

Workers of the World, Unite!

Franchise Extensions and the Threat of Revolution in Western Europe, 1820-1938.*

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Abstract

This paper tests the hypothesis that the threat of revolution caused the elite to extend the voting franchise, as suggested by Acemoglu and Robinson [Quarterly Journal of Economics 115, 1167-1199, 2000]. We propose new measures of the threat of revolution and estimate the relationship between these and measures of suffrage reform on a sample of 12 western European countries covering the period from 1820 to 1938. We find strong support for the "threat of revolution hypothesis". We also find some evidence that war mattered for the extension of the franchise, whereas modernization theory receives little support.

Key words: The extension of the voting franchise, democracy, threat of revolution, suffrage.

JEL classification: D7, P16.

1 Introduction

Why would the political elite of a country ever want to extend the voting franchise and introduce democracy? After all, by doing so, it would delude its own political base and expose itself to the risk of redistributive fiscal policy. In their seminal work, Acemoglu and Robinson (2000, 2006) answered the challenge posed by this question by showing that an

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elite might extend the franchise as a means to avoid a revolution.¹ To be sure, there are many other ways to head off a revolution, but voting rights have the advantage that once they are granted, they are hard to take back. Accordingly, a franchise extension can serve as a commitment to future redistribution in cases where it would not be credible for the elite to redistribute while retaining power.²

The threat of revolution plays a central role in this argument. The historical record provides some justification for this focus. For example, the work by Tilly (1995) suggests that contentious gatherings in Great Britain gained momentum in the period leading up to the critical vote on the Great Reform Act in the Autumn of 1831 and political historians, such as Lee (1994), suggest that the threat of violence also played a key role in relation to the Second Reform Act in 1867. Moreover, in Sweden the threat of revolution played, according to Tilton (1974), a central role in the franchise reforms of 1866 and 1909. Finally, the revolution of 1848 was a direct cause of franchise reform in France.

It is, however, more difficult to establish if the threat of revolution played a decisive role more generally when other potential causes of franchise reform are taken into account. A small literature has made some attempt at answering this challenge. Aidt and Franck (2009), for example, study the voting behavior of those Members of Parliament (MPs) who voted on the Great Reform Act in 1831 in the United Kingdom and find some evidence that the build up of the threat affected the vote decision of some reluctant MPs. Kim (2007) argues that strike activity within a country can be used as a proxy for the threat of revolution and shows that various measures of strikes are correlated with franchise reforms in a sample of 12 European countries after 1880.³ Przeworski (2009) studies a broader sample of countries but with a focus on the period after WWI. He uses data

¹Boix (2003) and Conley and Temini (2001) also stress the threat of revolution as a cause of franchise extension.

²For alternative theories of franchise extension, see Justman and Gradstein (1999), Lizzeri and Persico (2004), Gradstein (2007) or Congleton (2004, 2007).

³This estimate does not take into account unobserved fixed country, time effects or the very high degree of persistence in voting rights, and it must be viewed as tentative. Kim (2007) uses three measures of strike activity based on Flora et al. (1987). Data for France, Italy and Sweden are recorded from the 1880s; data for Austria, Belgium, Denmark, Germany and the UK are recorded from the 1890s. The data for the remaining four countries start in the 1900s. Thus, as documented below, many major franchise reforms are not covered by this sample.

on demonstrations, riots, and strikes to proxy the threat and shows a strong correlation between this measure and the probability of suffrage reform.

In this paper, we propose a new measure of the threat of revolution and show that it is a very strong predictor of franchise reforms in western Europe during the period from 1820 to 1938. We argue that the political elites contemplating franchise reform needed to assess or to estimate the likelihood of revolution. To this end, they would obviously use information about the situation locally, but they would also look to other countries. Observing revolutionary activities in neighboring countries would make the elites fear that revolution might spread to their own country. As a consequence, they would be more willing to extend voting rights. Based on the work by Tilly (1993) and others, we have recorded all "revolutionary events" in Europe during the period 1820 and 1938 and used this to construct three alternative measures of the *threat* of revolution as it might have been perceived by the political elites in different countries in the region at the time. By focusing on revolutionary events in the "neighborhood" of a country, it is plausible to assume that the variation in threat levels is exogenous with respect to the political process of that country. This allows us to identify the causal impact of the threat of revolution on suffrage reform and in that way to test Acemoglu and Robinson's (2000) theory of franchise extension. It is also important to stress that in comparison to Kim (2007) and Przeworski (2009) who use "national events" – in particular labour market unrest – to proxy the threat, our approach is to use information about revolutionary activities in the "neighborhood" of a country. Our approach also has the advantage that we can quantify the threat for the critical period in the 19th and early 20th century during which the franchise was extended in western Europe.

Using these measures, we estimate two alternative models of franchise reform using a sample of 12 western European countries from 1820 to 1938. In each model, we control for other potential determinants of democracy such as income, urbanization, education, war, trade integration, social learning etc. The first model is a dynamic panel model with fixed country and time effects. Here, the outcome variable is a measure of the number of voters in proportion to the potential electorate. The second model is an event history model.

Here, we seek to explain the conditional probability of a franchise reform. Both models show that the threat of revolution was a major cause of franchise extension in western Europe.

The rest of the paper is organized as follows. In section 2, we present our data on revolutionary events and suffrage reform. In section 3, we set out our estimation strategy. In section 4, we report the results. In section 5, we discuss robustness of the results. In section 6, we conclude and relate our contribution to the broader empirical literature on the determinants of democracy.

2 Revolutionary Events and Franchise Reform

We track the evolution of voting rights in 12 western European countries covering the period from 1820 to 1938. The countries in the sample are Austria, Belgium, Finland, Sweden, Norway, Denmark, the Netherlands, Germany, the United Kingdom,⁴ France, Italy, and Switzerland.⁵ We focus on these countries partly because reliable data is available, partly because they represent the gradual European transition from absolute monarchy or other types autocracy to constitutional democracy, and partly because Acemoglu and Robinson's (2000) theory of franchise extension is motivated by the experiences of these countries.⁶

Democratization of western Europe was a complex process that over a 100 years period gradually broadened political participation and extended the franchise from being the

⁴Not including Ireland.

⁵For Germany, Austria and Italy, we have excluded the periods with National Socialist and Fascist regimes, respectively. A country enters the sample when it becomes an independent state. This means that Belgium and the Netherlands enter the sample in 1830; that Switzerland enters in 1848 (when a federal structure was established); that Italy enters in 1861; that Germany enters in 1871. Norway did not gain full independence until 1905. However, during the Union with Sweden, it kept its liberal constitution and independent institutions, except for the foreign service, and could control its franchise rules. Finland was an autonomous Grand Duchy of the Russian Empire from the end of the Finnish War between Sweden and Russia in 1809 until 1917 when full independence was achieved. The old four-chamber Diet was re-activated in the 1860s and made new legislation concerning internal affairs. The Diet was replaced by the Parliament of Finland in 1906. This makes it reasonable to include both Norway and Finland in the sample from 1820 but none of our results depend on this choice.

⁶It is not possible to obtain sufficient data to include Spain, Portugal, Greece or countries on the Balkans in the analysis.

privilege of the few to being the right of all adult citizens.⁷ The process can be quantified in many different ways.⁸ Our main interest is to study whether the threat of revolution was a cause of enfranchisement of poorer social groups, as opposed to enfranchisement of, say, women or the young. This can best be done by tracking the size of the electorate in percentage of its reference age and sex group over time and space.⁹ Before women's suffrage, the reference group is all men of voting age and after, it is all citizens of voting age. This measure, which we shall call *suffrage*, quantifies the consequences of income, property holding, and wealth restrictions on the right to vote in isolation from the effect of women's suffrage. In Table 1, we have listed all major franchise reforms in the 12 countries. It is clear that democracy in the form of universal male suffrage arrived gradually in most countries through a sequence of piecemeal reforms. We also note two clusters of reform: 1848-50 and 1918-19. This is, perhaps, not a coincidence. In 1848, the Year of Revolution, a revolutionary wave swept over Europe. The epicenter was France but social unrest soon spread to the rest of the continent, with revolts in several German and Italian states, in the Habsburg Empire, in Greater Poland and elsewhere. It is well-known that the French revolution of 1848 resulted in suffrage reforms in France itself, but it is also noteworthy that countries, such as Denmark (1849), Switzerland (1848), the Netherlands (1848) and Belgium (1848), that were not directly affected by the revolutionary wave extended their franchises at the time. Likewise, the Russian Revolution of 1917 coincides with the second wave of franchise reform.

To quantify the threat of revolution, we have, based on Tilly (1993, 2004), Todd (1998) and Encyclopedia Britannica recorded all "revolutionary events" in Europe during the period.¹⁰ Revolutionary events are defined as "those instances when for a month or more at least two blocs of people backed by armed force and receiving support from a substantial

⁷Italy, Austria and Germany during the interwar period are, of course, examples of backlashes to democracy.

⁸Carstairs (1980) provides a brief account of the relevant reforms in each country.

⁹Our data refer to the right to vote in parliamentary elections and, in countries with bicameral systems, to elections for the lower chamber. We note that local and sub-national elections were often governed by a different franchise, as were the elections to the many upper chambers.

¹⁰We include Spain, Portugal, Russia, Poland, Ireland, Greece and the Balkans, as well as the 12 countries in the sample, in the definition of the "neighborhood".

part of the general population exercised control over important segments of the state organization" Tilly (2004, p. 73). Some of these events, however, refer to coup d'état and civil war. We have excluded those instances from our analysis in order to focus as closely as possible on situations where the ruling elite was threatened by a revolution as conceptualized in the work by Acemoglu and Robinson (2000). These instances are detailed in Tables A1 and A2 in the Appendix and the years of the various revolutionary events are recorded in Table 1. In the statistical analysis, we use three different indicators of the threat. *Revolution 1, weighted* takes into account that events that happened far away from a given country might have had less effect on the perceived threat level than events that happened closer to home. More specifically, *revolution 1, weighted* is defined as:

$$(\textit{revolution 1, weighted})_{it} = 1000 \sum_{j \neq i} \frac{R_{jt}}{d_{ij}}, \quad (1)$$

where d_{ij} is the distance in kilometers from country i to country j and R_{jt} is the number of revolutionary events that took place in country j in year t . The difference between this and our second measure, *revolution 2, weighted*, is that *revolution 1, weighted* only counts major events (recorded in Table A1) while *revolution 2, weighted* counts all events, some of which were minor and might not have been widely noted at the time in other countries. The latter are recorded in Table A2. The last measure (*revolution 1, unweighted*) is an unweighted version of revolution 1 where we simply count the number of revolutionary events in each year. We notice that we exclude "national" revolutionary events, i.e., events within a country itself. The rationale for doing so is strong. While revolutionary events in other countries can be viewed as exogenous to the politics of neighboring countries, events within a country itself is, by definition, related to local politics and could therefore be correlated with unobserved determinants of franchise reform. By excluding these events – e.g., the effect that the revolution in France in 1848 had on France itself – we avoid this problem.¹¹

<Table 1: The timing of Suffrage Reforms and Revolutionary Events in Europe, 1820-

¹¹We notice, however, that if we do include "national" events in the definition, it strengthens rather than weakens our results. These results are available upon request.

1938.>

3 Estimation Strategy

We estimate two different models: a dynamic panel model and an event history model. In the panel model, the dependent variable is *suffrage*, while the event history model seeks to estimate the time conditional probability of franchise reform. The panel model is specified as follows:

$$\begin{aligned} \text{suffrage}_{it} = & \beta_1 \text{suffrage}_{it-1} + \beta_2 \text{revolution}_{it} \\ & X_{it-5}v + \alpha_i + \eta_t + \varepsilon_{it}, \end{aligned} \tag{2}$$

where α_i is a country fixed effect, η_t is a time fixed effect¹² and ε_{it} is an error term with $E(\varepsilon_{it}) = 0$. We estimate equation (2) by OLS but allow for panel heteroskedasticity and for spatial correlations between the error terms across countries, and we base inference on panel corrected standard errors (PCSEs), as recommended by Beck and Katz (1995). Moreover, we include a lagged dependent variable. This captures the strong path dependency in the evolution of franchise institutions in western Europe.¹³ We stress that to identify the causal effect of the threat of revolution on the extension of the franchise, we require that *revolution 1* and *2* are conditionally independent of ε_{it} . This assumption is plausible as we exclude revolutionary events originating within each country itself.

The dynamics of the voting franchise is affected by many factors. We include as many of these as possible in the vector X .¹⁴ In the baseline specifications, we only include time-varying variables for which we have data covering the entire sample period from 1820. In

¹²We use 5-years time effects rather than yearly time dummies. While the distance weighted measures of the threat of revolution have cross-country and time variation, it is too collinear with the year dummies for estimation to be possible, as some year dummies have to be dropped to avoid a singular design matrix. The unweighted version of revolution 1 is perfectly collinear with the yearly time effects. We show in Section 5 that the results are similar if we use 2-years time dummies.

¹³The presence of a lagged dependent variable implies that the fixed estimator is biased. One possibility is to estimate the model with a GMM estimator (e.g., that of Arellano and Bond (1991)). However, this is not ideal because of the limited number of countries included in our study. Instead, we rely on Monte Carlo studies by Judson and Owen (1999) that show that the bias associated with the fixed effects estimator in partial adjustment models is small when the time horizon exceeds 20 years.

¹⁴The precise definitions and sources are given in the Data Appendix.

extensions, we add a number of other variables for which we only got partial coverage. According to Lipset's (1960) modernization hypothesis, higher income, education, and urbanization are major causes of democratization. We control for these forces by including *GDP per capita*, the *urbanization rate*, and a dummy variable, *educational attainment*, that is equal to one once enrollment in primary education surpasses 60 per cent and zero before than. Rigobon and Rodrik (2005), Lopez-Cordova and Meissner (2008) and others have argued that trade integration causes democratization. In the baseline specifications, we control for this by including a dummy variable, *gold standard*, that is equal to one if a country is on the gold standard and zero otherwise. The rationale is that being on the gold standard reduces trading costs and indirectly encourages trade integration. Data on trade flows is not available for all 12 countries for the full sample, but in some specifications, we use *trade volume*, measured as the sum of imports and exports over GDP, to capture trade integration more directly. Congleton (2004) stresses industrialization as a cause of democratization because it empowers pro-democracy lobby groups and in that way change the constitutional bargaining process. In some specifications, we control for this by including the variable, *agricultural share*, which measures the share of the workforce employed in agriculture and can be viewed as an inverse measure of industrialization. As argued by Mulligan and Shleifer (2005), a larger population means that there are more shoulders to bear the fixed cost associated with institutional innovations. Consequently, more populous countries should be more inclined to adopt franchise reforms with large fixed costs. To control for this, we include the variable *population*. It simply records the size of the population in each year. All these control variables are lagged by five years to reduce the risk of simultaneity bias.

Janowitz (1976) and, more recently, Ticchi and Vindigni (2009) have argued that mass conscription armies and war contributed to the development of democratic institutions in Europe and elsewhere. We control for this by including a dummy variable, *war*, that records if a country was at war in a given year.¹⁵ World War I (1914-1918) was a major shock to the political and economic order. It might not only have effected the countries

¹⁵We do not include colonial wars in this. Data on the size of armies exist (see, e.g., Flora et al., 1987), but do not cover much of the early part of the 19th century.

that were directly involved, but also the rest of Europe. To control for this and to isolate the effect of the Russian Revolution in 1917 from the general effect of the war, we include a dummy variable, *WWI*, that is equal to one for all countries during the period 1914-18. Finally, Brezis and Verdier (2003) argue, in the context of the recent political transitions in central and eastern Europe, that the decision to introduce democracy by the rulers of one country may affect the decision to democratize in other countries.¹⁶ Such learning or spillover effects could also have played a role during the 19th century. To investigate this possibility, we include the following measure of social learning in the model:

$$(\textit{social learning})_{it} = \sum_{k \neq i} d_{ik} D_{kt} \quad (3)$$

where d_{ik} is the distance in kilometers from country i to country k and D_{kt} is equal to the total number of franchise reforms undertaken by country k by year t . If social learning was important, we should find a positive coefficient to this variable.

For the event history study, we code the dependent variable y_{it} as 1 if country i introduced a franchise reform in year t and 0 in the years before and after that. A country drops out of the sample in the year after universal male suffrage was reached.¹⁷ We do not know precisely when a country became at "risk" of becoming democratic. So we deal with the problem of left censoring simply by assuming that countries enter the "risk set" either in 1820 or at the time of independence. These data are grouped duration data. It is, therefore, natural to use a duration model to estimate the relationship between the threat of revolution and the time conditional probability of introducing democracy (the hazard rate). We follow Beck et al. (1998) and estimate the following discrete logistic model:¹⁸

$$P(y_{it} = 1 | \textit{revolution}_{it}, X_{it}, M_{t-1} = 0) = \frac{1}{1 + e^{-(\lambda \textit{revolution}_{it} + X_{it}\gamma + H(\cdot))}} \quad (4)$$

where X_{it} is the vector of control variables (chosen from among those discussed above). The variable M is an indicator variable that is equal to 0 in each year before universal

¹⁶See also Gleditsch and Ward (2006).

¹⁷Since we focus on the process that led to universal male suffrage and exclude the democratic reversals during the interwar period, there is, by definition, no issue of "reentry".

¹⁸Beck et al. (1998) show that this is the discrete hazard model corresponding to the well-known continuous time proportional hazard model (Cox, 1975).

male suffrage and equal to 1 thereafter. It is unlikely that the hazard rate is independent of the political history of the countries. Our specification models duration dependence in a flexible way through the function $H(\cdot)$.¹⁹ We estimate $H(\cdot)$ using natural cubic splines and use the estimated spline coefficients²⁰ along with the cumulation of years since last reform (or since entry to the sample) to model duration dependence.

4 Results

The results of the panel model are reported in Table 2. Columns one to three show the estimates for the three different measures of the threat of revolution. All three measures are significant at the one percent level or better in all specifications. The threat of revolution was, therefore, a statistically significant and, we argue, a causal determinant of the franchise extension in western Europe. Not surprisingly, the effect is smallest when we use the broad definition of what constitutes a revolutionary event (column three). Since all countries in the sample, with the exception of the United Kingdom and France, were not democratic in 1820 (*suffrage* is coded zero) and all countries had universal male suffrage towards the end of the sample (*suffrage* is coded 100), the dependent variable is censored. In column four, we show what happens when we use a Tobit estimator to take this into account. We see that it does not make much difference to the results. Column five shows a specification that allows for country-specific AR(1) processes in the error terms. Combined with panel heteroskedasticity and spatial correlations between the error terms across countries, this effectively amounts to clustering the error terms at the country level. Again, we see that it does not make much of a difference. We can, therefore, rule out that our results are spurious artifacts of the fact that our measures of the threat of revolution are themselves serially correlated.²¹

The magnitude of the effect can best be grasped by considering the point estimate from

¹⁹The argument of the function is $t - t_i^p$ where t_i^p represents either the year in which country i enters the "risk set" (i.e., either 1820 or the year of independence) or the year of the previous franchise reform within the sample period.

²⁰We have determined the number of knots by a sequence of F-tests and have settled on a specification with two knots.

²¹See Bertrand et al. (2004) for a discussion of this point.

column two of Table 2. The short-run effect of an "extra" revolutionary event somewhere in Europe is to increase the franchise by just under two percentage points in the average country. The high degree of persistence in the franchise, however, implies that the long-run effect is much larger: around 33 percentage points. Seen in this perspective, 1848 – the Year of Revolution – in which we have recorded five different revolutionary events had a huge impact on the development of democratic institutions across western Europe.

<Table 2: Results for the Panel Model>

It is clear, however, that *suffrage* as well as several of the control variables on the right-hand side of the panel model are trending up and may be or behave as if there were nonstationary.²² This raises questions regarding the interpretation of the results reported in Table 2. However, we may begin by noting that, in the specification reported in column five of Table 2, where we allowed for country specific autocorrelation in the error term of equation (2), the estimated autocorrelation coefficients all were very low and far from a unit root. This goes a long way in dispelling the concern about the interpretation. However, we can go one step further and estimate an Error Correction Model (ECM) for suffrage using OLS with panel corrected standard errors, as suggested by Beck (2001). In particular, we estimate:

$$\Delta suffrage_{it} = \lambda_1 \Delta revolution_{it} + \Delta X_{it-5} \boldsymbol{\kappa} \quad (5)$$

$$+ \rho \left(suffrage_{it-1} - \gamma_1 revolution_{it-1} - X_{it-6} \boldsymbol{\omega} \right) + \varepsilon_{it}, \quad (6)$$

where the term in parenthesis is the long-run relation appropriately adjusted to match our other estimations. The estimated equation is:²³

$$\Delta suffrage_{it} = \underset{(0.23)}{1.19} \Delta revolution_{it} + \dots - \underset{(0.0097)}{0.039} \left(\underset{(6.28)}{suffrage_{it-1}} - 45.0 \underset{(6.28)}{revolution_{it-1}} - \dots \right), \quad (7)$$

²²Dickey-Fuller tests on the individual series suggest in several cases that we cannot reject nonstationarity of the series [not reported].

²³The figures in brackets underneath the coefficients are robust standard errors.

where we have suppressed the other control variables and only report results for *revolution 1, weighted* and the ρ parameter which measures if there is adjustment to a long-run equilibrium. The coefficients reported in equation (7) are all significant at the one per cent level. The equation implies a positive short-run effect of changes in the threat of revolution on changes in *suffrage*. More importantly, we find a substantial long-run effect which is in line with what we find in the partial adjustment model, though the effect is somewhat larger. The fact that $\hat{\rho} < 0$ implies adjustment to the long-run equilibrium. All in all, this suggests that the results reported in Table 2 are not an artifact of nonstationary data.

The results of the event history model are reported in Table 3.²⁴ They paint the same picture as those emerging from the panel model: the threat of revolution increased the probability of suffrage reform significantly across the board. The first three columns show the logit estimates for each of the three measures of the threat of revolution. We see that all three are statistically significant at the one percent level. A limitation of these specifications, however, is that the baseline hazard rate, while admitting duration dependency as discussed above²⁵ does not include a country-specific component. In column four, we report the results from a specification in which the baseline hazard rate is affected by idiosyncratic country-specific shocks. A comparison between the estimates from this random effects logit model and that reported in column one reveals very little difference. Another limitation is that democratizations are rare events.²⁶ This may magnify any systematic bias of the maximum likelihood estimate of the parameters of the logit model. King and Zeng (2001) have developed an estimator that corrects for this bias. We have re-estimated all the models using this estimator and report the results of one of these re-estimations in column five. We see that the coefficient on *revolution 1, weighted* continues

²⁴Germany and Switzerland are excluded from the sample used for the event history study, as they introduced full male suffrage at the same time as the two countries became independent nations.

²⁵A likelihood ratio test indicates strong duration dependence in the baseline hazard rate, and all the specifications shown allow the hazard rate to be time-dependent. Based on column one of Table 3, the likelihood ratio test for excluding *number of years since last reform* and the two splines is equal to 15.44 corresponding to significance at the one percent level.

²⁶In our sample, years with suffrage reform constitute less than four percent of the total number of cases.

to be statistically significant at the one percent level. The same is true with the two other measures of the threat of revolution (not reported). Finally, in column six we have clustered the standard errors at the country level. We see that this does not make much of a difference either. In the event history model, the magnitude of the effect of the threat of revolution can best be illustrated by considering the odds ratio. Based on the estimate reported in column two of Table 3, one extra revolutionary event increases the odds that a country will introduce a major suffrage reform by 108 percent. This is a substantial effect.

Besides the threat of revolution, being at war also appears, at least in the panel model, to be a driver of franchise extension.²⁷ This lends support to the theory that war and subscription armies were important for the development of suffrage institutions in Europe. Based on the point estimates reported in columns one to three Table 2, being at war, *ceteris paribus*, increases the franchise by about 3.6 percentage points in the short-run, with the long-run effect being about 17 times larger. It is interesting to notice that WWI by itself, rather than encouraging democratization, seems to have put a break on the process.

None of the other potential determinants of franchise extension seem to have mattered much. This includes, in particular, all the modernization variables. This rejection of the modernization hypothesis is in line with recent work by Acemoglu et al. (2008) but is at odds with the conclusions drawn from previous work on the question. We return to this issue in Section 6. We do not find any evidence either of social learning, nor do we find support for the notion that trade integration was a major driver of democracy. Occasionally, population size has a positive and significant impact. This lends some support to the idea that size might matter for the willingness to undertake reforms with large fixed costs.

<Table 3: Results from the Event History Model>

²⁷The ECM shows a similar picture with war affecting both the short-run dynamics of franchise reform and the long-term relationship, though the long-term coefficient is only significant at the ten percent level (not reported).

5 Additional Robustness Checks

In Table 4 (the panel model) and Table 5 (the event history model), we report on a number of robustness checks.²⁸ First, to investigate if using *gold standard* to proxy for trade integration is behind the rejection of trade as a cause of democratization, we have replaced it with the variable *trade volume*. Doing so reduces the sample size by about 200 observations. As can be seen from column one of Table 4, *trade volume* is marginally significant in the panel model, but insignificant in the event history model (see column one of Table 5). Both specifications, however, show the expected positive sign, giving some credence to the "trade-causes-democracy" thesis. Importantly, *revolution 1, weighted* is significant at the one percent level in both specifications and thus not effected much.

Second, we control for *agricultural share* to capture the impact of industrialization. We note from column two in Table 4 and 5, respectively, that it matters little for the coefficient on *revolution 1, weighted*, and that the coefficient on *agricultural share* is itself negative and insignificant in both the panel and event history model. Again, this is not encouraging for modernization theory.

Third, we have checked if the results are changed if we use two-years time dummies instead of five-years time effects. As seen from column three of Table 4, this does not change the main result for *revolution 1, weighted*. Thus, although we cannot control for yearly time effects, this strongly suggest that our result is not caused by unobserved common shocks (other than the threat of revolution) that occur on a yearly frequency.

Fourth, we have investigated the effect of restricting the sample to the "old world", i.e., the world before WWI (1820 to 1913). Many of the pivotal suffrage reforms had been undertaken by 1913, and we see from column four of Table 4 that the results are largely unaffected by this restriction on the sample. This underscores the importance of including the early suffrage reforms in the analysis and shows that information on what happened during WWI or during the interwar period is not essential for the conclusion that the threat of revolution was a major cause of franchise reform.

²⁸It has little consequence for the results reported in this section which of the three measures we use to capture the threat of revolution. For that reason, we only report the results with *revolution 1, weighted*.

Fifth, an alternative to our three measures of the threat of revolution is to use data on strikes as in Kim (2007). Doing this, however, restricts the sample to the period starting in 1880 (see footnote 2) and thereby we lose most of the early reforms. Substituting *revolution 1, weighted* with the variable *strike participants*, which measures the number of workers involved in strikes per 100,000 non-agricultural workers, in the panel or in the event history model results in an insignificant estimate (see column 5 of Table 4 and column 4 of Table 5).²⁹ This underscores two important features of our approach. Firstly, it is important to take a long historical perspective and include reforms undertaken early in the nineteenth century in the analysis. Secondly, it is important to focus on events outside the country under investigation as these seems to have played a major role for the perceptions about the seriousness of the threat of revolution.

Finally, we have tried to drop the other countries one at a time, and the results remain similar, leading us to conclude that the result is not driven by any single country [not reported].

<Table 4: Robustness checks for Panel model.>

<Table 5: Robustness check for the Event History Model.>

6 Discussion

This paper provides systematic, statistical evidence that the threat of revolution played a pivotal role for the evolution of suffrage rights in western Europe in the 19th and early 20th century. Our various measures of the threat are consistently one of the main determinants of the extension of the franchise during this period. The analysis lends strong support to one of the key building blocks of the theory of suffrage reform developed by Acemoglu and Robinson (2000, 2006). Of course, this is just one factor in accepting their theory. The evidence on the key implication of their theory, namely that the franchise extension

²⁹ *Strike participants* or any of the two other measures of strike activity recorded by Flora et al. (1987) is significant only in estimations in which the only control variable is *GDP per capita*. Once time dummies, country fixed effects or a lagged dependent variable are added, it becomes insignificant [results available upon request].

should be related to bigger government and more redistribution, is, on the other hand, more mixed (see, e.g., Aidt et al. (2006), Kenny and Winer (2006), or Aidt and Jensen (2009a,b)).

We conclude the paper by relating our findings to the broader literature on the causes of democracy. Most quantitative research on this question neither includes a measure of the threat of revolution, nor does it take a long historical perspective. By doing both, our study stands out. There is, however, some recent work that tests the "threat of revolution hypothesis" on more restricted data. The study by Kim (2007), discussed above, uses various measures of strike activity to proxy the threat of revolution, but only covers a small part of the 19th century. In a recent study, Przeworski (2009) proposes to use the sum of demonstrations, riots, and strikes to proxy for the threat of revolution in a world sample covering the period from 1919 to 1995. He finds that this measure of the threat of revolution is a significant predictor of franchise reform. As 1919 is, in fact, the year in which the last of the 12 countries included in our study gained universal male suffrage, Przeworski's evidence does not speak directly to the extension of the franchise in western Europe. Our evidence does, and the two sets of results, therefore, complement each other perfectly. Taking a long historical perspective also allows us to investigate the "Janowitz thesis" that war was an important impulse for democratic reform in western Europe. Here, in contrast to Przeworski (2009), we find some evidence that it was, lending support to the approach to endogenous democratization taken recently by Ticchi and Vindigni (2009).

Modernization theory has previously found support in the work by Barro (1999) and many others.³⁰ Our results put a serious question mark on the generality of this evidence. As noted above, a similar doubt has been raised by Acemoglu et al. (2008). They show that once country-specific fixed effects and the high degree of persistence in democratic institutions are taken into account, GDP per capita is no longer causally related to democracy. Gundlach and Paldam (2009) have, however, challenged this result. They argue that it is inappropriate, when testing modernization theory, to combine lagged values of measures of democracy and country fixed effects with yearly data, as it introduces so much multi-

³⁰Barro (1999) found that GDP per capita can explain a substantial fraction of the cross-country variation in the Gastil Index. See also Boix (2003).

collinearity that it becomes impossible to identify any link – even if one exists – between democracy and income. In our data, the within-country range of *suffrage* is wide (typically ranging from 0 to 100). We, therefore, have plenty of variation from which a relationship between income and democracy could, in principle, be identified, and we believe that the sort of multi-collinearity alluded to by Gundlach and Paldam (2009) is less of a concern in our context and not the reason why we cannot find evidence of ‘modernization’. This belief is strengthened by the fact that *GDP per capita* is insignificant also in the event history model where we cannot control for country fixed effects. Przeworski et al. (2000) reach a similar conclusion and point out that democratization is not caused by modernization, but GDP per capita *is* important for democratic consolidations. Furthermore, Gassebner et al. (2007) show in an extreme bounds analysis of the determinants of democracy using a large postwar sample of countries from around the world that GDP per capita is not a robust determinant of democracy.³¹

So in conclusion, this study provides new evidence in favor of "the threat of revolution thesis" for the critical period in western European history during which the franchise was gradually extended. Our study also adds to an increasing number of studies suggesting that the "modernization thesis" – at least in its simplest form – cannot explain why democratic institutions emerged in the West. We do acknowledge, however, that more research is necessary to settle the latter matter and we believe that western Europe during the 19th century constitutes a promising testing ground for doing so.

7 Data Appendix

7.1 Definitions and Sources

The variables used in the analysis are defined as follows:

1. *Suffrage* is the electorate in percentage of the enfranchised age and sex group; before the women’s suffrage, male population only (parliamentary elections). Sources: Flora et al. (1983), Marshall and Jaggers (2000), and Cook and Paxton (1998).

³¹We note that there are methodological problems with extreme bounds analysis, see Jensen (2009) and Jensen and Würtz (2006).

2. *Revolution 1 and 2, weighted* is revolutionary events weighted by geographic distance, excluding events in each country itself. The difference between *revolution 1* and *2* is that the former only include major events (listed in Table A1) while the latter in addition include minor events (listed in Table A2). *Revolution 1, unweighted* counts the number of (major) revolutionary events in each year. Sources: Tilly (1993, 2004), Todd (1998), Rose (2000) and Encyclopedia Britannica (2009).
3. *GDP per capita* is real GDP at international 1990 Geary-Khamis dollars, adjusted to exclude the impact of border changes, per capita. Source: Maddison (2003).
4. *Population* is the size of the total population in 1000s. Source: Maddison (2003).
5. *Agricultural share* is the number of individuals employed in agriculture, mining and fishing per 1000 employees. Source: Mitchell (2007).
6. *Urbanization rate* is the proportion of the population who lives in towns with more than 20,000 inhabitants. Source: Banks (2003).
7. *Education attainment* is a dummy coded 1 for the years after which enrollment in primary education as a percentage of all 5-14 year olds reached 60% and 0 otherwise. Sources: Flora (1983) and Mitchell (2007).
8. *Gold standard* is a dummy equal to 1 if a country is on the gold standard in a given year and 0 otherwise. Sources: Meissner (2004) and EH.net encyclopedia (eh.net/encyclopedia).
9. *Trade volume* is exports plus imports relative to GDP. Sources: Mitchell (2007), Netherlands Central Statistics Bureau (1999), Buyst (1997), Krantz and Schön (2007), Grytten (2004), Flandreau and Zumer (2004); The Swiss Economic and Social History online database (www.fsw.uzh.ch/histstat).
10. *Social learning* is defined as a distance weighed average of franchise reforms in other countries. Sources: Rose (2000) and the sources used to define years of franchise reform.

11. *War* is a dummy variable equal to 1 if a country is at war and 0 otherwise. Sources: Encyclopedia Britannica (2009).
12. *WWI* is a dummy equal to 1 during World War I and 0 otherwise.
13. *Strike participants* is the number of workers involved in strikes per 100,000 non-agricultural workers. Source: Flora et al. (1987).

7.2 Notes on the construction of the data sets

The original data contains gaps for some of the variables. This is obviously not true for reform years, but for *suffrage* these gaps arise because these political data are only recorded in election years. In off-election years, we have no observations. For *suffrage*, we have made the assumption that the variable stayed constant between elections. The series for GDP per capita and the urbanization rate are complete for most countries, but, in the few cases, where data are missing we have interpolated linearly. Linear interpolation seems a reasonable solution, although more sophisticated methods are available (see, e.g., Little, 1992).

<Table A1: Major revolutionary events (*revolution 1*).>

<Table A2: Minor revolutionary events (*revolution 2*).>

<Table A3: Summary Statistics for variables used in the analysis.>

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Table 1: Timing of Suffrage Reforms and Revolutionary Events in Europe, 1820-1938

Country	Reform years	Revolutionary events
United Kingdom	1832, 1867, 1884, 1918	
Austria	1873, 1896, 1907	1848-49
Italy	1861, 1882, 1912, 1919	1820, 1848-49
Norway	(1814) 1884, 1897	
Netherlands	1848, 1887, 1894, 1917	
Sweden	1866, 1907, 1919	
France	1824, 1830, 1848	1830, 1848, 1870-71
Germany	1871 ^a	1848-49
Finland	1869, 1906	
Belgium	1831, 1848, 1893, 1919	1830-3
Switzerland	1848	
Denmark	1849, 1915	
<i>Hungary</i>	n.a.	1848-49, 1918-19
<i>Russia</i>	n.a.	1905, 1917
<i>Ireland</i>	n.a.	1916
<i>Spain</i>	n.a.	1820-23, 1827 , 1836, 1840, 1842, 1843, 1854-6, 1866, 1868, 1873-4, 1890, 1909, 1930, 1933, 1934
<i>Portugal</i>	n.a.	1820 , 1910, 1915 , 1919, 1927
<i>Greece</i>	n.a.	1843, 1866-8, 1935, 1938
<i>Balkans</i>	n.a.	1826, 1861, 1885, 1888, 1907
<i>Greater Poland</i>	n.a.	1830-1, 1863-64

Sources: Carstairs (1980), Flora et al. (1983), Encyclopaedia Britannica, Todd (1998), Tilly (1993, 2004).

Notes: Countries listed in italic are not included in the sample as such, but revolutionary events in these countries are used to calculate the three measures of the threat of revolution. In column 3 the years in bold indicate the events included in the construction of *revolution 1* and *3*. The remaining years indicates the additional minor events included in the construction of *revolution 2* (see Appendix for details). a. Right from its unification, Germany had full male suffrage, and the Weimar republic of 1920 is therefore not regarded as a reform year. *Suffrage* is, in fact, close to 98 percent before 1920.

Table 2: Results for the Panel Model.
Dependent variable: Suffrage.

	(1)	(2)	(3)	(4)	(5)
Revolution 1, weighted	1.277***			1.428***	1.335***
	[6.331]			[5.817]	[6.643]
Revolution 1, unweighted		1.956***			
		[5.529]			
Revolution 2, weighted			0.651***		
			[3.446]		
Suffrage (lagged)	0.942***	0.941***	0.942***	0.960***	0.932***
	[75.70]	[74.93]	[75.09]	[95.68]	[69.78]
Log GDP per capita (lagged)	-0.574	-0.608	-0.588	-3.889*	0.719
	[-0.267]	[-0.282]	[-0.272]	[-1.851]	[0.294]
Log Population (lagged)	4.343*	4.598*	4.220*	8.921***	3.726
	[1.790]	[1.906]	[1.750]	[4.053]	[1.465]
Urbanization rate (lagged)	-0.00244	-0.00252	-0.00264	-0.0135**	0.00527
	[-0.344]	[-0.358]	[-0.373]	[-2.048]	[0.568]
War	3.682***	3.734***	3.736***	3.896***	4.289***
	[3.777]	[3.829]	[3.792]	[4.144]	[4.176]
WWI	-3.595***	-3.875***	-3.252**	-1.859	-3.498***
	[-2.748]	[-2.900]	[-2.364]	[-1.395]	[-2.786]
Social learning	-16.66	-22.2	-6.506	-39.61	34.12
	[-0.270]	[-0.357]	[-0.105]	[-0.788]	[0.530]
Gold standard	-0.0137	-0.0655	-0.0508	0.379	0.21
	[-0.0211]	[-0.0999]	[-0.0770]	[0.631]	[0.303]
Educational attainment (lagged)	-0.274	-0.248	-0.34	-0.0697	-0.884
	[-0.341]	[-0.309]	[-0.422]	[-0.0997]	[-1.059]
Constant	-26.03	-27.67	-25.05	-37.82*	-31.51
	[-1.076]	[-1.140]	[-1.030]	[-1.755]	[-1.230]
Observations	1069	1069	1069	1069	1069
Number of countries	12	12	12	12	12
Estimation technique	PCSE	PCSE	PCSE	Tobit	FGLS with individual AR-

Notes: Robust z statistics in brackets; * significant at 10%; ** significant at 5%; *** significant at 1%. All estimations include country fixed effects and five years average time effects.

Table 3: Results from the Event History Model
Estimates of the probability of a suffrage reform, 1820-1938.

	(1)	(2)	(3)	(4)	(5)	(6)
Revolution 1, weighted	0.511***			0.478***	0.511***	0.511***
	[4.210]			[5.231]	[4.210]	[7.304]
Revolution 1, unweighted		0.736***				
		[3.939]				
Revolution 2, weighted			0.450***			
			[3.703]			
Log GDP per capita (lagged)	-0.0904	0.21	-0.159	-0.233	-0.0903	-0.0904
	[-0.0459]	[0.107]	[-0.0814]	[-0.105]	[-0.0458]	[-0.0498]
Log Population (lagged)	1.002***	0.944***	0.882***	0.915***	1.002***	1.002***
	[3.280]	[3.143]	[3.061]	[2.922]	[3.280]	[4.357]
Urbanization rate (lagged)	-0.0031	-0.00276	-0.00255	-0.00245	-0.0031	-0.0031
	[-0.854]	[-0.768]	[-0.708]	[-0.585]	[-0.854]	[-1.012]
War	-0.278	-0.134	-0.0474	-0.204	-0.278	-0.278
	[-0.273]	[-0.135]	[-0.0483]	[-0.184]	[-0.273]	[-0.360]
WWI	-0.591	-0.92	-0.369	-0.423	-0.591	-0.591
	[-0.517]	[-0.815]	[-0.320]	[-0.344]	[-0.517]	[-0.476]
Social learning	42.68	39.3	38.89	41.57	42.68	42.68
	[1.366]	[1.261]	[1.257]	[1.248]	[1.366]	[1.545]
Gold standard	-0.487	-0.683	-0.411	-0.45	-0.487	-0.487
	[-0.802]	[-1.139]	[-0.672]	[-0.680]	[-0.802]	[-0.660]
Educational attainment (lagged)	0.469	0.341	0.481	0.431	0.469	0.469
	[0.654]	[0.482]	[0.677]	[0.597]	[0.655]	[0.679]
Number of years since last reform	0.143	0.121	0.13	0.109	0.143	0.143
	[1.014]	[0.837]	[0.921]	[1.007]	[1.014]	[1.408]
Spline 1	0.000333	0.000264	0.000281	0.00025	0.000333	0.000333
	[0.626]	[0.491]	[0.532]	[0.535]	[0.626]	[0.709]
Spline 2	-0.00015	-0.00012	-0.00012	-0.000116	-0.000148	-0.000148
	[-0.622]	[-0.495]	[-0.523]	[-0.532]	[-0.622]	[-0.688]
Constant	-14.16	-15.5	-12.65	-11.78	-14.16	-14.16
	[-0.994]	[-1.093]	[-0.901]	[-0.708]	[-0.995]	[-1.129]
Observations	647	647	647	647	647	647
Number of countries	10	10	10	10	10	10
Estimation technique	Logit	Logit	Logit	Random effects	Rare events	Clustering

Notes: z statistics in brackets; * significant at 10%; ** significant at 5%; *** significant at 1%. Only ten countries are included in the event history study as Germany and Switzerland had full male suffrage from the time they became unified countries.

Table 4: Robustness Checks for the Panel Model
Dependent variable: *Suffrage*.

Robustness check	(1)	(2)	(3)	(4)	(5)
	Add Trade	Add Agricultural share	Two-year dummies	Till 1913	Strike data
Revolution 1, weighted	1.711***	1.027***	1.224***	1.330***	
	[7.855]	[3.762]	[6.562]	[6.103]	
Strike Participants					-0.0000006
					[-0.0422]
Suffrage (lagged)	0.929***	0.926***	0.935***	0.929***	0.926***
	[58.38]	[60.87]	[73.89]	[54.54]	[25.97]
Log GDP per capita (lagged)	0.0838	-1.996	-1.661	0.838	3.053
	[0.0298]	[-0.760]	[-0.721]	[0.248]	[0.476]
Log Population (lagged)	4.722*	10.98***	5.108**	3.125	7.248
	[1.662]	[3.270]	[2.081]	[0.754]	[1.027]
Urbanization rate (lagged)	0.0105	-0.00862	-0.00361	-0.00777	0.00678
	[1.190]	[-1.087]	[-0.501]	[-0.613]	[0.473]
War	3.684***	1.818*	3.618***	7.484***	-1.215
	[3.629]	[1.650]	[3.550]	[5.720]	[-0.634]
WWI	-3.925**	-3.172**	-2.262		-2.192
	[-2.495]	[-2.206]	[-1.101]		[-1.370]
Social learning	-102.9	-110	-105.5	1.52	-106.8
	[-1.268]	[-1.357]	[-1.530]	[0.0172]	[-0.750]
Gold standard		-0.143	-0.0993	0.458	-0.188
		[-0.196]	[-0.139]	[0.427]	[-0.202]
Educational attainment (lagged)	-0.186	-1.276	-0.188	-0.371	-0.76
	[-0.202]	[-1.335]	[-0.231]	[-0.394]	[-0.384]
Trade volume (lagged)	0.0335*				
	[1.937]				
Agricultural share (lagged)		-0.00827			
		[-0.974]			
Constant	-34.45	-59.11**	-27.2	-26.63	-70.6
	[-1.156]	[-2.047]	[-1.110]	[-0.613]	[-1.545]
Observations	858	876	969	809	475
Number of countries	12	12	12	12	12
Estimation technique	PCSE	PCSE	PCSE	PCSE	PCSE

Notes: See notes to Table 2.

**Table 5: Robustness Checks for the Event History Model
Estimates of the probability of a suffrage reform, 1820-1938.**

	(1)	(2)	(3)
Robustness check	Add Trade	Add Agricultural share	Strike data
Revolution 1, weighted	0.573*** [4.009]	0.432*** [3.003]	
Strike participants			0.000335 [1.371]
Log GDP per capita (lagged)	2.563 [1.031]	-0.0178 [-0.00733]	8.725 [1.470]
Log Population (lagged)	1.123*** [2.792]	1.098*** [2.674]	2.371 [1.582]
Urbanization rate (lagged)	- 0.00918* [-1.803]	-0.00593 [-1.122]	-0.0280** [-2.320]
War	0.196 [0.184]	-1.377 [-1.070]	-3.238 [-1.117]
WWI	-0.158 [-0.133]	-0.183 [-0.157]	-0.166 [-0.0811]
Social learning	12.56 [0.315]	42.87 [1.255]	87.02 [0.950]
Gold standard	-0.133 [-0.193]	-0.781 [-1.177]	-3.727** [-2.117]
Educational attainment (lagged)	-0.347 [-0.382]	0.397 [0.548]	-0.496 [-0.208]
Trade volume (lagged)	0.0168 [1.262]		
Agricultural share (lagged)		-0.00208 [-0.622]	
Number of years since last reform	0.205 [1.373]	0.088 [0.500]	0.749 [0.773]
Spline 1	0.000548 [0.976]	3.31E-05 [0.0502]	0.00121 [0.390]
Spline 2	-0.00024 [-0.942]	-3.4E-06 [-0.0117]	-0.00035 [-0.266]
Constant	-34.28* [-1.789]	-13.52 [-0.720]	-98.40** [-2.037]
Observations	529	471	174
Number of countries	9 ^a	10	7 ^b
Estimation technique	Logit	Logit	Logit

Notes: See notes to Table 3. a. Data from Belgium are lost. b. Data from Norway, Finland and France are lost due to missing data the strike participants series.

Table A1: Major revolutionary events (*Revolution 1*).

Events	Region	Year
Revolution in Hungary	Balkans	1848-9
Bloodless revolution in Hungary, ending in foreign military intervention	Balkans	1918-19
Mutiny of Spanish troops under Colonel Rafael Riego, generalizing revolution to 1823, termination by French invasion	Iberia	1820-1823
Revolution at Oporto, Portugal	Iberia	1820
Revolt of malcontents in Spain	Iberia	1827
Portuguese insurrection of General Pimenta de Castro, followed by democratic revolution	Iberia	1915
Belgian revolution vs. Holland (French, British intervention)	Belgium	1830-3
Easter Rebellion in Ireland	British Isles	1916
July Revolution	French states	1830
French Revolution	French states	1848
State collapse, occupation, republican revolutions	French states	1870
Multiple communes	French states	1870-71
Russian revolution	Russian states	1905
Russian revolution	Russian states	1917
Naples	Italy	1820
Italian States	Italy	1848-9
Habsburg	Austria	1848-9
German states	Germany	1848-9

Table A2: Minor revolutionary events (*Revolution 2*)

Events	Region	Year
Janisary rebellion in Constantinople	Balkans	1826
Pro-constitutional uprising in Greece	Balkans	1843
Revolt in Hercegovina	Balkans	1861
Revolt in Crete	Balkans	1866-8
Insurrections in Bosnia, Herzegovina, Bulgaria	Balkans	1875-8
Pro-bulgarian revolution in Eastern Roumelia	Balkans	1885
Peasant insurrection in Romania	Balkans	1888
Peasant insurrection in Moldovia	Balkans	1907
Young Turk's revolution in Ottoman Empire, including insurrection in Macedonia	Balkans	1908-9
Albanian insurrection	Balkans	1910
Venezelist rising in Greece	Balkans	1935
Revolt in Crete	Balkans	1938
Royalist rising in Spain	Iberia	1822-23
Progressist insurrection in Andalusia, Aragon, Catalonia and Madrid, ending in constitution of 1837	Iberia	1836
Revolt of General Baldomero Espartero who seized power in Spain	Iberia	1840
Rising in Barcelona, temporary declaration of republic, crushed by Espartero	Iberia	1842
Coalition deposes Espartero; Narvaez president to 1851	Iberia	1843
Spanish revolution led by O'Donnell and Espartero	Iberia	1854-6
Failed insurrection of General Juan Prim	Iberia	1866
Pronunciamento of Admiral Juan Topete; generalization of insurrection	Iberia	1868
First Spanish Republic, Carlists rising	Iberia	1873-4
Anarchist outrages in Spain	Iberia	1890
Catalan general strike, insurrection	Iberia	1909
Insurrection in Lisbon, proclamation of republic	Iberia	1910
Royalist uprising in Northern Portugal	Iberia	1919
Failed insurrection against Portuguese military regime	Iberia	1927
Mutiny of garrison at Jaca, demanding republic	Iberia	1930
Barcelona rising of anarchists and syndicalists	Iberia	1933
Working-class insurrection in Asturias, general strike and insurrection in Catalo	Iberia	1934
Polish rebellion in Greater Poland	Russian states	1830-1
Polish rebellion in Greater Poland	Russian states	1863-4

Table A3: Summary statistics for variables used in the analysis.

Variable	#Observations	Mean	Std. Dev.	Min	Max
Suffrage	1297	47.463	38.60434	0	110.1
Revolution 1, weighted	1403	0.352	1.241	0	15.105
Revolution 1, unweighted	1403	0.246	0.742	0	5
Revolution 2, weighted	1403	0.719	1.374	0	15.105
Suffrage	1297	47.463	38.60434	0	110.1
Log GDP per capita	1280	7.74	0.45	6.66	8.76
Log Population	1413	8.85	1.21	6.79	11.10
Urbanization rate	1278	206.09	152.54	0.00	732.00
War	1330	0.04	0.21	0.00	1.00
WWI	1403	0.04	0.20	0.00	1.00
Social learning	1403	0.03	0.02	0.00	0.08
Gold standard	1403	0.40	0.49	0.00	1.00
Education attainment	1237	0.46	0.50	0.00	1.00
Strike participants	502	3449.63	10295.18	0.00	209389.00
Trade volume	948	45.01	25.63	2.24	140.17
Agricultural share	952	404.23	157.97	52.00	821.05