

# Workers of the World, Unite!

## Franchise Extensions and the Threat of Revolution in Europe, 1820-1938\*

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### Abstract

We test the hypothesis that the extension of the voting franchise in Europe was related to the threat of revolution. We contend that international diffusion of regime contention and information about revolutionary events happening in neighboring countries generate the necessary variation in the perceived threat of revolution. Using two samples of European countries covering the period from 1820 to 1938, we find robust evidence which is consistent with the ‘threat of revolution hypothesis’. We also find some evidence that war triggered suffrage reform.

*Key words:* Suffrage, economics of democratization, threat of revolution.

*JEL classification:* D7, P16.

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# 1 Introduction

Variations in the rules that govern who can vote have a fundamental impact on policy choices and on the longer term development prospect of a society. Historically, the power to elect or appoint leaders – Kings or Parliaments – was the privilege of small elites who derived substantial benefits from this privilege. Today in modern democracies, political power is more evenly spread and elections are governed by the principle of one (adult) person one vote. A major puzzle in political economics is why a powerful incumbent elite would want to share power with broader segments of the population. After all, by doing so, it dilutes its own political base and stands to lose significant economic rents. The ‘threat of revolution hypothesis’ suggests that the elite offers voting rights to avoid revolution (see, e.g., Acemoglu and Robinson, 2000, 2006).<sup>1</sup> They do so whenever they perceive the risk to be sufficiently real. Seen in this perspective, democratization is *preemptive* democratization triggered by threat perceptions.

The historical record provides many suggestive examples consistent with this hypothesis<sup>2</sup> as does the wave of democratic reform that swept across North Africa during the Arab Spring. Beyond such examples, however, there exists surprisingly little systematic, statistical evidence. The reason is that it, unlike civil war and actual revolution, is hard to quantify the *threat* of a revolution. In this paper, we develop a measure of the threat of a revolution and conduct a new test of the ‘threat of revolution hypothesis’. We argue that international diffusion of information related to regime contention—in particular actual large-scale revolutionary activities in other countries—may influence regime dynamics abroad through two channels. Firstly, those seeking a regime change through revolution might take inspiration from events in other countries. Secondly, the defenders of the old autocratic regime may update their assessment of how threatening the domestic situation is, and reevaluate the likely consequences of a revolution or the scope for repression. Based

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<sup>1</sup>For alternative economic theories of franchise extension, see Falkinger (1999), Justman and Gradstein (1999), Conley and Temini (2001), Boix (2003), Lizzeri and Persico (2004), Llavador and Oxoby (2005), Jack and Lagunoff (2006), Congleton (2007, 2011), Aidt et al. (2010), Engerman and Sokoloff (2012, chapter 4) or Aidt and Albornoz (2011).

<sup>2</sup>See, e.g., Tilly (1995) and Weyland (2010).

on this, they then decide whether to relinquish power and extend voting rights preemptively. In short, we use international diffusion of information about actual revolutions to quantify the perceived threat of revolution.

This approach has two major advantages that sets it apart from previous tests. The first advantage is that we can quantify the threat of revolution for the critical period in the 19th and early 20th centuries during which the franchise was in fact extended in Europe. Previous research has been unable to do so. Przeworski (2009), for example, tests the ‘threat of revolution hypothesis’ on a world sample after World War I and Kim (2007) studies the link between strike activity and franchise reform in a sample of 12 Western European countries between 1880 and World War II. Our data allow us to start the analysis in 1820. The second advantage is that we provide a direct test of the theory. Previous work by Brückner and Ciccone (2011) and Burke and Leigh (2010) establish causal links between *economic shocks* and democratic change which are consistent with the theory. Chaney (2013) uses deep historical data on deviant Nile floods to show that the political power of religious leaders increased during periods of economic downturn and interprets this as evidence that these leaders could coordinate a revolt. We go beyond this literature by assessing the link between the *threat of revolution* and democratic change directly.

We implement our test on two samples of European countries between 1820 and 1938. The focus on Europe is justified for at least two reasons. Firstly, Acemoglu and Robinson (2000) motivate their theory with detailed examples from Britain, Germany, Sweden, and France. Accordingly, although the theory of preemptive democratization is generally applicable, it is arguably particularly relevant for understanding regime dynamics in 19th and early 20th century Europe. Secondly, (modern) democratic institutions originated in Europe and spread to other parts of the world, first through colonization (Hall and Jones, 1999; Acemoglu et al., 2001) and in more recent times by providing a blueprint for the design of democratic constitutions. Seen in this perspective, gaining a better understanding of how democracy came about in Europe is an important stepping stone for understanding the spread of democracy across the world and, therefore, ultimately for understanding the influence of institutions on long-run economic development.

Our results, based on two very different research designs, show that the threat of revolution had a major effect, not only on franchise extension as measured by the fraction of the adult population with the right to vote but also on the timing of major suffrage reforms. The baseline result is that one extra revolutionary event somewhere in Europe is associated with a two percentage point expansion of the fraction of the male population with the right to vote in neighboring countries and with a 75 percent increase in the odds of a suffrage reform.

There are two ways to read these results. The first is to give them a causal interpretation. This requires that revolutionary events in other countries are uncorrelated with unobserved country and time specific causes of democratization and that these events only affect democratization in a particular country through the effect on the perceived probability of revolution in that country. We control for many potential determinants of democratization, such as national income, urbanization, education, war, trade integration, inequality, enlightenment shocks, etc. and for unobserved country specific fixed factors and common time shocks. Yet, it is possible that countries which were ‘ready’ to democratize happened to be more exposed to revolutionary shocks from abroad for reasons we do not observe. Using the method proposed by Altonji et al. (2005), we find that selection on ‘unobservables’ must be 2.5-5.9 times stronger than selection on ‘observables’ for our baseline result to be entirely explained by selection bias. While perhaps not impossible, we find this highly implausible. Taken together, this gives us reason to believe that there is a causal link. The second way to read the results is as suggestive conditional correlations which are consistent with the ‘threat of revolution hypothesis’. The value of uncovering these correlations is two-fold. Firstly, our data allow us to establish a strong positive correlation between the threat of revolution and democratization for the entire first wave of democratization. Previous research has focussed mostly on later waves or on part of the first wave only. Moreover, the correlation that we uncover is extremely robust to estimation method and different sets of variables capturing other theories of franchise reform.<sup>3</sup> Secondly, we emphasize international

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<sup>3</sup>The correlations uncovered by Kim (2007) for the period 1880 to 1938 are, for example, not robust to controlling for unobserved country or time fixed effects. Przeworski (2009) focuses on bivariate correlations between his measure of the threat (based on data on riots and strikes) and his measure of democratization in order to maximize the number of cases.

diffusion of information on regime contention as one possible mechanism through which the threat of revolution might have induced preemptive democratization.<sup>4</sup> This provides a new perspective on the theory.

The rest of the paper is organized as follows. In section 2, we present a two-country model of (preemptive) franchise reform which motivates our empirical investigation. In section 3, we present data on revolutionary threats and suffrage reform. In section 4, we present our two research designs. In sections 5 and 6, we report the results and discuss confounding factors. In section 7, we discuss other theories of franchise reform. In section 8, we conclude. The supplementary material contains a Theory Appendix with some proofs, three data appendices (Data Appendix A to C) detailing our data material and the sources consulted and providing a table with descriptive statistics. Appendix D contains additional econometric checks.

## 2 Theory

Our test of the ‘threat of revolution hypothesis’ is based on the idea that revolutionary events abroad represent shocks to the information set of the old regime elites and to potential revolutionaries and may, through those two channels, be triggers of suffrage reform. To illustrate this logic, we develop a two-country version of Acemoglu and Robinson’s (2000) model of preemptive franchise extension.<sup>5</sup>

### 2.1 Assumptions

We consider two countries, indexed by  $i \in \{1, 2\}$ , with an infinite time horizon,  $t = 0, 1, \dots, \infty$ . We omit, for simplicity, the country index on variables and parameters that, by assumption, are the same. The *political state* in country  $i$  ( $S_{it}^{Pol}$ ) at time  $t$  can be either democracy ( $\mathcal{D}$ ), autocracy ( $\mathcal{A}$ ), or a post-revolutionary regime ( $\mathcal{S}$ ), i.e.,  $S_{it}^{Pol} \in \{\mathcal{D}, \mathcal{A}, \mathcal{S}\}$ . Each country is populated by two groups, called the insiders and the outsiders, and indexed by  $g \in \{I, O\}$ .<sup>6</sup> Utility is discounted by the factor

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<sup>4</sup>The literature on civil war also emphasizes international diffusion (Sambanis and Hegre, 2006). We focus on the diffusion of the *threat* of revolution rather than full-blown civil war. Revolution and conflict have many causes including economic shocks (Berger and Spoerer, 2001) and ethnical conflict (Esteban et al. 2012).

<sup>5</sup>See Dorsch and Maarek (2014) for a related model.

<sup>6</sup>We use the terms ‘insiders’ and ‘outsiders’ to allow for alternative interpretations. Typically, the insiders represent the old regime elites (e.g., the landed aristocracy) while the outsiders may

$\beta$ . We specify the per-period utility per member of group  $g$  as functions of the prevailing political state and denote them by  $y_g(S_{it}^{Pol})$ .<sup>7</sup> Under autocracy, the insiders, despite being a minority, control the government and bias policy in their favour. The utility of each insider is  $y_I(\mathcal{A})$ , while that of each outsider is  $y_O(\mathcal{A}) < y_I(\mathcal{A})$ . Under democracy, the outsiders hold the majority and introduce policies that benefit them and harm the insiders. As a consequence,  $y_I(\mathcal{A}) > y_I(\mathcal{D}) > 0$  and  $y_O(\mathcal{A}) < y_O(\mathcal{D})$ . Finally, in the post-revolutionary regime, the insiders fare worse than under democracy, while the outsiders are better off, i.e.,  $y_I(\mathcal{S}) = 0$  and  $y_O(\mathcal{S}) \geq y_O(\mathcal{D})$ .<sup>8</sup> We treat the insiders and outsiders of each country as (four) players of a dynamic game and refer to them collectively as the decision makers.

The initial political state is autocracy in both countries. A regime transition happens either through a revolution or through democratization. We use the term ‘revolution’ broadly to mean any form of costly social transformation forced upon the insiders by the outsiders,<sup>9</sup> whereas democratization is understood as orderly political transformation designed and controlled by the insiders. We denote the outsiders’ decision to attempt a revolution by  $\rho_{it} \in \{Y, N\}$ , where  $\rho_{it} = Y$  means that an attempt was made and  $\rho_{it} = N$  that no attempt was made. A revolution attempt costs each outsider  $\mu$  units of utility and its success depends on the *social state* ( $S_t^{Soc} \in \{G, B\}$ ). In social state  $B$ , a revolution always fails. In social state  $G$ , it succeeds with probability  $p$ . The post-revolutionary regime is an absorbing

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represent the working class, moderate liberals recruited from the upper middle class and the liberal aristocracy, an emerging lower middle class, parts of new industrial elites, or intellectuals and discontented gentry. The post-revolutionary regime and democracy can, accordingly, be interpreted as socialism versus parliamentary government elected on universal suffrage, as a republic (with rules that are particularly biased against the old regime elites) versus a constitutional monarchy with aristocratic control of an upper chamber but popular elections to a lower chamber, etc.

<sup>7</sup>These can be derived from specific assumptions about endowments, production technologies, and tax instruments, as in Acemoglu and Robinson (2000, 2006). By not explicitly modeling policy choices, we rule out that the insiders may offer fiscal transfers to avoid a revolution. The choice between fiscal transfers and a franchise extension is vital for understanding why democratization has commitment value, but is not important for understanding our empirical strategy. We return to the question of transfers in Section 4.

<sup>8</sup>Tullock (1971) and Kuran (1989) stress that it is the private returns that matter for an individual’s incentive to participate in a revolution. We assume that non-participating outsiders are excluded from the benefits associated with the post-revolutionary regime (see Acemoglu and Robinson, 2000, p. 1172).

<sup>9</sup>Accordingly, revolution attempts can take many different forms, ranging from a challenge from an emerging liberal-minded or radical middle class to a full-blown communist challenge as in Russia in 1917.

state. The discounted utility of an outsider after a successful revolution is  $\frac{y_O(\mathcal{S})}{1-\beta} - \mu$ .

The key assumption of the model is that the social states in the two countries are (positively) correlated. Correlation can be induced by international business cycle shocks, by weather shocks or by disease-induced crop failures (e.g., the potato blight). Alternatively, the source of correlation may be purely informational. A successful revolution requires coordination amongst the revolutionaries. Seeing a successful revolution abroad may foster coordination either through a demonstration effect or by serving as a rally call. The correlation need not be equally strong between all pairs of countries and, in practice, its strength is a function of economic, social and geographical proximity. For the purpose of the theoretical analysis, however, we make the extreme assumption that the social state is the same in the two countries and is transitory, but all we need is some degree of correlation.

To avoid a revolution, the insiders can extend the franchise ( $d_{it} \in \{Y, N\}$ ) or they can repress ( $r_{it} \in \{Y, N\}$ ). A preemptive franchise extension transfers power permanently to the outsiders and is sufficient to prevent a revolution.<sup>10</sup> Repression makes any attempt at revolution unsuccessful but costs each insider  $\sigma > 0$  units of utility. Neither the insiders nor the outsiders observe the social state directly.<sup>11</sup> They must, therefore, estimate based on ‘reports’ ( $L_{it}$ ) what the social state is before acting. The decision makers of a given country observe the same reports and in the absence of an informative report, everyone agree that the social state is  $G$  with probability  $q < 1$ .<sup>12</sup> The substantive assumption is that the reports differ across countries. In country 1, the decision makers receive *local* reports, i.e., reports that are not directly observed by decision makers in country 2. These reports are either uninformative ( $L_{1t} = 1$ ) or informative ( $L_{1t} = l$ ) where  $l \in (q, 1)$ . Upon receiving a report, the decision makers update their belief that the social state is  $G$  to  $q_{1t} = \Pr(G|L_{1t}) = \frac{q}{L_{1t}}$ . Since  $l \in (q, 1)$ , the belief that the social state is  $G$  is revised upwards after receiving an informative report and not revised after an uninformative report, i.e.,  $\Pr(G|l) = \frac{q}{l} > \Pr(G|1) = q$ .<sup>13</sup> In country 2, the

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<sup>10</sup>A sufficient condition is that  $\mu > \frac{y_O(\mathcal{S}) - y_O(\mathcal{D})}{1-\beta}$ .

<sup>11</sup>See Andrews and Jackson (2005).

<sup>12</sup>Since at time  $t$  no reports for future periods have yet been received, all decision makers believe at time  $t$  that the social state is  $G$  with probability  $q$  from period  $t + 1$  onwards.

<sup>13</sup>The restriction on  $l$  implies that the beliefs are never downgraded after receiving an informative

decision makers observe what happened in country 1 as information diffuses from one country to another. In particular, they observe the political state of country 1 and the decisions made by the insiders ( $d_{1t}$  and  $r_{1t}$ ) and by the outsiders ( $\rho_{1t}$ ), i.e.,  $L_{2t} \in \{S_{1t}^{Pol}, d_{1t}, r_{1t}, \rho_{1t}\}$ , and base their decisions on these *international* reports.

At the beginning of each period, the social state  $S_t^{Soc} \in \{G, B\}$  is determined by Nature. The decision makers in country 1 act before those in country 2 and they only need to act if the political state is autocracy ( $S_{1t}^{Pol} = \mathcal{A}$ ).<sup>14</sup> In that case, the sequence of events is:

1. The decision makers in country 1 receive the local report  $L_{1t} \in \{1, l\}$  and update their belief about the threat of revolution to  $q_{1t} = \Pr(G | L_{1t})$ .
2. The insiders decide whether to extend the franchise ( $d_{1t}$ ) or to repress ( $r_{1t}$ ).
  - (a) If they extend, country 1 becomes a democracy ( $S_{1t}^{Pol} = \mathcal{D}$ ) and utilities for the period are  $y_g(\mathcal{D})$  for  $g \in \{I, O\}$ , and the period ends.
  - (b) If they repress, any attempt at revolution fails (so the outsiders never revolt). The political state continues to be autocracy ( $S_{1t}^{Pol} = \mathcal{A}$ ) and utilities, gross of the utility cost of repression  $\sigma$ , for the period are  $y_g(\mathcal{A})$  for  $g \in \{I, O\}$ , and the period ends.
  - (c) If they decide to neither extend nor to repress, then stage 3 applies.
3. The outsiders decide whether or not to initiate a revolution. If they do and the social state is  $G$ , country 1 experiences with probability  $p$  a transition to the post-revolutionary regime ( $S_{1t}^{Pol} = \mathcal{S}$ ) while with probability  $1 - p$ , the revolution fails and the country continues in autocracy ( $S_{1t}^{Pol} = \mathcal{A}$ ). If the social state is  $B$ , a revolution always fails. Utilities for the period are, gross of the utility cost of revolution  $\mu$ ,  $y_g(S_{1t}^{Pol})$  for  $g \in \{I, O\}$  and  $S_{1t}^{Pol} \in \{\mathcal{S}, \mathcal{A}\}$ , and the period ends. If the outsiders do not attempt a revolution, the country continues in autocracy and utilities are  $y_g(\mathcal{A})$  for  $g \in \{I, O\}$ , and the period ends.

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report. We could allow for this by adding a third type of report, but this complicates the analysis without adding extra insights. The restriction also ensures that  $\Pr(G | l)$  is bounded below 1.

<sup>14</sup>If  $S_{1t}^{Pol} \in \{\mathcal{D}, \mathcal{S}\}$  at time  $t$ , no further decisions are required.



The sequence of events in country 2 is similar, except for stage 1:

- 1'. The decision makers in country 2 receive the international report  $L_{2t} \in \{S_{1t}^{Pol}, d_{1t}, r_{1t}, \rho_{1t}\}$  and update their beliefs about the threat of revolution to  $q_{2t} = \Pr(G | L_{2t})$ .

We emphasize two features of the information structure. Firstly, nobody observes the social state directly. For this reason, the model exhibits equilibrium paths, along which revolutions actually happen.<sup>15</sup> Secondly, international diffusion of information cannot by itself explain preemptive suffrage extensions. An initial trigger is needed. This is the role played by the local reports in country 1.

## 2.2 Analysis

We first study pure strategy Markov perfect equilibria in country 1. Subsequently, we study how international diffusion of information about events in that country affects regime dynamics in country 2.

### 2.2.1 Regime Dynamics in Country 1

The so-called revolution constraint, which controls whether the outsiders in stage 3 revolt or not, plays an important role for regime dynamics and we begin the analysis with a discussion of it. Since the outsiders do not know the true social state, they revolt if their updated belief ( $q_{1t}$ ) that the state is  $G$  is greater than the threshold<sup>16</sup>

$$\widehat{q}_{REVOLT} \equiv \frac{1}{p} \frac{(1 - \beta)\mu}{y_O(\mathcal{S}) - y_O(\mathcal{A})} \quad (1)$$

and do not revolt if  $q_{1t} < \widehat{q}_{REVOLT}$ . We make the following assumption.

**Assumption 1**  $q < \widehat{q}_{REVOLT} < \frac{q(1-p)}{1-pq}$ .

This assumption guarantees that the outsiders never revolt after observing an uninformative report ( $L_{1t} = 1 \Rightarrow q_{1t} = q$ ) but they will revolt upon observing an

<sup>15</sup>In Acemoglu and Robinson (2000), where all parties are fully informed about the social state, revolutions cannot happen in equilibrium because the insiders always want to preempt it. Ellis and Fender (2011) study a richer environment in which information cascades can generate revolutions.

<sup>16</sup>Derivation of this and subsequent conditions are in the Theory Appendix included with the supplementary material.

informative report ( $L_{1t} = l \Rightarrow q_{1t} = \frac{q}{l} > \frac{q(1-p)}{l-pq}$ ) unless the insiders take preemptive action.<sup>17</sup> In the latter case, the revolution constraint binds.

In stage 2, the insiders foresee whether the revolution constraint binds or not. When it binds, they face a choice between three options: democratize, repress, or run the risk of a revolution. We rank these options as follows. First, franchise extension is better than repression if

$$[D] : \sigma > \frac{y_I(\mathcal{A}) - y_I(\mathcal{D})}{1 - \beta}. \quad (2)$$

Condition  $[D]$  shows that the insiders of a country with an ineffective repression technology (a high  $\sigma$ ) or in which democracy does not pose a serious threat to them ( $y_I(\mathcal{A}) - y_I(\mathcal{D})$  is small)—perhaps because income inequality is modest—are likely to prefer to extend the franchise preemptively rather than to repress. Second, if the updated belief following an informative report ( $q_{1t} = \frac{q}{l}$ ) is sufficiently large, then either repression or democratization dominates running the risk of a revolution. More specifically, if condition  $[D]$  holds, then a preemptive franchise extension is optimal if

$$\frac{q}{l} \geq \frac{1}{p} \frac{y_I(\mathcal{A}) - y_I(\mathcal{D})}{y_I(\mathcal{A})} \equiv \hat{q}_{DEMOCRACY} \quad (3)$$

and if  $[D]$  fails, then repression is optimal if

$$\frac{q}{l} \geq \frac{1}{p} \frac{\sigma(1 - \beta)}{y_I(\mathcal{A})} \equiv \hat{q}_{REPRESSION}. \quad (4)$$

We make the following additional assumptions.

**Assumption 2**  $\hat{q}_{REVOLT} < \min \{ \hat{q}_{DEMOCRACY}, \hat{q}_{REPRESSION} \}$ .

**Assumption 3**  $\sigma < \frac{py_I(\mathcal{A})}{1 - \beta}$ .

Assumption 2 ensures that the outsiders are willing to revolt in situations where the insiders are unwilling to preempt a revolt. Assumption 3 imposes a lower bound on how willing they are to run this risk. It plays a role for the regime dynamics in country 2. Proposition 1 characterizes the Markov Perfect equilibrium in country 1.

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<sup>17</sup>It is sufficient for the analysis of country 1 to assume that  $\hat{q}_{REVOLT} < \frac{q}{l}$ . We assume that  $\hat{q}_{REVOLT} < \frac{q(1-p)}{l-pq}$  because this restriction matters for the analysis of country 2.

**Proposition 1** (*Political transitions in country 1*) Assume that Assumption 1 and 2 hold and that country 1 is an autocracy at the beginning of period  $t$ .

1. Suppose that  $L_{1t} = 1$ . The outsiders never revolt and the insiders never repress or extend the franchise preemptively. The political regime continues to be  $\mathcal{A}$ .
2. Suppose that  $L_{1t} = l$ .
  - (a) If condition [D] holds and  $\frac{q}{l} \geq \widehat{q}_{DEMOCRACY}$ , then a preemptive franchise extension takes place. The political regime becomes  $\mathcal{D}$  and no revolt is attempted.
  - (b) If condition [D] fails and  $\frac{q}{l} \geq \widehat{q}_{REPRESSION}$ , then the insiders repress. The political regime continues to be  $\mathcal{A}$  and no revolt is attempted.
  - (c) If  $\frac{q}{l} < \min\{\widehat{q}_{REPRESSION}, \widehat{q}_{DEMOCRACY}\}$ , the insiders take no preemptive action and a revolt takes place. If it fails, the political regime continues to be  $\mathcal{A}$ . If it succeeds, the political regime becomes  $\mathcal{S}$ .

### 2.2.2 Regime Dynamics in Country 2

The decision makers in country 2 observe the political state and the choices made by the insiders (repression or suffrage reform) and the outsiders (revolt in country 1, i.e.,  $(L_{2t} = \{S_{1t}^{Pol}, \rho_{1t}, r_{1t}, d_{1t}\})$ ). Given this information, they update their beliefs,  $q_{2t} = \Pr(G|L_{2t})$ , about the social state rationally using Proposition 1 and, in turn, base their decisions to reform, repress, or revolt on this. The thresholds  $\widehat{q}_{REVOLT}$ ,  $\widehat{q}_{REPRESSION}$ , and  $\widehat{q}_{DEMOCRACY}$  and condition [D] are the same as in country 1. We summarize this diffusion process as follows:

1. Suppose the political state of country 1 is  $\mathcal{A}$ , that the insiders of country 1 did not repress ( $r_{1t} = N$ ), and that the outsiders did not revolt ( $\rho_{1t} = N$ ). Then the decision makers in country 2 conclude that  $L_{1t} = 1$  and believe that  $q_{2t} = q$ . Given that, the revolution constraint does not bind in country 2 (and the outsiders will not revolt) and the political state remains  $\mathcal{A}$ .
2. If the decision makers in country 2 observe either a preemptive franchise extension ( $d_{1t} = Y$ ) or repression ( $r_{1t} = Y$ ), then they conclude that  $L_{1t} = l$ .

This is not sufficient to establish if the social state is, in fact,  $G$  but makes it more likely that it is. The updated probability that the social state is  $G$  is  $\frac{q}{l} > q$ . Given Assumption 1, the revolution constraint binds. The insiders respond by imitating the choice made by the insiders in country 1.

3. If the decision makers in country 2 observe a successful revolution ( $\rho_{1t} = Y$  and  $S_{1t}^{Pol} = \mathcal{S}$ ) in country 1, they can unambiguously conclude that the social state is  $G$ . The revolution constraint binds. Given Assumption 3, the insiders want to preempt a local revolt, either through a preemptive franchise extension if condition  $[D]$  holds or by repression otherwise.
4. If the decision makers in country 2 observe an unsuccessful revolt ( $\rho_{1t} = Y$  and  $S_{1t}^{Pol} = \mathcal{A}$ ), they conclude that  $L_{1t} = l$  but cannot deduce if the social state is, in fact,  $G$ . The updated probability that the social state is  $G$  is

$$q_{2t} = \Pr(G | \{\mathcal{A}, Y, N, N\}) = \frac{\frac{q}{l}(1-p)}{1 - \frac{q}{l} + (1-p)\frac{q}{l}} = \frac{q(1-p)}{l-pq} < \frac{q}{l}. \quad (5)$$

By Assumption 1,  $\frac{q(1-p)}{l-pq} > \hat{q}_{REVOLT}$  and the revolution constraint binds. Since the insiders in country 1 did not do anything to prevent the revolt, Assumption 2 implies that

$$\hat{q}_{REVOLT} < \frac{q}{l} < \min\{\hat{q}_{DEMOCRACY}, \hat{q}_{REPRESSION}\}. \quad (6)$$

Since  $\frac{q(1-p)}{l-pq} < \frac{q}{l}$ , the insiders in country 2 do not want to preempt a revolt either. A failed revolution attempt in country 1 triggers a revolution attempt in country 2. This generates a revolution snowball effect.

We present the key insights from this analysis in two propositions.

**Proposition 2** (*Preemptive franchise extension*). *Let Assumptions 1 to 3 hold.*

*Suppose that*

$$\frac{q}{l} < \min\{\hat{q}_{DEMOCRACY}, \hat{q}_{REPRESSION}\}. \quad (7)$$

*A successful revolution in country 1 triggers a preemptive suffrage reform in country 2 if condition  $[D]$  holds and repression if not.*

The ‘revolution shock’ originating from country 1 diffuses to country 2 through two channels. On the one hand, it serves as a rally call for the outsiders who upon observing the successful revolution abroad believe that they will (most likely) be successful as well. This makes the threat of revolution credible in country 2. On the other hand, it provides conclusive evidence to the insiders that they must act preemptively to avoid a revolution.<sup>18</sup> They do so either through preemptive suffrage reform or, if they have access to an effective repression technology ( $\sigma$  is low) or feel particularly threatened by democracy ( $y_I(\mathcal{A}) - y_I(\mathcal{D})$  is large), by repression. This captures the logic behind our test of the ‘threat of revolution hypothesis’: we propose to study empirically if revolutionary events in other countries affect the likelihood of suffrage reform at home positively.

In addition to this main test, the theory suggests auxiliary tests. First, a given ‘revolution shock’ abroad has a smaller impact on suffrage reform (1) in countries where the insiders are particularly threatened by democracy and (2) in countries that are ‘distant’ from the source of the revolutionary event. The first auxiliary test follows directly from condition [D]. The second auxiliary test follows from the observation that a ‘revolution shock’ in country 1 has no effect on suffrage reform in country 2 if the social states were independent. Insofar as the strength of the correlation between the social states is related to economic, social and geographical distance between pairs of countries, the theory delivers this second auxiliary prediction. The next proposition states one further prediction:

**Proposition 3** (*Democracy begets democracy*). *Let Assumptions 1 to 3 hold. Suppose condition [D] holds and that*

$$\frac{q}{l} > \widehat{q}_{DEMOCRACY}. \quad (8)$$

*A franchise extension in country 1 triggers a franchise extension in country 2.*

This proposition highlights an indirect channel through which information about revolutionary threats can diffuse internationally and be a cause of preemptive democratization. It happens when the insiders in country 2 observe a preemptive

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<sup>18</sup>They learn that the social state is  $G$  and given Assumption 3, they act preemptively.

suffrage reform in country 1. From this, they deduce that the revolution constraint must be binding and that they must take action if they want to prevent a revolt at home. Since the insiders in country 1 democratized preemptively, it is optimal for the insiders in country 2 to imitate. The reason is not that democracy has any intrinsic value or that a certain spur of enlightenment has affected the insiders' attitude to reform. The reason is that the (preemptive) reform in country 1 warns them that they too must act to avoid a revolution. This provides an additional empirical implication which we can test empirically.

### **3 Franchise Reforms and the Threat of Revolution: Measurements**

To test the 'threat of revolution hypothesis', we need quantitative measures of democratization and the threat of revolution. We equate democratization with the extension of the franchise for two reasons. Firstly, the hypothesis is first and foremost a hypothesis of franchise extension. Secondly, the hypothesis does not imply that the old regime elites needed to introduce the full package of democratic institutions (voting rights to all adults, secret ballot, civil liberties, effective accountability, etc.). On the contrary, it suggests that the elites would seek to grant the minimum concession needed to 'calm the waters' and avoid a revolution. Accordingly, the preemptive reforms induced by the threat of revolution often involved sharing as little de facto power as possible, or as Tilly (1995, p. 24) puts it, in his discussion of the Great Reform Act of 1832 in Britain, the "expanded suffrage afforded resulted from the government's frightened, but astutely minimal concessions to popular mobilization".<sup>19</sup> Granting the right to vote is by far the most visible and immediate de jure democratic concession that an elite can make and, therefore, in practice the most likely candidate for preemptive democratization.<sup>20</sup> This, we believe, makes

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<sup>19</sup>De facto power conferred to newly enfranchised groups can be limited in many ways. For example, it was common to maintain bicameral systems. While the franchise for the lower chamber was widened, the old regime elites preserved control of the upper chamber and through that the right to veto policy. Another mechanism was outright electoral corruption, often maintained by keeping the ballot open. The material point is whether the concessions at the time they were offered were accepted by the potential revolutionaries and thus eliminated the threat of revolution, and not whether they with the benefit of hindsight reallocated a lot of de facto political power.

<sup>20</sup>As emphasized by Kuran (1989), revolutions require leadership as well as popular support to succeed. Consequently, democratization can, in principle, preempt revolution by granting conces-

composite measures of democratization, such as those proposed by Przeworski et al. (2000) or Boix (2003) and used extensively in research on the causes of democratic change during the third wave of democratization (see, e.g., Gassebner et al. (2013)), inappropriate for our test.

Given this choice, we measure enfranchisement of hitherto disenfranchised social-economic groups of adult men, as opposed to enfranchisement of, say, women or the young. We do this in two ways. Firstly, we record the size of the electorate (with the right to vote in national elections to the lower legislative chamber) in percentage of its reference age and sex group over time and space. Before women’s suffrage, the reference group is all men of voting age, and after, it is all citizens of voting age. This measure, which we call *suffrage*, quantifies on a scale from 0 to 100 the impact of income, property holding, and wealth restrictions on the right to vote in isolation from the effect of women’s suffrage. We assign the value of zero to *suffrage* for the years before national elections to the (lower) legislative body were based on a well-defined set of suffrage rules. This measure can, based on information from Flora et al. (1983), be constructed for the 12 Western European countries listed in panel A of Table 1. Secondly, we record in column two of Table 1 the year of all reforms that enfranchise new socioeconomic groups by lowering income and property requirements, etc.<sup>21</sup> Information on this can be obtained for the ten additional countries listed in panel B of the table.<sup>22</sup> We refer to the sample of 21 countries as the ‘broader European sample’ and the sample of the 12 Western European countries as the ‘Western European sample’. The transition to constitutional democracy was progressive and gradual in most countries. Yet, Italy, Austria, Spain, Portugal, and Germany during the interwar period and France during the Second Empire from 1852 to 1869 constitute examples of backlashes to democracy. The years of

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sions to the potential revolutionary leadership without offering much to the popular supporters of revolution. In particular, in the 19th century, where the potential leadership was typically found amongst the radicals and liberal-minded middle classes, small franchise extensions that benefited these groups could be effective in stopping a revolution in the making. A good example of this is the Great Reform Act of 1832 in Britain (see, e.g. Aidt and Franck, 2014).

<sup>21</sup>Data Appendix A contains a detailed discussion of the coding of each reform.

<sup>22</sup>A country enters the sample when it becomes an independent state and drops out if it regresses back into some form of autocracy or into civil war. Data Appendix A provides further details on the construction of the sample. We report the year of entry and, if applicable, year of exit for each country in column 1 of Table 1.

these and other examples of (de facto) franchise contractions are listed in column four. We explore this information to account for the durability of past franchise extensions.

Our test of the ‘threat of revolution hypothesis’ is, as discussed above, based on the idea that regime contention and information on revolutionary events diffuse internationally. To quantify this diffusion process, we have recorded 42 ‘revolutionary events’ in Europe during the period.<sup>23</sup> ‘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). We have excluded instances of coup d’état and civil war since they are conceptually different. The years of the ‘revolutionary events’ are listed in column three of Table 1.<sup>24</sup> These include the three major waves of revolution in Europe that occurred in 1820, 1830 and 1848 as well as the Russian revolutions and many other events.

Based on this information, we construct three different indicators of the threat of revolution as perceived in country  $i$  in year  $t$  ( $TR_{it}^k$ ). To understand how this is done, let  $R_{jt}$  be the number of revolutionary events that took place in country  $j$  in year  $t$  and let  $W_{ij}^k$  be the spatial weight attached to the revolutionary event in country  $i$  for country  $j$  where  $k \in \{u, g, l\}$  is the index for a particular weight. The indicators of the threat of revolution are then defined as:

$$TR_{it}^k = \sum_{j \neq i} W_{ij}^k R_{jt}. \quad (9)$$

The first indicator,  $k = u$ , is an unweighted sum of the number of revolutionary events in each year, i.e.,  $W_{ij}^u = 1$  for all  $i$  and  $j$  with  $i \neq j$ . The ‘threat of revolution hypothesis’ suggests, however, that the information content of events is likely to be larger for events that happen in countries that are geographically, economically, or culturally closer. Our two other indicators recognize this aspect of the diffusion process. The second indicator,  $k = g$ , uses geographical distance

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<sup>23</sup>These are coded based on the works by Tilly (1993, 2004) and Todd (1998) and supplemented with information from Encyclopaedia Britannica.

<sup>24</sup>All the events are detailed in Data Appendix B.



to define the weight and let  $W_{ij}^g = \frac{1}{D_{ij}}$ , where  $D_{ij}$  is the distance in kilometers between the capitals of the country pair. The third indicator,  $k = l$ , uses linguistic distance to define the weights and let  $W_{ij}^l = 1 - \frac{\sqrt{15 - \#common_{ij}}}{15}$ , where  $\#common_{ij}$  is the number of common branches (up to 15) in the linguistic tree for each pair of countries (Fearon, 2003). Arguably, sharing a common language and geographical proximity are both plausible diffusion channels. For the main analysis, we construct each of the three indicators using the 16 major events, indicated in boldface in Table 1. In robustness checks, we make use of all 42 events. We exclude revolutionary events within a country in all these calculations. The rationale for doing so is that they represent the impulse to the diffusion process but are not a consequence of it.

For these data to be useful for our proposed test, it must be true (1) that information about these events spread around Europe fast, and (2) that the information was, in fact, used by the governing classes and potential regime challengers in other countries to assess the likelihood of a successful home-grown revolution. We discuss each of these requirements before we proceed. Firstly, even in the early part of the 19th century, news spread fast within Europe. Stuurman (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. Likewise, English newspapers reported the July revolution in Paris in 1830 on August 3 (Brock, 1973, p. 102). Later on in the century, with the construction of telegraph lines, news from all corners of Europe could spread quickly, 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 demonstrating that the governing classes used information about revolutionary events abroad to assess the threat of revolution at home and that opposition groups took inspiration from events happening in neighboring countries. One example is the impact that the July 1830 revolution in France had on the attitude of British Members of Parliament towards franchise reform. Some commentators at the time, in fact, suggested that news of the July revolution triggered franchise reform in Britain by making the governing classes aware of the threat of revolution (Halevy, 1935) and when Lord

Grey introduced the reform bill to the House of Commons with the words, ‘the principal of my reform is to prevent the necessity of revolution[...] I am reforming to preserve, not to overthrow’, he made a clear reference to the perceived risk of violent social change. Another example is the impact that the European revolutions of 1848 had in Denmark and in the Netherlands. As Weyland (2010, p. 1162) puts it, ‘the Danish king in March 1848 had the opportunity to observe the daily advance of revolution across Central Europe: he could almost predict the hour when it would reach Copenhagen [...] On March 18, Frederic VII made hasty concession [including a franchise reform] to the restless masses gathered outside his palace to avert an explosion in Denmark’. 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.’ Yet another example is the Russian Revolution in 1917 where ‘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’ (Collier, 1999, p. 78). Likewise, Weyland (2010) contends that fear of bolshevism induced preemptive suffrage reforms in Britain, Sweden, Germany, and Finland in 1917-19. In all these examples, news about revolutions abroad informed reform decisions reached by the elites across the continent, and it did so because it served as a rally call and as inspiration for local revolutionaries.

<Table 1 to appear here>

## 4 Estimation Strategy

We use two different research designs to implement our test of the ‘threat of revolution hypothesis’. In the first design, the dependent variable is the continuous

variable *suffrage* and the baseline specification is a fixed effects panel model:

$$suffrage_{it} = \theta suffrage_{it-1} + \beta_1 TR_{it}^k + X_{it}v + \varphi_i + \lambda_t + \varepsilon_{it}, \quad (10)$$

where  $\varphi_i$  is a country fixed effect,  $\lambda_t$  is two-year time fixed effects and  $\varepsilon_{it}$  is an error term with  $E(\varepsilon_{it}) = 0$ . The vector  $X_{it}$  includes other potential determinates of the suffrage. To control for the ‘initial’ political state, we include a lagged dependent variable. The error terms  $\varepsilon_{it}$  are unlikely to satisfy the standard assumptions of temporal and spatial independence and homoskedasticity. In the baseline specification, we, therefore, take account of i) cross-country spatial correlation amongst the disturbances, ii) autocorrelation, and iii) panel heteroskedasticity. We adopt the panel correction recommended by Beck and Katz (1995) to model unrestricted spatial correlation and we cluster the error terms at the country level.<sup>25</sup> These standard errors cannot be estimated with one-year time fixed effects because of the high correlation between year effects and the threat of revolution variables. This is the reason why we include two-year time fixed effects in the baseline. In section 5.2.4, we show that the results are robust to controlling for one-year time fixed effects in specifications where we do not model unrestricted spatial correlation. Theory predicts that  $\beta_1$  is positive.

Our second research design is an event history model. Here, the objective is to investigate whether the threat of revolution explains the timing of suffrage reforms. We code, using the information from column two in Table 1, the dependent variable *reform<sub>it</sub>* as one if country *i* introduced a franchise extension in year *t* and as zero in the years before and after that. A country drops out of the sample in the year after it introduced universal male suffrage or if it, before that happened, became a dictatorship. We do not know precisely when a country became at ‘risk’ of extending the franchise. We deal with this problem of left censoring by assuming that countries enter the ‘risk set’ either in 1820 or at the time of independence (as recorded in column one of Table 1). As in Beck et al. (1998), we estimate the following discrete

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<sup>25</sup>The measures of the threat of revolution are serially correlated by construction. This can, as pointed out by Bertrand et al. (2004), generate spurious correlation. We use a parametric method to correct for this. For each country, we use the estimated autocorrelation coefficients from an AR(1) process to adjust the standard errors. With more than 100 years of data, it is unlikely that these coefficients are biased downwards. The estimated autocorrelation coefficients are small (around 0.05). We use the PCSE procedure in STATA 12 to make these adjustments.

logistic model

$$P(\text{reform}_{it} = 1 | TR_{it}^k, X_{it}, M_{t-1} = 0) = \frac{1}{1 + e^{-(\bar{\beta}_1 TR_{it}^k + X_{it}\bar{\nu} + H(\cdot))}}, \quad (11)$$

where  $X_{it}$  is the vector of other potential determinates of the suffrage. The indicator variable  $M_{t-1}$  is equal to zero in each year before universal male suffrage and equal to one thereafter. The function  $H(\cdot)$  captures duration dependence in the hazard rate.<sup>26</sup> We cluster the standard errors at the country level. Theory predicts that  $\bar{\beta}_1$  is positive.

The two research designs capture different aspects of the democratization process. The panel model captures the evolution of the fraction of the population with voting rights, over time and space. The event history model captures the *timing* of suffrage reform. In both cases, identification requires the assumption of conditional independence. We discuss how reasonable this assumption is and potential threats to it below, but first we introduce the co-variates ( $X_{it}$ ). They are motivated by theoretical considerations but necessarily constrained by data availability. In the baseline, we only include variables for which we have data for the whole sample period.<sup>27</sup> In extensions, we add variables (to be introduced later) with partial time coverage to address particular issues. Firstly, some co-variates are motivated by the modernization hypothesis, initially formulated Lipset (1960). He stressed the gradual increase in income and the improvement in education attainment as causes of democratization. We capture modernization by *GDP per capita* and a dummy variable, *educational attainment*, that is equal to one if enrollment in primary education is greater than 60 per cent and zero otherwise. The variable, *urbanization rate*, also captures aspects of modernization. As stressed by Przeworski (2009), it can, in addition, serve as a proxy for the demand for public goods and be used to control for a positive association between the value of public goods and suffrage reform, as predicted by Lizzeri and Persico (2004).<sup>28</sup> Secondly, Lopez-Cordova and

<sup>26</sup>The argument of the function  $H(\cdot)$  is  $t - t_i^p$ , where  $t_i^p$  represents either the year in which country  $i$  enters the ‘risk set’ or the year of the previous franchise extension within the sample period. We estimate  $H(\cdot)$  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. Based on a sequence of F-tests, we use a specification with two knots.

<sup>27</sup>See Data Appendix C for precise definitions and sources.

<sup>28</sup>See also Llavador and Oxoby (2005).

Meissner (2008) and others argue that trade integration causes democratization. We capture this via the 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 trade costs and indirectly encourages trade integration.<sup>29</sup> Thirdly, we include a measure of the size of the population (*population*) to capture scale effects. All these variables are lagged by five years to reduce the risk of simultaneity bias. Fourthly, Janowitz (1976) and, more recently, Ticchi and Vindigni (2009) and Dincecco (2011) argue that mass conscription armies and war cause democratization. We use the dummy variable, *war*, that records whether a country was at war in a given year to control for this. World War I was a major shock to the European political and economic order. To capture this and to isolate the effect of the Russian Revolution of 1917 from the effect of the war, we include a dummy variable, *WWI*, that is equal to one for all countries during the period 1914-18.

## 5 Evidence From the Panel Model

We organize the evidence from the panel model in five sub-sections. We begin with the baseline results. In the next sub-section, we evaluate various sources of bias. This includes a discussion of spatial correlation, the reflection problem, reverse causality, own revolutions, enlightenment shock (one-year time fixed effects), and the effect of the French revolution. This is followed by evidence on auxiliary predictions from the theory and a discussion of other robustness checks. The final sub-section discusses alternative estimation techniques.

### 5.1 Baseline Results

Table 2 reports the baseline results. Columns one to three show the results for the three different measures of the threat of revolution without any control variables (except for the lagged endogenous variable and the fixed effects). The subsequent three columns show the results with the vector of co-variates. In all specifications, the coefficient on the threat of revolution proxy is positive and significant at the

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<sup>29</sup>The main virtue of this imperfect proxy is that, in contrast to the alternatives considered in section 7, it can be coded for the entire sample period.

five percent level or better. Moreover, the parameter estimates are stable. Based on the estimate reported in column four, the short-run effect of an extra revolutionary event somewhere in Europe is to increase the franchise by almost two percentage points in the average country. The long-run effect is around 30 percentage points. This baseline result is consistent with the ‘threat of revolution hypothesis’.

This estimate represents a causal effect only if the assumption of selection on observables is satisfied. This assumption fails if countries which were ready to democratize for reasons that we do not observe happened to be more exposed to revolutionary shocks from abroad. It is not possible to formally test if this is the case or not, but Altonji et al. (2005) propose a method which can give a sense of how critical selection on unobservables is. The idea is to assume that selection on unobservable factors is as important as selection on the observable factors included in the regression model. Imposing this ‘equal selection’ assumption enables us explicitly to calculate the magnitude of the selection bias (*bias*). We can compare this to the point estimate  $\hat{\beta}_1$  obtained under the selection on observables assumption (reported at the top of Table 2) and calculate what we shall call the ‘selection-ratio’ as  $\frac{\hat{\beta}_1}{bias}$ . The selection ratio tells us how much stronger selection on unobservables than on observables needs to be for the OLS estimate ( $\hat{\beta}_1$ ) to be attributable entirely to selection bias. We report the estimated ratios in bottom row of Table 2.<sup>30</sup> The ratios range from 2.5 to 5.9. This means that selection on unobservables would have to be 2.5-5.9 times stronger than selection on observables for the baseline result to be entirely explained by selection bias. While perhaps not impossible, we find this highly implausible.

<Table 2: Baseline results>

## 5.2 Confounding Factors

In this section, we evaluate various sources of bias, discuss alternative interpretations of the baseline result, and demonstrate the remarkable robustness of the relationship between *suffrage* and our measures of the threat of revolution.

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<sup>30</sup>We thank Todd Elder for sharing the relevant STATA code with us.

### 5.2.1 Spatial Dependence and the Reflection Problem

The baseline specifications adjust the standard errors for unrestricted spatial correlation. An alternative, suggested by Conley (1999), is to use a notion of social distance to model spatial dependence. In our context, the most natural notion is geographical proximity. We capture this by the crow-fly distance between pairs of capital cities. Table 3 reports three specifications with ‘Conley-adjusted’ standard errors based on three different cut-offs for how far apart two countries must be for the spatial correlation to fade to zero. Although the standard errors are higher, the estimated coefficient on  $TR_{it}^g$  remains significant at least at the five percent level.

Our emphasis on international diffusion of information naturally brings the so-called reflection problem to mind. The reflection problem refers to the fact that, in general, it is hard to separate contextual effects from social effects (Manski, 1993). This is often a serious obstacle to inference because the object of interest is the size of the social effect. We, however, do not pretend to be able to separate the ‘threat’ that originates from being located in a geographical area which shares a common threat (a contextual effect) from the ‘threat’ that originates from the fact that ‘peers’ are threatened (a social effect).

Yet, Proposition 3 of the model points to a particular form of reflection which we can model. The issue is this: revolutionary events in country 1 may trigger a democratization in country 2. This democratization is observed in country 3 where the elite decide to imitate and also extend the franchise.<sup>31</sup> This generates an upwards bias in the estimate of  $\beta_1$  which then reflects a combination of direct and indirect effects of the threat. To separate these effects, we use the variable *suffrage reforms abroad*, defined as the number of suffrage reforms happening in other countries in the sample in a given year weighted by linguistic distance. If the indirect effect of revolutionary events abroad is important, we expect a positive coefficient on this variable and a reduction in the size of the estimated coefficient on  $TR_{it}^g$ . Table 3, column four shows that *suffrage reforms abroad* is insignificant and with the ‘wrong’ sign. Moreover, the size of the estimated coefficient on  $TR_{it}^g$

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<sup>31</sup>See also Gleditsch and Ward (2006) or Persson and Tabellini (2009).

is unaffected (1.21 versus 1.19).<sup>32</sup> We obtain similar results with other definitions of *suffrage reforms abroad*.

We interpret the coefficient on  $TR_{it}^g$  as evidence that the ‘threat of revolution’ affects the franchise extension. It is, however, possible that revolutionary shocks abroad simply reduce uncertainty surrounding the cost of reform at home. This might influence the balance of power within the elite itself and give reform-friendly factions a competitive edge over reform opponents. We can use information on suffrage reform abroad to investigate this possibility. The idea is that if this hypothesis is true, then both revolutionary shocks and the reform experience of other countries should help reform-friendly factions of the elite and make suffrage reform more likely. As noted, we neither find that the direct measure of a ‘favorable reform environment’ – *suffrage reforms abroad* – is significant, nor that it has any substantive impact on the coefficient on  $TR_{it}^g$ .

<Table 3 to appear here>

### 5.2.2 Reverse Causality

Suffrage reform in some country may inspire demands for democracy in neighboring countries but with the consequence that the situation gets out of hand and a revolutionary event is triggered. If so, the causality may run from suffrage reform to revolution rather than the other way. We evaluate the plausibility of this alternative interpretation in two ways. First, we introduce a one-year lag in the measures of the revolutionary threat. Table 3, column five shows a representative specification. The point estimate on  $TR_{it-1}^g$  is 1.02 as compared to 1.19 in the baseline, and is significant at the five percent level. A general concern with using one-year lags as a defence against reverse causality is that the residuals might exhibit autocorrelation. In practice, however, the estimated autocorrelation parameter in the error structure in the baseline specification in Table 2, column five is very low (around 0.05) and we explicitly model country-specific AR(1) processes in the error terms. Yet, we consider two alternative ways to engage further with the threat to revolving the reverse

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<sup>32</sup>When we add *suffrage reforms abroad* to the event history models discussed in Section 6, we obtain a positive and significant coefficient. The estimated coefficient on  $TR_{it}^g$  remains positive and statistically significant but is smaller.



causality problem with a timing restriction. The first alternative is to model the change in the franchise extension ( $\Delta\textit{suffrage}$ ) rather than its level ( $\textit{suffrage}$ ). The point is that specifications with  $\Delta\textit{suffrage}$  are less prone to autocorrelation in the error structure.<sup>33</sup> Table 3, column seven reports that the estimated coefficient on  $TR_{it-1}^g$  is 1.26 and significant at the one percent level in a regression with  $\Delta\textit{suffrage}$  as the dependent variable. The second alternative is to estimate an event history model. This is discussed in detail in Section 6. An event study model uses information on the timing of the reforms only. In our data, these reforms are spread out over time within a country and never occur in consecutive years in any country. This reduces the problem of autocorrelation. It is reassuring that the point estimate on  $TR_{it-1}^g$  is also positive and significant in these models (see Table 8 in Section 6).

Second, the frequency of our data is yearly. Revolutionary events and suffrage reforms happening in the same year are, therefore, coded as if they were simultaneous events. In reality, of course, they were not and it is instructive to look at the timing within a year. Table 4 shows for each of the years associated with a major revolutionary event, the date of the onset of the event or events (column two) and the date at which the suffrage concession was announced in the affected countries (column three). In some cases, the reforms were announced or conceded the year before they were actually adopted (which are the years recorded in Table 1 and repeated in bracket in Table 4). An example of this is Denmark in 1848 where the Danish King announced his willingness to broaden the franchise on March 18 1848, but the new constitution was not signed till June 5 1849. Another example is United Kingdom in 1918. The process that led to the fourth reform act arguably started in 1912 with the proposed Franchise and Distribution Act.

For the three first waves in 1820, 1830 and 1848, we observe that revolutionary events systematically preceded the suffrage reforms. The only exception is Switzerland. Here, the constitutional process triggered by the civil war of 1847 started before the revolution in France but can hardly be considered the cause of the 1848 revolutions.<sup>34</sup> The Russian revolution of 1905 preceded the reform process in Fin-

<sup>33</sup>The estimated autocorrelation parameter is 0.005 with two-year fixed effects and 0.0028 with one-year effects.

<sup>34</sup>Berger and Spoerer (2001, p. 320) “conclude that without the economic crisis of 1845-1848, ..., there would not have been the critical mass to support these new ideas” and thus attribute

land. The timing in the turbulent years around World War I is a little less clear-cut, but the reforms in the Netherlands, Sweden, Norway, Italy and Belgium clearly followed after the Russian revolution of 1917 and the revolution in Hungary in 1918. The constitutional reform in Germany – leading to the Weimar Republic – also follows after the two major revolutionary events. Based on the evidence recorded in Table 4 and on the detailed narrative provided by Weyland (2010), we find it implausible that the baseline result is due to reverse causality. The timing and historical narrative are simply not consistent with this.

<Table 4 to appear here>

### 5.2.3 Own Revolutions

Our coding of the threat revolution variables exclude revolutionary events that happened within a country. The reason for not doing so is that we are using the diffusion of information about revolutions that happened elsewhere to estimate the impact of the *threat* of revolution. Revolutions that actually happened within a country is an impulse to the process not a consequence of it. In practice, it is only France who had ‘own revolutions’ within the Western European sample. An objection to our choice of excluding these events is that we are confounding revolutions in France with revolutions elsewhere. We can deal with this by controlling directly for ‘own revolutions’ in the regressions. Table 3, column six reports a specification where we add the dummy variable *own revolution* which is equal to one for France in 1830 and 1848. We observe that the coefficient on  $TR_{it}^g$  is largely unaffected. The French revolutions of 1830 and 1848 themselves were related to franchise extensions in France and so the coefficient on *own revolution* is positive and significant. In conclusion, we can rule out that the baseline results are driven by our coding of the revolution variables.

### 5.2.4 Enlightenment Shocks

It is possible that revolution and suffrage reform were both driven by liberal ideas sweeping the continent, and our results could be interpreted as evidence of such

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the 1848 revolution to economic shocks.

simultaneous over-time change in the revolutionary mood and franchise extension caused by ‘enlightenment shocks’. The publication of important books, such as Alexis de Tocqueville’s *Democracy in America* from 1835, von Humboldt’s *The Limits of State Action* from 1851, John Stuart Mill’s *On Liberty* from 1859, or Karl Marx and Friedrich Engels’ *Manifesto of the Communist Party* from 1848, could, for example, induce such spurs of enlightenment.

All the baseline specifications include time fixed effects to control for common enlightenment shocks at the two-year frequency but it is, of course, possible that these shocks happened at the yearly frequency.<sup>35</sup> The reason for using two-year time fixed effects is that doing so enables us to estimate panel corrected standard errors with unrestricted spatial correlation. We cannot model unrestricted spatial correlation in the error structure if we include one-year time fixed effects. The problem is multicollinearity: one-year time and country fixed effects can explain 98 percent of the variation in the unweighted proxy for the threat of revolution ( $TR_{it}^u$ ) and 87 percent of the variation in the distance weighted measure ( $TR_{it}^g$ ). Since we do not include one-year time fixed effects in our baseline models, we may not be controlling adequately for enlightenment shocks.

To gauge whether this is the case or not, we carry out additional estimations and tests. Firstly, we demonstrate that the results obtained with the distance weighted proxy for the threat of revolution ( $TR_{it}^g$ ) are robust to including one-year time fixed effects. As noted, we cannot obtain standard errors that correct for unrestricted spatial correlation in this case. We can, however, obtain both clustered standard errors corrected for panel heteroscedasticity and Conley-adjusted standard errors which use crow-fly distance between capital cities to model spatial dependency. Table 5 reports two sets of results.<sup>36</sup> Columns 1 to 3 show, for ease of comparison,

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<sup>35</sup>In Data Appendix D in the supplementary material, we show that the baseline results are unaffected if we cluster the standard errors by year (or by two-year pairs).

<sup>36</sup>The models reported in Table 5 control for the variable *own revolution*. We include this variable for two reasons. First, we show in Table 3, column six that the variable is highly significant. Second, it is arguably more important to control for *own revolution* with one-year than with two-year time fixed effects. Table 4 demonstrates that France had “own” revolutions in 1830 and 1848 which led to subsequent franchise extensions. If we do not control for these, they will be absorbed by the one-year time fixed effects. This overestimates the importance of these time fixed effects, and makes it harder to distinguish them from  $TR_{it}^g$  or  $TR_{it-1}^g$ . As expected, we obtain smaller coefficient estimates on the threat of revolution variables if we exclude *own revolution*. Precision is also reduced, but the point estimates on  $TR_{it-1}^g$  remains statistically significant at the five

estimations that control for two-year time fixed effect effects. Columns four to six show estimations that control for one-year time fixed effects. Column four shows that the point estimate on  $TR_{it}^g$  is equal to 1.03 and significant at the 10 percent level. This is a little smaller and less precisely estimated than with two-year effects (reported in column one). Column five shows a specification with a one-year lag of  $TR_{it}^g$ . The estimated coefficient on  $TR_{it-1}^g$  is equal to 1.10 and significant at the five percent level. Column six reports on a specification where the outcome variable is  $\Delta suffrage$ . In this first difference model, the estimated coefficient on  $TR_{it-1}^g$  is 1.37 and significant at the one percent level. In both cases, the point estimate is slightly smaller than the one obtained with two-year year fixed effects.

Secondly, a classic solution to a multicollinearity problem is to expand the sample. We cannot do this for the panel model that uses *suffrage* as the outcome variable. It is, however, possible to expand the sample in the context of the event history study where we use the dummy *reform* as the outcome variable. As discussed in Section 6.2 in more detail, in that framework, we can estimate a linear probability model which includes two-way fixed effects, with time fixed effects defined at the yearly frequency, for the extended sample of 17-21 countries. We find that the estimated coefficient on  $TR_{it}^g$  is positive and significant at the ten percent level evaluated with standard errors that allow for unrestricted spatial correlation. Moreover, the coefficient is similar to the one obtained without one-year time dummies. This suggests that we are not merely capturing enlightenment shocks with the threat of revolution proxies.

Thirdly, another way to deal with multicollinearity is to increase cross-sectional variation in the data. In Section 5.3, we test two auxiliary hypotheses generated by ‘the threat of revolution hypothesis’. Both of them induce more cross-sectional variation in the threat of revolution proxies by introducing interactions, either by postulating that the revolutionary events are likely to have stronger effects on countries that are close to where the event takes place or by postulating that the threat of revolution is conditional on inequality. Both hypotheses receive support with one-year fixed effects.

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percent level.

Taken together these estimations do not suggest that the baseline results from Table 2 simply capture yearly time shocks and demonstrate that our results are robust to yearly enlightenment shocks. The one-year time fixed effects are, however, an inadequate defense against country-specific time varying ‘enlightenment shocks’. To capture such shocks, we draw on the work of Potrafke and Vaubel (2011) on European ‘authors of liberty’. ‘Authors of liberty’ are scholars, poets, journalists, politicians, civil servants, etc. who during their life-time were influential supporters of liberal or radical ideas. Examples include Stefan Zweig, John Stuart Mill, Victor Hugo, Max Weber, Jakob Mey, and 366 others selected by a panel of country experts and listed in the Appendix to Potrafke and Vaubel (2011). Based on these data, we construct two country-specific time-varying enlightenment indicators. The first,  $AL_{it}^{Home}$ , records the number of ‘authors of liberty’ who live in country  $i$  in year  $t$ . The second,  $AL_{it}^{Abroad}$ , records the number of such authors from country  $i$  who live abroad (often because they emigrated in response to repression at home) in year  $t$ , both measured per 1000 inhabitants. We conjecture that these ‘authors of liberty’ serve as exponents of liberal ideas at home and, if living abroad, that they provide a hub through which new liberal ideas developing abroad can be transmitted to liberal-minded connections in their home country. Re-estimating the panel model with  $AL_{it}^{Home}$  and  $AL_{it}^{Abroad}$  added, we find that<sup>37</sup>

$$suffrage_{it} = \underset{(0.014)}{0.94} suffrage_{it-1} + .. + \underset{(0.18)}{1.32} TR_{it}^g + .. + \underset{(0.98)}{1.20} AL_{it}^{Abroad} + \underset{(1.58)}{0.46} AL_{it}^{Home} + .. \quad (12)$$

The coefficients on the two enlightenment indicators are positive, as expected, but not statistically significant. More importantly, the coefficient on  $TR_{it}^g$  continues to be highly significant and is a little larger than previously. All in all, we find it unlikely that the positive baseline estimate of  $\beta_1$  simply reflects confounding ‘enlightenment shocks’. Enlightenment might have been part of the story, but so was the threat of revolution.

<Table 5 to appear here>

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<sup>37</sup>The (unreported) control variables are the same as in Table 2, column five. The standard errors reported in brackets under the coefficients allow for unrestricted spatial correlation, within country clustering, and panel heteroskedasticity.

### 5.2.5 The French Revolution and the Napoleonic Wars

In the years after 1792, the French army occupied a number of neighboring countries (or parts thereof) and imposed the French civil code and eliminated aristocratic privileges (Acemoglu et al. 2011). This attempt to ‘export’ the French revolution by force might have lingered in the backs of the minds of the European elites in the decades that followed. They might, therefore, have reacted to the revolutionary events in France (and elsewhere) in 1830 and 1848, not out of fear that they too were at risk of a revolution, but because they feared another attempt at exporting revolutionary ideas by force. If so, the positive correlation between the threat of revolution proxies and suffrage reform could reflect an attempt by the elites in other countries to diffuse such a threat by copying the political system of the country where the revolution originated. This alternative explanation, however, presumes that such a move would effectively preempt an invasion. This would only be the case if the main objective of such an invasion was to export revolutionary ideas and institutions. This appears implausible and, moreover, this alternative interpretation is not consistent with the interaction between inequality and the threat of revolution proxies reported below in Section 5.3.

### 5.2.6 Repression, Transfers and Business Cycle Shocks

The ‘threat of revolution hypothesis’ views repression or the promise of transfers as alternative ways to deal with the threat of revolution (see e.g. Acemoglu and Robinson, 2000, 2006). Logically, therefore, repression and transfers are negatively correlated with the franchise extension but positively correlated with the threat of revolution. Consequently, the estimate of  $\beta_1$  is biased downwards. The reason we do not correct for this bias in the baseline is data limitations. We can, however, at the cost of reducing the sample size obtain some rudimentary proxies for repression and transfers. As a proxy for ‘repression’, we use the share of the public budget spent on policing and defense (*repression*) and as a measure of ‘fiscal transfers’, we use the share of the public budget spent on health, education, housing and various government-sponsored insurance and welfare programs (*fiscal transfers*). Table 6, columns one and two report some estimations that control for these factors.

Despite the loss of observations, the threat of revolution variable continues to have a significant and positive effect on *suffrage*. The point estimates on *repression* and *fiscal transfers* are negative, as predicted by the theory, but not statistically significant.<sup>38</sup>

Economic hardship is likely to be negatively correlated with revolutionary threats. Insofar as the business cycle has an international component, this may also bias the estimate of  $\beta_1$  downwards. To evaluate the importance of this, we extract the cyclical component of GDP per capita, *cycle*, using a Hodrick-Prescott filter. Column three of Table 6 reports a specification with *cycle* and the trend component of GDP per capita, *trend*. The point estimate on  $TR_{it}^g$  is smaller, as one would expect if the cycle is negatively correlated with the threat of revolution and positively correlated across countries, but its significance is unaffected. The effect of *cycle* itself is insignificant. It is, however, questionable if the quality of the historical GDP data is sufficient to capture the business cycle element accurately. The problem is that the GDP data for the period is often constructed using incomplete data for subsets of sectors and years. An alternative proxy for the cycle is to use year-on-year variation in rainfall. This has been shown by, for example, Berger and Spoerer (2001), Miguel et al. (2004), Brückner and Ciccone (2011), Burke and Leigh (2010) and Franck (2014) to be a good predictor of economic activity in economies with a large agricultural sector and to predict social unrest in Aidt and Leon (2014). Using data from Casty et al. (2007), we code two variables, *rainfall*, and *rainfall growth*. They measure the logarithm of the yearly rainfall and the year-on-year change in rainfall. We focus on the period before World War I since the Western European economies were more agrarian then. In column four of Table 6, we include the contemporaneous and the lagged value of *rainfall* as in Brückner and Ciccone (2011), while in column five, we follow Miguel et al. (2004) and use *rainfall growth* and its lagged value. The coefficients on the rainfall variables are positive, but not statistically significant. The estimated effect of the threat of revolution is hardly affected.

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<sup>38</sup>Re-estimating the specifications without *repression* and *fiscal transfers* on the restricted sample makes almost no difference to the point estimate on  $TR_{it}^g$ . This suggests that the downward bias is small. It makes no difference to the results if we control for repression and fiscal transfers abroad in the estimations [not reported].

<Table 6 to appear here>

### 5.3 Tests of Auxiliary Hypotheses

The ‘threat of revolution hypothesis’ delivers two auxiliary predictions that we test. Firstly, information about revolutionary threats diffuse across borders and agents in other countries learn about the risks and opportunities at home from events happening abroad. This learning effect is likely to be stronger amongst pairs of countries which are geographically, economically, or culturally closer. By distinguishing common time variation in the threat of revolution from cross-country variation generated by differences in geographical or linguistic distance to the epicenter of each revolutionary event, we can test this prediction.<sup>39</sup> Specifically, we decompose the threat measure as follows:

$$\widetilde{TR}_{it}^g = \widetilde{\beta}_1 \sum_{j \neq i} R_{jt} + \widetilde{\gamma} \sum_{j \neq i} D_{ij} R_{jt}. \quad (13)$$

The first term captures over-time variation in the threat level that is common to all countries and we expect that  $\widetilde{\beta}_1 > 0$ . The second term isolates the cross-country variation generated by differences in distance to the events. This source of variation is plausibly exogenous and we expect that  $\widetilde{\gamma} < 0$ . Re-estimating the panel model with  $\widetilde{TR}_{it}^g$  instead of  $TR_{it}^g$ , we find that in a specification with two-year time fixed effects<sup>40</sup>

$$suffrage_{it} = \underset{(0.013)}{0.93} suffrage_{it-1} + .. + \underset{(0.49)}{3.38} \sum_{j \neq i} R_{jt} - \underset{(0.0005)}{0.0019} \sum_{j \neq i} D_{ij} R_{jt} + \dots \quad (14)$$

The estimate of  $\widetilde{\gamma}$ , which is identified purely from the cross-country variation generated by distance to the revolutionary events, is negative and statistically significant. This suggests that the revolutionary shocks had a larger effect in countries closer to the epicenter of each event. Moreover, we have re-estimated this specification with one-year time fixed effects.<sup>41</sup> The estimate of  $\widetilde{\gamma}$  is equal to  $-0.0035$  and is

<sup>39</sup>Cross-country variation is also created by the fact that we omit revolutionary events happening within a country itself. This variation is non-random. Since this only affects France (in 1830 and 1848), we check that excluding France makes no difference to the results [not reported]. We also note that the results are robust to controlling for the variable *own revolution* (see Table 3, column six).

<sup>40</sup>The (unreported) control variables are the same as in Table 2, column five. The standard errors reported in brackets under the coefficients allow for unrestricted spatial correlation, within country clustering, and panel heteroskedasticity.

<sup>41</sup>With one-year time fixed effects, we cannot identify  $\widetilde{\beta}_1$ .



statistically significant.

Secondly, the ‘threat of revolution hypothesis’ predicts that the elites are more inclined to seek alternatives to franchise extension when they consider democratization threatening. Democratization is arguably more threatening where inequality is high because this enhances the incentive of newly enfranchised voters to support state-sponsored redistribution. Reliable historical data on income inequality is scarce. However, from the work by Bourguignon and Morrisson (2002), we obtain data on the gini coefficient for a subset of countries. We test this auxiliary hypothesis by adding the gini coefficient,  $gini$ , and the interaction between  $gini$  and  $TR_{it}^g$  to the model. This creates additional cross sectional variation in the effect of  $TR_{it}^g$  and allows us to include one-year time fixed effects. The result is:<sup>42</sup>

$$suffrage_{it} = \underset{(0.013)}{0.93} suffrage_{it-1} + \dots + \underset{(5.73)}{15.95} TR_{it}^g + \dots + \underset{(27.10)}{40.15} gini_{it-5} - \underset{(10.91)}{29.96} (gini_{it-5} \times TR_{it}^g) + \dots \quad (15)$$

The negative and statistically significant coefficient on the interaction term suggests that as a country’s income distribution becomes less unequal, the elites become more willing to respond to the threat of revolution by extending the franchise. The coefficient on  $gini$  is not statistically significant.<sup>43</sup> The two auxiliary tests are consistent with our interpretation of the baseline result as evidence in favor of the ‘threat of revolution hypothesis’.

## 5.4 Other Robustness Checks

Table 5 reports the results from three additional robustness checks.<sup>44</sup> First, the baseline specification uses the sub-set of major revolutionary events recorded in boldface in Table 1. In column six, we report a representative specification that uses all the recorded events. The coefficient of  $TR_{it}^g$  is smaller, but continues to

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<sup>42</sup>The (unreported) control variables are the same as in Table 2, column five. The standard errors reported in brackets under the coefficients allow for within country clustering, and panel heteroskedasticity. There is not sufficient cross section variation to allow for unrestricted spatial correlation in the error structure, but using the Conley-adjusted standard errors yield similar results. We obtain similar results with the other proxies [not reported].

<sup>43</sup>Acemoglu and Robinson (2000), Boix (2003), and Ansell and Samuels (2010) stress that inequality may affect the franchise, although there is no agreement on the nature of the effect. The observed range for  $gini$  is 0.47 to 0.56. For the average value of  $gini$  (0.52) the (short-run) marginal effect of  $TR^g$  on  $suffrage$  is around 1.38.

<sup>44</sup>Appendix D in the supplementary material contains more robustness checks.

be significant at the one percent level. This is not surprising since the minor revolutionary events are associated with less informative signals. Second, the wave of reforms between 1915 and 1919, arguably, were different from those happening during the long 19th century. To check that the results are not influenced unduly by this, we report in column seven, a specification where we restrict the sample to the period from 1820 to 1913. This makes almost no difference. Third, Italy, Austria and Germany drop out of the sample when they become dictatorships during the interwar years. In column eight, we show a specification where we keep Italy and Germany until 1938.<sup>45</sup> This has almost no effect on the point estimate.

## 5.5 Alternative Estimation Techniques

In the baseline specifications, we adopt an OLS estimator and adjust the standard errors to take unrestricted spatial correlation, within country interdependency (clustering by country), and panel heteroskedasticity into account. In Table 7, we report results obtained by alternative estimation techniques. Firstly, to get a sense of how much the adjustment for spatial correlation and clustering in the error structure matters, we report in columns one to three specifications which do not correct for spatial correlation or for clustering (column one), correct for spatial correlation, but not for clustering (column two), or corrects for clustering but not for spatial correlation (column three), respectively.<sup>46</sup> Clustering by country has little effect on the standard errors. In contrast, modeling spatial correlation improves precision. In all cases, the point estimate on  $TR_{it}^g$  (which is not affected by the various adjustments) is statistically significant at the one percent level.

Secondly, we are aware that the lagged dependent variable may cause a Nickell bias, albeit with more than 100 years of data the bias is likely to be small.<sup>47</sup> The GMM-system estimator (Blundell and Bond, 1998) or the bias-corrected least-squares dummy variable (LSDV) estimator (Bruno, 2005) can correct this bias, if there is one.<sup>48</sup> Very similar results emerge with these estimators (columns three

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<sup>45</sup>*Suffrage* is equal to zero during years with dictatorship. Austria is not included post-1918 due to incomplete urbanization data. Results are similar when *urbanization rate* is excluded and Austria is included till 1938 [not reported].

<sup>46</sup>All estimations include country and two-year time fixed effects and allow for heteroskedasticity.

<sup>47</sup>Judson and Owen (1999) show that the bias is negligible with more than 20 years.

<sup>48</sup>With only 12 countries, the advantage of the GMM estimator is unclear.

and four).

Thirdly, 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), *suffrage* is a censored outcome variable. We can use the Tobit estimator or the fractional estimator, suggested by Papke and Wooldridge (1996) to take this into account. From the results reported in columns five and six of Table 7, we see that it does not make any difference to the significance of the results.

Finally, *suffrage* as well as several of the control variables are trending up and may be non-stationary. We have reformulated the model as an Error Correction Model and find similar results (reported in Appendix D).

<Table 7 to appear here>.

## 6 Evidence From the Event History Model

The panel model tests whether the threat of revolution affected the *degree* of democracy. The event history model tests whether the threat of revolution can explain the *timing* of suffrage reform.

### 6.1 The ‘Western European Sample’

Table 8 reports logit estimates of the effect of the threat of revolution variables on the probability of suffrage reform. We observe that the estimates are positive and statistically significant at the one percent level (columns one to three). A similar result is obtain with the one-year lag of  $TR_{it}^g$  (column four). Based on the estimate reported in column one, one extra revolutionary event increases the odds that a country will introduce a major suffrage reform in that year by 75 percent. This is a substantial effect which is consistent with the ‘threat of revolution hypothesis’. The specification in column five shows that suffrage reforms in neighboring countries have a positive and significant effect on the probability of suffrage reform in a given country. This is also consistent with the prediction of the theory (proposition 3).

As expected, controlling for this indirect channel reduces the point estimate on  $TR_{it}^g$  but not its statistical significance.

We have undertaken many robustness checks. Firstly, suffrage reforms are rare events. To correct for this, we have re-estimated all specifications using the rare events logit estimator (King and Zeng, 2001). The representative result reported in column six shows that the coefficient on  $TR_{it}^g$  continues to be statistically significant at the one percent level after the bias-correction and that the coefficient estimate is only marginally smaller. Secondly, we treat all franchise extensions as if they were nonreversible. We know that in some cases they were not. Using the coding of franchise contractions from column four of Table 1, we make a distinction between those reforms that lasted for at least ten years and those which were wholly or partly reversed within that time window. Column seven shows a specification that excludes suffrage extensions that were followed by a franchise contraction. The coefficient on  $TR_{it}^g$  is smaller, but continues to be statistically significant at the one percent level. Thirdly, column eight shows a specification with country-specific hazard rates.<sup>49</sup> The point estimate is larger than the corresponding estimate reported in column two and is significant at the one percent level. Finally, we have investigated specifications that control for the cyclic component of GDP, or spending on repression and fiscal transfers. In all cases, the estimate of the threat of revolution variable is significant.<sup>50</sup>

<Table 8 to appear here>

## 6.2 The ‘Broader European Sample’

All the countries in the ‘Western European sample’ 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 countries did not become fully consolidated democracies until the 1990s. Yet, they did take the first steps towards democracy by extending the franchise to broader segments of the populations before World War

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<sup>49</sup>These are estimated by conditional fixed effects logit. Results from a random effects model are similar [not reported].

<sup>50</sup>See Table D1 in Appendix D included with the supplementary material.

I or just after, following a pattern not all that dissimilar to the one followed in Western Europe (Seymour and Frary, 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 test the ‘threat of revolution hypothesis’ on a broader sample.

To this end, we add information on suffrage reforms in Spain, Portugal, Greece, Iceland, Luxembourg, Serbia, Hungary, Poland, Russia, and Romania to the sample. The down-side is that we can, due to data limitations, only control for the influence of war (*war* and *WW1*), and, in a few countries, for *GDP per capita* and *population*. Table 9 reports the results for the maximum sample of 21 countries (columns one to four). The last four columns report specifications with additional control variables, but fewer countries and less time coverage. We observe that the threat of revolution (measured by  $TR_{it}^g$ ) has a positive impact on the likelihood of suffrage reform. Moreover, it is evident that the significance and magnitude of the effect is independent of the estimation technique and coding choices. The historical narrative clearly demonstrates that repression was common currency in Russia and Eastern Europe. Since we cannot control for this, we expect a downwards bias. It is, therefore, not surprising that the estimated effects are smaller in magnitude than those reported for the ‘Western European sample’ in Table 7.

<Table 9 to appear here>

As discussed in Section 5.2.4,  $TR_{it}^g$  and the other measures of the threat of revolution are highly correlated with one-year time fixed effects. With the ‘broader European sample’, we have up to 21 countries. The extra countries makes it feasible to overcome the multicollinearity problem.<sup>51</sup> In particular, we estimate a linear probability model with country and one-year time fixed effects as well as unrestricted spatial correlation in the error structure. The result is:<sup>52</sup>

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<sup>51</sup>One-year dummies and country fixed effects now explain 80 percent of the variation in  $TR_{it}^g$  in the enlarged sample. They explain 98 percent of the variation in the  $TR_{it}^u$  making estimation infeasible with this proxy.

<sup>52</sup>The standard errors reported in brackets under the coefficients allow for unrestricted spatial correlation, within country clustering, and panel heteroskedasticity.

$$P(\text{reform}_{it} = 1 | TR_{it}^g, X_{it}, M_{t-1} = 0) = \dots \underset{(0.01675)}{0.032} TR_{it}^g + \underset{(0.035)}{0.0137} war_{it} \quad (16)$$

The coefficient on  $TR_{it}^g$  is significant at the ten percent level. While the year dummies are significant, they do not seem to matter much for the point estimate which is 0.028 when the year dummies are excluded. We also note that we get similar results when we control for *GDP per capita* and *population*. To reiterate, these results reinforce our interpretation of the baseline results as evidence of threat of revolution rather than of enlightenment shocks.

## 7 Other Results

Our regressions include a number of control variables motivated by other theories of suffrage reform than the ‘threat of revolution hypothesis’. We stress that our study is not designed explicitly to test these alternatives. The purpose of including these variables is to avoid confounding our estimate of threat of revolution effect in obvious ways. Yet, it is of independent interest to consider the findings related to these variables and to dig a little deeper by augmenting the baseline models estimated on the ‘Western European sample’ with additional variables for which we only got partial time or country coverage. Table 10 combines the additional results from the panel model and the event history study.

In the baseline specifications, we use the variable *gold standard* to proxy for trade integration and we find little support for the ‘trade-causes-democracy’ thesis. The two variables *trade volume* (the sum of imports and exports relative to GDP) and *wheat price spread* capture trade integration more directly.<sup>53</sup> The variable *wheat price spread* is a measure of trade costs based on convergence in wheat prices across time and space (Jacks, 2005). The coefficient on *trade volume* is positive, but not significant (column one and five). On the other hand, *wheat price spread* is significant in the event history model (column six). This gives some credence to the ‘trade-causes-democracy’ thesis.

The modernization variables—*GDP per capita*, *urbanization rate*, and *education*

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<sup>53</sup>We lose between 200 and 650 observations when we include these variables.

*attainment*—are mostly insignificant, with the exception that *GDP per capita* is significant in the event history model (see Table 8). The same message comes from the specifications shown in columns three and seven of Table 10 where we control for the impact of industrialization (*agricultural share*). This echoes the findings of Acemoglu et al. (2008) that rising income and education levels cannot explain democratization.<sup>54</sup> Occasionally, population size has a positive and significant impact.

The ‘Janowitz thesis’ that war in general and conscription armies in particular were important impulses for democratic reform receives some support. In the panel model, the coefficient on *war* is consistently positive and significant. Based on the point estimates reported in Table 2, being at war increases *suffrage* by between 3.2 and 4.3 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 8). To take the scale of war into account, we use a measure of the number of war deaths—*war intensity*. We obtain results that are qualitatively similar to those obtained with the dummy variable *war* (columns four and eight of Table 10). The dummy variable for World War I is not significant, except in the conditional fixed effects logit model reported in Table 8, column eight.

<Table 10 to appear here>

## 8 Conclusion

We provide robust econometric evidence that the threat of revolution was systematically related to the evolution of suffrage rights in Europe in the 19th and early 20th centuries. This is consistent with the ‘threat of revolution hypothesis’ and the framework for understanding democracy change developed by Acemoglu and Robinson (2000, 2006).

Yet, it is important to keep three points in mind. Firstly, by focusing on Europe during the first wave of democratization, we study a subset of the universe of all suffrage reforms. It is, therefore, possible that the threat of revolution played a

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<sup>54</sup>For evidence supporting ‘modernization theory’, see, e.g., Gundlach and Paldam (2009) and for a critical evaluation, see Przeworski and Limongi (1997).

different role for democratization elsewhere and that other theories might have greater explanatory power in other contexts, regions, or time periods. The work by Przeworski (2009) on the causes of suffrage reforms after World War I, the work by Aidt and Franck (2013, 2014) on the relationship between the Swing riots and support for the Great Reform Act in 1832 in Great Britain, and the work by Aidt and Leon (2014) on the causal relationship between economic shocks, riots and democratic change in Africa at the turn of the 20th century, however, give some reason to believe that the ‘threat of revolution hypothesis’ offers insights into the causes of suffrage reforms also outside our sample and for particular reforms inside our sample.

Secondly, Acemoglu and Robinson’s (2000, 2006) theory of democratization emphasizes that democratization happens at critical junctures in history. Our evidence support this interpretation. However, this does not rule out that complex interactions between underlying, slow-moving economic variables—industrialization, urbanization, income growth, international trade, inequality, etc.—and democratic triggers could be important, and nor does the theory rule this out. One can interpret the revolutions abroad as shocks that may push a country over a threshold, but only if the underlying fundamentals are such that the economy is ‘close’ to the threshold to begin with.

Third, we interpret revolutionary shocks as signals to the elites in other countries about the threat of revolution. It is, however, possible that revolutions abroad also capture signals about the cost of reform to factions within a divided ‘elite’. Revolutions abroad might give those in favor of reform, a bargaining chip vis-à-vis the reform opposition because they can threaten to encourage a domestic revolt or simply because they can ‘bully’ opponents within the elite into action with an argument that reform is needed to avoid a revolution. In this case, the relationship between franchise extension and the threat of revolution variables does not capture a direct threat effect. Instead, it captures an indirect threat effect through which the hand of reform-friendly factions within the elite is strengthened. Suffrage reform is then caused by a mixture of preemptive and proactive forces.



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

Country <sup>a</sup>	Franchise extensions	Revolutionary events	Franchise contractions
<b>Panel A</b>			
United Kingdom (1820)	1832, 1867, 1884, 1918		None
Austria (1867, 1934)	1867, 1896, 1907	<b>1848-49</b>	1934
Italy (1861, 1924)	1861, 1882, 1912, <b>1919</b>	<b>1820, 1848-49</b>	1924
Norway (1820)	(1814), 1884, 1897, 1919		None
The Netherlands (1830)	(1815), 1848, 1887, 1894, 1917		None
Sweden (1820)	1866, 1907, 1919		None
France (1820)	1820, 1830, <b>1848</b> , (1870)	<b>1830, 1848, 1870-71</b>	1852-1869
Germany (1871, 1933)	1871, 1919	<b>1848-49</b>	1933
Finland (1820)	1869, 1906		None
Belgium (1830)	1831, 1848, 1893, 1919	<b>1830-33</b>	None
Switzerland (1848)	1848		None
Denmark (1820)	1849, 1915		1866 1875-1901
<b>Panel B</b>			
Luxembourg (1820)	1841, 1848, <b>1857</b> , 1893, 1902, 1919		1860
Iceland (1874)	1874, 1908, 1916, 1934		None
Spain (1820, 1936)	(1812), <b>1820</b> , 1834, <b>1837</b> , 1865, <b>1869</b> , 1888, 1890, <b>1931</b>	<b>1820-23</b> , 1827, 1836, 1840, 1842, 1854-56, 1866, 1868, 1873-74, 1890, 1909, 1933, 1934	1823-33, 1845 1876, 1923 1936, 1938
Portugal (1820, 1926)	<b>1822</b> , 1838, 1852, 1878, 1911	<b>1820</b> , 1910, <b>1915</b> , 1919, 1927	1826, 1895, 1926
Serbia (1820)	1868, <b>1888</b> , 1903, 1920	1861	1893, 1894, 1901, 1931
Greece (1822)	1822, 1844	1843, 1866-68, 1935, 1938	None
Romania (1856, 1938)	1866, 1923		1938
Poland (1918)	<b>1921</b>	1830-31, 1863-64	1926, 1935
Hungary (1867, 1936)	1867	<b>1848-49, 1918-19</b>	1920, 1936
Russia (1820, 1917)	<b>1906</b>	<b>1905, 1917</b>	1907, 1917
<b>Panel C</b>			
Ireland (never)		<b>1916</b>	
Other part of Balkans (never)		1826, 1885, 1888, 1907	

*Notes:* See Data Appendix A and B for information on the coding. The franchise extensions in boldface are those which were followed by a franchise contraction within 10 years. The revolutionary events in boldface are those which we characterize as major events. a. The first year in brackets indicates the year in which the country enters our samples and the second year, if applicable, is the year in which the country regresses into autocracy or civil war. The 12 countries listed in panel A are included in the Western European Sample. The Broader European Sample includes, in addition, the 10 countries listed in panel B. The countries listed in panel C are not in the sample, but we make use of revolutionary events that happened in Ireland and on the Balkans in the construction of the measure of the threat of revolution.

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

	(1)	(2)	(3)	(4)	(5)	(6)
TR <sup>u</sup> (unweighted)	1.89*** [5.88]			1.86*** [5.73]		
TR <sup>g</sup> (geographical)		1.24*** [6.75]			1.19*** [6.44]	
TR <sup>l</sup> (linguistic)			3.13** [2.14]			3.33** [2.32]
Suffrage lagged	0.94*** [71.04]	0.94** [72.76]	0.94** [70.28]	0.93*** [69.00]	0.93*** [70.66]	0.93*** [68.25]
Log GDP per capita				0.12 [0.05]	-0.22 [-0.08]	0.34 [0.13]
Log population				3.92* [1.78]	3.77* [1.69]	3.98* [1.84]
Urbanization rate				0.003 [0.38]	0.003 [0.34]	0.003 [0.35]
War				4.21*** [3.97]	4.08*** [3.87]	4.32*** [4.08]
WWI				-2.00 [-1.05]	-1.98 [-1.07]	-2.00 [-1.08]
Educational attainment				-0.81 [-0.93]	-0.8 [-0.93]	-0.84 [-0.97]
Gold standard				0.24 [0.31]	0.2 [0.26]	0.26 [0.34]
Observations	1069	1069	1069	1069	1069	1069
Number of countries	12	12	12	12	12	12
Estimation technique	OLS	OLS	OLS	OLS	OLS	OLS
Selection-ratio				3.7	5.9	2.5

*Notes:*  $z$  statistics in square brackets are based on PCSE standard errors allowing for panel heteroskedasticity and contemporaneously correlated across panel units, and for a country specific AR(1) process over time (clustering by country); \* significant at 10%; \*\* significant at 5%; \*\*\* significant at 1%. All estimations include country and two-year time fixed effects. We estimate with the `xtpcse` command in STATA 13 (Beck and Katz, 1995). It estimates the parameters by OLS and calculates PCSE corrected standard errors. The selection-ratio (Altonji et al., 2005) indicates how many times stronger selection on unobserved factors needs to be relative to selection on the observable factors included in the three specifications for the point estimate on the threat of revolution variables to be attributable entirely to omitted variables bias.

**Table 3: Results for the Panel Model: Conley spatial dependence, the reflection problem, reverse causality and own revolutions.**

Dependent variable:	<i>Suffrage</i>						$\Delta$ <i>Suffrage</i>
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
TR <sup>g</sup>	1.19** (2.09)	1.19** (2.54)	1.19*** (3.48)	1.21*** [6.19]		1.18*** [6.37]	
TR <sup>g</sup> (lagged)					1.02** [2.55]		1.26*** [3.04]
Suffrage reforms abroad				-1.01 [-0.35]			
Own revolution						9.12*** [4.00]	
Observations	1069	1069	1069	1069	1069	1069	1069
Number of countries	12	12	12	12	12	12	12
Estimation technique	OLS	OLS	OLS	OLS	OLS	OLS	OLS
Spatial correction	500km	800km	1400km	PCSE	PCSE	PCSE	PCSE

*Notes:* z statistics in brackets are based on spatial (Conley, 1999) standard errors for three different radiuses; z statistics in square brackets are based on PCSE standard errors allowing for panel heteroskedasticity and contemporaneously correlated across panel units, and for a panel specific AR(1) process over time (clustering by country); \* significant at 10%; \*\* significant at 5%; \*\*\* significant at 1%. All estimations include country and two-year time fixed effects and the same set of control variables as in Table 2, column five. The results in columns one to three are obtained with the OLS estimator and the standard errors proposed by Conley (1999) which allow for contemporaneous spatial dependence amongst the disturbances. The metric used to measure distance between panel units is distance in kilometres between pairs of capital cities. The three specifications differ in the assumed cut-off after which the spatial dependence is zero. The results in columns four and five are obtained with the same estimation technique as in Table 2.

**Table 4: Within year Timing of Revolutionary Events and Suffrage Reforms**

Year	Onset of revolutionary event	Announcement of concession
1820	Spain: January Portugal: January Italy: July	France: November
1830	France: July. Belgium: August 25	France: July United Kingdom: November Belgium: December 20
1848	Southern Italy: January France: February 22 Germany: March 3 Austria: March 12 Northern Italy: March 22 Hungary: May	Switzerland: January 27 France: February 24 Luxemburg: March 20 Denmark (1849): March 18 Netherlands: March 27 Belgium: March 29
1871	France: May	Germany (1871): 1867
1905	Russia: January 9	Finland (1906): November 12 Russia: February 18
1915	Portugal: May 10	Denmark: May 7
1916	Ireland: Easter	
1917	Russia: February	United Kingdom (1918): 1912 The Netherlands: Summer.
1918	Hungary: November	Germany (1919): November. Sweden (1919): December
1919		Italy: 1919. Norway: June. Belgium: 1919.

*Notes:* For each of the major revolutionary events, and for each suffrage reform that happened in the same calendar year, we have recorded the date of the onset of the revolutionary event and the date of the announcement of the reform or the date at which the process that led to the suffrage reform started. In column three, we record in bracket the year in which the reform was finally adopted, if this is different from the year in which the reform was announced.

*Source:* The information used to record these dates is given in the Data Appendix.



**Table 5: Results for the Panel Model: One-year time fixed effects.**

Dependent variable:	<i>Suffrage</i>		$\Delta$ <i>Suffrage</i>	<i>Suffrage</i>		$\Delta$ <i>Suffrage</i>
	(1)	(2)	(3)	(4)	(5)	(6)
TR <sup>g</sup>	1.18*** [6.35]			1.03* {1.85} (1.81)		
TR <sup>g</sup> (lagged)		1.15*** [2.88]	1.41*** [3.42]		1.10** {2.14} (2.46)	1.37*** {2.67} (2.62)
Observations	1069	1069	1069	1069	1069	1069
Number of countries	12	12	12	12	12	12
One-year fixed effects	NO	NO	NO	YES	YES	YES
Two-year fixed effects	YES	YES	YES	NO	NO	NO
Estimation technique	OLS	OLS	OLS	OLS	OLS	OLS
Spatial correlation	PCSE	PCSE	PCSE	Conley	Conley	Conley

*Notes:*  $z$  statistics in square brackets are based on PCSE standard errors allowing for panel heteroskedasticity and contemporaneously correlated across panel units, and for a panel specific AR(1) process over time (clustering by country);  $z$  statistics in curly brackets are PCSE standard errors without spatial correlation, but with panel heteroskedasticity adjustment and clustering by country;  $z$  statistics in brackets are based on spatial (Conley, 1999) standard errors (1400km); \* significant at 10%; \*\* significant at 5%; \*\*\* significant at 1%. The estimations in columns one to three include country and two-year time fixed effects and the same set of control variables as in Table 3, column six, except that we exclude the dummy for WWI to make these estimations comparable to those reported in columns four to six. The estimations in four to six include country and one-year time fixed effects and the same set of control variables as in Table 3, column six, except that the dummy for WWI drops out because it is perfectly collinear with the one-year time fixed effects.

**Table 6: Results for the Panel Model: Repression, Transfers, Economics Shocks and Other Robustness Checks.**

**Dependent variable: *Suffrage*.**

	(1)	(2)	(3) <sup>b</sup>	(4) <sup>b,c</sup>	(5) <sup>b,c</sup>	(6)	(7) <sup>c</sup>	(8) <sup>d</sup>
TR <sup>g</sup>	1.91*** [4.49]	1.37*** [6.76]	1.21*** [6.53]	1.18*** [6.00]	1.17*** [5.87]		1.19*** [6.01]	1.18*** [5.55]
TR <sup>g</sup> (all)						0.84*** [4.89]		
Repression		-0.02 [-0.45]						
Fiscal transfers	-0.05 [-1.22]							
Trend			0.29 [0.10]					
Cycle			3.48 [0.54]					
Log Rainfall				2.70 [1.17]				
Log Rainfall, lag				0.52 [2.22]				
Rainfall growth					1.65 [0.92]			
Rainfall growth, lag					1.01 [0.59]			
Observations	618	875	1045	809	809	1069	809	1089
No. of countries	9 <sup>a</sup>	9 <sup>a</sup>	12	12	12	12	12	12
Robustness check	Repression	Fiscal transfers	Cycle GDP	Cycle Rain shocks	Cycle Rain shocks	All events	Till 1913 only	Democratic reversals

*Notes:*  $z$  statistics in square brackets are based on PCSE standard errors allowing for panel heteroskedasticity and contemporaneously correlated across panel units, and for a panel specific AR(1) process over time (clustering by country); \* significant at 10%; \*\* significant at 5%; \*\*\* significant at 1%. All estimations use the same estimation technique and include country and two-year time fixed effects and the same set of control variables as in Table 2, column five. a. Data from Austria, Germany and Switzerland are missing. b. Log *GDP per capita* is replaced by *cycle* and *trend* or by the rainfall variables as appropriate. c. Till 1913. d. Reversals are included.

**Table 7: Results for the Panel Model: Alternative Estimation Methods.****Dependent variable: *Suffrage*.**

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
TR <sup>g</sup>	1.19*** [4.91]	1.19*** [6.38]	1.19*** [5.04]	1.20*** [4.01]	1.32*** [4.70]	1.38*** [4.59]	0.11** [2.05]
Observations	1069	1069	1069	1069	1069	1069	1061
Number of countries	12	12	12	12	12	12	12
Estimation technique	OLS	OLS	OLS	Bruno	GMM	Tobit	Fractional
Spatial correlation	No	Unrestricted	No	No	No	No	No
Clustering	No	No	Country	No	No	No	Yes

*Notes:* *z* statistics in square brackets; \* significant at 10%; \*\* significant at 5%; \*\*\* significant at 1%. All estimations include country and two-year time fixed effects and the same set of control variables as in Table 2, column five. The standard errors in columns one to three are adjusted for panel heteroskedasticity. The estimator used in column four is the bias-corrected least squares estimator proposed by Bruno (2005). The results in column five are obtained with the system-GMM estimator proposed by Blundell and Bond (1998). The marginal effect conditional on *suffrage* being strictly between 0 and 100 is the same as the coefficient estimate reported in column six. The fractional estimator used in column seven is due to Papke and Wooldridge (1996) and it transforms *suffrage* with a logit link. The coefficient is not comparable to the others, but  $\exp(0.11)$  can be interpreted as an odds ratio.

**Table 8: Results from the Event History Study, Western European Sample.****Dependent variable: *reform*.**

	(1)	(2)	(3)	(4)	(5)	(6)	(7) <sup>a</sup>	(8)
TR <sup>u</sup> (unweighted)	0.75*** [5.25]							
TR <sup>g</sup> (geographical)		0.50*** [7.57]			0.40*** [4.84]	0.48*** [7.34]	0.39*** [9.75]	0.73*** [4.30]
TR <sup>l</sup> (linguistic)			3.93*** [5.38]					
TR <sup>g</sup> (lagged)				0.40*** [4.61]				
Suffrage reforms abroad					5.44*** [3.09]			
Log GDP per capita	2.45** [2.44]	2.29** [2.23]	1.66*** [2.59]	2.13** [2.18]	2.06** [2.17]	2.11** [2.09]	1.36 [1.44]	2.37 [0.82]
Log population	0.47* [1.75]	0.49* [1.80]	0.32 [1.03]	0.47* [1.72]	0.43* [1.69]	0.45* [1.68]	0.20 [1.05]	5.47 [1.26]
Urbanization rate	-0.003 [-1.53]	-0.003 [-1.55]	-0.002 [-0.85]	-0.003 [-1.29]	-0.003 [-1.34]	-0.003 [-1.37]	-0.0009 [-0.39]	-0.003 [-0.32]
War	-0.15 [-0.20]	-0.29 [-0.47]	0.18 [0.12]	0.101 [0.10]	-0.10 [-0.16]	-0.21 [-0.35]	-0.04 [-0.06]	-0.88 [-0.15]
WWI	-1.52 [-1.15]	-1.17 [-0.93]	-2.14 [-1.24]	-1.36 [-0.95]	-0.92 [-0.74]	-0.96 [-0.78]	-0.89 [-0.71]	-2.40** [-1.96]
Educational attainment	-0.06 [-0.09]	0.005 [0.007]	-0.24 [-0.40]	-0.03 [-0.05]	0.06 [0.08]	0.01 [0.02]	0.42 [0.68]	0.042 [0.05]
Gold standard	-1.12* [-1.93]	-0.95* [-1.68]	-0.70 [-1.47]	-1.15** [2.16]	-0.83 [-1.51]	-0.87 [-1.56]	-0.72 [-1.22]	-1.21** [-2.05]
Observations	713	713	713	713	713	713	739 <sup>b</sup>	713
Number of countries	11	11	11	11	11	11	11	11
Estimation technique	Logit clustered by country	Logit clustered by country	Logit clustered by country	Logit clustered by country	Logit clustered by country	Rare events logit	Logit clustered by country	Cond. fixed effects logit

Notes:  $z$  statistics based on clustered (by country) standard errors in square brackets, except in column five; \* significant at 10%; \*\* significant at 5%; \*\*\* significant at 1%. Constant term not reported. Only 11 countries are included in the event history study, as Switzerland had full male suffrage from the time it became unified. The dependent variable *reform* is coded as 1 if country  $i$  introduced a franchise extension 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, before that happened, regressed into dictatorship. A country enters the "risk set" either in 1820 or at the time of independence (as recorded in column one of Table 1). We follow Beck et al. (1998) and use a discrete logistic specification (logit) to estimate the conditional probability of a reform in a given year. In column five, we adopt the rare events logit estimator by King and Zeng (2001) and in column seven we include fixed effects directly in the logit model. All estimations allow for duration dependence of the hazard rate. A likelihood ratio test indicates strong duration dependence in the baseline hazard rate. a. *Reform* is coded to exclude franchise extensions that were followed by a franchise contraction within a 10 years window. b. The extra observations are due to the fact that France stays in the sample till 1870 and Italy stays till 1924.

**Table 9: Results from the Event History Study, Broader European Sample.****Dependent variable: *reform*.**

	(1)	(2)	(3)	(4) <sup>e</sup>	(5)	(6)	(7)	(8) <sup>e</sup>
TR <sup>g</sup>	0.31***	0.31***	0.31***	0.28***	0.34***	0.34***	0.40***	0.28***
	[4.78]	[4.79]	[5.11]	[4.40]	[4.15]	[4.17]	[3.97]	[3.69]
Log GDP per capita					0.14	0.16	0.707	0.60
					[0.29]	[0.33]	[0.38]	[0.87]
Log population					0.31**	0.30**	2.03	-0.46
					[2.47]	[2.45]	[0.81]	[1.37]
War	0.15	0.23	0.29	0.25	0.48	0.53	0.579	0.34
	[0.28]	[0.43]	[0.494]	[0.44]	[0.69]	[0.77]	[0.84]	[0.67]
WWI	0.04	0.14	-0.201	0.20	-0.58	-0.45	-1.15	0.15
	[0.06]	[0.21]	[-0.332]	[0.28]	[-0.66]	[-0.52]	[-1.45]	[1.37]
Observations	1700	1700	1700	1726 <sup>d</sup>	1132	1132	1063	1158 <sup>d</sup>
Number of countries	21 <sup>a</sup>	21 <sup>a</sup>	21 <sup>a</sup>	21 <sup>a</sup>	17 <sup>b</sup>	17 <sup>b</sup>	15 <sup>c</sup>	17 <sup>b</sup>
Estimation technique	Logit clustered by country	Rare Events logit	Condi- tional fixed effects Logit	Logit clustered by country	Logit clustered by country	Rare Events logit	Condi- tional fixed effects Logit	Logit clustered by country

*Notes:*  $z$  statistics based on clustered (by country) standard errors in square brackets, except in columns two and six; \* significant at 10%; \*\* significant at 5%; \*\*\* significant at 1%. The definition of *reform* and the details of estimation techniques are in notes to Table 7. All estimations allow for duration dependence of the hazard rate. A likelihood ratio test indicates strong duration dependence in the baseline hazard rate. a. The sample includes Austria, Belgium, Finland, Sweden, Norway, Denmark, the Netherlands, the United Kingdom, France, Germany, Italy, Spain, Hungary, Portugal, Greece, Iceland, Luxembourg, Serbia, Poland, Russia and Rumania. b. These specifications exclude Russia, Serbia, Iceland, and Luxembourg. c. This specification excludes Hungary and Poland as well as the countries listed under b. d. The extra observations are due to the fact that France stays in the sample till 1870 and Italy stays till 1924. e. The specification excludes franchise extensions that were reversed within ten years.

**Table 10: Additional Results for the Panel and Event History Models, Western European Sample**

	(1) <sup>b</sup>	(2) <sup>b</sup>	(3)	(4)	(5) <sup>b</sup>	(6) <sup>b</sup>	(7)	(8)
Robustness check	Add trade volumes	Add wheat price spread	Add agricultural share	Add war-intensity	Add trade volumes	Add wheat price spread	Add agricultural share	Add war-intensity
Dependent variable	<i>Suffrage</i>				<i>Reform</i>			
TR <sup>g</sup>	1.49*** [7.19]	1.51*** [3.97]	0.89*** [3.74]	1.19*** [6.47]	0.52*** [6.44]	0.51** [2.28]	0.41*** [4.31]	0.49*** [6.58]
Trade volume	0.02 [1.44]				0.01 [1.63]			
Wheat price spread		4.91 [1.44]				13.55** [2.42]		
Agricultural share			-0.01 [-1.41]				0.001 [0.20]	
War intensity				5.59** [2.26]				-0.21 [-0.16]
Observations	858	436	876	1069	585	355	533	713
Number of countries	12	7 <sup>a</sup>	12	12	11 <sup>a</sup>	7 <sup>b</sup>	11	11
Estimation technique	OLS with PCSE clustered by country	OLS with PCSE clustered by country	OLS with PCSE clustered by country	OLS with PCSE clustered by country	Logit clustered by country	Logit clustered by country	Logit clustered by country	Logit clustered by country

*Notes:* See notes to Table 2 and Table 7. The independent variable in the estimations in columns one to four is *suffrage*. These estimations include country and two-year time fixed effects and the same set of control variables as in Table 2. We only report the coefficients on the new variables which are added to each specification. The models in columns five to eight are all logit models for the probability of a suffrage reform. These estimations include the same control variables as in Table 7 and allow for duration dependence. a. Data from Denmark, Finland, the Netherlands, Sweden, and Switzerland are missing; b. *Gold standard* is replaced by the alternative measure(s) of trade integration.

# Supplementary Material

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

Supplementary material

## 1 Theory Appendix: Proofs

**Deriving  $\widehat{q}_{REVOLT}$ .** At stage 3, which is reached only if the insiders neither extended the franchise nor invested in repression in stage 2, the outsiders do not know the social state. Suppose they observe an uninformative report. In this case,  $q_{1t} = q$  (and is expected to be at this level in all future periods). If the outsiders do not revolt, then they get  $y_O(\mathcal{A}) + \beta V_O(\mathcal{A})$ , where  $V_O(\mathcal{A})$  is the outsiders' expected discounted utility when the political state is autocracy. If they decide to revolt, they face a lottery and their expected discounted utility is

$$(1 - qp)(y_O(\mathcal{A}) - \mu + \beta V_O(\mathcal{A})) + qp\left(\frac{y_O(\mathcal{S})}{1 - \beta} - \mu\right). \quad (1)$$

Suppose that  $q$  is such that the outsiders never start a revolution. Then  $V_O(\mathcal{A}) = \frac{y_O(\mathcal{A})}{1 - \beta}$ , and we can find the critical value of  $q$  at which they are indifferent between revolting and not revolting after receiving  $L_{1t} = 1$  as  $\widehat{q}_{REVOLT}$  defined in equation (1).

**Deriving condition [D],  $\widehat{q}_{DEMOCRACY}$  and  $\widehat{q}_{REPRESSION}$ .** Democratization yields  $\frac{y_I(\mathcal{D})}{1 - \beta}$ , while repression yields  $y_I(\mathcal{A}) - \sigma + \beta \frac{y_I(\mathcal{A})}{1 - \beta}$  because the revolution is avoided for sure in this period and there is no expectation of a revolution in the future given Assumption 1. A simple comparison shows that franchise extension is better than repression if condition [D] holds. The expected payoff of doing nothing after receiving the report  $L_{1t} = l$  is the lottery

$$\frac{pq}{l}0 + \left(1 - \frac{pq}{l}\right) \left(y_I(\mathcal{A}) + \frac{\beta y_I(\mathcal{A})}{1 - \beta}\right). \quad (2)$$

Accordingly, if condition [D] holds (fails), we know that democratization (repression) is better than repression (democratization) and we can compare this expected payoff to  $\frac{y_I(\mathcal{D})}{1 - \beta} (y_I(\mathcal{A}) - \sigma + \beta \frac{y_I(\mathcal{A})}{1 - \beta})$ . This yields conditions (3) and (4) in the text, respectively.

## 2 Data Appendix A

This appendix details the coding of suffrage reforms in Europe 1820-1938 and other aspects of the samples used in the study. The “Western European sample” includes Austria, Belgium, Finland, Sweden, Norway, Denmark, the Netherlands, Germany, the United Kingdom (excluding Ireland), France, Italy, and Switzerland. The “broader European sample” includes Spain, Portugal, Greece, Iceland, Luxembourg, Serbia, Hungary, Poland, Russia, and Romania in addition to those included in the “Western European sample”.

A country enters the sample in 1820 or when it becomes an independent state. The entry year is recorded in the country tables (*A1* to *A22*) below and in Table 1 in the text. In the cases, where it is not straightforward to determine the entry year, a justification for the choice is given. In the main specifications, a country drops out if it regresses into some form of autocracy or into civil war (e.g., Germany (1933), Austria (1934), Italy (1924), Spain (1936), Russia (1917)) before 1938. In some specifications, we keep countries in the sample after they regress to autocracy/dictatorship.

Tables *A1* to *A22* detail how we coded the years of “franchise extensions” and “franchise contractions”. We have also, in some cases, recorded additional information about the evolution of democratic institutions in the sample and other relevant historical facts. This includes the year of women’s suffrage, the introduction of the secret ballot or changes in the election rule, etc.

We use the following definitions to code the years of “franchise extensions” and “franchise contractions” as recorded in Table 1 in the main text and detailed below:

- 1) A “franchise extension” is a reform that relaxes socio-economic qualifications on the right to vote in elections to the lower chamber of parliament for men. This includes lowering of income, tax payment, and wealth qualifications; abolishment of education qualifications; enfranchisement of recipients of public support; and other such socio-economic qualifications, as well as the introduction of (qualified) voting rights in the first place.

Comment: We do not include women’s suffrage or reforms that lowered the voting age, except if these happen in conjunction with changes in socio-economic



qualifications, in the definition of a franchise extension. All the reforms recorded are de jure and varied in the de facto power they conferred on the newly enfranchised voters.

2) A “franchise contraction” is a) a reform that increases one or more of the socio-economic qualifications listed above and thus de jure (and de facto) disenfranchises voters who have previously enjoyed the right to vote or b) a discrete change in the rules that de facto restricts the right to vote which happens subsequent to a “franchise extension” as defined above and which do not coincide with that extension.

Comment: We want to record instances where voting rights which have been granted previously are subsequently taken back, either de jure or de facto. De facto franchise contractions require a discrete change and must happen subsequent to a franchise extension. Thus, this implies that we do not count the fact that the broad franchise in Imperial Germany introduced in 1871 was de facto restricted by the fact that electoral corruption was widespread, but we do count the de facto overturn of the Weimar Constitution by the Nazi regime in 1933. Likewise, we count the Second Empire from 1852 to 1869 in France, where elections were systematically manipulated, and the constitutional amendment in Denmark in 1866 as instances of franchise contraction.

In the tables below, we indicate in boldface the years that we count as years of franchise extension and indicate in italics the years that we count as years of franchise contraction.

The sources used to construct these data are: Flora et al. (1983),<sup>1</sup> Carstairs (1980), Seymour and Frary (1918), Campbell (1958), Cook and Paxton (1998) and Caramani (2000). We have, in addition, made extensive use of Encyclopaedia Britannica (1911, 2009), Ortega and Blanco (1990), and Bataković (2007).

[Appendix Table A1 to A22]

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<sup>1</sup>Notice that the dates given in chapter 3 of Flora et al. (1983) are the years of the first and last election under a given set of franchise rules, not the year in which the legislation was adopted.

### 3 Data Appendix B

This appendix details the coding of revolutionary events in Europe 1820-1938. The coding is based on Tilly (1993, 2004) and is crosschecked with Todd (1998) and Hobsbawm (1962) and supplemented with information from Encyclopaedia Britannica (1911, 2009). 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.

Tilly defines revolutionary events as “those instances when for a month or more at least two blocks of people backed by armed forces and receiving support from a substantial part of the general population exercised control over important segments of state organization” (Tilly, 2004, p. 73). This definition is intended to capture major instances of regime contention and includes examples of an array of popular rebellions, civil wars, and military and other types of coups d’état. Other events, such as the riots that took place at the time when the British Parliament deliberated the Great Reform Act in 1831, are too insignificant to be counted as a ‘revolutionary event’ according to Tilly’s definition. This does not mean that they were not important locally, but it does mean that we assume that they were unlikely to have made much of an impression abroad. Conceptually, we want to focus on (revolutionary) events that are concentrated over a short window of time, where the aim is fundamental social and political change, and where the revolt has some measure of popular support. For this reason, we have divided Tilly’s catalogue of revolutionary events into two broad groups.

The first group is the revolutionary events upon which our measures of the threat of revolution are based. This includes events characterized as revolutions, revolts, insurrections and rebellions, and risings. We further divide these events into major and minor events. The minor events are those which we, based on the discussion in Tilly (1993, 2004), and the coverage given to them in Encyclopaedia Britannica (1911, 2009) and by Hobsbawm (1962), judge to be less likely to have made an impact abroad while the major ones clearly did. We list these events in Tables *B1* (major) and *B2* (minor). In the estimations, we make use of both lists.

The second group includes events which we exclude from our analysis because

we judge them to be too far away from the theoretical concept of revolution. This includes events which are characterized as independence wars, military coups or coups d'état, mutiny, civil war, foreign invasion, general strikes, and prolonged periods of (low-level) insurrections. These events are, for completeness, listed in Table B3.

[Appendix Tables B1 to B3]

## 4 Data Appendix C

Descriptive statistics of the variables used in the estimations are reported in Table C1, and their precise definition and sources are as follows:<sup>2</sup>

1. *Suffrage* is the electorate in percentage of the enfranchised age and sex group; before the women's suffrage, male population only (parliamentary elections). 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. Sources: Flora et al. (1983), Caramani (2000), Cook and Paxton (1998), and Aidt and Jensen (2009).
2.  $TR_{it}^k$  is the measure of the threat of revolution. For  $k = u$  it is a simple count of the revolutionary events in a given year; for  $k = g$ , the events are weighted by the (inverse) geographic distance; for  $k = l$ , the events are weighted by

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<sup>2</sup>For further notes on the construction of many of the control variables, see Aidt and Jensen (2009).

linguistic distance,<sup>3</sup> in all cases excluding events in each country itself. The main specification includes major events (listed in Table B1) only. For robustness, we also calculate the measures using all events including those minor ones listed in Table B2. Sources: Tilly (1993, 2004), Todd (1998), and Encyclopaedia Britannica (1911, 2009). The source used to construct linguistic distance is Fearon (2003).

3. *Own revolution* is a dummy variable equal to one in country  $i$  in year  $t$  if that country experienced a major revolutionary event in that year.
4. *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).
5. *Population* is the size of the total population in 1000s. Source: Maddison (2003).
6. *Agricultural share* is the number of individuals employed in agriculture, mining, and fishing per 1000 employees. Source: Mitchell (2007).
7. *Urbanization rate* is the proportion of the population who lives in towns with more than 20,000 inhabitants. Source: Banks (2003).
8. *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).
9. *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](http://eh.net/encyclopedia)).
10. *Trade volume* is exports plus imports relative to GDP. Sources: Mitchell (2007), Statistics Netherlands (1999), Buyst (1997), Krantz and Schön (2007),

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<sup>3</sup>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. We are missing information on some of the countries in the 'broader European sample' and so we cannot construct the linguistic distance weights for that sample.

Grytten (2004), Flandreau and Zumer (2004), and the Swiss Economic and Social History online database ([www.fsw.uzh.ch/histstat](http://www.fsw.uzh.ch/histstat)).

11. *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).
12.  $SRN_{it}$  is defined as a linguistic distance weighted average of franchise reforms in other countries than country  $i$  in year  $t$ . Sources: Fearon (2003) and the sources used to define years of franchise reform recorded in Table 1.
13. *War* is a dummy variable equal to 1 if a country is at war and 0 otherwise. We do not include colonial wars. The available data on the size of armies have insufficient time and country coverage to be of use in our setting. Sources: Encyclopaedia Britannica (1911, 2009) and Singer and Small (1994).
14. *WWI* is a dummy equal to 1 during World War I (1914-18) for all countries and 0 otherwise.
15. *War intensity* is the number of deaths on the battle field per capita. Source: Singer and Small (1994) or <http://www.correlatesofwar.org/>.
16. *Repression* is the share of total central government spending on police, defence, general administration, and the judiciary. Source: Flora et al. (1983) and Fearon (2003).
17. *Fiscal 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) and Fearon (2003).
18. *Gini* is the Gini coefficient for income inequality. A value of zero expresses total equality and a value of one maximal inequality. Data are available only with 20-year intervals. We have interpolated the missing observations linearly. Source: Bourguignon and Morrisson (2001, 2002).

19.  $AL_{it}^{Home}$  is the number of ‘authors of liberty’ aged 20 and above who are born and living in country  $i$  in year  $t$  per 1000 inhabitants. Source: Potrafke and Vaubel (2011).
20.  $AL_{it}^{Abroad}$  is the number of ‘authors of liberty’ aged 20 and above who were born and grew up in country  $i$ , but at some time in adulthood emigrated to another country and in year  $t$  lived outside their home country per 1000 inhabitants. Source: Potrafke and Vaubel (2011).
21. *Rainfall* is yearly rainfall in millimeters. The rain data is constructed for a grit by Casty et al. (2007). We have constructed the country-year data by calculating the average of rainfall in the grit cells that falls inside a country, with appropriate weighting if there is only partial overlap. The data can be downloaded from <http://www.ncdc.noaa.gov/paleo/pubs/casty2007/casty2007.html>
22. *Rainfall, growth* is the change in the logarithm of rainfall.

[Table C1]

## 5 Appendix D: Robustness Checks

1. Table D1 reports on the results of some robustness checks for the event history study where we have added additional variables.

<Table D1 to appear here>

2. As an alternative check on the importance of enlightenment shocks, we can investigate the possibility that the enlightenment shocks left in the residuals are correlated across time. To this end, we cluster the standard error by year. The result is:<sup>4</sup>

$$suffrage_{it} = \underset{(0.013)}{0.94} suffrage_{it-1} + \dots + \underset{(0.41)}{1.19} TR_{it}^g + .. \quad (3)$$

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<sup>4</sup>The control variables are the same as in Table 2 column five of the main text. The standard errors are shown in brackets under the coefficient estimates and are clustered by year. Similar results are obtained with the other two proxies for the threat of revolution [not reported].

We observe this has little impact on the significance of the effect. If we cluster at the two-years frequency, the point estimate is unaffected (1.19) with a standard error of 0.54.

3. *Suffrage* as well as several of the control variables are trending up and may be or behave as if they were non-stationary.<sup>5</sup> To confront this issue, we estimate an Error Correction Model

$$\begin{aligned} \Delta \text{suffrage}_{it} = & \lambda_1 \Delta TR_{it}^g + \Delta X_{it} \boldsymbol{\kappa} \\ & + \rho \left( \text{suffrage}_{it-1} - \gamma_1 TR_{it-1}^g - X_{it-1} \boldsymbol{\omega} \right) + \varepsilon_{it}. \end{aligned} \quad (4)$$

The term in parentheses is the long-run relation appropriately adjusted to match our other estimations, and the parameter  $\rho$  captures the adjustment to the long-run equilibrium. The estimated equation is<sup>6</sup>

$$\Delta \text{suffrage}_{it} = \underset{(0.19)}{1.12} \Delta TR_{it}^g + \dots - \underset{(0.0096)}{0.039} \left( \text{suffrage}_{it-1} - \underset{(10.81)}{44.9} TR_{it-1}^g - \dots \right). \quad (5)$$

The estimates imply a significant positive short-run effect of changes in the threat of revolution on changes in *suffrage* and a substantial long-run effect. The negative estimate of  $\rho$  implies adjustment to long-run equilibrium. All in all, our results are not an artifact of non-stationary data.

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<sup>5</sup>Dickey-Fuller tests on the individual series show that we cannot, in several cases, reject non-stationarity [not reported].

<sup>6</sup>The figures in brackets underneath the coefficients are panel and AR(1) corrected standard errors, as suggested by Beck (2001). The time varying control variables are the same as in Table 2.

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## The Western European sample

Table A1. The United Kingdom (in sample from 1820)

Year	Franchise extensions/contractions	Other features
Pre-1832	Restricted and unequal manhood suffrage with relatively high, but locally different, economic requirement.	Electoral corruption widespread.
<b>1832</b>	The Great Reform Act. Reduction and standardization of income and property qualifications. The process started with Lord Grey's government formed in November 1830.	Redistribution of seats.
<b>1867</b>	The Second Reform Act. Reduction of income and property requirements.	Redistribution of seats.
1872		The Secret Ballot.
<b>1884</b>	The Third Reform Act. Reduction and standardization of economic qualifications for the county and borough constituencies.	Redistribution of seats and single member districts as a rule.
<b>1918</b>	The Fourth Reform Act. Universal and almost equal suffrage for male citizens over 21. The process had started in 1912 with Asquith's Liberal government introducing the Franchise and Distribution Act. This proposed to increase the number of male voters, but did not include rights for women. The Speaker's Conference of 1916 recommended universal male suffrage, votes for women and some proportional representation.	This includes the right to vote for married women above 30 years and over who are householders or wives of householders. Redistribution of seats.
1928		Women's suffrage.

Table A2: Austria (in sample from 1867; regress to autocracy in 1934)

Year	Franchise extensions/contractions	Other features
1815-1866		Member of the German Confederation (which the Austrian Empire established after the defeat of Napoleon).
1866		War with Prussia in 1866 dissolves the Confederation and Austria remains outside the German Empire established amongst the other German states in 1871 under the leadership of Prussia.
1867		The Austro-Hungarian Compromise of 1867, under which the House of Habsburg agreed to share power with a separate Hungarian government, dividing the territory of the former Austrian Empire between them. The Austrian and the Hungarian lands became independent entities enjoying equal status and we treat the two as independent units in the extended sample from 1867.
<b>1867</b>	The Constitution of 1867 introduced restricted and unequal manhood suffrage in four electoral classes (curia): <ul style="list-style-type: none"> <li>a) Great landowners who paid a minimum on estates.</li> <li>b) Members of chambers of commerce and trade.</li> <li>c) 24 year old male inhabitants of towns and cities who paid minimum direct taxes.</li> <li>d) 24 year old male inhabitants of rural communes who paid minimum direct taxes.</li> </ul>	Bicameral imperial parliament with an upper house and a house of representatives, the latter elected indirectly by the Landtags, but with the provision that a direct election could be held if a Landtag (provincial assembly) did not send representatives.
1873	Unchanged	All elections for the lower chamber were direct.
<b>1896</b>	Additional fifth curia introduced in which there was universal and equal suffrage for male citizens over 24; electors of the first four curia gained a second vote.	
<b>1907</b>	Universal and equal suffrage for male citizens over 24. The curial system abolished.	Street demonstrations. Direct elections. Secret ballot, compulsory voting in four provinces. Majority representation, single-member constituencies.
1918		Declaration of German Austria as a democratic republic.
1919	Universal and equal suffrage for men and women over the age of 20.	Proportional representation. Women's suffrage. Secret ballot. Compulsory voting in two provinces.
1920	Unchanged	Federal structure, with Nationalrat (lower

		house) which was directly elected by universal suffrage and an upper house (Bundesrat) which represented the Lander (states).
1929	Unchanged	Direct election of the president by the enfranchised population instead of by parliament, but no presidential election held till 1951.
1934		Nazi party takes over.

Table A3: Italy (in the sample from 1861, regress to autocracy in 1924<sup>1</sup>).

Year	Franchise extensions/contractions	Other features
1820-1860	The individual Italian states had their own constitutions.	By 1860, there were four Italian states (the Austrians in Venetia, the Papal States, the Kingdom of Piedmont-Sardinia, and the Kingdom of the Two Sicilies). On March 17, 1861, the Parliament proclaimed Victor Emmanuel II King of Italy and the Kingdom of Italy was established. Venetia and the Papal States (Rome) were not integrated till 1866 and 1870, respectively.
<b>1861</b>	Equal but restricted manhood suffrage; limited to citizens of 25 years and over who paid minimum direct taxes and who could read and write.	Direct elections with secret ballot, Majority representation in single member districts.
<b>1882</b>	Equal but restricted manhood suffrage; limited to citizens of 21 years and over; same restrictions as in 1861 except that tax minima and equivalent wealth requirement were reduced and suffrage granted to higher officials and other citizens with certain educational qualifications without further census restrictions.	Direct elections with secret ballot. Majority rule. Majority representation in multi member districts.
1894		Revision of electoral registers resulted in a considerable reduction in the size of the electorate.
<b>1912</b>	Almost universal and equal suffrage for male citizens of 30 years and over and in addition for male citizens of 21 years and over, who had completed their military service, or had finished primary school, paid minimum tax, and exercised official functions.	Direct elections with secret ballot. Majority rule. Majority representation in multi member districts.
<b>1919</b>	Universal and equal manhood suffrage for citizens of 21 years and over, in addition	Direct elections with secret ballot. Proportional representation.

<sup>1</sup> We let Italy exit in 1924. In 1923, the Acerbo law had been passed, which effectively allowed Mussolini to be a sure winner of the 1924 election, and thus after 1923, Italy was effectively non-democratic.

	suffrage for all men who participated in the war without age restrictions.	
1924		Mussolini.
(1946)	Universal and equal suffrage for all citizens of 21 years and over.	Women's suffrage.

Table A4: Norway (in the sample from 1820).

Year	Franchise extensions/contractions	Other features
(1814)	Equal but restricted manhood suffrage with relatively high occupational and property requirements.	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. Indirect elections with open voting; majority representation.
<b>1884</b>	Extension of the franchise to citizens paying taxes on income above given minima.	Secret ballot.
<b>1897</b>	Almost universal and equal suffrage for male citizens of 25 years and overs. Suffrage suspended in cases of bankruptcy and for paupers receiving public assistance.	Indirect, secret ballot. Majority representation.
1905		Union with Sweden dissolved.
1905		Direct elections.
1907	Extension of suffrage to women if own or husband's income exceeded minima.	(This has caused an artificial drop in <i>suffrage</i> as recorded by Flora et al. (1983) which we have corrected.)
1913	Almost universal and equal suffrage for men and women of 25 years and over. Suspended only for paupers.	Women's suffrage.
<b>1919</b>	Lowering of the voting age from 25 to 23. Paupers allowed to vote.	Proportional representation.

Table A5: The Netherlands (in the sample from 1830).

Year	Franchise extensions/contractions	Other features
(1815)	The members of the lower chamber were elected by the 18 provincial councils which consisted of different Estates.	The union with Belgium established in 1814 was codified in the constitution adopted in 1815. This constitution established a two-chamber system. The upper chamber was nominated by the Crown. The lower chamber was indirectly elected and initially intended to have equal representation from the Netherlands and from Belgium.
1830	Unchanged	The Belgian Revolution; the union with Belgium dissolved.
1840	Unchanged	After the independence of Belgium in 1830, a revision of the constitution was adopted in 1840, but both the 1815 and 1840 constitutions were based on the same franchise rules and indirect elections. As a consequence, the constitution of 1840 does not represent a franchise extension, just an adjustment of the existing rules to accommodate the fact that Belgium no longer was part of the Union.
<b>1848</b>	Equal but restricted manhood suffrage for citizens of 23 years and over. Vote contingent on surpassing relatively high direct tax minima.	Direct elections with secret ballot. Majority representation. 1848-68: the powers of the elected parliament were de facto in question till 1868.
<b>1887</b>	The right to vote was made dependent on education and property. The franchise was expanded, to about 29% of adult men. The direct tax minima and equivalent requirement of renting or owing a house above a certain rental value reduced.	
<b>1894</b>	Equal but restricted suffrage for male citizens of 25 years and over, with relatively low qualifications.	
<b>1917</b>	Universal male suffrage with voting age of 25. Pacification Act of 1917. Autumn.	Direct elections. Proportional representation. Compulsory voting. In 1917, like in 1848 influenced by the tense international situation, manhood suffrage was introduced combined with a system of proportional representation to elect the House of Representatives, the States-Provincial and the municipality councils. By the revision of 1922 universal suffrage was explicitly adopted in the constitution, after it had already

		been introduced by law in 1919
1922	Universal suffrage with voting age of 25.	Women's suffrage.

Table A6: Sweden (in sample from 1820)

Year	Franchise extensions/contractions	Other features
(1809)	Four estate system with some form of election to the Estate of Farmers and the Estate of Burghers.	
<b>1866</b>	Equal but restricted manhood suffrage with relatively high economic qualifications. Alternatively, property above a certain value; leasing of farmland above a certain value; income above given minima. Voting age 21.	Partly direct, partly indirect elections. Secret ballot. Majority representation, mostly in single-member constituencies.
<b>1907</b>	Almost universal and equal suffrage for male citizens of 24 years and over, excluding recipients of public poor relief.	Direct elections; secret ballot; proportional representation.
<b>1919</b>	Universal and equal suffrage for men and women of 23 years and over. Recipients of public poor relief enfranchised.	Women's suffrage. Adopted in special session of the parliament in December 1918.

Table A7: France (in sample from 1820).

Year	Franchise extensions/contractions	Other features
(1815)	Almost universal and equal manhood suffrage (excluding dependents) for primary elections. Restricted and unequal manhood suffrage in the electoral colleges because of strict criteria of eligibility.	Indirect elections. Primary assemblies elect lifetime members of electoral college. Electoral college elect member of parliament. Majority representation at both stages.
<b>1820</b>	Restricted and unequal suffrage for male citizens of 30 years and over. General electorate restricted by high direct tax minima; the upper 25% of the general electorate paying higher direct taxes constituted an additional electoral body. Under Richelieu, the franchise was changed to give the wealthiest electors a double vote, in time for the November 1820 election.	Direct elections: the general electorate elects 60% of member of parliament; the remaining 40% are elected by the assemblies of higher taxpayers. No strict provision for secrecy. Majority representation: absolute majority system in single member constituencies with third ballot run-off between top two candidates. Charles X dissolved the Chamber in 1827 and 1830, so the de facto power of the Chamber was curtailed.
<b>1830</b>	The Constitution of 1830 and the electoral law of 1831 introduce restricted but almost equal manhood suffrage for citizens of 25 years and over; reduced direct tax minima. The property qualification was reduced to include everyone who paid a direct tax of 200 (formerly 300) francs.	Direct elections with secret ballot. Majority representation: absolute majority system as before.
<b>1848</b>	Universal and equal suffrage for all male	



	citizens of 21 years and over. (The Second Republic).	
1852-1869	The de jure franchise rules were not changed.	During the Second Empire from 1852 to 1869, elections were systematically manipulated by the government to secure the return of compliant body of members.
1870	Reintroduction of the suffrage rules from the Second Republic. These had formally been in operation during the Second Empire, so the franchise rules were not changed de jure. We count this as a reform year in the specifications where we take franchise contractions into account, but not in those where we don't.	The Third Republic: elections were able to function freely and fairly.
(1945)	Universal and equal suffrage for all men and women of 21 years and over.	Women's suffrage. Direct elections with secret ballot. Proportional representation in multi-member constituencies till 1951.

Table A8: Germany (in sample from 1871, regresses to autocracy in 1933-34<sup>2</sup>).

Year	Franchise extensions/contractions	Other features
1815-1867	No directly elected assembly at the confederal level. Each of the 41 member states had their own suffrage rules and there were direct elections to state assemblies in some states.	The (Second) Germany Confederation. It was established at the Vienna Congress and reduced the number of independent German polities to 41. The two dominant powers were Prussia and Austria. The confederation was governed by a diet, a council of state, and a prime minister. Each state selected a representative to the diet (where a weighted voting system was used to make decisions). The states of the confederation retained most powers and could decide on their own constitutional rules, but the confederation required the states to adopt written constitutions. The federation could not raise taxes and was charged with the task of securing internal and external security.
1848-1849	Elected parliament proposed but not adopted. Many states liberalized their suffrage rules.	An attempt to reform the German confederal government was made in 1848-49 at the constitutional convention in Frankfurt. The members of the convention were elected on a broad suffrage that included wealth or tax-payment thresholds similar to those used by many state elections for their lower chambers. The convention could propose but not adopt reform. It proposed a constitutional monarchy for Germany with an elected parliament

<sup>2</sup> We code Germany as democratic in 1933 since elections were held in that year. Accordingly, Germany exits in 1934 when it effectively became a dictatorship.

		and ministers responsible to parliament and a new German crown (offered to the King of Prussia). The King of Prussia refused and the attempt at reform stopped there.
1866		War between Austria and Prussia breaks the confederation up.
1867	Similar to those applicable during the German Empire.	Northern German Confederation established by Prussia.
<b>1871</b>	<p>Universal and equal suffrage for all male citizens of 25 years and over in elections to the Reichstag.</p> <p>Each of the 25 member states had their own constitutions and rules for elections to their parliaments.</p>	<p>Imperial Germany established when the southern states join the Northern German Confederation. Austria did not join and is treated as independent from 1867. Electoral corruption widespread. Direct elections, majority rule. The government was not constitutionally responsible to parliament. Under the constitution of 1871, executive power lay with the Bundesrat. The Bundesrat consisted of representatives of the member states according to a system of population weighting. This gave Prussia dominant influence. The appointment of the German Chancellor was retained by the King of Germany (and Prussia). The main duty of the central government was initially defense and the chancellor had control over the military and over foreign policy. This restricted the de facto influence of the electorate on federal policy and since the state constitutions often used wealth restrictions, voters also had limited influence on state policy. In practice, great powers were granted to the emperor, as the president of the federal council. The Reichstag was elected by universal and direct election with a secret ballot.</p>
<b>1919</b>	<p>Universal and equal suffrage for men and women of 20 years and over.</p> <p>All states (Länder) adopt this suffrage.</p>	<p>The Weimar Republic. Direct and secret elections. Proportional representation. Women's suffrage. The constitution of the German Empire adopted in 1871 required that the Reichstag (the lower chamber) was elected by universal and direct election with a secret ballot. The Duchies, Kingdoms, and Free Cities, however, had their own constitutions and these typically prescribed that their lower chambers were elected on a wealth-based suffrage. The most important example of the three class system is the one operating in Prussia from 1848 till 1919. The Weimar Constitution required that the delegates [of the Reichstag] are elected by universal, equal, direct, and secret suffrage by men and women over twenty years of age, according to the principle of proportional representation and as pointed out by Congleton (2011, p. 479), the "final step to parliamentary democracy in Germany required a substantial increase in parliament's authority over</p>

		public policy, rather than suffrage expansion”. However, the Weimar Constitution (Article 17) required that every state must have a republican constitution and that all representatives must be elected according to the same rules as for the Reichstag. This implies that the Weimar Constitution extended the franchise in the states and as such can be viewed as a de jure franchise extension. This coding choice does not affect any of our results.
1933		Though It did not obtain a majority in the Reichstag in March 1933, the Nazi government was able to pass the Enabling Act which effectively ended democracy. The Weimar Constitution of 1919 was never officially repealed, but the legal measures taken by the Nazi government in March 1933 meant that the constitution became irrelevant.

Table A9: Finland (in the sample from 1820).

Year	Franchise extensions/contractions	Other features
1820		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. We let Finland enter the sample in 1820, but none of the results depends on this choice.
<b>1869</b>	The Diet Act of 1869 introduced a restricted and unequal franchise to the Diet based on 4 estates: nobility, clergy, town, and peasants. In the estate of burgesses, plural voting according to local taxes paid; in the estate of peasants, the suffrage was restricted to owners of real estate or leaseholders.	Direct elections to the estate of burgesses, indirect elections to the estate of peasants. No provisions for secrecy.
<b>1906</b>	Universal and equal suffrage for all men and women over 24 years. The process that led to the November Manifesto and the parliament of Finland started with a general strike of 1905 (12–19 November). During the general strike, the Red Declaration, written by Finnish politician and journalist Yrjö Mäkelin, was given in Tampere, demanding dissolution of the Senate of Finland, universal suffrage, political freedoms, and abolition of censorship. Leader	The Diet was replaced by the Parliament of Finland. Direct elections with secret ballot. Proportional representation in multi-member constituencies. Women’s suffrage.

	of the constitutionalists, Leo Mechelin crafted the November Manifesto that led to the abolition of the Diet of Finland and of the four Estates, and to the creation of the modern Parliament of Finland.	
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Table A10: Belgium (in the sample from 1830).

Year	Franchise extensions/contractions	Other features
1830		Independence from the Netherlands.
<b>1831</b>	Equal but restricted manhood suffrage with high direct tax minima differing in urban and rural areas. Voting age 25.	Leopold I enthroned as king on 21st July 1831. Constitutional monarchy. Direct elections but no provision for secrecy. Majority representation in multi-member constituencies; absolute majority required on first ballot, relative majority on second ballot. On 20 December, 1830 at the London Conference of 1830 Belgium's independence recognized.
<b>1848</b>	Reduction and standardization of direct tax minima.	
1871	Voting age reduced to 21	
1877		Secret ballot.
<b>1893</b>	Universal but unequal manhood suffrage. Plural voting with one additional vote for house owners and owners of real estate above a certain minima; two additional votes for citizens with higher education diploma and certain officials; maximum votes per person is 3 and minimum voting age is 21.	Direct elections with secret ballot. Compulsory voting; majority representation as before.
1899		Proportional representation
<b>1919</b>	Universal and equal manhood suffrage for men over 21. Plural voting abolished. Suffrage for mothers and widows of soldiers who had died in the war.	Direct elections with secret ballot. Compulsory voting. Proportional representation.
(1948)	Universal and equal suffrage for all men and women over 21.	Women's suffrage.

Table A11: Switzerland (in sample from 1848)

Year	Franchise extensions/contractions	Other features
1830	The cantons had their own constitutional arrangements.	“The July Revolution of 1830” in France launched a liberal movement in Europe which in Switzerland had the aim of transforming the union of separate and independent cantons into a united federation and converting the central council into an assembly elected by direct, universal and equal suffrage.” (Carstairs, 1980, p. 135). The 1832 constitution draft did not gain unanimity, and it was not until after the civil war in the mid-1840s that a revised constitution was adopted in May 1848 and the Swiss Confederation was established.
<b>1848</b>	Universal and equal suffrage for male citizens of 20 years and over. This franchise applied to the national council. The rules for election to the states council was within the jurisdiction of the individual cantons and thus varied.	The Swiss Confederation. Direct elections, secret or oral voting according to canton legislation. Majority representation: constituencies varied in size (each voter having as many votes as seats to be filled); absolute majority required on first and second ballot, simple majority on third. The Constitution of 1848 established a federal assembly, which included a popularly elected national council and a council of the states in which the cantons were each equally represented. A federal council (the national executive) consisting of 7 members was directly elected by the federal assembly. The Swiss constitution of 1848 was the outcome of civil war (3–29 November 1847). The first attempt at a constitution, which split the district of Schwyz in two and moved the cantonal capital away from Schwyz, was narrowly defeated on 27 January 1848. The second constitution, which removed the mentioned points and merged the former districts of Wollerau and Pfäffikon in the district of March, was then approved by the electorate on 27 February 1848. The Swiss Constitution was established with the promulgation of the Constitution of 12 September 1848.
1850		The federal law of 1850 laid down the detailed rules governing elections to the national council.
1864		Serious riots in Geneva in protest against misrepresentation. The canton fell into a

		state of anarchy, from which it was rescued only by the intervention of the other members of the Swiss Confederation.
1872	Unchanged	Secret ballot according to federal legislation.
1919	Unchanged	Proportional representation.
(1971)	Universal and equal suffrage for men and women of 20 years and over.	Women's suffrage.

Table A12: Denmark (in the sample from 1820).

Year	Franchise extensions/contractions	Other features
1820		Absolute monarchy.
1831	Franchise based on property ownership. Allowed the nobility, the property owners in the cities and major farmers to vote.	Advisory regional councils (Staenderforsamlingerne) re-established by the King. First meetings in 1835.
<b>1849</b>	Equal but restricted suffrage for male citizens of 30 years and over, except servants and farm laborers not having their own household and those receiving or having received public poor relief.	Bi-cameral system. Direct elections; voting by ballot or by show of hands. Majority representation: simple plurality system in single-member constituencies. The announcement of constitutional reform was made from the balcony of the King's palace on March 18, 1848.
1866		A constitutional revision in which the major landowners and the wealthier middle classes succeeded in introducing changes which conferred electoral privileges upon themselves, and ensured that there would be a Conservative majority in the Landsting [the Upper Chamber]. The franchise rules for the Lower Chamber were unchanged.
1875-1901		The Liberals gained a majority, and between 1875 and 1901 the Conservative government ruled by means of provisional legislation without the authority of the parliament.
1901	Unchanged	From 1901, a system that made the government responsible to the government was introduced along with the secret ballot.
<b>1915</b>	Universal and equal suffrage for all men and women of 29 years and over. The election required for the change in the constitution took place on May 7.	Direct elections with secret ballot. Proportional representation. Women had obtained the right to vote for local elections in 1909.
1920	Voting age reduced to 25.	

## The broader European sample

Table A13: Hungary (in the sample from 1867).

Year	Franchise extensions/contractions	Other features
1820	A Hungarian Diet	Part of the Habsburg Empire and under Austrian control within the German Confederation.
1848		Hungarian Revolution. After the unsuccessful revolution, Emperor Franz Joseph again assumed absolute control and divided the non-German part of the Austrian Empire into four distinct territories: Hungary, Transylvania, Croatia-Slavonia, and Vojvodina.
<b>1867</b>	See under Austria.	Autonomy within the Austrian-Hungarian Empire. Fiscal and foreign policy jointly determined.
1918-20		Involved in wars to establish borders.
1920	New franchise rules more restricted than under the 1867 constitution.	New constitution.
1921-31		Governed by a conservative leader.
1929		Social unrest.
1932-36		Fascist parties gained power and made close alliances with Germany.
1936		The Prime Minister Gyula Gombos promises a “model Nazi state” within two years, but it was not achieved before 1938. We keep Hungary in the sample till 1938 but since one could argue that it regressed to autocracy in 1936, we have checked that none of the results depends on this choice.

Table A14: Russia (in sample from 1820, regressed into civil war 1917-18<sup>3</sup>).

Year	Franchise extensions/contractions	Other features
1814		The control of the Kingdom of Poland confirmed at the Congress of Wien.
1820	The Governing Senate was a legislative, judicial, and executive body of Russian Monarchs and lasted until the end of the Russian Empire (in 1917).	Russian Empire.
1825		Rebellion.
1830		Zar Nikolai frightened by the French revolution. This leads to enhanced control and repression.
1848		Zar Nikolai frightened by the French and German revolutions. This leads to

<sup>3</sup> The civil war is dated as starting in November 1917, and we let Russia exit the sample in 1918.

		enhanced control and repression.
1853-56		Kremlin War with Turkey, the UK and France.
1861		Freedom for the peasants and other reforms, including the introduction of locally elected bodies to run social security etc.; operational in 1864 (country) and 1870 (town). Legal system reformed.
1864		Conquest of central Asia starts.
1874		Conscription army.
1881		New Zar rolls back some of the liberal reforms.
1904-05		Russian-Japanese war.
1905		First Russian Revolution. Peaceful demonstration ends with bloodbath, but demonstrations continue.
<b>1906</b>	All men of voting age, but indirect elections and multiple votes. On 18 February 1905 the Tzar offered to hold elections to a consultative assembly to calm the situation.	Restricted constitutional rule. Bicameral system with broad suffrage for the lower chamber. Veto retained with the Tzar.
<i>1907</i>	Restrictions on suffrage to insure a conservative Duma. Elections held between 1907 and 1917.	
<i>1917</i>		October revolution and subsequent civil war.

Table A15: Poland (in sample from 1918).

Year	Franchise extensions/contractions	Other features
1815		The Kingdom of Poland is established at the Congress of Vienna but the King is the Russian Emperor Alexander 1, so Poland is not an independent state.
1830		November uprising inspired by events in France and Belgium. Brutally suppressed by the Russians in 1831.
1846		Peasant revolt in part of Poland that is under Austrian control, the result is that Krakow becomes part of Austria. Repression used.
1848		Rebellion against Prussia and Austria under inspiration from revolutionary events elsewhere in Europe. Repression very bloody.
1863		Rebellion in the Kingdom of Poland, demanding freedom for peasants. Repression by Russia was the result in 1864.
1867		Galicja (the south-eastern part of Poland) obtains national independence, Polish culture can flourish in



		Krakow. Other parts under German control are suppressed.
1890s		Two political parties develop. Right-wing, anti-Semitic, and anti-German party; Left-wing socialist party.
1918		An independent Poland established at the peace conference in Paris, but must fight the neighbors to establish borders (lost to Czechoslovakia in 1919-20 but gained Vilnius in 1920).
<b>1921</b>	The May Constitution: A broad franchise.	Republic of Poland: Democratic constitution; bi-cameral system with weak president (“very democratic constitution”). 13 governments between 1919 and 1926.
1926	Franchise rules nominally unchanged.	Coup. August Novelization was a set of amendments to the 1921 (May) Constitution by which the power of the executive was enhanced. There were four main clauses in the amendments: (1) The President may dismiss the Sejm (parliament) and the Senate. (2) The President may issue acts having statutory power with the approval of the Sejm. (3) The Sejm may not dissolve itself. (4) If the Sejm cannot agree on the state budget, the budget may be passed by the government.
1935	Franchise rules nominally unchanged.	The April Constitution was the general law passed by the act of the Polish Sejm on 23 April 1935. It introduced a presidential system with certain elements of authoritarianism. The President had wide-ranging power to dismiss parliament and to veto legislation.

Table A16: Serbia (in sample from 1820).

Year	Franchise extensions/contractions	Other features
1815		National Assembly, the Skupstina, established. It consisted of 22 district heads, who were nominated by the Prince, and representatives of the town and parish counties, whose members—prominent merchants, priests, etc.—were nominated by the district heads. A purely consultative body that convened once a year.
1830	No elections held	Serbia became an internationally recognized autonomous principality under Turkish sovereignty and Russian protection.
1835	No elections held.	Demands for representative institutions and suppression of five rebellions. The Prince eventually agreed to let the Skupestina pass a constitution (Constitution of 1835) to limit his power, but this was abolished under

		pressure from England among others, and the Constitution of 1835 did not become operational.
1838 (operative in 1839)	No elections held.	Constitution of 1838. New constitution negotiated under Turkish influence. A new state council of 17 life members and a cabinet of four were duly instituted.
1839-58		Period of oligarchy.
1858-60		The power of the Skupstina undermined by Prince.
1860-68		Enlightened absolutism
<b>1868</b>	Elections with relatively wide suffrage.	Constitution of 1868. The constitution drew to some extent upon the early nineteenth-century German constitutional monarchies. The main features were: recognition of the hereditary rights of the Obranovic dynasty, the legislative power to be shared by the Prince and the Skupstina, the council remained a consultative body, the government only responsible to the Prince, principle of freedom of press, and judges to be independent.
1870	General elections in 1874 and 1875.	Election law adopted.
1875		Rebellion against the Turks in Hercegovina.
1876		War with Turkey. Peace in 1877 on the basis of the status quo.
1877		Russo-Turkish war; Serbia gets involved in 1878. Unrestricted independence declared. Territorial gains.
1881	Election won by Progressists and Radicals (the Liberal lost).	First time organized political parties compete for power.
1881		Secret Serbo-Austrian convention which prevented Serbia from entering political treaties with other countries without the consent of Austria.
1882		Kingdom of Serbia declared.
1883	Election with Radical victory, but a Progressists government appointed by the King.	
1885		Serbo-Bulgarian war. Status quo preserved.
1886	Election, one-seat majority to the Progressists.	
1888	Election resulting in Progressists majority.	

<b>1888</b>	In the elections to the Skupstina, apart from soldiers of the active army, all male citizens of full age may vote if they pay 15 dinars in direct taxes. The tax payment requirement is higher for elections (60 dinars).	Constitution of 1888. The constitution, proposed by the Radical Party in response to the egalitarian aspirations of the nation's agrarian majority, adopted a French constitutional model - with a unicameral system and frequent coalition governments. Shaped on the model of the Belgian Constitution of 1831, which in its turn was a modified version of the French Charter of 1830, it restored a French influence, expressed for the first time in the 1835 Constitution. It guaranteed civil and political rights; including the freedom of speech, abolished summary courts; gave the Skupstina the right to table bills, to pass the budget and introduced direct elections and the secret ballot.
<i>1893</i>	Election, rigging gives Liberal majority.	Coup d'etat when a minor declares himself of age. No change in the constitution as such.
<i>1894</i>	This represents a de jure franchise contraction.	Constitution of 1868 replaces that of 1888.
<i>1895</i>	Election returns Progressits majority.	
<i>1901</i>		Constitution of 1901 introduces a bi-cameral system in an attempt of upholding the influential role of the ruler, while limiting that of the Radical Party, which had enjoyed an ample electoral support since the 1888 Constitution.
<b>1903</b>	Elections in 1905, 1906, 1908, 1912. Restoration represents a de jure (and de facto) franchise extension relative to 1868 and 1901 constitution.	Restore the Constitution of 1888. Serbia becomes a parliamentary democracy, with a pre-dominant two party system.
<i>1912-13</i>		Balkan Wars.
<i>1918</i>		Serbia becomes part of Yugoslavia (at first the Kingdom of Serbs, Croats and Slovenes).
<b>1920</b>	Elections and political competition, but with a ban on the Communist party. Franchise relatively broad.	Election to a constitutional convention adopted centralist constitution.
<i>1931</i>	1931 constitution was accompanied by an electoral law which ensured a large government majority. Anti-democratic.	King Alexander's dictatorship (1929-34). Repression of, in particular, communists.
<i>1935</i>	Election held in conditions of greater freedom, but still with a system that allowed the government to hold on to the majority.	

(1939)	Election with Radical majority as a result.	
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Table A17: Rumania (in sample from 1856, regress to autocracy 1938)

Year	Franchise extensions/contractions	Other features
1856	Some limited elections via “Landsdag”.	Independence in part by the treaty of Paris (1856) and in full by the treaty of Berlin (1878).
<b>1866</b>	The franchise was governed by property, income and literacy requirements.	The constitution established a liberal monarchy with a responsible ministry and a parliament of two elective chambers, but with most of the power with the Prince.
1907		Peasant rising suppressed violently.
<b>1923</b>	Universal male suffrage.	Most of the power with the King.
<i>1938</i>		Military dictatorship.

Table A18: Greece (in sample from 1822)

Year	Franchise extensions/contractions	Other features
1821		War of independence, with independence declared in 1822.
<b>1822</b>	Universal but unequal male suffrage.	Indirect elections.
<b>1844</b>	Universal and equal male suffrage with voting age of 25.	Direct majority elections in multi-member constituencies. Multiple voting. Secret ballot whereby voters could write the name of the candidate in private. Illiterates helped by pooling officers.
1864	Voting age reduced to 21.	Lead ball system replaced ballots. Candidates elected by plurality system in one ballot in which a yes or no vote was expressed by putting the ball into the appropriate box. From 1923 paper ballots in some provinces. From 1926 paper ballots in all provinces
1926	Unchanged	Proportional representation introduced.
1928	Unchanged	Majority system introduced.
1929	Unchanged	Compulsory voting introduced.

Table A19: Iceland (in sample from 1874).

Year	Franchise extensions/contractions	Other features
1814-1873		In 1814, following the Napoleonic Wars, Iceland remained a Danish dependency.
1845		The modern parliament, Alþingi (English: Althing), was founded in 1845 as an advisory body to the Danish monarch.
<b>1874</b>	Right to vote restricted to men aged 25 and over in charge of own finances and property and not receiving poor relief. Census and capacity conditions: 1) farmers paying tax over a given minimum; 2) town burghers and fishermen paying communal tax over eight crowns; 3) owners of real estate paying a communal tax of 12 crowns on house property; 4) civil servants; and 5) graduates of university and divinity school.	Denmark granted Iceland a constitution and limited home rule. Direct elections of 30 MPs; majority system with multiple votes. 6 MPs appointed by the Danish King.
1903		Simple plurality system introduced.
1904		Home rule expanded.
<b>1908</b>	As before with two modifications: 1) tax for non-farm laborers lowered to 4 crowns; 2) graduates of medical schools granted voting rights.	Directly elected MP increased from 30 to 34. 6 appointed by the Danish King.
<b>1916</b>	Universal suffrage for men and women with age restrictions for women and servants; recipients of poor relief not included.	Direct elections and all 40 members now elected. Women's suffrage.
1918		The Danish-Icelandic Act of Union, an agreement with Denmark signed on 1 December 1918 and valid for 25 years, recognized Iceland as a fully sovereign state in a personal union with the King of Denmark.
1920	Age restrictions for women and servants abolished.	Mixed PR-plurality system introduced.
<b>1934</b>	Voting age lowered for men and women. Recipients of poor relief enfranchised.	
(1944)		The Republic of Iceland was established.

Table A20: Luxembourg (in sample from 1820).

Year	Franchise extensions/contractions	Other features
1815		Luxembourg was disputed between Prussia and the Netherlands. The Congress of Vienna established Luxembourg as a Grand Duchy in personal union with the Netherlands. Luxembourg also became a member of the German Confederation. Since the country exists as an independent state from 1815 to 1838, we let it enter the sample in 1820.
1839		The Belgian Revolution of 1830–1839 reduced Luxembourg's territory by more than half. The First Treaty of London reaffirmed Luxembourg's independence.
<b>1841</b>	Suffrage limited to men of 25 years and over paying a yearly tax of 10 guilders. The tax was 20 for the electors.	Indirect elections.
<b>1848</b>	Census (tax) reduced from 10 guilders to 10 francs.	Direct elections. The Constitution of Luxembourg was acutely amended on 20 March, 1848.
<b>1857</b>	Census of 10-125 francs for indirect elections in the cantons and over 125 for direct elections in the districts. But the same year, in which direct elections were abolished, the census was generally established at 10 francs.	Mixed indirect and direct elections.
<i>1860</i>	Census requirement raised to 30 francs.	Direct elections replace indirect elections.
1867		The Second Treaty of London affirms Luxembourg's independence and neutrality.
1868	Census requirement kept at a payment of 30 francs.	Direct and equal elections.
1879		Secret ballot.
<b>1893</b>	Census requirement reduced to 15 francs.	
<b>1902</b>	Census requirement reduced to 10 francs.	
<b>1919</b>	Universal and equal suffrage for both men and women. Voting age 21.	Proportional representation introduced. Direct elections.

Table A21: Portugal (in from 1820, regress to autocracy in 1926).

Year	Franchise extensions/contractions	Other features
1820	All free male citizens entitled to vote for the constitutional assembly.	Indirect elections.
<b>1822</b>	Franchise for all men aged 25 and over, except for males under guardianship, servants, and friars.	Direct elections. Secret ballot.
<i>1826</i>	Franchise for all men aged 25 and over with an income of 100 milreis a year, except for males under guardianship, servants and friars. Voting age of 21, and over for members of clergy, married men, military officers and persons with higher education.	Indirect elections. The Carta constitution. Two-chamber parliament. This charter formed the basis for election till 1910 (the
<b>1838-42</b>	Census requirement reduced to 80 milreis.	Direct elections.
<b>1852</b>	Census requirement abolished for teachers, graduates of universities, and the clergy.	Direct elections in multi-member constituencies.
<b>1878</b>	Franchise extended to all men aged 21 and over who were heads-of-households or who could read and write.	
<i>1895</i>	Income requirement for illiterates halved. Head-of-household qualification discontinued. This on the allowed more illiterates to vote, but the reduced caused by the discontinuation of the head-of-household franchise reduced the number of voters by more. So the net result was a contraction. Mackie and Rose (1991, Chapter 20) suggest that the contraction was from 70% to 47%.	
<b>1911</b>	Enfranchisement of all men of 21 years and over who could read and write plus heads-of-households (similar to the 1878 electoral law). Tax qualifications were abolished.	Mixed electoral system. The Monarchy was overthrown and a republic declared.
<i>1913</i>	The head-of-household qualification abolished thus contracting the size of the electorate.	
<i>1926</i>		Dictatorship.

Table A22: Spain (in sample from 1820, civil war from 1936)

Year	Franchise extensions/contractions	Other features
(1812)	Constitution of Cadiz (or of 1812): Universal manhood suffrage with voting age at 21; all financially-independent men enfranchised.	The constitution introduced the principles of universal male suffrage, national sovereignty, constitutional monarchy and freedom of the press, and supported land reform and free enterprise.
(1814)		Constitution abolished by Ferdinand VII.
<b>1820-1823</b>	Constitution of Cadiz reinstated.	Ferdinand VII's misrule provoked a revolt in favor of the Constitution of 1812 and he was held prisoner till 1823.

1823-1833	Absolutism. Ferdinand VII used his veto powers to prevent the liberal governments from functioning.	Ferdinand VII regained power after French invasion. Relentless restoration of reactionary absolutism.
1827		Reactionary revolt (known as "War of the Agraviados"). Suppressed.
1834-1836	Royal statute (Estatuto Real): census introduced on the basis of tax payment and voting age raised to 30 years. This reduced the suffrage relative to the Constitution of Cadiz but expanded it relative to the de facto franchise between 1823 and 1833.	Indirect elections in two steps. Bicameral parliament with elected lower house and appointed Senate.
1836	Restoration of 1812 constitution, while preparing the Constitution of 1837.	A coup by sergeants of the Spanish Royal Guard.
1837	Constitution of 1837: franchise for all men aged 25 and over who either paid annual tax of 200 reales, had yearly income of 1500 reales, paid 3000 reales of lease holding or rent, or were inhabitants of a house of a sufficient rental value. About five percent of the population had the right to vote. Broader franchise than under the Royal Charge, but narrower than under the Constitution of 1812.	Direct elections with a broad electorate choosing a lower house (the Chamber of Deputies), while the upper house (the Senate) was appointed by the monarch.
1840-43		Constitution of 1837 partially suspended by Regent Baldomero Espartero, who ruled by decree between 1840 and 1843.
1845	Constitution of 1845: census requirement increased to 400 reales, census of 200 reales only for some professional categories. Narrowed the franchise to less than one percent of the population, i.e., more restricted than the Constitution of 1837.	Direct elections (two ballots).
1854		Election law of 1837 used to elect a constitutional assembly.
1856		New constitution passed by the Parliament but not enacted by the Queen.
1865	Census halved to 200 reales (100 for certain professional categories).	Single-ballot with plurality rule.
1869	Constitution of 1869: universal male suffrage, voting age 25.	Glorious Revolution Constitution.
1876	Constitution of 1876: Universal suffrage repealed. Voting age 25. Franchise limited to persons paying a property tax of 25 pesetas, a yearly trade tax of 50 pesetas, or possessing a higher educational qualification.	Restoration Constitution. While theoretically democratic, elections were routinely rigged by the governing party, and in practice power was shared by two alternating parties (the "turno" system).



<b>1888</b>	Universal male suffrage reintroduced. Voting age 25.	
<b>1890</b>	Members of certain corporations formed special constituencies to ensure representation.	
1907	Voting age reduced to 23.	Compulsory voting.
1923		During Primo de Rivera's dictatorship (1923–1930) many of the Constitution of 1876's articles were suspended in a de facto dictatorship.
<b>1931</b>	Second Republic Constitution. Universal and equal male suffrage with voting age of 23.	Secret ballot.
1936-1939		Civil war. During the Civil War (1936–1939) the Constitution of 1831 was abolished by the Nationalists and widely disregarded in the Republican zone.
1938	Franco's dictatorship.	

**Table B1: Major Revolutionary Events, 1820-1938.**

<b>Events</b>	<b>Region</b>	<b>Year</b>
Revolution in Hungary (Moravia, Transylvania and Wallachia). Started in May 1848.	Eastern Europe	1848-49
The “aster flowers” revolution in Hungary, ending with foreign military intervention and counter revolution in 1919. The initial revolution was led by Károlyi with support from the army and supporters of the social democratic party wearing aster flowers. Started in November 1918.	Eastern Europe	1918-19
Generalized revolution from 1820 to 1823 in Spain (Liberal Revolution in Spain of January 1, 1820). It was started by Mutiny of Spanish troops under Colonel Rafael Riego. This was an example of the military pronunciamiento: Liberal colonels organised in their own secret officers’ brotherhoods ordered their regiments to follow them into insurrections. It was termination by a French invasion in 1823.	Iberia	1820-23
Revolution at Oporto, Portugal. This liberal revolution started January in Porto, quickly spreading without resistance to several other Portuguese cities and towns, culminating with the revolt of Lisbon. The revolutionaries demanded the immediate return of the royal court to continental Portugal, demanded a constitutional monarchy to be set up in Portugal and restoration of Portuguese exclusivity in the trade with Brazil.	Iberia	1820
Portuguese insurrection of General Pimenta de Castro. The republicans supported by the Navy and violent civil groups revolted. Started in May.	Iberia	1915
Belgian revolution against Holland. Started August 25.	Belgium	1830-33
Easter Rebellion in Ireland. The rising was suppressed after seven days of fighting, and its leaders were court-martialled and executed.	British Isles	1916
July Revolution	French states	1830
French Revolution, February	French states	1848
State collapse, occupation, republican revolutions.	French states	1870
Multiple communes. In Paris, resentment against the government arose and from April – May 1871 Paris workers and National Guards revolted and established the Paris Commune, which maintained a radical left-wing regime for two months until its bloody suppression by Thiers' government in May 1871.	French states	1871
Russian revolution (unsuccessful), January 9	Russian states	1905
Russian revolution (successful), February	Russian states	1917
Revolutions in Naples and Sardinia. The Carbonari organized anti-absolutist riots in Naples in July. This led to the 1820 revolution which forced King Ferdinand I of the Two Sicilies to promise a constitutional monarchy and the King of Sardinia to accept liberal reform. The revolutions were in the end repressed.	Italy	1820

Italian states. Insurrection in Palermo, Sicily, spreads to the mainland in January, Risings in the Habsburg provinces in early March.	Italy	1848-49
Habsburg. Street fighting in Vienna in March.	Austria	1848-49
German states. First event March 3.	Germany	1848-49

**Table B2: Minor Revolutionary Events.**

Events	Region	Year
Janissary rebellion in Constantinople	Balkans	1826
Pro-constitutional uprising in Greece	Balkans	1843
Revolt in Herzegovina, supported by Montenegro	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 Moldavia	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
Reactionary revolt (known as "War of the Agraviados" (malcontents)) in Catalonia and other regions of Spain.	Iberia	1827
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
Spanish revolution led by O'Donnell and Espartero	Iberia	1854-56
Failed insurrection of General Juan Prim	Iberia	1866
Generalized insurrection (Pronunciamento of Admiral Juan Topete)	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 (by 1930, Salazar in power).	Iberia	1927
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 B3: Other Revolutionary Events.**

<b>Events</b>	<b>Region</b>	<b>Year</b>
Independence war in Moldavia, Wallachia	Balkans	1821-24
Independence war in Crete	Balkans	1821-25
Independence war in Greece	Balkans	1821-31
Independence war in Albania	Balkans	1830-35
Independence war in Bosnia	Balkans	1831-36
Independence war in Moldavia	Balkans	1848
Independence wars in Montenegro	Balkans	1852-59
Military coup in Greece, king deposed	Balkans	1862
Independence war in Bosnia	Balkans	1862
Independence war in Serbia	Balkans	1862
Independence wars in Bosnia, Herzegovina, Thessaly during Russo-Turkish war	Balkans	1878
Independence war in Crete	Balkans	1878
Independence war in Crete, Greek and British intervention	Balkans	1896-98
Independence war in Macadonia	Balkans	1902-03
Independence war in Crete	Balkans	1905
Independence war in Romania	Balkans	1909
Independence war in Albania during the Balkan war	Balkans	1912
Overthrow of Stamboliski in Bulgaria	Balkans	1923
Portuguese civil war	Iberia	1823-24
British landing in Portugal, supporting constitutionalists	Iberia	1827
Portuguese coup d'etat by Dom Miguel, followed by Miguelite wars to 1834	Iberia	1828
Carlist war in Spain	Iberia	1833-39
Frequent insurrections in Portugal	Iberia	1834-53
Spanish coup on behalf of Queen Cristina, defeated	Iberia	1841
Coalition deposes Espartero; Narvaez president until 1851	Iberia	1843
Portuguese civil wars	Iberia	1846-50
Military coup, continuation of Carlist wars to 1876, then another coup on behalf of Alfonso, son of deposed Queen Isabella	Iberia	1874,1876
Sporadic revolts, strikes and conspiracies in Portugal	Iberia	1889-1908
Lisbon general strike	Iberia	1912
Uprising and seizure of power by General Sidonio Paes (assassinated 1918) in Portugal, defeated general strike	Iberia	1917
Mutiny of Barcelona garrison, outbreak of separatist movement, coup of Primo de Rivera	Iberia	1923
Attempted coup in Portugal	Iberia	1925
Successful coup in Portugal	Iberia	1926
Attempted coup in Catalonia	Iberia	1926
Mutiny of garrison at Jaca in Spain, demanding republic	Iberia	1930
Spanish elections with large majority for Republicans; Alfonso leaves, new constitution	Iberia	1931
Military revolt led by General Jose Sanjurjo (in Spain)	Iberia	1932
Spanish civil war	Iberia	1936-39
Civil war in Ireland, Irish independence	British	1919-23

	Isles	
Louis Napoleon's coup	France	1851
Kirghiz vs. Russia (violence that erupted over conscription of Muslims for service in World War I)	Russian states	1916
Russian civil war, broad international intervention	Russian states	1917-21
Civil war	Switzerland	1845-47

**Table C1: Summary Statistics for the Variables used in the Analysis.**

Variable	#Observations	Mean	Std. Dev.	Min	Max
Suffrage	1297	47.46	38.60	0.00	110.10
TR <sup>u</sup> (unweighted, major)	1403	0.24	0.73	0.00	5.00
TR <sup>g</sup> (geographical, major)	1403	0.35	1.28	0.00	15.11
TR <sup>l</sup> (linguistic, major)	1403	0.03	0.16	0.00	1.44
TR <sup>g</sup> (geographical, all)	1403	0.71	1.40	0.00	15.11
Suffrage reforms abroad	1448	0.02	0.06	0.00	0.61
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	0.0004	0.03	-0.18	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	0.008	0.08	0.00	1.00
Social learning	1403	0.02	0.06	0.00	0.61
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
Fiscal transfers	1007	9.46	10.96	0.00	50.40
AL <sup>Home</sup>	1192	0.44	0.55	0.00	3.05
AL <sup>Abroad</sup>	1192	0.10	0.21	0.00	1.73
Log Rainfall	809	4.2	0.34	3.32	5.18
Rainfall, growth	809	-0.0005	0.14	-0.45	0.44
Gini coefficient	1315	0.52	0.02	0.47	0.56

**Table D1: Results from the Event History Study I, Western European Sample.****Dependent variable: *reform*.**

	(1)	(2) <sup>c</sup>	(3)	(4)
TR <sup>g</sup>		0.50*** [7.76]	0.67*** [3.62]	0.51*** [7.79]
TR <sup>g</sup> (all)	0.46*** [5.73]			
Trend		1.49 [1.23]		
Cycle		-3.42 [0.49]		
Repression			-0.05** [-2.12]	
Fiscal transfers				0.04 [0.55]
Authors of Liberty, home				
Authors of Liberty, abroad				
Observations	713	699	394	628
Number of countries	11	11	9 <sup>b</sup>	9 <sup>b</sup>
Estimation technique	Logit	Logit	Logit	Logit

*Notes:* *z* statistics based on clustered (by country) standard errors in brackets; \* significant at 10%; \*\* significant at 5%; \*\*\* significant at 1%. Only ten countries are included in the event history study as Switzerland had full male suffrage from the time it became unified. All estimations allow for duration dependence of the hazard rate and include the same control variables as in Table 8 in the main text. a. Data from Finland are missing. b. Data from Austria and Germany are missing. c. Log *GDP per capita* is replaced by *cycle* and *trend*.