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by

Toke S. Aidt

and

Peter S. Jensen

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FURTHER INFORMATION
Department of Business and Economics
Faculty of Social Sciences
University of Southern Denmark
Campusvej 55
DK-5230 Odense M
Denmark

Tel.: +45 6550 3271
Fax: +45 6550 3237
E-mail: lho@sam.sdu.dk
http://www.sdu.dk/osbec

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Workers of the World, Unite! Franchise Extensions and the Threat of Revolution in Europe, 1820-1938*

Toke S. Aidt, University of Cambridge†
Peter S. Jensen, University of Southern Denmark‡

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Abstract

This paper tests the hypothesis that the extension of the voting franchise was caused by the threat of revolution, as suggested by Acemoglu and Robinson (2000). We approximate the threat of revolution in a given country by revolutionary events happening in neighboring countries. We investigate the relationship between this new measure of the threat of revolution and measures of suffrage reform in two samples of European countries covering the period from 1820 to 1938. We find strong support for the 'threat of revolution theory'. We also find some evidence that war triggered suffrage reform, whereas 'modernization theory' receives little support.

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

JEL classification: D7, P16.

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†Corresponding Author: Faculty of Economics, University of Cambridge, Cambridge CB3 9DD, United Kingdom. E-mail: toke.aidt@econ.cam.ac.uk. Phone: +44 1223 335231. Fax: +44 1223 335475.

‡Department of Business and Economics, University of Southern Denmark, Campusvej 55, 5230 Odense M, Denmark. E-mail: psj@sam.sdu.dk.

1 Introduction

Why would the political elite of a country ever want to extend the voting franchise to broader segments of the population? After all, by doing so, it dilutes its own political base and exposes itself to the risk of redistribution. One answer to the challenge posed by this question is that the governing classes extend the franchise to avoid revolution or other forms of radical social transformation.[1] To be sure, there are many other ways to head off a revolution, but, as Acemoglu and Robinson (2000, 2006) point out in their seminal work on this question, voting rights have the advantage that once they are granted, they are hard to take back.[2] 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. Along similar lines, Conley and Temini (2001) argue that the extension of franchise occurs when the interests of the enfranchised and disenfranchised groups conflict and the disenfranchised group presents a credible threat. Boix (2003) also views the threat of revolution as a process that strengthens the hand of the disenfranchised, but emphasizes the interaction with structural and organizational parameters that makes democracy cheap relative to autocracy rather than the commitment value of democracy.

The threat of revolution plays a central role in these arguments. The historical record provides 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 when Lord Grey introduced the reform bill in Parliament earlier that year with the words 'the principal of my reform is to prevent the necessity of revolution[...] I am reforming to preserve, not to overthrow' he was making a clear reference to the perceived risk of violent and radical social change. 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. The perceived link between franchise extension and the threat of revolution is by no means unique to Britain.

[1] See Tullock (1971) for a classical exposition of why revolutions happen.

[2] For alternative theories of franchise extension, see Justman and Gradstein (1999), Lizzeri and Persico (2004), Llavador and Oxoby (2005), Falkinger (1999) or Congleton (2004, 2007, 2011). Jack and Lagunoff (2006) formulate a general theory of dynamic enfranchisement that encompasses many of these competing theories.

For example, Tilton (1974) grants the threat of revolution a central role in the Swedish franchise reforms of 1866 and 1909, and, of course, the revolution of 1848 was a direct cause of subsequent franchise reform in France, as was the (unsuccessful) 1905 revolution in Russia.

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 and yet more challenging to establish if any observed correlation between the threat of revolution and suffrage reform represents a causal effect or is just coincidental. A small literature has made some attempt at addressing these challenges. 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 western European countries between 1880 and World War II. Przeworski (2009) studies a broader sample of countries but his focus is on the period after the Great War. He uses data on demonstrations, riots, and strikes to proxy the threat and establishes a strong correlation between these measures and the probability of suffrage reform.

In this paper, we propose a new measure of the threat of revolution. According to the 'threat of revolution theory' the political elites contemplating franchise reform needed to assess the likelihood of revolution in their country. To this end, they would obviously use information about the situation locally, e.g. about riots, strikes or other types of social unrest. But they would also observe what was happening elsewhere and use reports about revolutionary activities in neighboring countries to update their assessment of the likelihood of revolution at home. Based on this, they would then decide whether to relinquish power and to extend voting rights as a preemptive measure. Our proposed measure of the threat of revolution is based on this logic of international transmission of information. 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 new measures of the threat of revolution as it might have been perceived by the governing elites in different countries in the region at the time. The underlying logic suggests that the governing elites would learn more from revolutionary events 'closer to home' and we construct threat

measures based on geographical and linguistic distance to the event.

Our approach has two major advantages that sets it apart from previous tests of the 'threat of revolution theory'. The first advantage is that our measure can, under two plausible conditions, be used to access the causal link, if any, between the threat of revolution and suffrage reform. First, the governing elites in each country revised their assessment of the risk of a home-grown revolution upwards after observing revolutionary events in neighboring countries. As we discuss in section 3, there exists convincing historical evidence that the elites did in fact pay attention to revolutionary events as and when they happened abroad and that these played a pivotal role in decisions to extend the franchise. Second, by focusing on revolutionary events in the 'neighborhood' of a country and by excluding events that happened within a country itself, these events represent exogenous shocks to the information set of the elite. Our measure of the threat level is therefore unlikely to be correlated with other (observed and unobserved) determinants of suffrage reform – such as political rivalry between factions within the national elites or general enlightenment trends – originating within that country. In contrast, threat measures based on 'national events' – in particular labor market unrest and riots – are likely to be endogenous to the political situation of the country in which they take place. In fact, one may conjecture that reform politics and local riots and strikes might be driven by the same largely unobserved political and economic factors. Accordingly, while Kim (2007) and Przeworski (2009), on the basis of such measures, have uncovered a suggestive correlation between strikes and riots on the one hand, and suffrage reform on the other, it remains unclear whether this represents a causal mechanism or not.[3] The second advantage of our approach is that we can quantify the threat for the critical period in the 19th and early 20th centuries during which the franchise was in fact extended in Europe. Due to data limitations previous work focused on the period after the Great War (Przeworski, 2009) or had 1880 as starting point (Kim, 2007).[4] Our data allow us to start the analysis in 1820 and thus to cover the period in

[3]Kim (2007), for example, does not take into account unobserved country or time fixed effects when estimating the effect of strikes on suffrage reform. The estimated correlation could, therefore, represent unobserved political factors at the national level or international political shocks.

[4]It is difficult to reconstruct data on strikes for the 19th century. The earliest data are from France, Italy and Sweden and are recorded from the 1880s; data for Austria, Belgium, Denmark, Germany and the

European history during which the franchise was actually extended.

Using these new measures of the threat of revolution, we estimate two alternative models of franchise reform using two samples of European countries from 1820 to 1938. We control for other potential determinants of democracy such as income, urbanization, education, war, trade integration, social learning etc. as well as for alternative strategies that the governing classes might have used to cope with the threat of revolution. 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 as a proportion of 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 during the first wave of democratization in Europe. This lends strong support to the 'threat of revolution theory'.

The rest of the paper is organized as follows. In section 2, we present a model of franchise reform based on a simple extension of Acemoglu and Robinson (2000). In section 3, we present our data on revolutionary events and suffrage reform. In section 4, we discuss issues related to identification. In sections 5 and 6, we report the evidence on the 'threat of revolution theory' coming from the dynamic panel and the event history model, respectively. In section 7, we discuss evidence related to competing theories of franchise reform. In section 8, we conclude.

2 Theory

Our test of the 'threat of revolution theory' is based on the idea that revolutionary events abroad represent exogenous shocks to the information set of the elites in other countries and may, through that channel, be a trigger of suffrage reform. International transmission of such information could work through a number of alternative channels.

One possibility is that the governing elites are unsure about whether a revolution is in the making or, if it is, about what the consequences of a revolution might be. In this case,

UK are recorded from the 1890s and for other countries not until the 20th century. Thus, as documented below, many major franchise reforms are not covered by the sample studied by Kim (2007).

revolutionary events abroad offer an opportunity for the elites to learn from the experience of others and base their decision regarding political reform on that. To formalize this logic, we develop a simplified version of the model of franchise extension presented by Acemoglu and Robinson (2000).

2.1 Assumptions

We consider a society with an infinite time horizon, $t = 0, 1, ...\infty$. It is populated by two groups of individuals, the rich and the poor. The majority of the population is poor. The *political state* (S_t^{Pol}) of the society can be either democracy ($\mathcal{D}$), autocracy ($\mathcal{A}$), or a post-revolutionary regime ($\mathcal{S}$), i.e., $S_t^{Pol} \in \{\mathcal{D}, \mathcal{A}, \mathcal{S}\}$. Utility is linear in income and is discounted by the factor β. We specify the per-period incomes of the members of the two groups directly as functions of the political states and denote them by $y_g\left(S_t^{Pol}\right)$ for $g \in \{R, P\}$.[5] Under autocracy, the rich control the government and no redistribution takes place. The income of the rich is $y_R(\mathcal{A})$ while that of the poor is $y_P(\mathcal{A}) < y_R(\mathcal{A})$. Under democracy, the poor hold the majority and use the state to redistribute income from the rich. As a consequence, $y_R(\mathcal{A}) > y_R(\mathcal{D}) > 0$ and $y_P(\mathcal{A}) < y_P(\mathcal{D})$. Finally, in the post-revolutionary regime, the rich fare worse than under democracy while the poor are better off. To capture this, we assume that $y_R(\mathcal{S}) = 0$ and $y_P(\mathcal{S}) \geq y_P(\mathcal{D})$. The post-revolutionary regime can be interpreted as socialism under which wholesale expropriation of the assets of the rich take place, but it can also be understood simply as another type of 'democracy' where the rules are (particularly) biased in favor of the poor.

The initial political state is autocracy. Regime transitions happen either through a revolution or through democratization. We use the term revolution broadly to mean any form of costly social transformation, whereas democratization is understood as orderly and costless (or less costly) social transformation. The opportunities for a revolution depend

[5] These incomes can be derived from more fundamental assumptions about endowments, production technologies, and tax instruments as in Acemoglu and Robinson (2000, 2006). By specifying the incomes directly, we rule out one of the strategies that the rich may use to avoid a revolution: temporary welfare spending. The choice between welfare spending and a franchise extension is vital for understanding why democratization has commitment value, but is less important for understanding our empirical strategy. For this reason, we maintain this assumption, but return to the question of welfare transfers as an alternative to franchise extension in section 4.

on the *social state* ($S_t^s \in \{G, B\}$). In social state B, a revolution is impossible, while in social state G, the poor pose a real threat to the rich and may, at a cost, stage a revolution. This results in a transition to the post-revolutionary regime, which we assume is an absorbing state. The cost of a revolution is measured as a fraction of the income of the poor, $(1-\mu) \in (0,1)$, so that the discounted income of a poor citizen after a revolution is $\frac{\mu y_P(\mathcal{S})}{1-\beta}$. To avoid a revolution, the rich can extend the franchise. This is better for them than a revolution and, under the assumption that $\mu < \frac{y_P(\mathcal{D})}{y_P(\mathcal{S})}$, it can, in fact, prevent a revolution from happening. With this assumption, we can ignore, in the rest of the analysis, the possibility that the poor could, in principle, overthrow a democracy through a revolution.

We assume that the poor observe the social state directly, so they know if a revolution is feasible or not[6] and what the cost is. The rich, on the other hand, do not observe the social state directly. Instead they receive reports about the likely state from two sources: a national and an international source. The national source represents information gathered by the police and the army about home-grown revolutionary activities, news reports about local riots, uprisings and other types of unrest, information about business cycle conditions and so on. The international source represents information received about revolutionary activities abroad. Such activities provide vital information to the rich about the social state and allow them to access the risk of a revolution in their own country better. To formalize this, we assume that at time t the posterior belief of the rich that the social state is G at that time is

$$q_t = F\left(q, \omega_t^N, \omega_t^I\right) \tag{1}$$

with $q_t \in (0,1)$. The two variables ω_t^N and ω_t^I represent national and international reports received at time t by the rich about local and international revolutionary events, respectively. We assume that q_t is increasing in both arguments. If no reports are received at all, $q_t = F(q, \emptyset, \emptyset) = q \in (0,1)$ and if no international reports are received the posterior belief is determined solely by national reports, $q_t = F\left(q, \omega_t^N, \emptyset\right) = f\left(q, \omega_t^N\right)$. We interpret

[6] This is an extreme assumption. It could be relaxed by assuming that the poor, like the rich, observe the social state imperfectly. Doing so, would strengthen the importance of information about revolutionary events abroad.

q as the 'baseline' threat of revolution and notice that since at time t the rich have yet to receive reports for the future ($\left\{\omega_{t+j}^N, \omega_{t+j}^I\right\}$ for $j = 1, 2...$), they believe at time t that $q_{t+j} = q$ for all $j = 1, 2.....$

The timing of events within each period is as follows. If a revolution has happened in the past, then the political state is the post-revolutionary regime, and incomes are $y_g(\mathcal{S})$ for $g \in \{R, P\}$. If a franchise extension has happened in the past, the political state is democracy and incomes are $y_g(\mathcal{D})$ for $g \in \{R, P\}$. If the political state is autocracy at the beginning of period t ($S_t^{Pol} = \mathcal{A}$), then the following sequence of events takes place:

1. The social state $S_t^s \in \{G, B\}$ is revealed to the poor, but not to the rich.

2. The rich receive the reports $\left\{\omega_t^N, \omega_t^I\right\}$ and update their assessment of the threat of revolution to $q_t = F\left(q, \omega_t^N, \omega_t^I\right)$.

3. The rich decide whether or not to extend the franchise. If they do, the society becomes a democracy ($S_t^{Pol} = \mathcal{D}$) and incomes for the period are $y_g(\mathcal{D})$ for $g \in \{R, P\}$ and the period ends. If not, stage 4 applies.

4. The poor decide whether or not to initiate a revolution. If they do, the society experiences a transition to the post-revolutionary regime ($S_t^{Pol} = \mathcal{S}$) and incomes for the period are $y_g(\mathcal{S})$ for $g \in \{R, P\}$ and the period ends. If no revolution takes place, the society continues to be an autocracy and incomes are $y_g(\mathcal{A})$ for $g \in \{R, P\}$ and the period ends.

We treat the members of the two groups as two players of a dynamic game and restrict attention to pure strategy Markov perfect equilibria.

2.2 Analysis

Suppose that the political state is $\mathcal{A}$. We begin by considering the choice of the poor in stage 4. They know the social state and can base the revolution decision directly on that. If the social state is B, they do not revolt and the society stays autocratic for the period.

If, on the other hand, the social state is G, they revolt if

$$\mu > \frac{y_P(\mathcal{A})}{y_P(\mathcal{S})}. \tag{2}$$

We assume that this condition is satisfied. This implies that the poor will revolt in social state G and the rich must therefore give them voting rights to avoid a revolution. Given that, we can interpret q_t as a direct measure of the (perceived) threat of revolution.

Anticipating this in stage 3, the rich decide whether or not to extend the franchise based on the imperfect information about the social state they hold. It is useful first to consider the case in which the rich do not receive any reports at all so that $q_t = q$. If the rich decide to extend the franchise, a transition to democracy takes place and their lifetime income is $\frac{y_R(\mathcal{D})}{1-\beta}$. If, on the other hand, they decide not to extend the franchise, they face a lottery. Given their assessment of the current threat, q, the expected value of that lottery is $q \cdot 0 + (1-q)\, V_R$ where

$$V_R = y_R(\mathcal{A}) + \beta \left[q \cdot 0 + (1-q)\, V_R\right] \tag{3}$$

is the expected discounted value of not extending the franchise in political state $\mathcal{A}$ given the 'baseline' threat of revolution. We can solve this equation to get that

$$V_R = \frac{y_R(\mathcal{A})}{1 - \beta\,(1-q)}. \tag{4}$$

We can now define the critical value of the 'baseline' threat of revolution, $\widehat{q}$, such that without any reports, the rich are indifferent between extending ($\frac{y_R(\mathcal{D})}{1-\beta}$) and not extending the franchise ($(1-\widehat{q})\, V_R$):

$$\widehat{q} = \frac{(1-\beta)\,(y_R(\mathcal{A}) - y_R(\mathcal{D}))}{(1-\beta) y_R(\mathcal{A}) + \beta y_R(\mathcal{D})}. \tag{5}$$

For $q < \widehat{q}$, the society will – in the absence of any intelligence about the threat of revolution – suffer a revolution the first time the social state is G. Given their prior beliefs about the threat of revolution q, the rich are willing to run the risk and not extend the voting franchise, and a transition to the post-revolutionary regime will eventually happen. Democratization, accordingly, happens when the rich receive timely intelligence reports. The following proposition formalizes this logic.

Proposition 1. Assume that $q < \widehat{q}$ and that $\mu > \frac{y_P(\mathcal{A})}{y_P(\mathcal{S})}$. Let t' be the first period in which the social state is G. The rich extend the franchise at time t if

$$q_t = F\left(q, \omega_t^N, \omega_t^I\right) > 1 - \frac{y_R(\mathcal{D})(1-\beta\,(1-q))}{y_R(\mathcal{A})(1-\beta)} \equiv \overline{q} \tag{6}$$

and $t \leq t'$. Otherwise, a revolution takes place at time t' and the society transits to the post-revolutionary regime.

Proof. See Appendix A■

The proposition links intelligence reports from national and international sources to the franchise extension. Reports that induce the rich to update their assessment of the threat of revolution to the critical value $\overline{q}$ triggers a franchise extension: despite the fact that the reports are not conclusive, the rich judge that the threat at time t is so significant that a revolution is imminent and must be prevented by giving voting rights to the poor. In the absence of such reports a revolution happens and the society transits to the post-revolutionary regime. In this way, the model demonstrates how shocks to the information set of the rich originating inside *and* outside the country can cause suffrage reform and prevent revolution.[7] We shall build our estimation strategy on this, but before we discuss this in detail, it is useful to introduce and motivate our empirical measures of the two key variables: the threat of revolution and the extension of the franchise.

[7] Our theory illuminates one possible channel through which international transmission of information affects reform politics at home. This is, however, not the only possible channel through which this could happen. Another possibility is that revolutionary events abroad are focal points for revolutionaries in other countries. To see how this might work, consider the model of information cascades and revolutionary regime transitions developed by Ellis and Fender (2010). In their model, the (potential) revolutionaries do not know the true cost of revolution, while the governing elites do and can adopt various strategies to preempt revolution, including extending the franchise. Each revolutionary has private information about the cost. Based on this information and on observing what others are doing, each of them decides whether to participate in a revolt. This can create an information cascade and lead to a revolution. Suppose now that a revolutionary event happens in some other country. This could serve as a rally call for the revolutionaries at home, making it more likely that the critical participation level to make a revolution successful would be reached. Realizing this, the governing elites might after observing such a revolutionary event happen in a neighboring country adopt their behavior, and rather than running the risk that a revolution may happen, simply preempt it by extending the franchise.

3 Data on Revolutionary Events and Franchise Reform

Europe is a natural choice for a test of 'the threat of revolution theory'. During the period from the end of the Napoleonic Wars to the beginning of the Second World War, the major European powers went through the gradual transition from absolute monarchy or other types of autocracy to constitutional democracy. Moreover, Europe was the stage for many of the major revolutionary events of the era and it was the existing governing classes that in the vast majority of cases took the decision to extend the political franchise. The development of democracy in North America and Oceania followed a very different path as did the evolution of political institutions in Latin America, Africa and Asia. It is for this reason that we focus our empirical investigation on a sample of European countries covering the period from 1820 to 1938. Our main sample (the 'western European sample') includes 12 western European countries – Austria, Belgium, Finland, Sweden, Norway, Denmark, the Netherlands, Germany, the United Kingdom,[8] France, Italy, and Switzerland[9] – for which we have comprehensive data for the entire period. We also study a 'broader European sample' that includes Spain, Portugal, Greece, Iceland, Luxembourg, Serbia, Hungary, Poland, Russia and Romania. For these additional countries, we have much less comprehensive data.

In order to test the 'threat of revolution theory', we need two primary inputs: a quantitative measure of the franchise extension and a quantitative measure of the threat of revolution. 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

[8] Not including Ireland.

[9] For 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.

or the young. It is therefore natural to quantify the extension of the voting franchise by tracking the size of the electorate in percentage of its reference age and sex group over time and space.[10] Before women's suffrage, the reference group is all men of voting age, and after, it is all citizens of voting age.[11] This measure, which we shall call *suffrage,* quantifies on a scale from 0 to 100 the impact of income, property holding, and wealth restrictions on the proportion of the adult population allowed to vote in elections to the main legislative chamber of the country in isolation from the effect of women's suffrage.[12] It is available for the 12 countries in our main sample only. An alternative way to characterize the process of franchise extension is simply to record the timing of all major franchise extensions. Information on this can be obtained for 10 additional European countries and allows us to extend the sample with countries in eastern Europe, on the Iberian peninsula and in the Balkans. We record the year of all major suffrage reforms in column two of Table 1.[13] Democracy arrived gradually in many of the countries, through a sequence of piecemeal changes,[14] but we note two clusters of reforms: 1848-50 and 1918-19. This is, perhaps, not

[10] Our data refer to the right to vote in parliamentary elections and, in countries with bicameral systems, in elections for the lower chamber.

[11] This definition of democracy identifies differences in the *de jure* restrictions on political participation across time and space. These restrictions sometime differed from the *de facto* restrictions on political influence. In Germany before 1918, for example, the franchise was fairly wide, but the executive was largely unaccountable to the parliament and, thus, to voters, and in the countryside voting was to a large extent controlled by the landlords. Similarly, in Denmark from 1870 to 1901 the executive branch of government was controlled by a small group of large landowners against the wishes of the majority of the parliament and against (the spirit of) the constitution. By contrast, in Belgium the franchise was fairly narrow till 1893, but the executive was accountable to the electorate.

[12] The data are constructed from Flora et al. (1983), Caramani (2000), and Cook and Paxton (1998). We assign the value of zero to *suffrage* for the years before the first franchise reform allowed national elections to the main legislative body based on a well-defined set of suffrage rules. In some countries these reforms were pre-dated by various elected or appointed advisory bodies. Examples of this include elections for a farmer's chamber in Sweden in the 1820s and in Denmark before the constitution of 1849. In the Netherlands, the suffrage was quite broad for a while, but was curtailed by the French and reduced under its new royal constitution after the Vienna Congress (see Congleton, 2011). No quantitative information exists for how broad these suffrages were, but the historical narrative clearly indicates that they were very narrow and often did not lead to any real influence on public policy.

[13] We continue to focus on reforms that enfranchise lower socioeconomic groups by lowering income and property requirements etc., and do not include reforms that enfranchised women, unless women's suffrage, as was the case in a number of countries, was part of a broader reform package that also relaxed economic restrictions on the right to vote for men. The reform years are constructed with input from Flora et al. (1983), Caramani (2000), Carstairs (1980), Seymour and Frary (1918) and Encyclopaedia Britannica (1911, 2009).

[14] While the transition to full democracy was progressive and gradual in most countries, Italy, Austria and Germany during the interwar period are, of course, examples of backlashes to democracy. In France,

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), which 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.

Our test of the 'threat of revolution theory' is, as discussed above, based on the idea that revolutionary events abroad represent exogenous shocks to the information set of the elites in other countries who used this information to assess the (local) threat level and to judge if a franchise extension was needed or not. To implement this, we have, based on the works by Tilly (1993, 2004) and Todd (1998) and supplemented with information from Encyclopaedia Britannica, recorded all 'revolutionary events' in Europe during the period.[15] 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 part of the general population exercised control over important segments of the state organization' Tilly (2004, p. 73).[16]

We argue i) that information about these events spread around Europe fast and, ii) that the information was, in fact, used by the governing classes in other countries to assess the likelihood of a home-grown revolution. We discuss each of these postulates in turn. Firstly, even in the early part of the 19th century, news did spread fast within Europe. Stuurman

the suffrage was narrowed after the defeat of Napoleon. Similar anti-democratic restrictions were imposed at various points in time in Spain, Poland, and Serbia.

[15] Besides revolutionary events that took place in the countries included in the 'broader European sample', we also include events that took place in other countries in the Balkans and in Ireland.

[16] Some of the events recorded by Tilly (1993, 2004) 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). Other events, such as the riots that took place at the time when the British Parliament deliberated the Great Reform Act in 1831, were too insignificant to be counted as a 'revolutionary event' according to Tilly's definition. This does not mean that they were not important locally (in terms of our model they would be part of national intelligence), but it does mean that we assume that they were unlikely to have made much of an impression abroad.

(1991), for example, discusses how news of the French Revolution in 1848 reached Dutch merchants off the coast of Africa within weeks and presumably long after the news was known in the Netherlands itself. Likewise, news of the July revolution in Paris in 1830 was reported in English newspapers on August 3, 1830 (Brock, 1973, p. 102). Later on in the century, as the construction of telegraph lines proceeded, news from all corners of Europe could be spread almost instantly, not just amongst the European elites but also, as printed media and literacy spread, amongst the general population.

Secondly, the historical record contains plenty of examples suggesting that the governing classes in other countries did use information about revolutionary events abroad to assess the threat of revolution at home. One example is the impact that the July 1830 revolution in France had on the attitude of many British MPs to franchise reform. Some commentators at the time, in fact, suggested that news of the July revolution triggered the demand for franchise reform in Britain by making the governing classes aware of the threat of revolution. While it is probably an exaggeration that intelligence about the revolution in France had a major impact on the election to the British parliament in July 1830, 'it helped determine, not who was elected, but how those elected behaved when the new House met' (Brock, 1973, p. 103) and in that way served as a source of information about the risk of revolution. In a similar fashion, the news of the revolution in France in 1848 gave the Chartists in Britain a boost and triggered mass rallies across the country.[17] The fact that 80,000 volunteered as Special Constables in support of the existing political order again shows how news about revolutions abroad spurred the governing classes and their supporters into action to preempt revolution. Another example is the impact that the European revolutions of 1848 had in Denmark and in the Netherlands. In Denmark throughout the 1830s and 1840s, a fast growing bourgeoisie had demanded a share in government. It was, however, not until news of the bloody revolutions in France and Germany in 1848 and of the attempt by Schleswig and Holstein to get integrated into the German Confederation spread to Copenhagen that King Frederick VII gave in to the reform demands and accepted a constitutional monarchy and franchise extension (see, e.g. Collier,

[17] See Saville (1987, p. 227-228).

1999, pp. 33-34). Along similar lines, Stuurman (1991, p. 464) summarizes the situation in the Netherlands in 1848 as follows: 'although the Netherlands did not experience anything like a violent revolution in 1848, the political events of that year assuredly deviated from the normal course of Dutch politics[...] the fundamental cause of the non-violent revolution in the Netherlands is without doubt to be found in the European revolutions, notably those in France, Germany and Austria.' Finally, the Russian Revolution in 1917 played a key role for the wave of reforms at the end of World War I. In her discussion of the effect of the war on suffrage reform in western Europe, Collier (1999, p. 78) remarks that 'heightened working-class pressure [in Germany, Belgium, Sweden and Finland] was surely activated as much by the Russian Revolution as by World War I. From the side of the working class, what perhaps changed most was not the greater force of its pro-democratic agitation, but the revolutionary rather than the democratic example of the Russian Revolution'. In all these examples, news about revolutionary events abroad play a central role to the reform decisions reached by the elites.

Tables $A1$ and $A2$ in Appendix B detail all the revolutionary events included in our sample, but we have for illustrative purposes singled out the years of the events and recorded them in Table 1. For the purpose of the statistical analysis, we construct three different indicators of the threat of revolution as perceived by the governing class of country i in year t (TR_{it}^k). Let R_{jt} be the number of revolutionary events that took place in country j in year t and let D_{ij}^k be the 'distance' between country i and country j where $k \in \{u, g, l\}$ is an index for particular distance measures. Then, we can define the threat of revolution in country i at time t as

$$TR_{it}^k = \sum_{j \neq i} \frac{R_{jt}}{D_{ij}^k}. \tag{7}$$

The most elementary indicator, $k = u$, is just an unweighted sum of the number of revolutionary events in each year, i.e., $D_{ij}^u = 1$ for all i and j with $i \neq j$. The two other indicators recognize 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. This would be consistent with the theoretical framework developed above. We use two alternative measures of 'distance'. The first distance measure, $k = g$, is geographical

distance in kilometers between the capitals of the country pair. The second distance measure, $k = l$, is linguistic distance. Following Fearon (2003), we use the number of common branches in the linguistic tree for each pair of countries to measure how closely related their languages are.[18] Arguably, sharing a common language and geographical proximity are both plausible transmission channels for information about revolutionary events.[19] We construct each of the three indicators using the subset of major events indicated with bold face in Table 1 as well as all the events.[20] We stress that we exclude 'national' revolutionary events, i.e., events within a country itself, in all these calculations. The rationale for doing so is strong. While revolutionary events in other countries are, as argued above, exogenous to the reform politics of neighboring countries, events within a country itself are, by definition, related to local politics and could therefore be correlated with unobserved determinants of franchise reform that have nothing to do with the threat of revolution. By excluding these events – e.g. the effect that the revolution in France in 1848 had on France itself – we hope to avoid this problem.

<Table 1: The timing of Suffrage Reforms and Revolutionary Events in Europe, 1820-1938.>

4 Estimation Strategy

To introduce our strategy for testing the 'threat of revolution theory', let i denote the index for a particular country and let f_{it} be a measure of the franchise extension (or reform) at time t in country i. We can then express the franchise as a function of the perceived threat of revolution q_{it}, other observable determinants of the franchise X_{it}, country-specific fixed effects η_i, time-specific effects ϕ_t, and unobserved time-varying determinants of the

[18] We use the dominant language group, except for Switzerland and Belgium where we base the calculation on a population weighted average. The linguistic tree contains up to 15 nested categories and D^l_{ij} is defined as $\sqrt{\frac{15-\#common_{ij}}{15}}$, where $\#common_{ij}$ is the number of common branches in the tree between the language of country i and j.

[19] One could also consider using some measure of economic proximity, such as the extent of bilateral trade. For most countries, we can only trace this variable back to 1870. This alternative is therefore impractical for our purpose.

[20] The reason for zooming in on the major events is that some of the minor events might not have been widely noted at the time in other countries and so should be given zero weight.

franchise ρ_{it}:

$$f_{it} = \delta q_{it} + X_{it}\upsilon + \eta_i + \phi_t + \rho_{it}. \tag{8}$$

Motivated by the model presented in section 2, we approximate the perceived threat of revolution in country i at time t by a linear equation:

$$q_{it} = \alpha_1 \omega_{it}^N + \alpha_2 \omega_{it}^I + \gamma_i + \nu_t, \tag{9}$$

where γ_i is the country-specific baseline threat and ν_t represents common shifts in the threat level that affect all countries at the same time, e.g., the 'ghost of revolution' or 'rally calls'. Combining these two equations, we get

$$f_{it} = \delta\alpha_1 \omega_{it}^N + \delta\alpha_2 \omega_{it}^I + X_{it}\upsilon + (\delta\gamma_i + \eta_i) + (\delta\nu_t + \phi_t) + \rho_{it}. \tag{10}$$

In practice, we do not observe the national intelligence reports and they become part of the error term. We can write equation (10) as

$$f_{it} = \beta \omega_{it}^I + X_{it}\upsilon + \varphi_i + \lambda_t + \varepsilon_{it}, \tag{11}$$

where $\beta = \delta\alpha_2$, $\varphi_i = (\delta\gamma_i + \eta_i)$, $\lambda_t = (\delta\nu_t + \phi_t)$ and $\varepsilon_{it} = \rho_{it} + \delta\alpha_1 \omega_{it}^N$. This is the equation that we take to the data. To estimate the causal effect of the threat of revolution on the franchise, i.e., the parameter β, it must be true that our empirical proxy for ω_{it}^I, conditional on the (other) observable determinants of the franchise included in the vector X_{it} (to be discussed below) and on country and time fixed effects, is uncorrelated with all unobserved determinants of the franchise, i.e. that $cov(\varepsilon_{it}, \omega_{it}^I \big| X_{it}, \varphi_i, \lambda_t) = 0$. This, in turn, requires (1) that the error term ρ_{it} is (conditionally) uncorrelated with ω_{it}^I and (2) that ω_{it}^I is (conditionally) uncorrelated with ω_{it}^N. We discuss these two conditions in turn.

An important strand of literature argues that franchise extension originates from internal political competition within the elite of a country and, as such, is unrelated to the threat of revolution (e.g., Lizzeri and Persico, 2004; Llavador and Oxoby, 2005). The extent and importance of such internal rivalry is hard to quantify for the purpose of statistical analysis and will, therefore, in practice be captured by ρ_{it}. Internal political competition

and other such unobserved factors[21] are unlikely to be a cause of revolutions abroad. Neither the Danish liberal and national movements – the main advocates for suffrage reform in Denmark in the 1840s – nor the Danish King had any influence on the revolutionary events in France and Germany. Likewise, the power balance between pro-reform Whigs and Radicals and anti-reform Tories (and the King) in Britain did not affect the July revolution in France in 1830. The Russian Revolution was not caused by the political situation in Germany, Belgium, Sweden, or Finland and so on. It is therefore reasonable to maintain that revolutionary shocks abroad are (conditionally) uncorrelated with this type of internal political rivalry.[22]

However, there is another reason why ρ_{it} could be correlated with ω_{it}^{I}. In the formulation of the theoretical argument above, we did not allow the rich to adopt other strategies than franchise extension to combat the threat of revolution. In practice, as well as in Acemoglu and Robinson's (2000) model, other strategies play a role. Firstly, the rich could offer welfare transfers or other benefits to the poor and in that way eliminate their incentive to participate in a revolution.[23] Secondly, the rich may invest in repression and use the police or the army to eliminate any threat of revolution. The implication of this, then, is that the perceived threat of revolution may trigger franchise reform, but only if that particular coping strategy is chosen over the alternatives. Importantly, according to this logic, repression and welfare transfers are negatively correlated with the franchise extension but positively correlated with the threat of revolution. As a consequence, if we cannot control for the degree of repression or for welfare spending – and as we shall see, it is difficult to do so – the extent to which these strategies were used become part of ρ_{it}.

[21] As we discuss below, there are other theories of franchise extension (e.g. modernization and enlightenment, war, and trade integration) than those based on rivalry within the elite, which are also unrelated to the threat of revolution. However, we are able to control for each of these in the estimations and so, they are less likely to show up in the error term.

[22] Although it is possible that some domestic political factions would have an incentive to block news about revolutionary events abroad to further domestic political ends, it is virtually impossible to imagine that this could be done effectively in the European countries in the sample. In contrast, it is interesting to note that this did in fact happen throughout the Caribbean following the Haitian Revolution. This was made possible by strong social control and low levels of literacy.

[23] The drawback of this coping strategy is that it is only credible when the threat of revolution is real (in the model when the social state is G). For this reason, it will often be insufficient to head off a revolution and the rich must resort to franchise reform.

Our estimate of β will then be biased down towards zero.

We also require that the correlation between national and international intelligence reports is zero. Unconditionally, this is unlikely to be the case. In fact, our causal mechanism – international transmission of information about the risk of revolution – presumes that the elite of one country can learn about the risk of a home-grown revolution by observing revolutionary events abroad. This requires (positive) cross-country correlation between factors that make revolutions likely or not (in the model between the social states). If we, moreover, presume that national and international reports about revolutionary activities are more likely to emerge when there really is a risk of revolution, national and international reports will, *ceteris paribus*, tend to be (positively) correlated. If unchecked, this creates upward bias in the estimate of β, and it is therefore important that we condition on the factors that generate this correlation in order to ensure that ω_{it}^I and ω_{it}^N are conditionally uncorrelated.

The most important factor that could cause such a correlation is the business cycle. If, for example, the business cycle contains an international component *and* the threat or revolution is systematically related to economic hardship, then the conditions for revolution would be (positively) correlated across countries. This, in turn, means that national intelligence reports would be correlated with the international business cycle which, in turn, may be a driver of revolutionary activities abroad. In practice, we deal with this concern by controlling for local business cycle conditions, both directly by conditioning on variables related to the state of the national trade cycle and indirectly by including common time fixed effects in the estimations.

The 'rally call effect' is another factor that could generate a positive correlation between national and international reports about the threat of revolution. Suppose, for example, that a revolutionary event happens in some country. This is observed by revolutionaries abroad for whom it serves as a rally call, thus making revolution more likely there. As a consequence, national intelligence reports about the risk of revolution may reach the local elite. This phenomenon, however, is captured by the common time fixed effects in equation (9) and is, we believe, less of a concern.

With this discussion in mind, we estimate two versions of equation (11) to test the 'threat of revolution theory': a dynamic panel model (section 5) and an event history model (section 6). The panel model is estimated on the 'western European sample', while the event history model also makes use of the 'broader European sample'.

5 The Dynamic Panel Model

In the dynamic panel model, the dependent variable is *suffrage* and, as the baseline, we consider the following specification:

$$suffrage_{it} = \theta suffrage_{it-1} + \beta TR_{it}^{k} + X_{it-5}\upsilon + \varphi_i + \lambda_t + \varepsilon_{it}, \tag{12}$$

where φ_i is a country fixed effect (for each of the 12 countries in the western European sample), λ_t is a time fixed effect and ε_{it} is an error term with $E(\varepsilon_{it}) = 0$. In the baseline specification, we use two-year time fixed effects rather than yearly time dummies to avoid a multicollinearity problem, but we return to this issue below.[24] The vector X_{it-5} includes other potential determinates of the suffrage, typically lagged by five years (to be discussed below). To capture the strong path dependency in the evolution of franchise institutions, we include a lagged dependent variable on the right-hand side.

The evolution of the voting franchise is likely to be affected by many factors other than the threat of revolution. We include as many of these as possible in the vector X.[25] Firstly, the modernization hypothesis, as formulated by, for example, Lipset (1960), stresses the gradual increase in income, improvement in education attainment, and the process of urbanization as major causes of democratization. We control for these factors 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 then. Secondly, Lopez-Cordova and Meissner (2008) and others have argued that trade

[24] The problem is that the year fixed effects are (almost) perfectly collinear with the three indicators of the threat of revolution.

[25] The precise definitions of all control variables and their sources are given in Appendix B. In the baseline specifications, we only include variables for which we have data covering the entire sample period from 1820. In extensions, we add a number of other variables for which we only have partial coverage. We postpone the discussion of these other variables to section 7.

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 idea is that being on the gold standard reduces trading costs and indirectly encourages trade integration. We acknowledge that this is an imperfect proxy. Its main virtue is that it, in contrast to more direct measures of trade integration considered in Section 7, can be tracked back to 1820. Thirdly, the size of the country may matter. One reason, suggested by Mulligan and Shleifer (2005), is that 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 a measure of the size of the population (*population*). All these control variables are lagged by five years to reduce the risk of simultaneity bias.

Fourthly, 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 whether a country was at war in a given year.[26] World War I (1914-1918) was a major shock to the political and economic order. It might not only have affected the countries 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 Great War, we include a dummy variable, *WWI*, that is equal to one for all 12 countries during the period 1914-18. Finally, Gleditsch and Ward (2006), Persson and Tabellini (2009) and others have argued that the decision to introduce democracy by the political elites of one country may affect the decision to democratize in other countries. To allow for such spillover effects, we include the following measure of social learning in the model:

$$social\ learning_{it} = \sum_{j \neq i} \frac{DEM_{jt}}{D_{ij}^{k}}, \tag{13}$$

where D_{ij}^{k} is either the distance in kilometers from country i to country j ($k = g$) or the linguistic distance between the two ($k = l$) and DEM_{jt} counts the total number of

[26] We 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 and are not suitable for our purpose.

franchise reforms undertaken by country j by year t. If social learning was important, we should find a positive coefficient to this variable.

5.1 Evidence from the Dynamic Panel Model

The main results of the panel model are reported in Table 2.[27] Columns one to three in Table 2 show the results for the three different measures of the threat of revolution when equation (12) is estimated with a fixed effects estimator.[28] In all specifications, the threat of revolution is significant at the five percent level or better. Not surprisingly, the effect is smaller, but still statistically significant, when we use the broadest definition of what constitutes a revolutionary event (see column four). The presence of the lagged dependent variable on the right-hand side, however, implies that the fixed effects estimator is biased, albeit the bias is likely to be very small since our panel covers more than 100 years.[29] The bias can be avoided by using the GMM-system estimator[30] (Blundell and Bond, 1998) or the bias-corrected least-squares dummy variable (LSDV) estimator (Bruno, 2005). Very similar results emerge when the model is estimated using these estimators (see columns five to eight). Overall, we therefore conclude that the threat of revolution was a statistically significant and, we argue, a causal determinant of the franchise extension in western Europe. The magnitude of the effect can best be grasped by considering the point estimate from column one of Table 2, which reports the specification with the unweighted measure of the threat. The short-run effect of an 'extra' revolutionary event somewhere in Europe is that it increases 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.

The estimations shown in Table 2 do not make any attempt to control for three factors

[27] We postpone the discussion of evidence on competing theories to section 7.

[28] We 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).

[29] For a fixed number of countries, the bias disappears in the limit as the number of time periods goes to infinity. In practice, however, Judson and Owen (1999) have shown that the bias is negligible for panels that cover more than 20 years.

[30] With only 12 countries, it is not clear, however, that a GMM estimator is preferable to the fixed effects estimator.

that the Acemoglu and Robinson's (2000) theory of suffrage reform highlights as being important: business cycle shocks, repression of revolutionary activities, and temporary transfers to the poor. Table 3 shows some specifications that take these factors into account. Firstly, we have constructed a measure of the business cycle, *cycle*, by extracting the cyclical component of GDP per capita using a Hodrick-Prescott filter.[31] Since riots and other types of social unrest typically build up during times of hardship, we expect that the governing classes revised their estimate of the likelihood of a revolution upwards during a recession making suffrage reform more likely then and vice versa. A specification including *cycle* (and the trend component of GDP per capita, *trend*) is shown in column one of Table 3. We see that controlling for the state of the cycle reduces, as one would expect if the cycle is negatively correlated with threat of revolution and positively correlated across countries, the size of the point estimate on TR^g_{it} but not its significance. The effect of *cycle* itself is insignificant.

Secondly, if the elites could prevent a revolution through repression or by offering temporary transfers to the poor, it would, according to Acemoglu and Robinson's (2000) theory, be preferred to a (permanent) extension of the franchise. As discussed above, failure to control for this biases the estimate of the threat of revolution downwards. As a proxy for 'repression', we use data on the share of the public budget spent on policing and defence (*repression*) and as a measure of 'temporary transfers' we use the share of the public budget spent on health, education, housing and various government-sponsored insurance and welfare programs (*temporary transfers*). The results are reported in columns two and three of Table 3. Despite the fact that the sample size is significantly reduced, the threat of revolution continues to have a highly significant and positive effect on the franchise. The point estimates on *repression* and *temporary transfers* are negative as predicted by the theory but not statistically significant.[32]

Table 3 shows three further specifications. Firstly, the baseline specifications assume

[31] We have tried a number of other filters. It makes no difference to the results which one is being used.

[32] We have re-estimated the specifications without including the two variables *repression* and *temporary transfers* on the restricted sample to judge the size of the potential downwards bias. We find that the coefficient to TR^g is 1.95 (compared to 1.98) for the 'repression sample', whereas it is 1.33 (compared to 1.33) for the 'temporary transfers sample'. These point estimates suggest that the bias induced by not being able to control for these alternative coping strategies in the main specifications is very small.

that international news about revolutionary events abroad reached the governing classes in other countries within a year. As discussed in detail above, the historical evidence supports this assumption. Yet, it is important to check the robustness of the results by allowing for a longer window of opportunity. The specification shown in column four lags TR_{it}^{g} by one year and thus allows for a two-year window. The point estimate is 0.99 as compared to 1.22 with a one-year window, but still significant at the five percent level. Secondly, since all countries in the western European sample, with the exception of the United Kingdom and France, did not have regular elections by 1820 (*suffrage* is coded zero) and all countries had universal male suffrage towards the end of the sample period (*suffrage* is coded 100), the dependent variable, *suffrage*, is censored. In column five of Table 3, we show what happens when we use a Tobit estimator to take this into account. We see that it does not make much different to the results. Thirdly, our measures of the threat of revolution are serially correlated by construction. This can, as pointed out by Bertrand et al. (2004), generate a spurious correlation, in our case, between *suffrage* and TR_{it}^{k}. To see if this is a problem, we show in column six a specification that clusters the error terms at the country level. Again, we see that it does not make much of a difference.

<Table 2: Results for the Panel Model I>

<Table 3: Results for the Panel Model II>

There are three other issues – related to identification, to common time effects, and to stationarity – that are sufficiently important to warrant detailed considered before we present the results from the event history study.

Decomposing the Variation in the Threat of Revolution The variation in the (weighted) measures of the threat of revolution comes from three sources: firstly, over time variation; secondly, cross-country variation due to the fact that we omit revolutionary events happening within a country itself; and thirdly cross-country variation generated by differences in geographical or linguistic distance to the epicenter of each revolutionary

event. The variation created by excluding national revolutionary events is non-random.[33] Since only France is affected by own revolutionary events in the 'western European sample', we can confront the non-randomness generated in this way simply by dropping France from the sample. Doing so, makes no difference to the results [not reported].

More importantly, we can disentangle the two remaining sources of variation by postulating that

$$\widetilde{TR}_{it}^{k} = \widetilde{\beta} \sum_{j \neq i} R_{jt} + \widetilde{\gamma} \sum_{j \neq i} R_{jt} D_{ij}^{k} \tag{14}$$

The first term picks up the over-time variation in the threat level (and we expect that $\widetilde{\beta} > 0$). This may, in the absence of year fixed effects, be confounded by simultaneous movements in political unrest and franchise extension (see below). The second term isolates the cross-country variation generated by differences in distance to the events. This source of variation is unquestionably exogenous and we expect that $\widetilde{\gamma} < 0$. Re-estimating the partial adjustment model with $\widetilde{TR}_{it}^{g}$, we find that[34]

$$suffrage_{it} = \underset{(0.012)}{0.941} suffrage_{it-1} + .. + \underset{(0.23)}{3.49} \sum_{j \neq i} R_{jt} - \underset{(0.011)}{0.0019} \sum_{j \neq i} D_{ij}^{g} R_{jt} + ... \tag{15}$$

We see that the signs are as expected and that both sources of variation are contributing to the identification of the effect of the threat of revolution. The fact that the estimate of $\widetilde{\gamma}$, which is identified purely from the cross-country variation generated by distance to revolutionary events, is negative and statistically significant is a strong indication that we have identified a causal mechanism.

Common Time Fixed Effects We are aware that the results reported in Tables 2 and 3 could be interpreted as evidence of a simultaneous over-time change in revolutionary mood – the 'ghost of revolution' – and franchise extension, rather than as a causal effect of revolutionary threat. It is also possible that a sudden spur of 'enlightenment' in a particular

[33] Countries that experience a revolutionary event are coded as being exposed to lower revolutionary threat in that year than the rest of the countries. To the extent that revolutionary events within a country are correlated with suffrage reform, this creates a spurious correlation between the three measures of the threat of revolution and *suffrage*. If the correlation is positive, the consequence is a downwards bias.

[34] The control variables are the same as in the specifications reported in Table 2, but to conserve space, we do not report the point estimates here. Robust standard errors are reported in the bracket under the coefficient.

year across all the countries in the sample could reduce the threat of a revolution and simultaneously trigger suffrage reforms. The time fixed effects are included to address this concern. However, since we use two-year average time effects to avoid a multicollinearity problem, it is possible that 'ghost of revolution' or 'enlightenment' shocks could play a role if within a two-year period there happened to be more revolutionary events and more suffrage reforms in the second than in the first year. To investigate this further, we adopt the method of Plümper and Troeger (2007) to estimate the time effects separately from the impact of our measure of the threat of revolution. In particular, we, first, estimate a specification of equation (12) with one-year time fixed effects but without *WWI* and TR^k_{it}. Next, we regress the estimated year fixed effects on these two variables. The residuals from this regression along with *WWI* and TR^k_{it} are then included in the original specification. The results are

$$\begin{aligned} suffrage_{it} &= \underset{(0.011)}{0.938} suffrage_{it-1} + ... + \underset{(0.23)}{1.70} TR^u_{it} + \underset{(0.66)}{0.79} WWI_t && (16) \\ suffrage_{it} &= \underset{(0.011)}{0.935} suffrage_{it-1} + ... + \underset{(0.13)}{0.99} TR^g_{it} + \underset{(0.66)}{1.32} WWI_t && (17) \\ suffrage_{it} &= \underset{(0.011)}{0.929} suffrage_{it-1} + ... + \underset{(3.83)}{12.8} TR^l_{it} + \underset{(0.48)}{1.38} WWI_t. && (18) \end{aligned}$$

We do not report the coefficients for the control variables (which are the same as in Table 2) and the standard errors are shown in brackets under the coefficient estimates. The estimated effect of TR^u_{it} and TR^g_{it} are somewhat smaller than before while the effect of TR^l_{it} is a little larger, but all estimates continue to be highly significant. However, for *WWI* the effect is more dramatic. In fact, the variable changes sign from negative to positive suggesting that common time fixed effects are more of an issue when evaluating the 'Janowitz thesis' than the 'threat of revolution theory'.

Stationarity *Suffrage* as well as several of the control variables are trending up and may be or behave as if they were non-stationary.[35] This raises questions regarding the interpretation of the results reported in Tables 2 and 3. To confront this issue, we estimate an Error Correction Model for *suffrage*, using OLS with panel corrected standard errors,

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

as suggested by Beck (2001). In particular, we estimate

$$\begin{aligned} \Delta suffrage_{it} &= \lambda_1 \Delta TR^g_{it} + \Delta X_{it-5}\boldsymbol{\kappa} \\ &\quad + \widetilde{\rho}\left(suffrage_{it-1} - \gamma_1 TR^g_{it-1} - X_{it-6}\boldsymbol{\omega}\right) + \varepsilon_{it}, \end{aligned} \tag{19}$$

where the term in parenthesis is the long-run relation appropriately adjusted to match our other estimations and the parameter $\widetilde{\rho}$ captures the adjustment to the long-run equilibrium. The estimated equation is[36]

$$\Delta suffrage_{it} = \underset{(0.23)}{1.38}\Delta TR^g_{it} + ... - \underset{(0.0097)}{0.041}\left(suffrage_{it-1} - \underset{(11.03)}{48.0}\, TR^g_{it-1} - ..\right). \tag{20}$$

Again, we have suppressed the control variables (which are the same as in Table 2) and only report results for TR^g_{it}. The coefficients reported in equation (20) are all significant at the one percent 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. The negative estimate of $\widetilde{\rho}$ implies adjustment to the long-run equilibrium. All in all, this suggests that our results are not an artifact of non-stationary data.

6 Event History Model

The variable *suffrage* records the size of the electorate and allows us to study the gradual evolution of the franchise over time and space. An alternative approach is to record and study the timing of major franchise reforms. One advantage of this shift in emphasis is that we can then extend the sample with countries from Eastern Europe, the Iberian peninsula and the Balkans.

To facilitate such an event history study, we code, using the information from Table 1, the dependent variable y_{it} as 1 if country i introduced a franchise reform in year t and as 0 in the years before and after that. A country drops out of the sample in the year after universal male suffrage was reached or if it regressed into dictatorship. We do not know precisely when a country became at 'risk' of becoming democratic. So we deal with the problem of left censoring by assuming that countries enter the 'risk set' either in 1820 or at

[36] The figures in brackets underneath the coefficients are robust standard errors.

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 suffrage reform (the hazard rate). We follow Beck et al. (1998) and estimate the following discrete logistic model:

$$P\left(y_{it}=1|\,TR_{it}^{k},X_{it},M_{t-1}=0\right)=\frac{1}{1+e^{-\left(\overline{\beta}TR_{it}^{k}+X_{it}\overline{\nu}+H(.)\right)}},\tag{21}$$

where X_{it} is the vector of control variables (chosen from among those discussed above). The variable M_{t-1} is an indicator variable that is equal to 0 in each year before universal male suffrage and equal to 1 thereafter. We allow for duration dependence in the hazard rate through the function $H(.)$.[37]

6.1 The 'Western European Sample'

We begin by reporting results based on the 'western European sample'.[38] We control for the same co-variates as before. The main results paint the same picture as that emerging from the dynamic panel model: the threat of revolution increased the probability of suffrage reform significantly. The first four columns of Table 4 show the logit estimates for each of the measures of the threat of revolution. We see that they all are positive and statistically significant at the five percent level or better. The magnitude of the effect can be illustrated by considering the odds ratio. Based on the estimate reported in column one of Table 4, one extra revolutionary event increases the odds that a country will introduce a major suffrage reform in that year by 108 percent. This is a substantial effect.

These estimations, however, do not take into account that democratizations are rare events.[39] The fact that they are may magnify any systematic bias of the reported maximum likelihood estimates. King and Zeng (2001) have developed an estimator that corrects for

[37] The 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. We estimate $H(.)$ using natural cubic splines and use the estimated spline coefficients along with the cumulation of years since the last reform (or since entry to the sample) to model duration dependence. We have determined the number of knots by a sequence of F-tests and have settled on a specification with two knots.

[38] Germany and Switzerland cannot be included in the event history study because they introduced full male suffrage at the time they became nation states.

[39] In the 'western European sample', years with suffrage reform constitute less than four percent of the total number of cases.

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 TR_{it}^{g} continues to be statistically significant at the one percent level. The same is true with the other measures of the threat of revolution [not reported]. Another limitation of the logit model is that the baseline hazard rate, while admitting duration dependency as discussed above,[40] does not include a country-specific component. In column one of Table 5, 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 those reported in Table 4 reveals very little difference. Table 5 also reports specifications that allow for clustering of the standard errors at the country level, control for the cyclic component of GDP, spending on repression and temporary transfers or allow for a one-year delay in the flow of information. In all cases, the threat of revolution continues to be a significant predictor of the timing of franchise reforms.

<Table 4: Results from the Event History Model I>

<Table 5: Results from the Event History Model II>

6.2 The 'Broader European Sample'

All the countries in the main sample are western European and in actual fact achieved universal manhood suffrage within the sample period. In other parts of Europe, in particular in eastern Europe, on the Balkans, and on the Iberian peninsula, the evolution of democracy was more sporadic and many of these countries did not become fully consolidated democracies until the third wave of democratization. Yet, they did take the first steps towards democracy by extending the franchise to broader segments of the populations before World War I or just after, following a pattern not all that dissimilar to that followed in western Europe. Seymour and Frary (1918: pp. 151-152), for example, note about Russia in 1905 that 'by these extensions [of the franchise], the right to vote was given to the vast majority of the people'. Although, voting continued to be indirect and subject

[40] A 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.

to official control, this did constitute a significant broadening of political participation. Spain extended the franchise gradually over the course of the 19th century and arrived at universal male suffrage by 1890 (Ortega and Blanco, 1990). On the Balkans, Greece had a relatively democratic constitution from 1844 onwards, while Serbia had a parliament (Skupshtina) from 1869, which was elected by universal suffrage, whose only aristocratic element consisted of a certain number of deputies appointed by the prince (Seymour and Frary, 1918, pp. 251-252). A further franchise reform took place in 1888. Bulgaria was created after the Russian-Turkish war (1877-78) with semi-democratic institutions based on universal manhood suffrage but with significant powers vested in the King. Romania had a very restricted franchise throughout the 19th century but introduced a manhood suffrage subject to a literacy test in 1918. Consequently, seen from the perspective of the 19th century, it is not so clear that our sample of western European countries is systematically different from the 'full' European sample. Nevertheless, it is important to subject the 'threat of revolution theory' to a test based on a broader sample of countries.

To this end, we have, as noted above, collected information on suffrage reforms in Spain, Portugal, Greece, Iceland, Luxembourg, Serbia, Hungary, Poland, Russia and Romania (see Table 1) and re-estimated the event history model on this broader sample of countries.[41] The down-side is that we are unable to control for competing theories of franchise reform, except for the influence of war, and, in a few countries, for *GDP per capita* and *population*. The results are shown in Table 6. The first four columns report the results for the maximum sample of 20 countries, while the last four columns report specifications with additional control variables, but without Russia, Serbia, Iceland, and Luxembourg. Looking across the two top rows of the table, we see that all measures of the threat of revolution have a positive and highly statistically significant impact on the likelihood of suffrage reform. The historical narrative clearly demonstrates that repression was common currency in Russia and eastern Europe. Since we are not controlling for this in the estimations reported in Table 6, we expect a downwards bias in the estimate of the

[41] Hungary, Poland and Serbia are included from independence till the end of the sample period in 1938. Romania drops out in 1937, when it becomes a dictatorship. Russia drops out in 1923 when the civil war ended. Spain and Portugal drop out in 1936 and 1926, respectively, when they become dictatorships.

threat of revolution effect. It is, therefore, not overly surprising that the estimated effects are smaller in magnitude than those reported for the western European sample in Table 4. In conclusion, our results suggest that the threat of revolution was a major impulse of democratization not only within western Europe narrowly defined, but in Europe more widely.

[Table 6: Results from the Event History Model III.

Estimates of the probability of a suffrage reform, 1820-1938, 'Broader European Sample']

7 Other Results

Our main purpose is to test the 'threat of revolution theory'. However, our empirical investigation can also speak to the relevance of competing theories of suffrage reform. Some of the alternative theories require us to augment the baseline models estimated on the western European sample with additional variables for which we only got partial time or country coverage. The results of these additional estimations are reported in Tables 7 and 8. We notice that in all these additional specifications the evidence supporting the 'threat of revolution theory' remains strong and so, in what follows, we focus our discussion on the evidence related to the competing theories.

The 'Janowitz thesis' that war and the emergence of conscription armies were important impulses for democratic reform in Europe receives some support. In the panel model, the coefficient on *war* is consistently positive and highly significant. Based on the point estimates reported in columns one to four in Table 2, being at war, *ceteris paribus*, increases the franchise by between 3.5 and 4.2 percentage points in the short-run, with the long-run effect being about 17 times larger. The effect is, however, not significant in the event history study (see, e.g., Table 4). It is interesting to notice that WWI, according to our estimates, by itself did not contribute significantly to the extension of the franchise in Europe. In the panel model, this may, however, be due to the high correlation with the two-year time effects. This suspicion is confirmed when we estimate the time effects using the method suggested by Plümper and Troeger (2007). In this case, the effect of WWI falls

into line with the 'Janowitz thesis' showing a positive and sometimes significant effect on the franchise extension (see equations (16) to (18)). The dummy variables *war* and *WWI* are crude proxies for the effect of war and do not take into account the scale of war. In column four of Tables 7 and 8, we have replaced the two dummy variables with a measure of the number of war deaths – *war-intensity* – and we find a positive and statistically significant effect of this on the franchise in the panel model, but not in the event history model.[42]

In the baseline specifications, we use the variable *gold standard* to proxy for trade integration, and based on this, there is no evidence supporting the 'trade-causes-democracy' thesis. To investigate the robustness of this non-finding, we have replaced the gold standard dummy variable with two more direct measures of trade integration, represented by the variables *trade volume* and *wheat price spread*, respectively in Tables 7 and 8. The variable *trade volume* records the sum of imports and exports relative to GDP. The variable *wheat price spread* is a measure of trade costs proposed by Jacks (2005) based on convergence in wheat prices across time and space.[43] It is a problem for both of these measures that we lose between 200 and 650 observations. As can be seen from column one of Table 7, *trade volume* is significant in the panel model, but insignificant in the event history model (see column one of Table 8). Both specifications, however, show the expected positive sign, giving some credence to the 'trade-causes-democracy' thesis. The measure of trade costs based on the wheat price spread is insignificant.

In contrast to Persson and Tabellini (2009), we find no evidence that being located in a 'democratic neighborhood' encourages democracy. On the contrary, in the few cases where the variable *social learning* is statistically significant, it has a negative sign, suggesting democratic reforms in neighboring countries had a negative effect on democratic reforms at home. Occasionally, population size has a positive and significant impact.

None of the modernization variables, i.e. *GDP per capita*, *urbanization rate* and *education attainment,* seem to have mattered much. In the few cases in which a modernization variable is significant it appears to have a negative effect on democracy. The same message

[42] This is based on information from the 'correlates of war' data set, see Singer and Small (1994).
[43] See also Jacks et al. (2010).

comes from the specifications shown in Tables 7 and 8 where we control for *agricultural share* to capture the impact of industrialization.[44] This is not encouraging for 'modernization theory'.[45]

<Table 7: Additional Results for Panel Model.>

<Table 8: Additional Results for the Event History Model.>

8 Conclusion

This paper provides systematic, statistical evidence that the threat of revolution played a pivotal role for the evolution of suffrage rights in Europe in the 19th and early 20th centuries. Our various measures of the threat are consistently one of the main determinants of the extension of the franchise during this period and we believe that the results represent a causal effect. 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 the theory. The evidence on the key implication of the theory, namely that the franchise extension should be related to bigger government and more redistribution, is, on the other hand, more mixed. In particular, the evidence from western Europe for the period casts doubt on the simple hypothesis that suffrage reforms caused a big and immediate expansion of government, see e.g. Aidt et al. (2006) and Aidt and Jensen (2009a,b).[46]

Taking a long historical perspective also sheds new light on the 'Janowitz thesis' that war was an important impulse for democratic reform. Here, in contrast to, for example, Przeworski (2009), we find some evidence that it was. This lends support to the approach to endogenous democratization taken recently by Ticchi and Vindigni (2009). Our results

[44] In contrast to 'modernization theory', Congleton (2004) emphasizes that structural change empowers new pro-democracy lobby groups and influences the constitutional bargaining process in that way.

[45] For further discussion of the relevance of 'modernization theory', see Barro (1999), Boix (2009) and Gundlach and Paldam (2009) who reported evidence of a positive relationship between GDP and various measures of democracy, and Acemoglu et al. (2008) who cast doubt on the causal nature of this evidence. See also Przeworski et al. (2000) and Przeworski and Limongi (1997).

[46] In contrast, Husted and Kenny (1997) do find evidence of a large positive effect of suffrage reform on redistribution among US states in the post-WWII period and Boix (2003) and Lindert (1994) report similar results for broader samples of countries.

regarding 'modernization theory' echoes the finding by Acemoglu et al. (2008) that this theory – at least in its simplest form – cannot explain why democratic institutions emerged. We do acknowledge, however, that it is a challenge to estimate the impact of slow moving social processes on discrete events like suffrage reforms and that more research is needed on this and on the related question of the link between trade integration and democracy. We believe that western Europe during the 19th century constitutes a promising testing ground for doing so. It would also be of interest to delve deeper into the question of social learning and democratization.

9 Appendix A

Proof of proposition 1. Let q_t be the updated threat level in period t. The rich get $\frac{y_R(\mathcal{D})}{1-\beta}$ if they extend the franchise. If they do not extend, then their expected income is

$$q_t \cdot 0 + (1 - q_t) \left(\frac{y_R(\mathcal{A})}{1 - \beta (1 - q)} \right) \tag{22}$$

where we have used the fact that $q < \widehat{q}$, so that the rich given their assessment of the 'baseline' threat of revolution q do not expect to extend the franchise in the future. Rearranging gives equation (6). The critical value $\overline{q} < 1$ for all $q < \widehat{q}$. To see this, evaluate $\overline{q}$ at $\widehat{q}$ to get

$$\frac{(1 - \beta) (y_R(\mathcal{A}) - y_R(\mathcal{D}))}{(1 - \beta) y_R(\mathcal{A}) + \beta y_R(\mathcal{D})} > 0. \tag{23}$$

Notice that $\frac{\partial \overline{q}}{\partial q} = -\frac{\beta y_R(\mathcal{D})}{(1-\beta) y_R(\mathcal{A})} < 0$ and that $\overline{q}$ for $q = 0$ is positive. If the rich receive reports that induce them to update their assessment of the threat of revolution to $q_t \geq \overline{q}$ at $t \leq t'$, a transition to democracy takes place, otherwise a revolution will cause a transition to the post-revolutionary regime.

10 Appendix B

The variables used in the analysis are defined as follows:[47]

1. S*uffrage* 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), Caramani (2000), and Cook and Paxton (1998).

2. TR_{it}^{k} is the measure of the threat of revolution. For $k = u$ it is a simple count of the events in a given year; for $k = g$ the events are weighted by geographic distance;

[47] For notes on the construction of the data set see Aidt and Jensen (2009b).

for $k = l$ the events are weighted by linguistic distance, in all cases excluding events in each country itself. The main specification includes major events (listed in Table $A1$) only. For robustness, we also calculate the measures using all events including those minor ones (listed in Table $A2$). Sources: Tilly (1993, 2004), Todd (1998), and Encyclopaedia Britannica (1911, 2009). The source of linguistic distance is Fearon (2003).

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. *Wheat price spread* is an estimate of the trade cost between two locations in a given period based on differences in wheat prices at the two locations. Source: Jacks (2005).

11. *Social learning* is defined as a distance (geographical or linguistic) weighed average of franchise reforms in other countries. Sources: Fearon (2003) and the sources used to define years of franchise reform.

12. *War* is a dummy variable equal to 1 if a country is at war and 0 otherwise. Sources: Encyclopaedia Britannica (1911, 2009) and Singer and Small (1994).

13. *WWI* is a dummy equal to 1 during World War I and 0 otherwise.

14. *War intensity* is the number of deaths on the battle field per capita. Source: Singer and Small (1994) or http://www.correlatesofwar.org/.

15. *Repression* is the share of total central government spending on police, defence, general administration and the judiciary. Source: Flora et al. (1983).

16. *Temporary transfers* is the share of total central government spending on health, education, housing and various government-sponsored insurance and welfare programs. Source: Flora et al. (1983).

<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	
The 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	**1830-33**
Switzerland	1848	
Denmark	1849, 1915	
Luxembourg	1841, 1848, 1857, 1868, 1893, 1902, 1919	
Iceland	1874, 1908, 1916	
Spain	1836, 1865, 1869, 1888, 1890, 1931	**1820-23**, **1827**, 1836, 1840, 1842, 1843, 1854-56, 1866, 1868, 1873-74, 1890, 1909, 1930, 1933, 1934
Portugal	1822, 1838, 1852, 1878, 1895, 1911	**1820**, 1910, **1915**, 1919, 1927
Serbia	1869, 1889, 1920	1861
Greece	1822, 1844	1843, 1866-68, 1935, 1938
Romania	1866, 1923	
Poland	1921	1830-31, 1863-64
Hungary	1867	**1848-49, 1918-19**
Russia	1906	**1905, 1917**
Ireland	Not in the sample	**1916**
Other part of Balkans	Not in the sample	1826, 1885, 1888, 1907

Sources: Carstairs (1980), Flora et al. (1983), Caramani (2000), Encyclopaedia Britannica (1911, 2009), Seymour and Frary (1918), Todd (1998), Tilly (1993, 2004).

Notes: In column 3 the years in bold indicate the major revolutionary events included in the construction of the three main measures of the threat of revolution (TR^k, major events). The remaining years indicate the additional minor revolutionary events included in the construction of (TR^g, all events) (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 I.
Dependent variable: *Suffrage*.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
TR^u (unweighted, major events)	1.89***				2.02***		1.87***	
	[5.78]				[4.24]		[2.68]	
TR^g (geographical, major events)		1.22***				1.38***		1.20***
		[6.56]				[4.92]		[3.02]
TR^l (linguistic, major events)			12.00**					
			[1.96]					
TR^g (geographical, all events)				0.82***				
				[4.80]				
Suffrage (lagged)	0.93***	0.93***	0.92***	0.93***	0.88***	0.88***	0.95***	0.94***
	[69.94]	[73.89]	[66.78]	[70.83]	[35.53]	[35.57]	[51.54]	[51.00]
Log GDP per capita (lagged)	-0.64	-1.66	-0.38	-0.81	3.75	3.31	-1.43	-1.58
	[-0.25]	[-0.72]	[-0.16]	[-0.32]	[0.98]	[0.87]	[-0.35]	[-0.41]
Log Population (lagged)	4.79*	5.11**	5.66**	4.42*	-0.35	-0.36	2.67	2.14
	[1.95]	[2.08]	[2.26]	[1.80]	[-0.56]	[-0.58]	[0.40]	[0.38]
Urbanization rate (lagged)	0.002	-0.004	0.002	0.002	0.001	0.002	-0,003	-0,002
	[0.20]	[-0.50]	[0.26]	[0.21]	[0.16]	[0.20]	[-0.22]	[-0.13]
War	4.20***	3.62***	3.50***	4.14***	3.83**	3.83**	3.54**	3.50**
	[3.97]	[3.55]	[3.43]	[3.92]	[2.44]	[2.44]	[2.26]	[2.38]
WWI	-2.21	-2.26	-2.44	-2.10	-2.66	-2.60	-2.10	-2.03
	[-1.15]	[-1.10]	[-1.17]	[-1.11]	[-0.96]	[-0.94]	[-0.51]	[-0.52]
Educational attainment (lagged)	0.069	-0.099	-0.29	0.12	2.00	2.05	-0.22	-0.34
	[0.09]	[-0.14]	[-0.40]	[0.15]	[1.22]	[1.26]	[-0.15]	[-0.24]
Gold standard	-0.49	-0.19	0.40	-0.56	-0.48	-0.44	-0.04	-0.004
	[-0.60]	[-0.23]	[0.51]	[-0.68]	[-0.36]	[-0.33]	[-0.03]	[-0.003]
Social learning (geographic)	-76.7	-105.5		-64.7	-122.0	-129.8*	-73.8	-51.6
	[-1.07]	[-1.53]		[-0.92]	[-1.58]	[-1.69]	[-0.52]	[-0.39]
Social learning (linguistic)			-2.65*					
			[-1.77]					
Observations	1069	1069	1069	1069	1069	1069	1069	1069
Number of countries	12	12	12	12	12	12	12	12
Estimation technique	Fixed Effects	Fixed Effects	Fixed Effects	Fixed Effects	GMM-System	GMM-System	Bruno LSDV bias corrected estimator	Bruno LSDV bias corrected estimator

Notes: Robust *z* statistics in brackets; * significant at 10%; ** significant at 5%; *** significant at 1%. All estimations include two-year average time fixed effects.

Table 3: Results for the Panel Model II.
Dependent variable: *Suffrage*.

	(1)[b]	(2)	(3)	(4)	(5)	(6)
TR^g (geographical, major events)	0.99***	1.97***	1.33***		1.27***	1.23***
	[5.33]	[4.65]	[6.43]		[5.03]	[6.65]
TR^g (geographical, major events, lagged)				0.99**		
				[2.38]		
Trend	-1.13					
	[-0.45]					
Cycle	2.89					
	[0.42]					
Repression		-0.049				
		[-1.14]				
Temporary transfers			-0.016			
			[-0.35]			
Observations	1057	618	875	1069	1069	1069
Number of countries	12	9 [a]	9 [a]	12	12	12
Estimation technique	Fixed Effects	Fixed Effects	Fixed Effects	Fixed Effects	Tobit	Clustering

Notes: Robust *z* statistics in brackets; * significant at 10%; ** significant at 5%; *** significant at 1%. All estimations include two-year average time fixed effects and the same set of control variables as in Table 2. a. Data from Austria, Germany and Switzerland are missing; b. Log *GDP per capita* is replaced by *cycle* and *trend*.

Table 4: Results from the Event History Model I.
Estimates of the probability of a suffrage reform, 1820-1938, Western European Sample

	(1)	(2)	(3)	(4)	(5)
TR^u (unweighted, major events)	0.74***				
	[3.94]				
TR^g (geographical, major events)		0.51***			0.48***
		[4.21]			[5.23]
TR^l (linguistic, major events)			7.17**		
			[2.46]		
TR^g (geographical, all events)				0.45***	
				[3.70]	
Log GDP per capita (lagged)	0.21	-0.09	2.42	-0.16	-0.23
	[0.11]	[-0.046]	[1.28]	[-0.08]	[-0.11]
Log Population (lagged)	0.94***	1.00***	0.85***	0.88***	0.92***
	[3.14]	[3.28]	[2.85]	[3.06]	[2.92]
Urbanization rate (lagged)	-0.002	-0.003	-0.006	-0.003	-0.003
	[-0.77]	[-0.85]	[-1.46]	[-0.71]	[-0.59]
War	-0.13	-0.28	0.06	-0.05	-0.20
	[-0.14]	[-0.27]	[0.064]	[-0.048]	[-0.18]
WWI	-0.92	-0.59	-0.45	-0.37	-0.42
	[-0.82]	[-0.52]	[-0.39]	[-0.32]	[-0.34]
Educational attainment (lagged)	0.34	0.47	0.50	0.48	0.43
	[0.48]	[0.65]	[0.72]	[0.68]	[0.60]
Gold standard	-0.68	-0.49	-0.60	-0.41	-0.45
	[-1.14]	[-0.80]	[-1.01]	[-0.67]	[-0.68]
Social learning (geography)	39.3	42.7		38.9	41.6
	[1.26]	[1.37]		[1.26]	[1.25]
Social learning (linguistic)			-0.32		
			[-0.36]		
Observations	647	647	647	647	647
Number of countries	10	10	10	10	10
Estimation technique	Logit	Logit	Logit	Logit	Rare events

Notes: *z* statistics in brackets; * significant at 10%; ** significant at 5%; *** significant at 1%. Constant term not reported. 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. All estimations allow for duration dependence of the hazard rate.

Table 5: Results from the Event History Model II.
Estimates of the probability of a suffrage reform, 1820-1938, Western European Sample

	(1)	(2)	(3)[b]	(4)	(5)	(6)
TR^g (geographical, major events)	0.51***	0.51***	0.51***	0.67***	0.50***	
	[4.21]	[7.30]	[4.60]	[2.69]	[4.07]	
TR^g (geographical, major events, lagged)						0.36**
						[2.53]
Trend			-1.01			
			[-0.55]			
Cycle			-1.22			
			[-0.17]			
Repression				-0.020		
				[-0.60]		
Temporary transfers					0.067	
					[1.32]	
Observations	647	647	633	372	602	647
Number of countries	10	10	10	9 [a]	9 [a]	10
Estimation technique	Random effects	Clustering	Logit	Logit	Logit	Logit

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. All estimations allow for duration dependence of the hazard rate and include the same control variables as in Table 4. a. Data from Austria are missing. b. Log *GDP per capita* is replaced by *cycle* and *trend*.

Table 6: Results from the Event History Model III.
Estimates of the probability of a suffrage reform, 1820-1938, Broader European Sample

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
TR^u (unweighted, major events)	0.36***	0.37***			0.47***	0.48***		
	[3.18]	[3.29]			[3.08]	[3.35]		
TR^g (geographical, major events)			0.27***	0.27***			0.31***	0.32***
			[4.32]	[4.40]			[3.51]	[3.97]
Log GDP per capita (lagged)					0.23	0.24	0.18	0.20
					[0.53]	[0.49]	[0.43]	[0.42]
Log Population (lagged)					0.35*	0.35*	0.36*	0.35*
					[1.91]	[1.89]	[1.95]	[1.94]
War	-0.09	-0.09	-0.24	-0.13	0.30	0.37	0.18	0.26
	[-0.15]	[-0.15]	[-0.40]	[-0.21]	[0.45]	[0.55]	[0.26]	[0.38]
WWI	0.43	0.43	0.51	0.60	-0.24	-0.13	-0.12	-0.0005
	[0.73]	[0.69]	[0.88]	[0.97]	[-0.33]	[-0.17]	[-0.17]	[-0.0006]
Observations	1755	1755	1755	1755	1063	1063	1063	1063
Number of countries	20[a]	20[a]	20[a]	20[a]	16[b]	16[b]	16[b]	16[b]
Estimation technique	Logit	Rare events	Logit	Rare events	Logit	Rare events	Logit	Rare events

Notes: *z* statistics in brackets; * significant at 10%; ** significant at 5%; *** significant at 1%. a. The sample includes Austria, Belgium, Finland, Sweden, Norway, Denmark, the Netherlands, the United Kingdom, France, Italy, Spain, Portugal, Greece, Iceland, Luxembourg, Serbia, Hungary, Poland, Russia and Rumania. b. These specifications include Russia, Serbia, Iceland, Luxembourg. All estimations allow for duration dependence of the hazard rate.

Table 7: Additional Results for the Panel Model
Dependent variable: *Suffrage*.

	(1)[b]	(2)[b]	(3)	(4)
Robustness check	Add Trade volumes	Add Wheat price spread	Add Agricultural share	Add war-intensity
TR[g] (geographical, major events)	1.56***	1.75***	1.056***	1.23***
	[7.60]	[4.19]	[4.26]	[6.61]
Trade volume (lagged)	0.038**			
	[2.12]			
Wheat price spread (lagged)		2.56		
		[0.48]		
Agricultural share (lagged)			-0.013	
			[-1.56]	
War-intensity				5.06**
				[2.11]
Observations	858	405	876	1069
Number of countries	12	7 [a]	12	12
Estimation technique	Fixed Effects	Fixed Effects	Fixed Effects	Fixed Effects

Notes: See notes to Table 2. All estimations include the same control variables as in Table 2 and we only report the new variables. a. Data from Denmark, Finland, the Netherlands, Sweden, and Switzerland are missing; b. *Gold standard* is replaced by the alternative measure of trade integration.

Table 8: Additional Results for the Event History Model.
Estimates of the probability of a suffrage reform, 1820-1938, Western European Sample

	(1)	(2)	(3)	(4)
Robustness check	Add trade volumes	Add wheat price spread	Add agricultural share	Add war-intensity
TR[g] (geographical, major events)	0.58***	0.45**	0.43***	0.53***
	[4.32]	[2.17]	[3.00]	[4.58]
Trade volume (lagged)	0.017			
	[1.26]			
Wheat price spread (lagged)		0.21		
		[0.07]		
Agricultural share (lagged)			-0.002	
			[-0.62]	
War-intensity				0.50
				[0.21]
Observations	529	280	471	647
Number of countries	9[a]	6 [b]	10	10
Estimation technique	Logit	Logit	Logit	Logit

***Notes*:** See notes to Table 3. All estimations include the same control variables as in Table 2 and allow for duration dependency. We only report the new variables. a. Data from Belgium are lost. a. Data from Denmark, Finland, the Netherlands, and Sweden are missing. c. *Gold standard* is replaced by the alternative measure of trade integration.

Table A1: Major Revolutionary Events.

Events	Region	Year
Revolution in Hungary	Eastern Europe	1848-9
Bloodless revolution in Hungary, ending in foreign military intervention	Eastern Europe	1918-19
Mutiny of Spanish troops under Colonel Rafael Riego, generalizing revolution to 1823, termination by French invasion	Iberia	1820-23
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	1830-33
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	1848-49
Habsburg	Austria	1848-49
German states	Germany	1848-49

Table A2: Minor Revolutionary Events.

Events	Region	Year
Janissary rebellion in Constantinople	Balkans	1826
Pro-constitutional uprising in Greece	Balkans	1843
Revolt in Herzegovina	Balkans	1861
Revolt in Crete	Balkans	1866-68
Insurrections in Bosnia, Herzegovina, Bulgaria	Balkans	1875-78
Pro-Bulgarian revolution in Eastern Roumelia	Balkans	1885
Peasant insurrection in Romania	Balkans	1888
Peasant insurrection in Moldovia	Balkans	1907
Young Turks' revolution in the Ottoman Empire, including insurrection in Macedonia	Balkans	1908-09
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 until 1851	Iberia	1843
Spanish revolution led by O'Donnell and Espartero	Iberia	1854-56
Failed insurrection of General Juan Primenta	Iberia	1866
Pronunciamento of Admiral Juan Topete; generalization of insurrection	Iberia	1868
First Spanish Republic, Carlists rising	Iberia	1873-74
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 Catalonia	Iberia	1934
Polish rebellion in Greater Poland	Russian states	1830-31
Polish rebellion in Greater Poland	Russian states	1863-64

Table A3: Summary Statistics for the Variables used in the Analysis.

Variable	#Observations	Mean	Std. Dev.	Min	Max
Suffrage	1297	47.463	38.604	0	110.1
TR^u (unweighted, major events)	1403	0.246	0.742	0	5
TR^g (geographical, major events)	1403	0.352	1.241	0	15.105
TR^l (linguistic, major events)	1403	0.0146	0.059	0	0.803
TR^g (geographical, all events)	1403	0.719	1.374	0	15.105
Log GDP per capita	1280	7.74	0.45	6.66	8.76
Trend GDP	1110	7.82	0.42	6.85	8.76
Cycle GDP	1110	0.00035	0.028	-0.179	0.16
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
War intensity	1413	0.0079	0.075	0	1.00
Social learning, geographic	1403	0.03	0.02	0.00	0.08
Social learning, linguistic	1403	1.008	0.7809	0	3.242
Gold standard	1403	0.40	0.49	0.00	1.00
Education attainment	1237	0.46	0.50	0.00	1.00
Trade volume	948	45.01	25.63	2.24	140.17
Wheat price spread	529	0.46	0.17	0.22	1.13
Agricultural share	952	404.23	157.97	52.00	821.05
Repression	657	46.92	14.48	16.70	89.20
Temporary transfers	1007	9.46	10.96	0	50.4