium. Causality runs, it is contended, from changes in political institutions to changes in the size of government. As stressed by Engerman and Sokoloff (2005, 2011:Chap. 4), one of the most fundamental political institutions in any society is the one governing elections. Historically, the power to elect or appoint leaders – kings or parliaments – was the privilege of small elites, who derived substantial benefits from this privilege.<sup>3</sup> Today in modern democracies, political power is more evenly spread as elections are governed by the principle of one (adult) person, one vote. Since franchise extensions often grant the right to vote to citizens who tend to benefit particularly from more government spending, there is a presumption that suffrage reforms are associated with growth in the size of government. The seminal paper by Meltzer and Richard (1981) formalizes the logic behind this presumption. They consider a situation where the government is elected by majority vote by the subset of citizens who can vote. The government provides a universal public good which is financed by a proportional tax on income. The equilibrium policy is determined by average income in the entire population relative to median income amongst those who can vote. A reduction in the median income of those with the right to vote, brought about by, for example, a liberalization of income or wealth qualifications on the franchise, results in higher spending and taxation. The reason is that the fiscal system redistributes to those with lower incomes. Accordingly, as the median voter becomes poor, he votes for higher spending (and taxes).<sup>4</sup> This we refer to as the franchise extension hypothesis.

There are, however, reasons to believe that the consequences of franchise extension might be more complex. Some have argued that the relationship is, in fact, U-shaped rather than monotonically increasing in the share of the population with the right to vote (Lindert 2004a:Chap. 7; Plumper and Martin 2003; Hausken et al. 2004; Aidt et al. 2010). Starting from a very narrow franchise which effectively allows only the members of the elite to vote (or where no formal voting takes place at all), an extension of the right to vote to broader segments of the elite or to the middle classes may lead to retrenchment. As the franchise is further extended to include (relatively poor) working class voters the trend reverses and the franchise extension becomes associated with larger government. While this U-shaped relationship can be rationalized in a number of ways, the formulation by Plumper and Martin (2003) is particularly appealing in our context. They consider a more complex fiscal system than Meltzer and Richard (1981). In addition to a universal public good, the government also provides rents. The key difference between the two categories of spending is that a public good, once provided, can be enjoyed by all, while rents are specific (private) to the individuals who get them. Consequently, the fiscal cost of providing rents is increasing in the size of the group that is targeted. Rents and public goods are financed by a proportional tax on income. The government's objective is to maximize a political support function. The function trades off the benefit of providing public goods to all citizens, rents to the "constituency" of

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<sup>3</sup>See Congleton (2007, 2011) for an insightful account of how and why the allocation of power between king and parliament shifts over time.

<sup>4</sup>Meltzer and Richard (1981) build on the median voter model. This model is not ideal for thinking about complex fiscal systems with many policy dimensions. Hettich and Winer (1999) and Tridimas and Winer (2005), however, show that the franchise extension hypothesis holds within the context of the more appropriate probabilistic voting model.

the government, and the fiscal cost of the spending program. In a non-democracy, the government is appointed by a king and his constituency is a narrow elite of nobles. It is optimal for the king to buy their support using rents because the group of nobles is small. In democracy, the government is elected by a subset of citizens that extends beyond the nobility. Its constituency is larger than under non-democracy (at least 50% of the enfranchised). A franchise extension has two effects in this framework. First, an extension from non-democracy to limited democracy with a restricted franchise leads to a fall in spending and taxation. The fundamental reason is the possibility that the composition of spending changes. While under non-democracy the king maximizes his support by providing rents to the nobles, under limited democracy this strategy becomes too expensive and the nobility's rents are eliminated. The point is that the group of enfranchised voters is too big. It is cheaper for the elected government to buy support by providing public goods which, once provided, benefit all. The net result of this is a fall in total taxation. Second, starting from a limited democracy, further enfranchisement of citizens with lower incomes leads to an increase in taxation and spending on public goods, basically for the same reason as in Meltzer and Richard (1981). We refer to this as the retrenchment hypothesis.

The allocation of voting rights is not the only aspect of the election process that matters for fiscal choices. In particular, Anderson and Tollison (1990) point out that the ballot rules are important.<sup>5</sup> Under open voting, electoral corruption often thrives and the old elites can, even for fairly large electorates, use social control or direct vote buying to keep the demands for larger government at bay.<sup>6</sup> This suggests that the introduction of the secret ballot should be associated with larger government as vote buying becomes uneconomical.<sup>7</sup> We refer to this as the ballot hypothesis.

These are the three main hypotheses that we propose to test and for which we believe a long historical perspective is essential for proper inference. In addition, however, our data allow for rudimentary tests of the war-finance hypothesis and of two leading structural explanations for growth in government. The war-finance hypothesis links growth in government to the need to mobilize resources to fight wars. While it is beyond doubt that war is costly and is inevitably associated with large spikes in public spending, it is less obvious that these spikes will have longer term effects. One mechanism through which this could happen, however, is the so-called displacement effect (Peacock and Wiseman 1961; Higgs 1987). The hypothesis is that while government can always find new ways of spending revenue, citizens do not want taxation to exceed a 'tolerable' level. During wars, taxation necessarily exceeds the pre-war 'tolerable' level and once the war is over the 'tolerable' level has shifted up. This, the argument goes, leaves room for peace-time fiscal expansion. Another, perhaps more plausible,

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<sup>5</sup>Persson and Tabellini (2003) have demonstrated that the election rule (majoritarian versus proportional rule) and the distinction between presidential and parliamentarian democracies exert important influences on the size and composition of the public finances in the modern period (after World War II). All of the countries in our sample, with the exception of Belgium where proportional rule was introduced in 1894, employed majority rule until 1913. This makes it impossible to use our data to study the effect of the election rule on growth in government.

<sup>6</sup>For insightful discussions of the economics of vote markets in a range of different contexts, see Baland and Robinson (2007, 2008), Collier and Vicente (2012), Stokes (2005, 2011) or Heckelman (1995).

<sup>7</sup>Stokes (2005), in an interesting study of electoral corruption in Argentina in the 1990s, shows that vote markets can operate even under secret ballot. Yet, it is clear that secrecy makes it more difficult to trade votes.

possibility recently pursued by Besley and Persson (2011) and by Dincecco (2009, 2011) and tested in Dincecco and Prado (2012) is that wars foster state capacity building, in particular in terms of improved tax collection technologies. Once the need for war-finance is over, the capacity to raise more revenue persists and may, if there is demand for public services and redistribution, lead to a permanent fiscal expansion.<sup>8</sup>

The structural explanations center on the link between development, economic growth and globalization, on the one hand, and growth in government on the other.<sup>9</sup> One structural explanation, known as the modernization hypothesis or simply as Wagner's law (Wagner 1883), contends that economic development (such as rising incomes, urbanization and higher educational standards) in various ways leads to greater demand for public services, social insurance and redistribution, and ultimately to larger governments. Another structural explanation contends that globalization – understood as more openness in trade of goods and services and less restrictions on capital flows – exposes economies to new risks. Societies may respond to these by increasing the size of the public sector because doing so provides social insurance (Rodrik 1998). We refer to this as the globalization hypothesis.<sup>10</sup>

To summarize, the main purpose of the paper is to test the following political hypotheses regarding growth in government:

1. **The franchise extension hypothesis:** the extension of the franchise leads to larger government.
2. **The retrenchment hypothesis:** the extension of the franchise leads to smaller government initially, but causes an increase eventually.
3. **The ballot hypothesis:** the secret ballot leads to larger government.

In addition, we also test the following secondary hypotheses:

1. **The war-finance hypothesis:** participation in war leads to larger peace-time government.
2. **The modernization hypothesis:** income growth, urbanization, and rising education standards lead to larger government.
3. **The globalization hypothesis:** trade integration leads to larger government.

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<sup>8</sup>Sabine (1966) illuminates this logic very clearly in his analysis of the British income tax.

<sup>9</sup>See Aidt and Dutta (2007) for theoretical analysis of the relationship between economic growth and growth in the relative size of government.

<sup>10</sup>Another structural explanation for growth government – Baumol's cost disease explanation – emphasizes the limited scope for productivity growth in the production of public services (Baumol 1967). The cost inflation that follows from wage growth in the private sector then pushes up government expenditures. Yet another theory centers on variations in the deadweight cost of taxation (Becker 1983; Becker and Mulligan 2003; Aidt 2003). We are unable to test these explanations with the data at hand.



### 3 Data

Our analysis is based on two samples of Western European countries. Our main sample covers the long 19th century from the end of the Napoleonic wars (1820) to the onset of World War I (1913) for nine Western European countries.<sup>11</sup> The countries are Italy,<sup>12</sup> Austria,<sup>13</sup> Belgium,<sup>14</sup> France, the Netherlands,<sup>15</sup> Sweden, Denmark,<sup>16</sup> the United Kingdom, and Prussia.<sup>17</sup> The secondary sample covers only the last part of the 19th century (from 1870) and also runs to 1913. It consists of the following eight countries: Italy, France, the Netherlands, Sweden, Denmark, the United Kingdom, Norway,<sup>18</sup> and Switzerland. For mnemonic purposes, we refer to the main sample as the *long sample* and the secondary sample as the *short sample*. The long sample buys us deep time coverage but restricts our ability to measure the size of government. The short sample buys us flexibility in how we measure the size of government, but at the cost of less deep time coverage. The differences between the two samples in terms of country coverage are dictated by data availability.

For the long sample, we measure the size of government by the variables *revenue per capita* and *spending per capita*. These variables are constructed by Dincecco (2011) from a large number of primary and secondary sources. They measure total revenue and total expenditure of the central government in each country per capita in gold grams. The conversion into gold grams makes the data internationally comparable and real (see Dincecco 2011:Appendix A.2. for details on how this is done). These are measures of the absolute size of government. It is not possible to measure the size of government relative to GDP for the long sample.

Figure 1 shows the evolution of *revenue per capita* and *spending per capita* for each country in the long sample over time. We observe a number of facts. First, most of the increase in the absolute size of government came late in the 19th century. For a number of countries, *revenue* and *spending per capita* are effectively flat until around 1860. Second, we observe spikes that can be attributed to war. Third, we observe that there is substantial heterogeneity in the growth experience across the nine countries. Fourth, *spending per capita* largely follows *revenue per capita* reflecting that balanced budget rules were common in Europe at the time. Yet, we note that peace-time deficits sometimes emerged and persisted, as in the Netherlands and Austria.

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<sup>11</sup>Since we cannot trace all variables back to 1820 for all nations and because some of the countries were created within the sample period, the resulting panel is unbalanced consisting of 655-670 observations. The entry and exit years for each country are listed in column one of Table 2.

<sup>12</sup>Unified in 1861.

<sup>13</sup>Data from 1820 refer to the geographical unit comprised of Austria-Hungary.

<sup>14</sup>Independent of the Kingdom of Netherlands in 1831.

<sup>15</sup>Data from 1820. For the period 1820 to 1831, the fiscal data exclude the net transfer from what in 1831 becomes Belgium.

<sup>16</sup>Independent polity in 1820, but fiscal data not available until 1864.

<sup>17</sup>Prussia exits the sample in 1867 prior to the creation of the German Empire in 1871.

<sup>18</sup>Norway was in union with Sweden until 1905. However, it had its own parliament (Storting) from 1815. The parliament decided on taxation and expenditure (except for military spending). Foreign policy was, in contrast, controlled by the Swedish King (and parliament). This justifies treating Norway as an independent unit for the purpose of studying the evolution of the size of government.

Figure 1: Revenue and spending per capita 1820-1913 for nine Western European countries.

For the short sample, we can measure the size of central government spending or revenue in percentage of GDP by the two variables *spending/GDP* and *revenue/GDP*.<sup>19</sup> These are measures of the relative size of government. Table 1 reports decade averages by country. Perhaps the two most striking facts about the evolution of the relative size of government over the period from 1870 to World War I are, firstly, that there was very little growth in government in any country and, secondly, that there were substantial differences in the size of government across countries. With respect to the latter fact, it is interesting to note that the size of government relative to the economy in Denmark and Sweden – two of the ‘big-government’ countries of the modern period – was about half the size of those of France and Italy at the turn of the 19th century. It is also interesting to notice that the relative size of government in France actually contracted during the period.

Table 1: The evolution of the relative size of government in eight Western European countries, 1870-1913.

We have collected empirical proxies aimed at testing each of the six hypotheses discussed above. The franchise extension and the retrenchment hypotheses require a quantitative measure of the extension of the franchise. For both samples, we use the variable *suffrage* for this purpose. It records the size of the electorate in percentage of all men of voting age.<sup>20</sup> We interpret it as a measure of income, property holding, and wealth restrictions on the right to vote. We code *suffrage* as being equal to zero during periods (in the early) 19th century when and where no elections were held. The trend towards universal franchise is well-documented elsewhere (Aidt et al. 2006) and was reached around 1920 for adult males. Table 2 reports, in column two, the years of major suffrage reform in each of the countries we study. In columns three to five, we report the value of *suffrage* for the year in which the country enters the relevant sample, in 1870 (the starting year of the short sample), and the year when they exit (1913, except for Prussia). For the countries in the long sample, we observe the full range from non-democracy to universal male suffrage. For the short sample, in contrast, we observe only the transition from limited democracy to universal male suffrage, with Italy and the Netherlands having the most restricted franchise (around 10%) in 1870.

<sup>19</sup>The source for these data is Flora et al. (1983).

<sup>20</sup>The data refer to the right to vote in parliamentary elections to the lower chamber. Prussia had a franchise that divided voters into three classes according to tax payment. Although more than 80% of adult males could vote in the third class, this group of voters had little influence on who got elected and, based on data from Kock (1984:Table 3a) and census data, we define *suffrage* as the percentage of voters in classes one and two relative to the adult male population. Austria had a system based on a number of Curia (the members of which elected a subset of the members of parliament). We define *suffrage* using voters in Curia III and IV (electing 70% of the seats) until 1891 and Curia V after that. For the other countries, the franchise to the lower chamber was almost equal and we have not made any adjustments. The source of these data is Kock (1984), Flora (1983) and own coding as explained in Aidt and Jensen (2011).

It is important to keep in mind, however, that the influence of the electorate on government policy could and often was restricted in ways that are not captured directly by the number of qualified voters. A leading example of this is the Prussian three-class franchise. Under this system all males over 25 could vote, but each voter in each electoral constituency was ranked by tax contribution and placed in one of three categories or classes. The contributors to the top third of the tax revenues in a district were in the first class, the second third was in the second group and the rest were in the third group. The result of the election in each class determined the same number of electors who in turn elected the winning candidate. Consequently, the many voters of the third class had very little influence and we use the number of voters in class one and two as our measure of the franchise extension in Prussia. On top of this, in Prussia as in most other European countries, significant political power was retained by un-elected upper chambers and/or by the king or his appointed ministers. These and others restrictions on how much influence voters had on policy tend to make it difficult to uncover evidence of the franchise and retrenchment hypothesis. Yet, we contend that *suffrage*, when appropriately adjusted as in the case of Prussia to take into account that the franchise was unequal, remains the best available proxy. It helps us isolate the effect of expansions in the size of the electorate on fiscal choices from other institutional features affecting the mapping from political representation to fiscal policy. Finally, none of the countries in our two samples introduced full women's suffrage before 1913.<sup>21</sup> Our sample period is, therefore, not suitable for new tests of the fiscal consequences of women's suffrage. The existing evidence from Western Europe (Aidt and Dallal 2008) and the United States (Lott and Kenny 1999), however, shows that women's suffrage did contribute to growth in government, but, in the case of Europe, only after World War I.

To test the ballot hypothesis, we record the year in which the ballot in each country became secret (see column six of Table 2 for the years). Secrecy in voting is often a matter of degree. We say that a country adopts the secret ballot in the year when it either adopts the Australian ballot<sup>22</sup> or, if some other system is used, our sources are clear that the (new) ballot rules were such that electoral corruption, vote buying, and intimidation were reduced to a minimum.<sup>23</sup> We code the dummy variable *secret ballot* equal to one for each year after the secret ballot was introduced and equal to zero before that.

We want to make sure that we isolate the impact of the franchise and the ballot rule from other aspects of the political institutions. To this end, we draw on the Polity IV database (Marshall and Jaggers 2000). It records various aspects of political authority patterns along five dimensions.<sup>24</sup> The so-called polity2 index quantifies these on a rising scale from -10

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<sup>21</sup>The first European country to grant voting rights to women was Finland (which is not in our samples) in 1906. Norwegian women who either themselves or whose husbands had income or wealth above a certain threshold were allowed to vote from 1909 onwards, but full women's suffrage was not achieved until 1913. In contrast, it was not uncommon for US states on the frontier to grant voting rights to women before the turn of the nineteenth century and New Zealand was also amongst the frontrunners by granting the vote to women in 1869.

<sup>22</sup>The Australian ballot requires that an official ballot is printed at public expense and distributed only at the polling stations. The official ballot lists the names of the nominated candidates of all parties and it is marked in secret at the polling station.

<sup>23</sup>Aidt and Jensen (2012) provide detailed justifications for the code choices.

<sup>24</sup>The five aspects are: i) constraints on the executive, ii) competitiveness, iii) openness in the process of

to +10, where -10 corresponds to nondemocracy and +10 to democracy. As pointed out by Vreeland (2008), it is important to consider carefully which sub-components of the overall index are relevant to the research question at hand and which might at worst distort proper inference. Our aim here is to control for aspects of institutions other than voting rights and secret ballot. One could, therefore, argue that we should exclude the two sub-components of the polity2 index that code aspects of political participation and effectively use the x-polity index proposed by Vreeland (2008).<sup>25</sup> While this, in principle, has merit, in practice, for our samples, it makes virtually no difference to any of the results if we use the x-polity index instead of the original polity2 index. This is also true for our instrumental variables estimates. Here, the concern is that the political competition sub-components partly code civil unrest. This creates a mechanical correlation between one of our instruments (*revolutionary threat*) and the polity2 index. Again, however, in practice, this does not matter for the results. Based on these considerations, we decided to use the polity2 index. After all, the two participation sub-components of the polity2 index do mostly capture things other than voting rights, so it is worth controlling for them. For the purpose of our estimations, we define the dummy variable *polity*, as being equal to one if the polity2 index is positive and zero otherwise, and include that in all estimations as a control. Column seven of Table 2 records the years for which the polity2 index is positive for each of the countries. We see that this shift towards ‘democracy’ rarely coincides with suffrage or ballot reforms.

The war-finance hypothesis requires a proxy for the cost of participation in war. The simplest way to quantify this is to define a dummy variable which is equal to one in years of war. This, however, fails to take into account the intensity of war and treats every war as being equally costly. It is for this reason that we prefer to follow Dincecco (2009, 2011) and use the variable *external war death* to test the hypothesis. This variable records the number of deaths on the battlefield per capita in each year of war and is zero in years of peace.<sup>26</sup>

To test the modernization hypothesis, we use data on national income, urbanization, and educational attainment.<sup>27</sup> More specifically, the variable *GDP per capita* is real GDP in international 1990 Geary-Khamis dollars, adjusted to exclude the impact of border changes, per capita, sourced from Maddison (2003). The variable *urbanization rate* is the proportion of the population that lives in towns and cities.<sup>28</sup> School enrollment data are not available

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executive recruitment, iv) competitiveness and v) regulation of political participation. The sum of the scores on the components is used to construct two summary variables, measuring democracy on a scale from 0 to 10 and autocracy from -10 to 0. The polity index is the sum of these two, and thus ranges from -10 (autocracy) to +10 (fully developed democratic institutions). In the estimations, we use the polity2 index, which is the version of the polity index that has been adjusted to make it suitable for time series analysis.

<sup>25</sup>Vreeland (2008) constructs the x-polity index by adding up the scores on the three sub-components that refer to the executive and excluding the two sub-components related to political participation.

<sup>26</sup>The source of these data is Singer and Small (1994) or <http://www.correlatesofwar.org/>.

<sup>27</sup>None of these socioeconomic variables are recorded with the same accuracy as we expect from modern data and thus are measured with substantial errors. GDP data, for example, are constructed from production data and often not on an annual basis. Other data are constructed from periodic censuses. The quality of the data improves towards the end of the sample period. This makes it a challenge to test the secondary hypotheses which we have put forward.

<sup>28</sup>We use the the proportion of the population who live in towns with more than 10,000 inhabitants in the short sample and the proportion living in cities with more than 20,000 inhabitants in the long sample. The sources for these data are Flora (1983) and Dincecco (2011), respectively.

for the early part of the 19th century. Accordingly, for the long sample, we use the dummy variable *school enrollment dummy* as a proxy. It is coded one for the years after which enrollment in primary education as a percentage of all 5-14-year-olds reached 60% and zero otherwise.<sup>29</sup> For the short sample, we can use actual data on the *school enrollment rate*, defined as enrollment in primary education as a percentage of all 5-14-year-olds.<sup>30</sup>

The globalization hypothesis is difficult to test on the long sample. The problem is that it is impossible to get data on, say, trade volumes or trade policy restrictions for the first half of the 19th century for most of the countries in the sample. For three of the countries (the United Kingdom, the Netherlands, and France), we gathered data on import-weighted average ad valorem tariffs (AVEs), calculated as the ratio of customs duty revenue to total imports for domestic consumption from the 1830s, but for the rest, such data become available around 1860 only (Lampe and Sharp In Press). Consequently, for the long sample, we are forced to use a very rudimentary proxy for globalization. We construct, based on information given in Meissner (2005),<sup>31</sup> the dummy variable *gold standard*. It is equal to one if a country is on the gold standard in a given year and zero otherwise. The logic is that being on the gold standard fosters trade integration. This is our measure for globalization for the long sample. For the short sample, which starts in 1870, better proxies are available and we use two different ones. Firstly, as a direct measure of globalization, we build on Lopez-Cordova and Meissner (2008) and use the variable *trade volumes* defined as exports plus imports relative to GDP. Secondly, Lampe and Sharp (In Press) have constructed import-weighted average ad valorem tariffs for all the countries in our short sample from 1870 onwards. We use this variable, which we call *trade protection*, to capture trade policy restrictions. The main problem with this as a measure of globalization is that countries with low taxation capabilities tend to generate considerable amounts of income from tariffs on consumption goods with low income elasticities. This can cause an upward bias in the measured level of protection. It also means that *trade protection* will partly capture a revenue effect and partly a protection or globalization effect. In the estimations, we use both measures.

The 19th century witnessed a rise of ideologies, most notably liberalism and socialism. As emphasized by Congleton (2011), this process may have influenced both suffrage reforms and fiscal choices. While this certainly rings true, from an econometric point of view, it is difficult to incorporate and test for this in an analysis of the long sample. However, insofar as the rise of new ideologies can be viewed as common shocks, we can (and do) control for them by including time fixed effects.<sup>32</sup> In the analysis of the short sample, we can do better than that. We include a measure of the seat share of left-wing parties in the lower chamber of parliament.<sup>33</sup> This variable, *left-wing party share*, captures at least indirectly the rise of socialism during the second half of the century.

To get a sense of how these variables evolved over time and space, we report in the last

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<sup>29</sup>The dummy variable is constructed with enrollment data from Flora (1983), Mitchell (2003), and Becker and Woessmann (2010) and is based on the assumption that enrollment rates are non-decreasing over time.

<sup>30</sup>The sources are Flora (1983) and Mitchell (2003).

<sup>31</sup>We also consulted EH.net encyclopedia (eh.net/encyclopedia).

<sup>32</sup>These dummies are significant in all specifications reported below.

<sup>33</sup>As pointed out by Congleton (2011: p.263), left-wing parties were often left-liberals, rather than revolutionary reformers.

two columns of Table 2 the year in which school enrollment hits the 60% threshold as well as the years on the gold standard. Table A1 in the Appendix reports descriptive statistics.

[Table 2: Descriptive information]

## 4 Empirical strategy

It is clear from Figure 1 and Table 1 that the evolution of the size of government exhibits a high degree of persistence. The likely reason is that fiscal commitments and institutions carry over from one year to the next. To capture this, we estimate a partial adjustment model. We include one lag of the outcome variable (*revenue per capita*, *spending per capita*, *revenue/GDP* or *spending/GDP*) amongst the explanatory variables. We also include country and time fixed effects in all specifications. Consequently, all our estimates are exploiting within-country variation around a common trend. They are, therefore, not confounded by time invariant country-specific factors or by aggregate shocks common to all countries in the relevant sample. The partial adjustment model, however, raises a number of econometric issues. One potential issue is the Nickell bias (Nickell 1981). With almost 100 years of data in the long sample and with more than 40 years in the short sample, the simulations of Judson and Owen (1999) suggest that this is not an issue of great concern.<sup>34</sup> Another potentially more serious issue is auto- and spatial correlation in the error structure. The former may inflate the z-statistics and cause invalid inference in a fixed effects model (Bertrand et al. 2004). We deal with this by clustering the standard errors at the country level.<sup>35</sup> The latter possibility is taken into account by panel-correcting the standard errors, as recommended by Beck and Katz (1995).

The partial adjustment model with fixed effects rules out certain types of contaminating factors. However, it leaves open the possibility that the size of government and the franchise extension are driven by the same unobserved country-specific dynamic forces. To deal with this possibility, we take an instrumental variables approach. As always the challenge is to find an instrument that is sufficiently strongly correlated with the franchise extension process but uncorrelated with the unobserved time-varying component of the process that drives the size of government. To put it slightly differently, we need a variable that affects only the size of government through its effect on the franchise. Many theories of suffrage reform stress that the incumbent elites only reluctantly and only when faced with a credible threat of revolution and no other alternative will share political power with broader segments of the (male) population; see e.g. Acemoglu and Robinson (2000) or Boix (2003). Based on this

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<sup>34</sup>Judson and Owen (1999) show that the bias is very small in panels with more than 20 years of data. We have, as a robustness check, re-estimated the partial adjustment model on both samples with the bias-corrected least-squares dummy variable (LSDV) estimator proposed by Bruno (2005a, b). The point estimates are virtually the same as those reported in the text, but the standard errors are larger. The LSDV estimator is preferable to the GMM estimator proposed by Arellano and Bond (1991) in panels with few cross section units.

<sup>35</sup>In practice, we allow for country-specific first order serial correlation. The estimated coefficients are all small. Panel unit root tests reject that the errors have unit roots.

theoretical foundation, we propose to use the measure of the threat of revolution constructed by Aidt and Jensen (2011) as an instrument for the franchise in the ‘size of government’ equation. This measure records, based on Tilly (2004) and other sources, actual revolutionary events in Europe between 1820 and World War I. We use a distance-weighted average of events happening in a particular year to proxy for the threat of revolution as perceived by the incumbent elites in a particular country in that year. We call this variable *revolutionary threat*.<sup>36</sup> Of the four revolutionary waves identified by Hobsbawm (1962), three happened in the first half of the century (1820, 1830, and 1848) and the fourth happened in 1917. This means that the instrument is not well-suited (relevant) for the short sample (1870-1913) and we apply it only to the long sample.

We argue that these revolution shocks alert the elites to the danger of revolution and increase the likelihood of preemptive suffrage reform.<sup>37</sup> While this measure is (as we show below) correlated with the suffrage,<sup>38</sup> its validity as an instrument can be challenged if revolutionary shocks cause the elites to offer fiscal transfers instead of democracy.<sup>39</sup> The implication, then, is that the IV estimator might have a positive bias and could be picking up a combination of franchise extension and concession effects. On the other hand, if the concession effect is the only time-varying omitted factor and franchise extension and concessions under the threat of revolution are substitutes and thus negatively correlated, then the OLS estimate would be biased downwards. Accordingly, the two estimators, in this sense, give upper and lower bounds. In addition to this measure of the threat of revolution, we also use past values of the franchise (and its square, when appropriate) as instruments.

## 5 Results

The results for the long sample are reported in Tables 3 and 4. Table 3 shows the fixed effects panel OLS estimates while Table 4 shows the instrumental variables estimates. The results for the short sample are reported in Tables 5 and 6.

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<sup>36</sup>To be precise, *revolutionary threat* for country  $i$  in year  $t$  is defined as

$$\sum_{j \neq i} W_{ij} R_{jt}$$

where  $R_{jt}$  is an indicator variable equal to one if country  $j$  (different from  $i$ ) is affected by a major revolutionary event in year  $t$  and  $W_{ij}$  is the inverse distance in kilometers between the capitals of country  $i$  and  $j$ . We note that we exclude revolutionary events in country  $i$  itself from the calculations.

<sup>37</sup>A complementary mechanism is that those who are seeking a regime change through revolution might take inspiration from events in other countries. In particular, revolutions abroad could serve as rallying cries and help revolutionaries or other regime opponents at home to coordinate their actions effectively and transform sporadic discontent into a serious and well-organized regime challenge.

<sup>38</sup>Przeworski (2009) also establishes a strong correlation between proxies for the threat of revolution and suffrage extension, but for the period after World War I.

<sup>39</sup>This would be consistent with Acemoglu and Robinson’s (2000) theory which stresses that the elites will prefer to offer such transfers temporarily if that is sufficient to avoid a revolution. Since they cannot commit to such transfers in the absence of a credible threat, they are often insufficient, and the elites will then have to extend the franchise and in that way commit themselves to future transfers.

**Results from the long sample (1820-1913)** Columns one and two of Table 3 report the results for *revenue per capita*. The specification in column one shows that *suffrage* is positively related to *revenue per capita* but the estimate is significant only at the 10% level. Importantly, the result in column two, where we have added the square of *suffrage* suggests that the relationship is, in fact, U-shaped. That is, starting from a low base, a franchise extension reduces *revenue per capita* but after reaching a threshold (located at the point where 38% of the male population is granted the right to vote) further extension is associated with an increase in *revenue per capita*. The revenue data for the long 19th century are consistent with the retrenchment hypothesis. The franchise extension hypothesis is therefore not universal but a feature of the last stages of the franchise extension process. Figure 2 graphs the effect of the franchise extension from non-democracy (*suffrage* equal to zero) to universal male suffrage using the point estimates from column two in Table 3. The marginal effect of a small increase in *suffrage* on *revenue per capita* is

$$\frac{\partial(\text{revenue per capita})}{\partial \text{suffrage}} = \hat{\beta}_1 + 2\hat{\beta}_2(\text{suffrage}) \quad (1)$$

where  $\hat{\beta}_1$  is the estimate coefficient on *suffrage* and  $\hat{\beta}_2$  is the estimated coefficient on its square. The marginal effect depends on the relative size of the electorate. As a consequence, the precision with which the slope of the U-shaped relationship is estimated varies with *suffrage*. In Figure 3, we graph the marginal effect along with the corresponding 95% confidence interval.<sup>40</sup> We observe that the marginal effect is negative and significant until a franchise that allows about 5% of the adult male population to vote is reached. The marginal effect is positive and significant after the right to vote has been granted to 50% of the adult male population. In between 5% and 50%, the 95% confidence bounds contain both positive and negative values. The turning point of the U-shaped relationship is, therefore, imprecisely estimated.

[Figure 2: Retrenchment and the franchise extension over the long 19th century]

[Figure 3: The marginal effect of the franchise extension on revenue per capita over the long 19th century]

The main results for *spending per capita* are reported in Table 3, columns three and four. In contrast to the results for *revenue per capita*, the evidence of retrenchment is much weaker. While the Wald test at the bottom of the table shows that the two relevant coefficients are jointly significant at the 10% level, they are not individually significant. The estimate of the coefficient on *suffrage* in column three is, on the other hand, significant. The difference

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<sup>40</sup>The variance of the estimated marginal effect of *suffrage* is:

$$\text{var}(\hat{\beta}_1) + 4(\text{suffrage})^2 \text{var}(\hat{\beta}_2) + 4(\text{suffrage}) \text{cov}(\hat{\beta}_1, \hat{\beta}_2).$$

This is used to compute 95% confidence intervals, using the point and variance estimates from column two of Table 3.



in the results for the revenue and expenditure side of the budget can plausibly be traced back to the fact that, at least for the first half of the 19th century, royal appointment of ministers and cabinets normally would determine the manner in which public money was spent. Elective lower chambers would have, or obtained over time, veto power over new taxes. It is, therefore, natural to expect retrenchment to show up in total tax revenues, which were affected directly by the franchise rule, and the effect on spending, which was indirect owing to budget control, to be less pronounced.

In columns five and six of Table 3, we have interacted suffrage with the country dummy variables in order to allow the effect of the franchise extension to be country-specific. We observe a great deal of country heterogeneity. The association between *suffrage* and *revenue per capita* is positive (and statistically significant) in Austria, Belgium, and France; negative (and statistically significant) in Italy, the Netherlands, Sweden, and Denmark; and not significant in the United Kingdom and Prussia. When we also allow the square of suffrage to vary across countries (not reported in the Table), we find equally large country differences. In Austria, Belgium, the Netherlands, Sweden, and the United Kingdom, the U-shaped relationship is significant, in Prussia it is insignificant, in Denmark and France it is inverted and significant, and in Italy the relationship is negative. The country-specific associations between *suffrage* and *spending per capita* are mostly insignificant, with the exceptions of Belgium, where it is positive, and Sweden, where it is negative. The pooled estimates reported in columns one to four mask this heterogeneity. This should be borne in mind when interpreting the results.

In Table 4, we report the IV estimates where we instrument *suffrage*. Since the main instrument – *revolutionary threat* – is collinear with year fixed effects, we can control only for two-year fixed effects in the IV estimations. For comparison, we report at the bottom of the table the corresponding fixed effects panel OLS results for the three main variables (*suffrage*, *suffrage squared*, and *secret ballot*). We observe that the U-shaped relationship becomes less pronounced, but that the two coefficients remain jointly significant at the 10% level both for *revenue* and *spending per capita*. The first stage regressions are reported in columns two and five. We observe that the two instruments are individually significant and jointly significant with very high F-statistics. They also pass the J-test for over-identification for both fiscal outcome variables.<sup>41</sup> In columns one and three, we report the second stage results for *revenue per capita*. Here, we observe that *suffrage* (instrumented) loses its significance (column one). More importantly, in column three, where we instrument *suffrage* and its square (using the square of the lag as an additional instrument), we observe that the U-shaped relationship is no longer significant. Taken together, these results cast doubt on the causal nature of evidence from the revenue side of the budget on the retrenchment hypothesis reported in Table 3. The results for *spending per capita* reported in columns four and six, on the other hand, reinforce the results from Table 3: *suffrage* (instrumented) is significant and a little smaller (0.014) than the corresponding OLS panel estimate (0.015). According to these estimates, the full transition from non-democracy to universal suffrage would increase

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<sup>41</sup>We show only the first stage regressions for *suffrage* since the ones for *suffrage squared* are of little economic interest. We note, however, that the instruments are jointly significant with a high F-statistic and that they pass the over-identification test.

*spending per capita* in the average country by 1.4-1.5 gold grams (the average value is about 14 gold grams).

The ballot hypothesis is rejected outright by the data for the long 19th century. The secret ballot dummy variable is mostly insignificant and where it is not, it has the opposite sign of that predicted by the hypothesis. We must conclude that the secret ballot did not contribute to growth in the absolute size of government. In contrast, the war-finance hypothesis does receive support: *external war death* is positively associated with the size of government albeit less so in the IV estimations. This adds to an already substantial evidence base in favor of the war-finance hypothesis accumulated by Dincecco (2009, 2011), Dincecco et al. (2011) and others. The evidence on the modernization hypothesis is mixed. In the specification with two-year time effects, *GDP per capita* is significant (see Table 4), but this is not robust to controlling for common shocks at the yearly frequency (see Table 3). *Urbanization rate* is mostly insignificant, but in the few cases where it is not, it has, with one exception, the opposite sign of that implied by the modernization hypothesis. The only variable that consistently supports the modernization hypothesis is *school enrollment dummy*. It is positively associated with *revenue per capita*, but this is unlikely to represent a causal effect and we observed that the correlation with *spending per capita* is insignificant. Finally, the globalization hypothesis does not receive much support. If anything, being on the gold standard seems to be negatively, not positively, correlated with the size of government.

Table 3: The fixed effects results for revenue and spending per capita from the long sample (1820-1913).

Table 4: Instrumental variables results for revenue and spending per capita from the long sample (1820-1913).

**Results for the short sample (1870-1913)** The short sample allows us to investigate our hypotheses in the context of the relative as well as the absolute size of government. The main results for the relative size of government are reported in Table 5. As far as the franchise extension and retrenchment hypotheses go, for the period 1870-1913, the action is on the spending side. In particular, neither the franchise extension hypothesis, nor the retrenchment hypothesis receives support from the revenue side of the budget. On the expenditure side, in contrast, we find, in column three that *suffrage* has a positive and significant effect on spending as a percentage of GDP. This continues to be the case when we add the square term in column four, which by itself is negative and significant. This is the *opposite* of the retrenchment hypothesis: it suggests that *spending/GDP* increases at first and then falls when suffrage reaches the turning point around 66% of the male population voting. This is illustrated in Figure 4. The “dip” in spending associated with the move from broad elite/middle class suffrage to universal (male) suffrage is visible but not particularly marked. The rejection of the retrenchment hypothesis is in itself not overly surprising for this sample. After all, as is clear from Table 2, we do not capture the full transition from non-democracy to universal (male) suffrage in this sample. It is, however, surprising to find a fall in the relative size

of government as universal (male) suffrage is approached. Whether the small “dip” should be viewed as a flattening out of the positive relationship or as a true reduction in the relative size of government is investigated in Figure 5. The figure graphs the marginal effect of *suffrage* on *spending/GDP* as a function of *suffrage* along with the 95% confidence bounds. We observe that the marginal effect is positive and significantly so until around 60% of the adult male population is enfranchised. After this point, the direction of the marginal effect is ambiguous up to roughly 70%, after which it is negative and statistically significant. The size of the effect is, however, modest: at most a fall of 0.2 percentage points and it is possible that the quadratic functional form is misspecified. To investigate the question of misspecification, we estimate a more flexible model that categorizes *suffrage* into five groups and interacts *suffrage* with dummy variables thus created. The result, shown in column five, reveals that the marginal effect is positive and constant at 0.039 until suffrage reaches around 80%. After that the marginal effect falls to 0.028 but remains significantly different from zero. This suggests that the “dip” estimated with the quadratic formulation may, in fact, be a result of misspecification of the functional form. We are, therefore, cautious not to read too much into it.

The evidence on the ballot hypothesis is more favorable for this sample than for the long sample. On both the revenue and the spending side, we see that the coefficient on the ballot dummy variable is positive, as suggested by the hypothesis, and statistically significant at the 5% level. This suggests that the secret ballot did, in fact, contribute to an increase in the relative size of government. The effect is about twice as big on the spending side as on the tax side, suggesting that part of the expansion might have been deficit financed. The effects themselves, however, are modest. In the short run, the secret ballot adds about 0.2 percentage points to *revenue/GDP* and about 0.5 percentage points to *spending/GDP*.

To investigate if the difference in results related to the ballot hypothesis between the long and short samples is due to the change in the sample period or to the change in the way the size of government is measured, we have estimated models for *revenue* and *spending per capita* for the period 1870-1913.<sup>42</sup> A summary of the results is reported in Table 6. We see that the coefficient on *secret ballot* is insignificant across the board. We can, therefore, conclude that the difference in this case *is* driven by the change in outcome variable. With regard to *suffrage*, the inverted U-shaped relationship in spending is not present when we use *spending per capita* as the outcome variable. In fact, the results for *revenue* and *spending per capita* both point to a monotonic positive effect, albeit a somewhat weaker effect for *revenue per capita* than for *spending per capita*. This is broadly consistent with the findings for the relative size of government. In this case, we therefore conclude that the difference in results between the long and short samples is mostly due to the change in sample period.

Figure 4: Spending in percentage of GDP and the franchise extension, 1870-1913.

Figure 5: The marginal effect of the franchise extension on spending/GDP, 1870-1913.

<sup>42</sup>Revenue and spending data measured in gold grams are not available for Norway and Switzerland. The sample underlying these estimations therefore excludes these countries, but includes Austria and Belgium.

Table 5: The fixed effects results for revenue/GDP and Spending/GDP from the short sample (1870-1913).

Table 6: Summary of fixed effects results for revenue and spending per capita, 1870-1913.

The war-finance hypothesis is supported in the specification with *spending/GDP*, but not in those with *revenue/GDP* where the point estimate on *external war death* is negative and significant. The modernization hypothesis receives little support overall and the same is true for the globalization hypothesis, where the coefficient on *trade volumes* is negative, not positive, and significant in the *revenue/GDP* estimations. *Trade protection* is positively related to the size of government. This suggests that the variable is picking up a revenue effect rather than a globalization effect. Finally, *left-wing party share* is negatively, and significantly, associated with *spending/GDP* and *revenue/GDP*. This is surprising and suggests that the rise of left-wing parties during the second half of the 19th century was not a major driver of fiscal expansion.

## 6 Perspectives on the political causes of growth in government

In this section, we reflect on our findings and relate them to the existing empirical literature. In doing so, we focus on the three political hypotheses that constitute the main objective of our analysis and to which we believe our data are well-suited to speak. The tests of the other hypotheses are of limited interest because of the relatively poor quality of the proxies we employ. Consequently, we shall not dwell on the literature related to them and instead refer the reader to Peter Lindert's book(s) *Growing Public* (Lindert 2004a,b) for an authoritative and insightful discussion of the relevant literature.<sup>43</sup>

### 6.1 The franchise extension and the retrenchment hypothesis

The franchise extension and the retrenchment hypothesis have received substantial attention from economists, political scientists, and economic historians in the past two decades. This has resulted in a battery of tests, both for the modern period and for the period before the two world wars, having appeared in print. The historical data exhibit much larger variation in the franchise rules and correspondingly large variations, both within a given country and across countries, in the number of citizens who could vote than modern post-war data. This allows researchers to track the entire spectrum from non-democracy (with no voting), to limited democracy with a restricted franchise to universal male suffrage, often within the same national unit. The studies for the modern period cannot do this and must often resort to using broader summary measures of democracy, e.g., based on the Polity IV database, the Freedom House indices, or the binary indicators of democracy developed by Boix (2003), Alvarez et al. (1996), Cheibub et al. (2010) and others.

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<sup>43</sup>Tridimas and Winer (2005) provide an excellent survey of the theoretical literature and Holsey and Borcherding (1997) and Borcherding (1985) review the evidence from the United States.

These studies are, therefore, less well-targeted at testing the franchise extension and/or the retrenchment hypothesis and better targeted at improving our understanding of the fiscal effect of the complete packages of democratic versus non-democratic institutions.

Table 7 provides a concise overview of 17 quantitative studies that have tested the two hypotheses (and many others as well). For each study we report the time period considered, the sample, the estimation technique, and the results (on the two hypotheses). We have organized the table such that the studies that employ historical data are at the top and those that use data from the modern period are at the bottom.<sup>44</sup>

Table 7: Overview of empirical studies of the relationship between democracy and the size of government.

Focusing, first, on the historical studies, we observe that the franchise extension hypothesis receives quite a bit of support, but mostly from studies that did not consider the retrenchment hypothesis. Once the possibility of retrenchment is taken into account and the underlying data material has sufficiently deep time or country coverage to insure that the full range from non-democracy to universal (male) suffrage is represented, the evidence for the franchise extension hypothesis becomes much weaker and the historical evidence points toward a U-shaped relationship between the extension of the franchise and various measures of the size of government. This is in line with the results for government revenues that we reported in Table 3 for the sample covering the long 19th century. It is also consistent with the finding by Aidt and Jensen (2009a). They find that the relationship between the probability of introducing the (personal) income tax and the franchise follows a U-shaped pattern. Lindert (2004a:Chap. 7) stresses the distinction between non-democracies, elite/middle class democracies and full democracies in his insightful interpretation of this pattern. It is the shift from non-democracy to elite/middle class (or limited) democracy that lies behind the fall in spending and taxation, while the move from elite/middle class democracy to universal male suffrage is associated with the increase. Elite/middle class democracy gave voice to a sufficiently large portion of the elite fringe or to the emerging middle classes and these groups did not wish to shoulder the cost of high public spending. The ruling elites under non-democracy were less concerned about these costs. They were often able to shift the tax burden onto other, disenfranchised groups and might even, in some contexts, have viewed spending as an important lever to pull to keep themselves in power, as means of extracting rents, or viewed investments in social infrastructure as being particularly beneficial to them, as argued by Aidt et al. (2010). This logic can also explain why we fail to find evidence of retrenchment in the shorter sample from 1870 to 1913: we do not have a sufficiently large number of non-democracies in that sample.<sup>45</sup>

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<sup>44</sup>Some well-known studies, e.g., Kristov et al. (1992), Becker and Mulligan (2003), Kenny and Winer (2006) and Ferris et al. (2008) are not included in Table 7. This is either because they do not include franchise extension variables or because they do not include direct tests of the two hypotheses of interest to us. Likewise, we do not include studies that focus on women's suffrage in the overview.

<sup>45</sup>We notice that Lindert (1994, 2004b) also studies a relatively short sample running from 1880 to 1930.

The evidence from the modern period is more mixed and seems to vary from context to context. Profeta et al. (2010), Lindert (2004b), and Mulligan et al. (2004) find little evidence of any link between measures of democracy and the size of government in samples covering both developing countries and developed countries. In contrast, strong evidence for the franchise extension hypothesis has been found by Boix (2001) in a world sample and by Husted and Kenny (2003) in a sample of U.S. states.<sup>46</sup> Plumper and Martin (2003) and Hausken et al. (2004) find strong evidence in favor of the retrenchment hypothesis. We note that these authors also test for the franchise extension hypothesis, but find that the retrenchment hypothesis outperforms it. This is in line with our reading of the historical evidence but only on the revenue side of budget. Consistent with this, Kenny and Winer (2006) find that moderate improvements in political freedom have little effect on the share of income tax revenues and that it is only when full political freedom is granted that more reliance on income taxation is observed in their post WWII world sample.

Our results, as previously noted, cast some doubt on the causal nature of the link between franchise extension and the (absolute) size of government measured from the revenue side. Some of the historical studies also use IV techniques to overcome the endogeneity problem inherent in estimating the association between voting institutions and fiscal outcomes and they are more optimistic about the causal nature of the association. Kim (2007) and Aidt and Jensen (2009b) use measures of the threat of revolution as instruments for suffrage reform in Western Europe<sup>47</sup> and Aidt et al. (2010) use a natural experiment along with measures of inequality as instruments for the extension of franchise in local elections in England and Wales in the 1870s. We note that Kim (2007) and Aidt et al. (2010) study the expenditure side of the budget and that the claims of causality all relate to the effect of the suffrage on different aspect of government spending. These claims are consistent with our finding that the IV estimate of the effect of franchise extension of government spending per capita is positive and significant. The only study that also looks at the income side of the budget (Aidt and Jensen 2009b) finds that the IV estimate for total revenue out of GDP is insignificant. The previous evidence is, therefore, perfectly in line with the finding of the present paper and one interpretation of this, then, is that the causality runs primarily from suffrage reform to government spending and only indirectly to taxation. This interpretation is also consistent with our findings from the short sample reported in Table 5.

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Yet, he finds evidence of retrenchment. We conjecture that this is because his sample includes a number of nondemocracies in Latin America as well as Japan. Since he uses between-country variation to estimate the effects, he can compare social spending by these nondemocracies to the elite/middle class democracies in Europe during the late 19th century.

<sup>46</sup>To be precise, Boix (2001) shows that the impact of a binary index of democracy on the size of government is conditional on the level of development and that the positive effect is present above a certain GDP per capita threshold.

<sup>47</sup>Kim (2007) uses data on local strikes as an instrument for the suffrage. These data are available for a few countries from 1880, but are not recorded for the majority until the turn of the century. Aidt and Jensen (2009a,b) use an instrument similar to the one we employ here, i.e., revolutionary events in other countries. Aidt and Jensen (2009a) cover a large portion of the 19th century, whereas Aidt and Jensen (2009b) start in 1860.

## 6.2 The ballot hypothesis

While the franchise extension and the retrenchment hypotheses have been thoroughly tested, the ballot hypothesis has received almost no attention. The only exceptions that we are aware of are Anderson and Tollison (1990), Lott and Kenny (1999), and Aidt and Jensen (2009a).

Anderson and Tollison (1990) study 48 US states in 1915 and ask if the states with constitutions that required voting to be secret (or in which voting machines were mandatory) experienced higher total state government expenditure per capita than states without such requirements. They find a strong positive correlation, after controlling for a range of economic and political factors, between the secret ballot and the level of spending. Lott and Kenny (1999) also explore differences in ballot rules across US states between 1870 and 1940. In contrast to Anderson and Tollison (1990), they ask if a change in the ballot rules from open to secret within a state had an effect on real per capita state revenue and expenditure. They find that the introduction of the secret ballot in a state had little effect on subsequent real per capita spending and revenues, but they do find a negative, significant effect on real per capita social services which is hard to reconcile with the ballot hypothesis.

Aidt and Jensen (2009a) study the factors that determined the introduction of personal income tax in Western Europe, North America, Oceania and Japan between 1815 and 1939. They show that the introduction of secret voting increases the probability of income tax adoption; a finding, which insofar as income taxation facilitates growth in government, is consistent with the ballot hypothesis.

Our finding that there is no correlation between the ballot and *revenue per capita* or *spending per capita* between 1820 and 1913 is in line with the results obtained by Lott and Kenny (1999). On the other hand, the fact that we find a positive association between the ballot and shares of total revenue and spending out of GDP between 1870 and 1913 complicates rather than clarifies the picture. What we can tentatively say based on our evidence from Europe, is that the ballot correlates positively with the size of government relative to the economy but not with the absolute size. The need for further research on the economic effect of the secret ballot is clearly very large.

## 7 Conclusion

The link between democracy and the size of government has been extensively studied over the years. The presumption that democracy is associated, perhaps even causally, with growth in government is widespread and several very influential theories of suffrage reform are predicated on that assumption, but the reality is more complex. In this paper, we present new evidence on the association between franchise extension, ballot rules and the size of government from Western Europe during the long 19th century. We find evidence of a U-shaped relationship between the within-country variations in the extension of the voting franchise and real per capita government revenue. This, however, masks significant cross-country heterogeneity and our IV estimations cast doubt on whether a causal interpretation is appropriate. We find a monotonic positive relationship between the franchise and government spending per capita which is robust to instrumentation. Our data reject the hypothesis that the secret

ballot led to an increase in the absolute size of government, but leaves open the possibility that there was a positive effect on the relative size of government. Our data, at least for the long 19th century, are consistent with the war-finance hypothesis that external wars were an important impulse for state capacity building and in that way contributed to expanding the public sector.

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## 8 Appendix

[Table A1: Descriptive statistics].

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Table 1: The evolution of the relative size of government between 1870 and 1913.

Country	Central Government Spending as a percentage of GDP				Central Government Revenue as a percentage of GDP			
	1870- 79	1880- 89	1890- 99	1900- 13	1870- 79	1880- 89	1890- 99	1900- 13
Norway	5.5	5.7	6.8	7.3	3.14	3.67	4.33	4.86
France	14.5	11.8	11	10.2	9.35	10.81	10.53	9.41
Italy	12.2	16.6	13.7	14.4	8.73	10.72	11.24	10.15
Netherlands	n/a	n/a	n/a	8.3	n/a	n/a	n/a	6.33
Sweden	5.63	6	5.79	6.9	4.01	4.74	5.46	5.26
Denmark	5.8	6.7	5.9	5.4	4.81	5.26	5.23	4.94
United Kingdom	7.6	6.8	8.2	8.2	5.32	5.71	5.59	6.55
Switzerland	n/a	1.92	2.3	2.78	n/a	1.54	1.93	2.08

Source: Flora (1983)

Table 2: Descriptive information

Country (entry year, exit year)	Suffrage reforms	Suffrage (percentage of adult males who could vote)			Secret Ballot	Polity > 0	School enrollment >60%	Gold standard
		Entry year	1870	Exit year				
<b>Both samples<sup>c</sup></b>								
Italy (1861-1913)	1861, 1882,1912	8	9	90	1861	After 1913	After 1913	1884
Netherlands (1820 <sup>d</sup> -1913)	(1815), 1848, 1887, 1894	0	11	67	1849	After 1913	1859	1875
Sweden (1820-1913)	1866, 1907	0	22	78	1907	After 1913	1871	1873
Denmark (1820 <sup>b</sup> -1913)	1849	0	73	88	1901	1849-65 After 1913	1893	1873
United Kingdom (1820-1913)	1832, 1867,1884	3	31	63	1872	1837-1913	1891	1820
France (1820-1913)	1820, 1830,1848	1	87	92	1913	1848-50 1877-1913	1858	1878
<b>Long sample only</b>								
Prussia (1820-1867)	1848, 1850	0	n.a.	10 <sup>e</sup>	1913 <sup>a</sup>	After 1871	1820	1872
Austria (1820-1913)	1867, 1896,1907	0	0	95	1907	After 1913	1889	Never
Belgium (1831-1913)	1831, 1848,1893	4.5	9	92	1877	1853-1913	1895	1878
<b>Short sample only</b>								
Norway (1870-1913)	1814, 1884,1897	21	21	70	1884	1898-1913	1886	Never
Switzerland (1880-1913)	1848	80	80	76	1882	1848-1913	1870	1878

Notes: a secret ballot introduced for the German Empire. b *revenue per capita* available only from 1864; c. the entry year refers to the long sample, for the short sample all countries enter in 1870 or when data becomes available. d. the fiscal data is available only from 1900 where *suffrage*=61; e the franchise is defined as voters in class I and II relative to the adult male population.

Table 3: The fixed effects results for revenue and spending per capita from the long sample.

	(1)	(2)	(3)	(4)	(5)	(6)
	Revenue per capita	Revenue per capita	Spending per capita	Spending per capita	Revenue per capita	Spending per capita
Lagged dependent variable	0.893***	0.876***	0.826***	0.810***	0.776***	0.679***
	[60.35]	[53.27]	[22.19]	[22.13]	[35.21]	[11.58]
Suffrage	0.00389*	-0.0166**	0.0125*	-0.0254		
	[1.769]	[-2.061]	[1.748]	[-0.882]		
Suffrage, squared		0.00022***		0.000406		
		[2.655]		[1.357]		
Secret ballot	-0.188	-0.252	-0.421	-0.529	-0.0825	0.258
	[-0.988]	[-1.318]	[-1.076]	[-1.428]	[-0.483]	[0.514]
Suffrage, Italy					-0.0243***	0.0374
					[-3.252]	[1.247]
Suffrage, Austria					0.0156*	0.026
					[1.860]	[0.953]
Suffrage, Belgium					0.00342*	0.0333***
					[1.660]	[3.102]
Suffrage, France					0.0125***	0.0122
					[3.329]	[1.169]
Suffrage, Netherlands					-0.0540***	-0.0763***
					[-7.130]	[-4.516]
Suffrage, Sweden					-0.0297***	-0.0357*
					[-3.830]	[-1.840]
Suffrage, Denmark					-0.120***	0.0104
					[-7.808]	[0.134]
Suffrage, United Kingdom					-0.00832	-0.00143
					[-1.148]	[-0.040]
Suffrage, Prussia					0.0411	-0.0188
					[1.588]	[-0.206]
External war death	1.079***	1.097***	1.360**	1.358**	1.081***	1.505**
	[5.623]	[6.021]	[2.307]	[2.381]	[5.261]	[2.186]
GDP per capita	0.0000779	-0.0000702	0.00077	0.000393	-0.0001850	0.000116
	[0.408]	[-0.363]	[1.200]	[0.619]	[-0.745]	[0.139]
Urbanization rate	-3.296**	-3.731**	-7.998*	-7.930*	-1.691	15.64***
	[-2.187]	[-2.432]	[-1.774]	[-1.832]	[-1.283]	[2.598]
School enrollment	0.343***	0.350***	-0.142	-0.172	0.547***	0.197
	[2.644]	[2.723]	[-0.393]	[-0.474]	[3.782]	[0.410]
Gold standard	-0.238*	-0.296**	-0.427	-0.566	0.284	-0.285
	[-1.893]	[-2.323]	[-0.739]	[-1.079]	[1.277]	[0.197]
Polity	0.613***	0.550***	0.862*	0.715	0.324***	0.69
	[3.812]	[3.407]	[1.942]	[1.592]	[2.667]	[1.519]
Observations (countries)	656 (9)	656 (9)	655 (9)	655 (9)	656 (9)	655 (9)
Wald-test of retrenchment		10.66***		5.14*		
Turning point for suffrage		37.7		31.3		

Notes: z-statistics in brackets; \*\*\* p<0.01, \*\* p<0.05, \* p<0.1. Estimations by OLS with panel corrected standard errors including an AR(1) adjustment. We include country fixed effects and year fixed effects.

Table 4: IV results for revenue and spending per capita from the long sample (1820-1913)

	(1)	(2)	(3)	(4)	(5)	(6)
	Revenue per capita	Suffrage	Revenue per capita	Spending per capita	Suffrage	Spending per capita
Stage	2nd	1st	2nd	2nd	1st	2nd
Revenue per capita, lagged	0.91***	0.19*	0.91***			
	[26.62]	[1.704]	[26.49]			
Spending per capita, lagged				0.821***	0.103	0.818***
				[25.22]	[1.243]	[24.82]
Suffrage (instrumented)	0.004		-0.003	0.0136**		-0.0033
	[1.066]		[-0.223]	[2.200]		[-0.151]
Suffrage, squared (instrumented)			0.00007			0.00018
			[0.594]			[0.803]
Secret ballot	-0.353*	1.798	-0.379*	-0.633*	1.645	-0.699**
	[-1.751]	[1.616]	[-1.837]	[-1.956]	[1.479]	[-2.118]
External war deaths	1.034	1.223	1.027	1.468*	1.096	1.454*
	[1.163]	[0.745]	[1.156]	[1.774]	[0.665]	[1.764]
GDP per capita	0.001**	0.003*	0.001**	0.0015***	0.0034**	0.0015***
	[2.217]	[1.941]	[2.187]	[3.492]	[2.129]	[3.428]
Urbanization rate	-2.351	-10.47	-2.118	-5.04	-12.64	-4.404
	[-0.761]	[-0.750]	[-0.655]	[-1.352]	[-0.912]	[-1.144]
School enrollment	0.340*	-2.031*	0.343*	0.0949	-1.887*	0.0986
	[1.648]	[-1.873]	[1.653]	[0.283]	[-1.741]	[0.294]
Gold standard	-0.0448	0.326	-0.0413	-0.13	0.5	-0.127
	[-0.238]	[0.326]	[-0.218]	[-0.369]	[0.500]	[-0.358]
Polity	0.492	0.906	0.447	0.952***	1.233	0.831**
	[1.537]	[0.680]	[1.256]	[2.931]	[0.943]	[2.125]
Suffrage, lagged		0.940***			0.944***	
		[52.36]			[53.24]	
Revolutionary threat		0.521**			0.521**	
		[2.088]			[2.082]	
Observations (countries)	655 (9)	655 (9)	655 (9)	654 (9)	654 (9)	654 (9)
F-test of IVs		1363.93			1417.74	
J test	1.259		1.255	0.046		0.038
Wald-test of retrenchment			1.46			5.45*
Turning point for suffrage			18.21			10.2
Suffrage (PCSE) <sup>a</sup>	0.005**		-0.0001	0.0147**		-0.00426
	[2.228]		[-0.014]	[2.172]		[-0.165]
Suffrage, squared (PCSE) <sup>a</sup>			0.00005			0.000208
			[0.603]			[0.756]
Secret ballot (PCSE) <sup>a</sup>	-0.343		-0.361*	-0.585		-0.678*
	[-1.567]		[-1.661]	[-1.461]		[-1.879]

Notes: z-statistics in brackets; \*\*\* p<0.01, \*\* p<0.05, \* p<0.1. All estimations include country fixed effects and two-year fixed effects. a. Fixed effect (OLS) panel regressions similar to those reported in Table 3, but with two-year fixed effects.



Table 5: The fixed effects results for revenue/GDP and spending/GDP from the short sample (1870-1913)

	(1)	(2)	(3)	(4)	(5)
	Revenue/GDP	Revenue/GDP	Spending/GDP	Spending/GDP	Spending/GDP
Revenue/GDP, lagged	0.463***	0.457***			
	[7.146]	[6.761]			
Spending/GDP, lagged			0.323***	0.170**	0.218***
			[4.204]	[2.137]	[2.786]
Suffrage	0.00481	0.00932	0.0158**	0.162***	0.0386***
	[1.603]	[0.674]	[2.493]	[4.664]	[3.049]
Suffrage, squared		-0.00004		-0.00124***	
		[-0.336]		[-4.351]	
20<Suffrage≤40					0.086
					[1.052]
40<Suffrage≤60					0.0849
					[0.960]
60<Suffrage≤80					-0.0715
					[-0.774]
80<Suffrage≤100					-0.0106***
					[-2.830]
Secret ballot	0.222**	0.220**	0.537**	0.611**	0.664**
	[2.033]	[2.007]	[2.140]	[2.422]	[2.151]
External war deaths	-1.355**	-1.368**	2.406***	2.395***	2.618***
	[-2.399]	[-2.429]	[3.838]	[4.394]	[4.430]
GDP per capita	0.000382	0.0156	0.0982	0.335	0.316
	[0.00224]	[0.0883]	[0.360]	[1.318]	[1.186]
Urbanization rate	0.00769	0.00824	0.0107	0.0253	0.0342*
	[1.187]	[1.280]	[0.568]	[1.533]	[1.847]
School enrollment rate	0.0303	0.0283	0.104	0.0132	0.0151
	[1.206]	[1.122]	[1.536]	[0.218]	[0.226]
Trade volumes	-0.000678***	-0.000666***	-0.000391	-1.30E-05	8.24E-05
	[-2.729]	[-2.621]	[-0.783]	[-0.0262]	[0.160]
Trade protection	6.789***	6.856***	8.211**	9.858**	10.45**
	[3.167]	[3.171]	[2.013]	[2.475]	[2.500]
Polity	-0.0135**	-0.0132*	0.00592	-0.000635	0.0188
	[-1.968]	[-1.928]	[0.470]	[-0.0583]	[1.128]
Left-wing party share	-0.0247***	-0.0243***	-0.0351***	-0.0276***	-0.0346***
	[-3.788]	[-3.758]	[-3.629]	[-3.368]	[-3.845]
Old	0.023	0.0237	-0.106	-0.0259	-0.0539
	[0.329]	[0.339]	[-0.711]	[-0.207]	[-0.381]
Observations	271	271	271	271	271
Number of countries	8	8	8	8	8
Turning point for suffrage		n.a.		65.70	

Notes: z-statistics in brackets; \*\*\* p<0.01, \*\* p<0.05, \* p<0.1. Estimations by OLS with panel corrected standard errors including an AR(1) adjustment. We include country fixed effects and year fixed effects. The estimations include two extra controls which are common in the literature: Left-wing party share is the share of seats held by left-wing parties in parliament and old is the fraction of the population above 65 years of age. The source for both is Flora (1983).

Table 6. Results for revenue and spending per capita (1870-1913)

	(1)	(2)	(3)	(4)
	Revenue per capita	Revenue per capita	Spending per capita	Spending per capita
Suffrage	0.00607*	-0.00746	0.0263***	0.00923
	[1.931]	[-0.541]	[3.814]	[0.319]
Suffrage, squared		0.000137		0.000171
		[0.985]		[0.577]
Secret ballot	-0.153	-0.205	-0.0231	-0.111
	[-0.588]	[-0.747]	[-0.0523]	[-0.247]
Wald test for retrenchment		4.39		14.38***
Observations	340	340	340	340
Countries	8	8	8	8

Notes: Countries is the sample: Italy, the Netherlands, Sweden, Denmark, the UK, France, Austria and Belgium. All regressions include fixed country and time effects and the same control variables as in table 3. Data for revenue and spending per capita in gold grams not available for Norway and Switzerland.

Table 7: Overview of empirical studies of the relationship between democracy and the size of government

Type	Study	Democracy and size of government measure	Period	Sample	Method	Franchise extension hypothesis	Retrenchment hypothesis
Historical	Aidt et. (2006)	Suffrage G/Y	1860-1938	Western European sample with 12 countries	Fixed effects panel with OLS and PCSE.	Some	Not tested
Historical	Aidt and Jensen (2009b)	Suffrage T/Y and G/Y	1860-1938	Western European sample with 9 countries	Fixed effects panel with OLS and PCSE. IV estimations	Some	Not tested
Historical	Kim (2007)	Binary index: Multi-party elections; responsible government; franchise of at least half the adult population. Social security per adult.	1880-1945	Western Europe	Fixed effects panel with IV. No time fixed effects.	Strong	Not tested
Historical	Aidt et al. (2010)	Suffrage Spending on urban amenities per capita	1868-1886	Municipalities in England and Wales	Fixed effects panel with IV.	Weak	Strong
Historical	Aidt and Eterovic (2011)	Voter turnout in proportion of total population. T/Y and G/Y	1920-2000	18 Latin American countries	Fixed effect with OLS and PCSE (clustered at country level).	Strong	Not tested
Historical	Dincescoco (2009, 2011)	Binary index: parliament gain constitutional right to control national budget annually and had the right for at least two decades. Revenue per capita	1650-1913	11 Western European nations	Fixed effect with OLS and PCSE.	Strong	Not
Historical	Lindert (2004b) <sup>a</sup>	Proportion of the total population with	1880-1930	21 countries in Europe and the	Cross section with Tobit	Weak	Strong

Type	Study	Democracy and size of government measure	Period	Sample	Method	Franchise extension hypothesis	Retrenchment hypothesis
Modern	Profeta et al. (2010)	right to vote. (Social spending)/Y Polity IV and Freedom House civil liberty index T/Y and G/Y	1990-2005	Americas and Oceania. Asia Latin America New EU countries	Fixed effects panel with OLS with clustered standard errors.	None	None
Modern	Husted and Kenny (1997)	Poll tax requirement Literacy test Welfare spending per capita,	1950-88	46 US states	Fixed effects with OLS.	Strong	Not tested.
Modern	Plumper and Martin (2003) and Hausken et al. (2004)	Polity IV G/Y	1975-1997	World sample with 83 countries	Pooled panel with OLS.	None	Strong
Modern	Boix (2001, 2003)	Binary index: some governmental offices are filled as a consequence of contested elections. Receipts for general government.	1950-90 1970-90	World sample with 65 countries	Pooled panel with OLS and PCSE	Strong but interacts positively with GDP.	Not tested
Modern	Mulligan et al. (2004)	Polity IV G/Y and T/Y	Average 1960-90	World sample with about 125 countries	Cross section with OLS	Rejection for T/Y (lower T/Y in democracies) None for G/Y.	Not tested
Modern	Lindert (2004b)	Proportion of the total population with right to vote. (Social spending)/Y	1962-81 1978-95	19-21 developed countries	Cross section with Tobit	None	None
Modern	Mueller and Stratmann (2003)	Turnout rate G/Y	1960-90	World sample with about 78 countries.	Pooled panel with OLS and IV	Strong	+ larger in strong than in weak democracies.

Note: T =total central government revenue; G=total central government expenditure; Y=GDP. a. Lindert (1994) analyses the same data but use a binary index for democracy rather than the proportion of population that could vote to capture democratization. He finds that the binary index of democracy reduces the probability of social spending programmes, but turnout and women's suffrage increase the probability. In the specifications reported in the 2004 book with the measure of the franchise indicator, the baseline democracy dummy is not significant and we choose only to list the 2004 results in the table.

Table A1: Descriptive statistics

Variable	Obs	Mean	Std. Dev.	Min	Max
<b>Sample 1820-1913</b>					
Revenue per capita	745	13.27	7.39	2.22	42.04
Spending per capita	708	14.94	7.81	1.86	41.83
Suffrage	1039	34.39	34.35	0.00	94.50
Secret ballot	831	0.31	0.46	0.00	1.00
External war deaths	846	0.02	0.16	0.00	1.54
GDP per capita	1053	2129.56	849.41	781.01	4920.55
Urbanization rate	846	0.19	0.10	0.05	0.46
School enrollment (dummy)	1091	0.43	0.50	0.00	1.00
Gold standard	846	0.42	0.49	0.00	1.00
Polity	1410	0.51	0.50	0.00	1.00
Revolutionary threat	1222	0.39	1.37	0	15.11
<b>Sample 1870-1913</b>					
Revenue/GDP	310	6.15	2.84	1.44	12.05
Spending/GDP	312	7.82	3.78	1.60	18.90
Suffrage	342	53.44	28.34	8.90	92.40
Secret ballot	342	0.86	0.35	0.00	1.00
War	342	0.02	0.15	0.00	1.00
GDP per capita	342	2711.00	874.97	1432.28	4920.55
Urbanization rate	304	27.12	13.85	5.88	59.91
School enrollment (rate)	311	65.26	13.36	34.10	86.00
Trade protection	342	0.07	0.05	0.006	0.19
Trade volume	342	61.12	51.51	18.04	259.36
Polity	342	0.38	0.49	0.00	1.00
Leftwing party share	342	4.36	8.90	0.00	53.70
Old	342	6.50	1.21	4.19	10.59

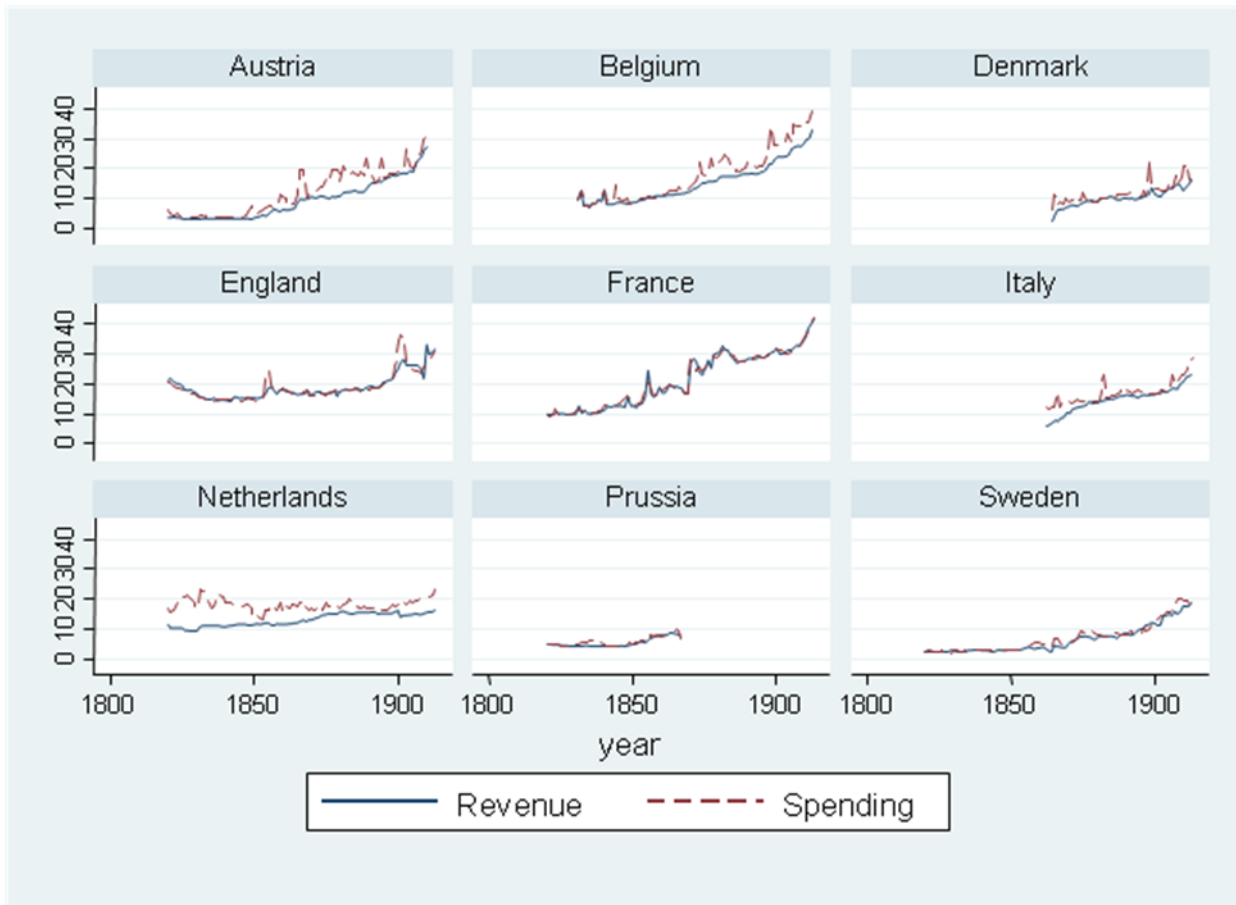
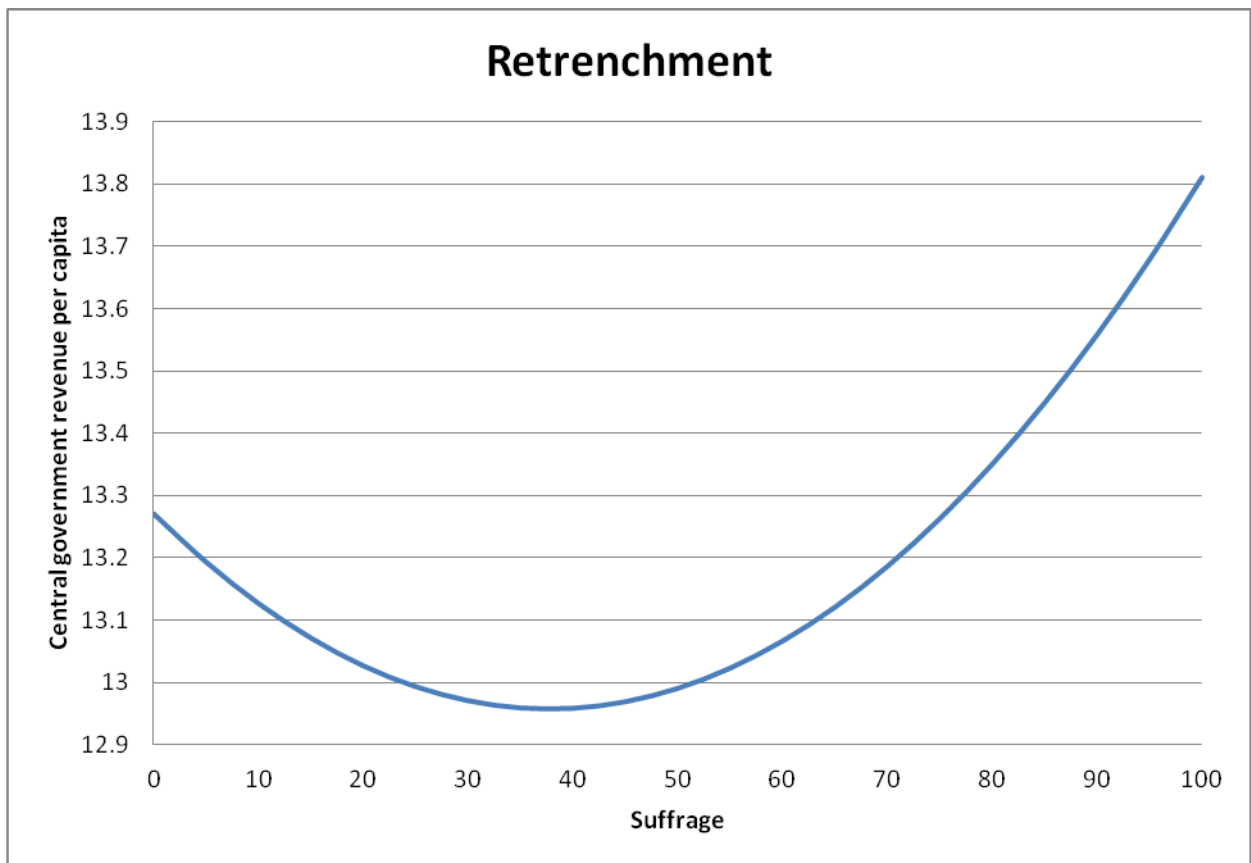


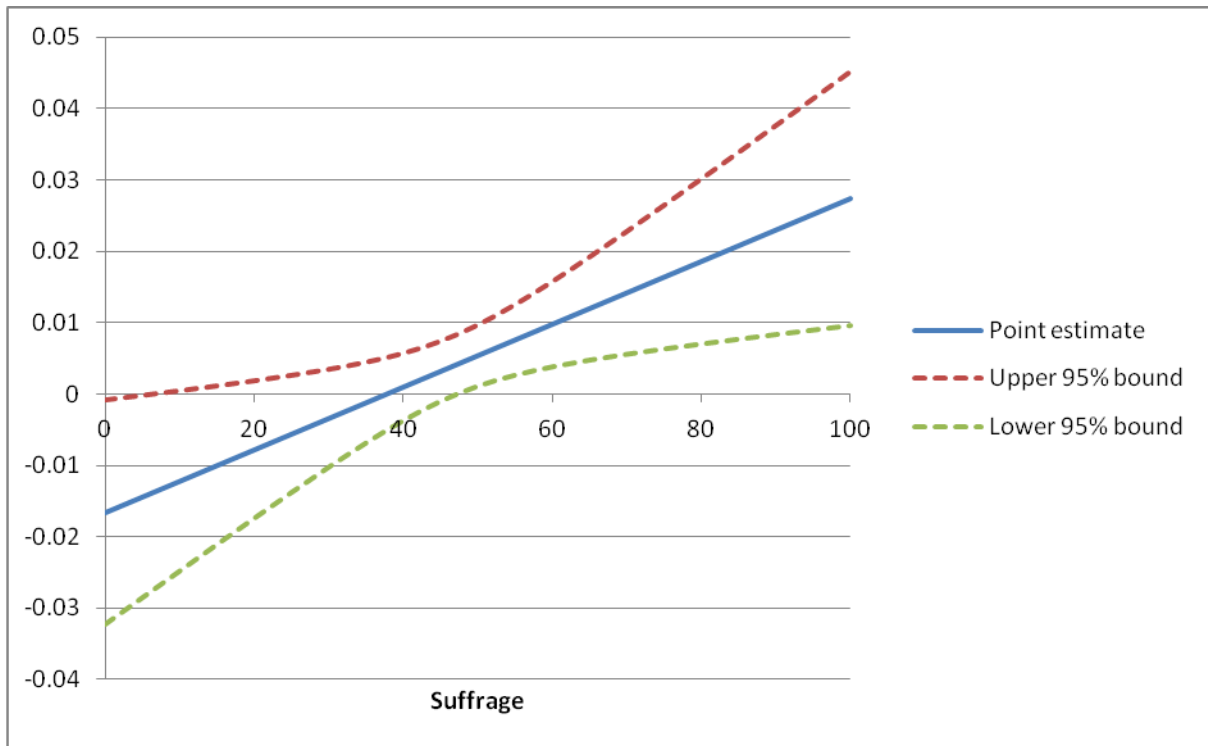
Figure 1: The evolution of the revenue and spending per capita in the nine countries in the long sample

Figure 2: Retrenchment and the franchise extension over the long 19th century



Note: The illustration is based on the estimated coefficients from Table 4 column 4. We use the time-country average for *revenue per capita* as the baseline value at suffrage=0.

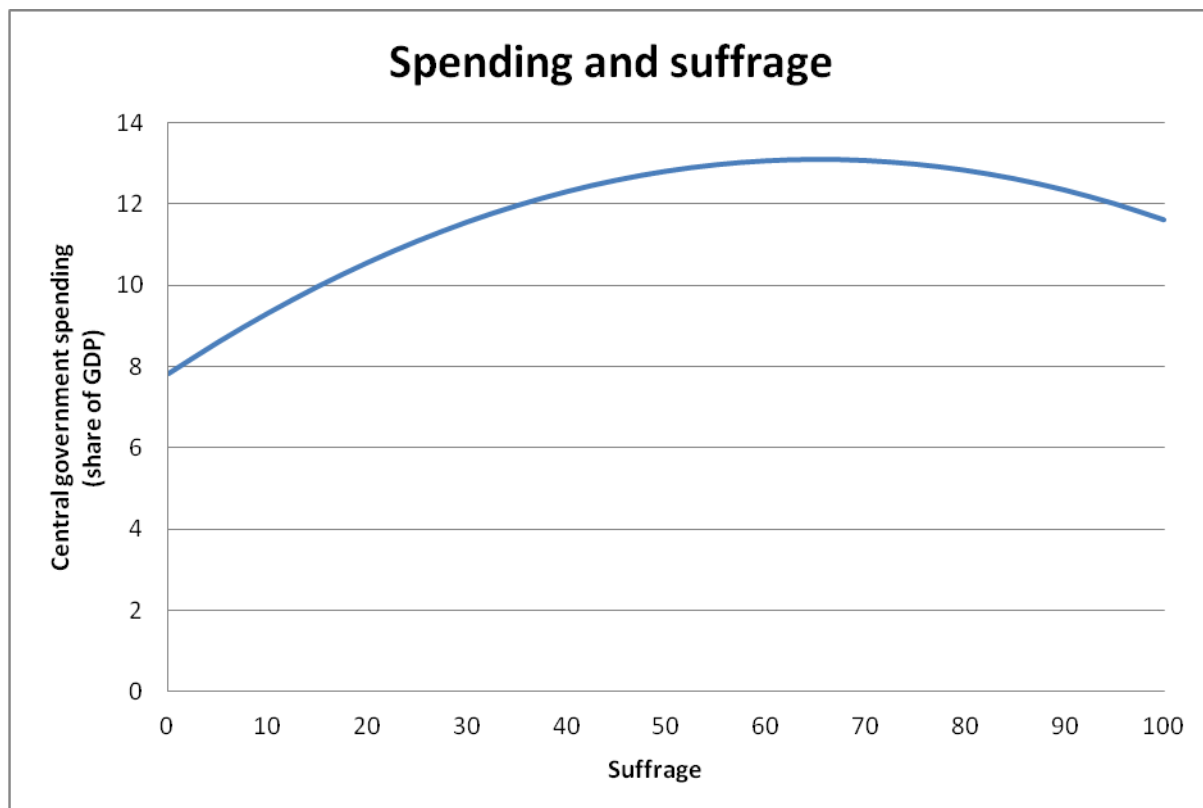
Figure 3: The marginal effect of the franchise extension on revenue per capita over the long 19th century.



Note: Point estimate is the marginal effect of a small change in *suffrage* at each level of *suffrage*. The broken lines are the 95% confidence bounds associated with the marginal effects.

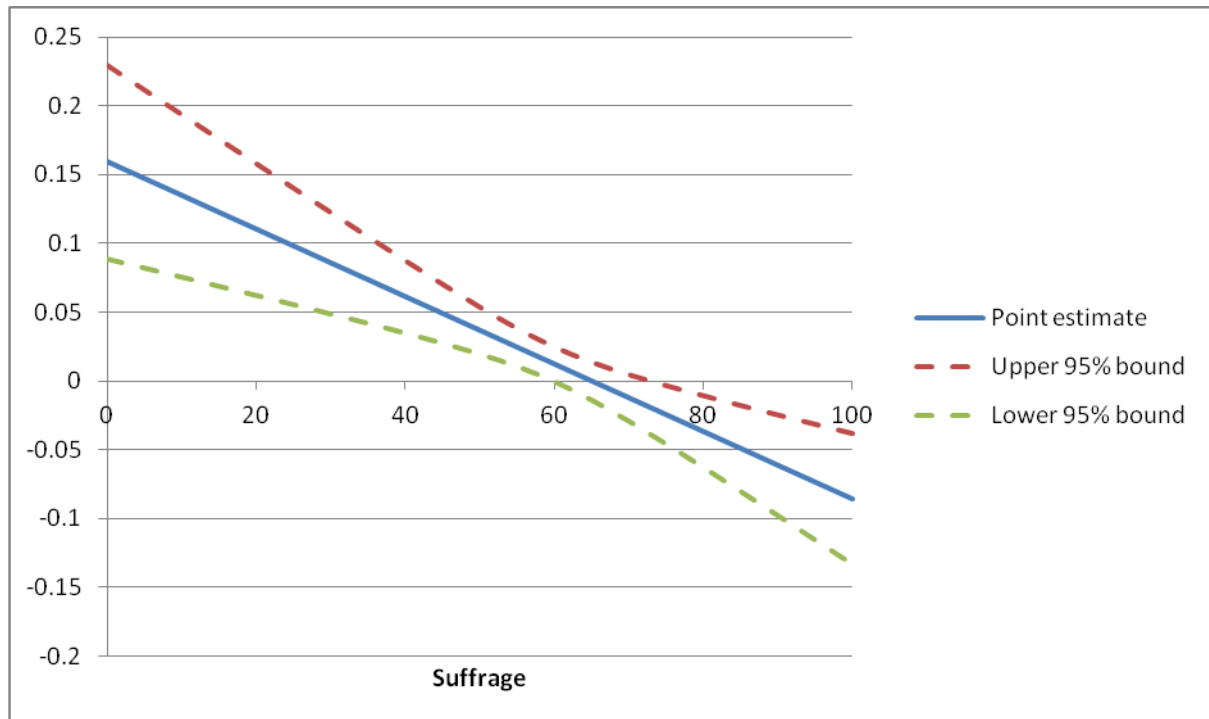


Figure 4: Spending is percentage of GDP and the franchise extension, 1870-1913.



Note: We use the coefficient estimates from Table 5 column 4 for the illustration and the time-country average as the baseline value for suffrage=0.

Figure 5: The marginal effect of the franchise extension on *spending/GDP*, 1870-1913.



Note: See note to Table 3.