

# **Democratization under the threat of revolution: Evidence from the Great Reform Act of 1832\***

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Preliminary

Comments are welcome

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**Abstract.** This paper proposes a test of the “threat of revolution” hypothesis that links democratisation to the threat of political violence based on evidence from the Great Reform Act adopted by the British Parliament in 1832. It relies on a time-varying constituency-specific measure of the threat of revolution and uses a panel data strategy with fixed effects and instrumental variables. We find that political violence affected the outcome of the critical 1831 general election which was essential for the subsequent success of the reform process. However, the vote of the Members of Parliament on the bill was not influenced by political violence but by orderly social contention such as demonstrations, political meetings, and petitions.

*Key words:* Democratisation, franchise extension, threat of revolution, 1832 Great Reform Act.

*JEL classifications:* D72.

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“At a crucial moment in British politics, a larger part of the country was in rebellion, respectable men were refusing to serve as special constables, landowners were pressed to the wall. There is no politician born who would not ponder the implications of such a situation very carefully” (Hobsbawm and Rudé, 1973, p. 257).

“The principal of my reform is to prevent the necessity of revolution..... I am reforming to preserve, not to overthrow” (Lord Grey speaking in the House of Commons, March 22, 1831).

“The Reform Bill owed more the country labourers than to all the rest of the nation put together”. (William Cobbett in the *Political Register*, June 16 1832)

## 1. Introduction

It is widely acknowledged that institutions play a critical role for long-term economic growth (e.g., North and Thomas (1973), Hall and Jones (1999)). At critical junctures in history, societies establish persistent institutional structures that set the limits for subsequent economic and social development and put different societies on divergent economic and political paths (Acemoglu et al. 2001, Acemoglu and Robinson, 2006). Alongside economic and legal institutions, related to markets, property rights and contract enforcement, democratic governance plays a pivotal role. Democracy, as we understand it today, originated in Western Europe less than two centuries ago through a process which is still a matter of debate among economists, political scientists, and historians.

Broadly speaking, one can make a distinction between two economic theories of democratization.<sup>1</sup> “Rivalry” theories argue that democratization is achieved as a by-product of political rivalry among members of the ruling elites whose objectives ultimately pertain to the preservation of their own political and economic powers.<sup>2</sup> In contrast, “structural” theories of democratization<sup>3</sup> emphasize the distributive conflict between the enfranchised old elites and the disenfranchised classes. Democratization is thus an indirect fight over the distribution of resources where the rich and powerful old elite have strong incentives to block any attempt at

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<sup>1</sup> See the surveys by Robinson (2006), Ziblatt (2006), and Congleton (2011, chapter 1).

<sup>2</sup> One leading idea is that suffrage reform is part of an ongoing constitutional bargaining process where voting rights and control rights over policy making along many dimensions are exchanged on a voluntary basis (Congleton, 2007, 2011). Another view, which can be traced back to Himmelfarb (1966) and which constitutes one of Collier (1999)’s paths to democracy, is that factions within the old elite see an electoral advantage in expanding the electorate (Llavador and Oxoby, 2005) or anticipate a favorable shift in the composition of public spending under a broader suffrage (Lizzeri and Persico, 2004).

<sup>3</sup>See Acemoglu and Robinson (2000, 2006), Conley and Termini (2001), Boix (2003); Ellis and Fender (2011); Gradstein and Justman (1999).

overthrowing the status quo. It is only under the threatening shadow of political violence and radical social change that these elites will relinquish power and include broader segments of the population in the political calculus. Democracy is, to paraphrase Przeworski (2009), conquered rather than voluntarily granted<sup>4</sup> and the threat of revolution plays a causal part in bringing this outcome about.<sup>5</sup> The ruling elites form perceptions about the threat by observing events happening around them and then subsequently act upon those perceptions or updated beliefs in a predictable and observable fashion.<sup>6</sup> Although the term “threat of revolution” is a useful metaphor, it is important to emphasize that it should be interpreted broadly as “the existence of an alternative to the established government commanding substantial support in significant segments of the national population” Tilly (1995, p. 90). The threat can therefore come from segments of the middle class as well as from the poor and need not result in pre-emptive democratization if repression or other less costly strategies are sufficient to preserve the status quo.<sup>7</sup>

The purpose of this paper is to provide new evidence on the “threat of revolution” hypothesis and in that way to further our understanding of the economic and social origins of democracy. We propose to do this in the context of the Great Reform Act of 1832 because it constitutes a promising testing ground for three related reasons. First, the act was the first of several reforms on Great Britain's long journey to universal and equal suffrage. It is thus often viewed as “a major turning point in English history” (Maehl, 1967, p. 1) and it arguably triggered important economic reforms in the decades that followed (e.g., the reform of the Poor

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<sup>4</sup>The classical work by Moore (1966) and the more recent work by Rueschemeyer et al. (1992) also stress structural factors such as landholding inequality and the social control that this grant to landed interests but they stress how changes in the political bargaining power of key socio-economic groups can cause democratization rather than the threat of revolution as such. Ansell and Samuels (2010) take a contractarian approach and emphasize that democratization is not about redistribution from the elite but about different social groups seeking protection from the state. In this theory, landholding inequality and income inequality play fundamentally different roles.

<sup>5</sup> There are various ways of formalizing this basic idea: Acemoglu and Robinson (2000, 2006) emphasize the commitment value of institutional reform and the effects of economic factors (e.g., inequality) on the value of this commitment relative to alternative coping strategies. Boix (2003) and Conley and Termini (2001) focus on changes in structural and organizational parameters which make democracy cheap relative to autocracy or increase the cost of revolution more than the commitment value of democracy.

<sup>6</sup> Aidt and Jensen (2011) contains a simply extension of the baseline model of Acemoglu and Robinson (2000) that formalizes this logic. See also Ellis and Fender (2011) which develops a model of information cascades to explain how and why revolutions happen and how democratization may or may not be able to eliminate this risk.

<sup>7</sup> Scholars may attribute a different meaning to the term "threat of revolution" but it seems to us that Tilly (1995, p. 90)'s definition is the most sensible and straightforward. It notably makes it clear that the threat does not need to include elements of socialism, as was most likely the case with the Great Reform Act of 1832, which is the focus of this study.

Laws in 1834, the repeal of the Corn Laws in 1846, and the reform of municipal government in 1835). Second, the act was debated and voted on in the House of Commons and in the House of Lords whose election rules and constituencies had not been modified in nearly two centuries. Thus, we can relate the MPs' support or opposition to the reform as expressed through their roll-call votes as well as the results from the general election held in June-July 1831 to the threat of revolution. Third, Tilly (1995)'s seminal work on popular contention in Britain between 1828 and 1834 shows that the reform crisis years of 1830-1832 were associated with a spike in social unrest across the country, with the so-called Swing Riots, which were a rising of agricultural laborers against poor working and living conditions in 1830-1831, contributing much to this general picture of social contention. Although not all historians agree<sup>8</sup>, many perceive a connection between this spike in social unrest and the Great Reform Act. For example, Hobsbawm and Rudé (1973, p. 256) write about the Swing Riots of 1830-1831:<sup>9</sup>

“It would be surprising if a movement so widespread, and which frightened the government so much – for however brief a spell – had been without influence on the reform legislation of the first half of the 1830s. Contemporaries certainly thought there was a connection between Swing and Reform; Cobbett and Wakefield believed it to have done more to turn parliamentary reform in to practical politics than the urban agitations [...] It is in any case probable that, in so far as fear of revolution influenced the legislators, then this most widespread rising of the oppressed, acting so often in concert with the discontented urban Radicals, must have been in the minds of those who weighted the dangers of Reform against those of social upheaval.”

To test the “threat of revolution” hypothesis, we construct new constituency-specific data on the threat as perceived by the relevant decision makers at the time. We base our measures primarily on the Swing Riots. These riots are particularly suitable for our purpose because they, firstly, had a very uneven geographical impact and, secondly, they were concentrated over a relatively short

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<sup>8</sup> While some historians, e.g., Butler (1914, p. 410-23), Cole and Postgate (1961, p. 246-55), Hamburger (1963, p. 94-101) and Halevy (1950, p. 3-9), agree with Tilly (1995, chapter 7) others do not. For example, Trevelyan (1937, p. 224-39) and Congleton (2011, chapter 13) among others view the reform as a leading example of democratization by peaceful means that avoided strife and disharmony by finding a suitable constitutional compromise and Moore (1961) emphasizes the ideological divide between Members of Parliament and attribute the reform to a split within the old Tory elite.

<sup>9</sup> Smith (1972) goes one step further and argues for a direct connection between the riots and the fall of the Duke of Wellington's government in the last week of November 1830, an event that put the Whig government and with it the need for political reform firmly on the political agenda.

period of time so that they constituted a clear spike above the general level of social unrest in Great Britain at the time. These two features allow us to construct measures of the threat of revolution that vary across space and across time. We use these sources of variation to identify the impact of the threat of revolution *as perceived* by parliamentary electors, members of parliament (MPs), local patrons and other decision makers with a stake in reform on reform support at two critical junctures: the first roll-call vote in the House of Commons which took place on 22 March 1831 and where the bill passed with just one vote, and the subsequent general election held in June-July 1831 which was fought solely on the reform question. Moreover, to overcome potential endogeneity issues, we take advantage of the fact that the riots spread systematically along the road networks across the low wage wheat growing regions of England (Charlesworth, 1979). This allows us to construct exogenous spatial variation in the perceived threat level for each constituency by using geographical distance to the riots in the other counties. In that way, we establish if a causal link between the threat of revolution and reform support in each parliamentary constituency exists.

Our results show that the threat of revolution, as measured by exposure to the Swing Riots, had a major impact on the outcome of the 1831 general parliamentary election. In fact, our cross-section, fixed effects, and instrumental variables estimations all suggest that the reform-friendly Whigs and Radicals would not have obtained a majority of seats in the House of Commons without the shadow of the threat induced by the Swing Riots. The effect was most pronounced in the open constituencies in which electors enjoyed some degree of independence in their vote decisions. In the close constituencies, which were controlled by local patrons and where contested elections were rare, the threat of revolution had a smaller effect, if at all. In contrast, we find that exposure to the Swing Riots did not directly influence the votes of the MPs in the critical first roll-call vote on the bill on 22 March 1831, once we condition on their party affiliation, on their personal circumstances, and the characteristics of their constituencies. Still, we do find evidence that the MPs were influenced by the broader pattern of nonviolent social contention, such as peaceful demonstrations, political meetings and petitions. The channel through which social unrest operated was thus different in the two contexts. Overall, our evidence strongly supports the view that the threat of violence and radical social transformation played at key role in the success of the Great Reform Act.

In this way, the paper relates directly to a small emerging empirical literature on the “threat of revolution” hypothesis but it provides a different perspective. The existing empirical literature, e.g., Kim (2007), Przeworski (2009), and Aidt and Jensen (2011), is based on cross-national comparisons. This study, in contrast, shifts the focus from such cross-national comparisons of many suffrage reforms to the detailed study of a particular reform, the Great Reform Act. By doing so, it avoids many of the well-known pitfalls associated with cross-national studies, but it obviously comes at the cost of external validity: the Great Reform Act is hardly representative for all reform acts. Yet, this shift in emphasis has proved useful in a number of recent studies that also explore information on election results and roll-call votes to gain insights into the causes of political and economic reform. Ziblatt (2008) and Aidt and Franck (2012), for example, study the connection between structural factors at the constituency level and the success or failure of suffrage reform while Schonhardt-Bailey (1998) studies roll-call votes on trade reform to cast light on Imperial Germany’s coalition of landed aristocracy and heavy industry.<sup>10</sup> None of these studies, however, focus explicitly on the threat of revolution and the only empirical evidence that we are aware of on the connection between parliamentary reform and social unrest in Britain is related to the Second Reform Act of 1867. Here, Berlinski and Dewan (2010) as part of their study of the rise of the liberal party argue that the threat of revolution played a lesser role than it is commonly thought.<sup>11</sup> Turner and Zhan (2010)’s study of the London stock market also casts doubt on the threat of revolution explanation for the 1867 reform act.<sup>12</sup> This paper, in contrast, provides direct evidence from the Great Reform Act of 1832 suggesting that the threat of revolution played a leading role.

The rest of the paper is organized as follows. In section 2, we discuss the relevant historical background. In Section 3, we introduce our new measure of the threat of revolution. In

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<sup>10</sup> See also Schonhardt-Bailey (2006) for a study of trade liberalisation which makes use of roll-call votes and election returns to cast a new light on the role of special interests and ideology in the repeal of the Corn Laws in Britain in the 1840s.

<sup>11</sup> An argument often put forward to support the hypothesis that the threat of revolution must have played a role is that the reform was carried through by a conservative government which subsequently lost out in electoral terms to the liberals. Insofar as the reform induced this outcome, electoral expedience is an unlikely motivating factor and the reform, the argument goes, should therefore, partly or wholly, be attributed to the threat of revolution (Acemoglu and Robinson, 2000). Berlinski and Dewan (2010), however, demonstrate that the reform was not causally linked to the subsequent rise of the Liberal party by exploring the differential impact of the reform on the size of the electorate across parliamentary districts.

<sup>12</sup> Turner and Zhan (2010) study the stock market reaction to the bill and find that the market reacted negatively. They argue that insofar as the reform was meant to prevent a revolution then one should expect a positive and not a negative reaction of stock market participants.

Section 4, we discuss the three research designs we use to study the 1831 general election and report the results we obtain. In Section 5, we report the results related to the study of the 22 March 1831 roll-call vote in the House of Commons. In Section 6, we present results related to the role that the French and Belgium revolution in 1830 played in the success of the Great Reform Act. In Section 7, we provide some concluding remarks. The Appendices at the end contains some details on the construction of the dataset and descriptive statistics.

## **2. The Great Reform Act of 1832 and the Swing Riots**

In this section, we provide background information on the Great Reform Act as well as on the occurrence of social unrest and riots in the early 1830s, with a particular focus on the Swing Riots.

### *2.1. The passing of the Great Reform Act*

The Great Reform Act of 1832 was the first in a sequence of reforms that over the course of a century transformed the British political system from one based on privilege and corruption to one based on universal suffrage and the secret ballot. The immediate consequences of the reform were limited to redistribute parliamentary seats from the small “rotten” boroughs<sup>13</sup> to the large and fast growing industrial cities, as well as to extend the franchise to “respectable” segments of the middle class.<sup>14</sup> However, in the broader historical perspective, it must be viewed as a watershed that set a long political and economic reform process in motion.

The reform bill was voted on under the rules of the unreformed parliament which had not been modified in nearly two centuries. Most of the large expanding industry cities were not represented while small rural boroughs established in the middle-ages with few inhabitants could return two MPs. The voting franchises varied from one constituency to another depending on when it had been granted and were usually very restricted. This situation created many close constituencies which were dominated by patrons who were typically large local landowners and who effectively nominated those who got “elected”. In the open constituencies, elections were

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<sup>13</sup> “Rotten” boroughs were also known as “nomination” or “pocket” boroughs. They were constituencies that had been created in the Middle Ages and still returned MPs in the nineteenth century, even though they had by then lost the bulk of their population. The classic example of a “rotten” borough was Old Sarum, which only numbered 11 inhabitants in 1831.

<sup>14</sup> Brock (1973) estimates that the reform approximately doubled the size of the electorate to twenty percent of adult males. It introduced a uniform franchise in the boroughs that allowed resident householders with a property rated for tax purposes at ten pounds per year to vote and gave the vote to fifty-pound renters in the counties.

more competitive and the electors could exert some degree of independent voice although electoral corruption was ripe in many places. By modern standards, elections were neither fair nor open and the voting franchise was restricted to at most three percent of the adult male population.

The reform bill was introduced in the House of Commons in 1831 by Lord Grey. It was voted on in the House of Commons four times, critically amended at the committee stage, and was rejected by the Lords before it eventually entered the statute books in June 1832. The bill's journey through the political process was, therefore, far from smooth and it could have failed at a number of critical junctures along the way. Figure 1 presents a timeline of the main events following the historical narrative of Brook (1973) and Cannon (1973).

[Figure 1: The timeline of the main events as the bill passes through the political process.]

The first critical juncture was the formation of the Whig government under the leadership of Lord Grey in November 1830 after the general election in June-July of that year. This was the first reform-friendly government for more than a quarter of a century which had been dominated by a long stretch of Tory governments firmly committed to preserving the old political order. The second critical juncture was the vote in the House of Commons on 22 March 1831 where the bill was approved by the slightest of majorities: 302 in favor and 301 against. Lord Grey was keenly aware that this was not sufficient to get the bill through the House of Lords without major concessions and asked the King to dissolve parliament. The general election in June-July 1831 – the third critical juncture – was effectively a referendum on parliamentary reform since it was the single issue that dominated the campaign. Many Tories were not returned and the election result gave the Whigs the majority they needed to pursue the reform.<sup>15</sup> In the three subsequent votes in the House of Commons in July, September, and December, the bill was supported by large majorities, as is illustrated by the roll-call data reported in Table 1. These data also highlight that the reform question was essentially a party question. Almost all Whig MPs (as well as all Radical MPs) supported the bill, while the vast majority of Tories were against it.<sup>16</sup>

[Table 1. The Vote Counts for the Four Roll-Call Votes in the House of Commons.]

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<sup>15</sup> In fact, Hill (1996, p. 193) estimates that 90 percent of the elected Tories came from constituencies which stood to lose their representation if the initial Whig bill passed.

<sup>16</sup> The Tory support for reform was shared by "liberal" Tories and by the so-called "ultra" Tories who shifted their support away from the Tory leadership because of their dissatisfaction with its role in the passing of the Catholic relief act in 1829 (see Moore, 1961).



The fourth critical juncture occurred in September 1831 when the House of Lords rejected the bill and send it back to the House of Commons. After the second reading on 17 December 1831 where some concessions to the Lords were made, a frantic period of lobbying of individual Lords followed until the House of Lords approved the bill by a 6-vote-majority in April 1832. Again, this was insufficient to get the bill through the committees, and Lord Grey offered his resignation if the King did not promise to create enough new Lords, if needed, to carry the bill. The King eventually made that promise and the bill passed the fifth and last critical juncture as the Tory opposition did not wish to risk losing their majority in the House of Lords as well as having to concede an expanded franchise and a redistribution of seats in the House of Commons. The bill finally received royal assent on 7 June 1832 after nearly two years of bitter struggle in parliament and fierce agitation all over the country.

## *2.2 Political violence in the early 1830s and the Swing Riots*

The late 1820s and early 1830s saw unprecedented levels of social unrest in Britain (Tilly, 1995). The so-called Swing Riots contributed much to this general picture of unrest. They were named after the mythical avenger Captain Swing whose signature could be found on many of the threatening letters received by tenant farmers, parsons and landowners. They started in June 1830 and continued throughout 1831, but peaked in intensity in November 1830. They were essentially a rising of agricultural laborers against low wages, unemployment, and poor living conditions in general<sup>17</sup> and originated in Kent but spread to other counties, such as Sussex, Hampshire, and Wiltshire in the South and West and eventually engulfed East Anglia and parts of the midlands. The North (e.g., Yorkshire) and the most-Western part of the country (e.g., Cornwall and Wales) were relatively unaffected.

We use two sources to obtain quantitative information on the geography and timing of the riots. The first source is Hobsbawm and Rudé (1973, Appendix III). They record 1475 Swing-related events between 1 January 1830 and 3 September 1832. The second source is Tilly (1995) who records 285 Swing related events as part of his general catalogue of contentious gatherings, which he defines as events “at which 10 or more people gather in a publicly accessible place and make claims on other people, including holders of power, claims which if realized would affect

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<sup>17</sup> Unlike many other countries at the time, the agricultural economy in Great Britain had three broad groups: the large landowners, the tenant farmers, and the agricultural workers. The tenant farmers rented relatively large pieces of land and employed farm labour on wage contracts, typically of short duration and with no obligation to pay if no work was done. The farm labourers were not self-sufficient farmers, i.e., they did not own or rent any land.

the interests of their object” (p. 63).<sup>18</sup> Table 2 reports the number of swing incidents per county, with the most affected counties at the top and the least affected at the bottom. The first column records the catalogue of Swing Riots compiled by Hobsbawm and Rudé (1973). The information in the following three columns draws upon Tilly (1995)’s catalogue of contentious gatherings. The Swing Riots that qualify on this account are listed in column two, the distribution of all contentious gatherings, which include orderly meetings, petitions, delegations and demonstrations, are listed in column three, while column four singles out the subset of contentious gatherings that involved violence. Figure 2 shows the number of Swing Riots per month throughout 1830 and 1831 using data from the two catalogues.

[Table 2: The spatial distribution of the swing riots, contentious gatherings, and violent contentious gatherings, 1828-1832].

[Figure 2: The distribution of the Swing Riots over time, 1830-1831.]

The data reported in Table 2 and in Figure 2 paint the same picture of the Swing Riots: they followed a very uneven geographical pattern and were concentrated within a relatively short time window. The Riots involved different types of incidents which were characterized by either a high degree of violence or a threat of violence. Common tactics included arson, the burning of farm houses and haystack, the destruction of threshing machines owned by tenant farmers as well as outright extortion of money and provisions from farmers, landowners and parsons.<sup>19</sup> Hobsbawm and Rudé (1973) contend that the riots were caused by harsh rural socio-economic deprivation in general, and poor harvest in 1828-1829 in particular, whose effect was more severely felt across the wheat-growing, low-wage southern and western counties of England, in the Midlands, and in East Anglia. Charlesworth (1979) documents how news about local riots spread in a systematic manner following the road network and provoked new riots along the way.

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<sup>18</sup>The number of Swing events differs between these two catalogues for three main reasons. Firstly, Hobsbawm and Rudé count events, such as threatening Swing letters, which do not qualify under Tilly’s definition of a contentious gathering. Secondly, Tilly combines events that Hobsbawm and Rudé count separately if they happened on the same day and involved the same people. Thirdly, Tilly only includes events which are documented in eight London-based periodicals (Gentleman’s Magazine, the Annual Register, the London Chronicle, the Times, Morning Chronicle, Mirror of Parliament, Hansard’s Parliamentary Debates and Votes and Proceeding of Parliament.). Hobsbawm and Rudé make use of these sources as well, but also draw on Home Office documents and other official archival reports. Both catalogues are incomplete and leave out events which did not register in the national press or figured in Home Office and other official documents. See e.g., Reed and Well (1990) and Holland (2010).

<sup>19</sup> The main onus to deal with the rebellion rested on local magistrates who reacted very differently and often found it difficult to recruit farmers and tradesman to serve as special constables. Lord Grey and Lord Melbourne, in the Home Office, took decisive action to suppress the disturbances after they took office in November 1830. The sentences were particularly harsh in terms of deportations to Australia.

The Swing Riots were not the only riots that took place during the turbulent reform years, as the data in Table 2 show. In fact, they only constitute a small fraction of the violent contentious gatherings recorded by Tilly (1995) for the 1828-32 period. Nevertheless, the Swing Riots stand out by their intensity and differentiated geographical spread: the hinterland of London was literary burning between October 1830 and February 1831. In a sense the Swing Riots represent a discrete and very visible notch up relative to the general underlying level of social unrest. This is clearly illustrated by Figure 3 which shows the number of violent contentious gatherings per month between 1828 and 1832. Even the violence triggered by the veto by the House of Lords in October 1831 remained small relative to the violent social unrest triggered one year before by the Swing Riots.

[Figure 3: The distribution of violent contentious gatherings, 1828-1832.]

The fact that the early 1830s was both a period of social unrest *and* franchise reform does not, of course, in itself prove that the former caused the latter. Nevertheless, the historical narrative suggests the electors and patrons in small rural constituencies in the directly affected counties most certainly paid very close attention to what they saw and heard. Hobsbawm and Rudé (1973, p. 242), for example, remark that

“what shocked framers and landlords painfully was not the feebleness but the strength of the labourers’ activities in 1830. For them [farmers and landlords] the rising was not the last kick of a dying animal, but the first demonstration that a hitherto inert mass [...] was capable of large-scale, coordinated or at last uniform movement over a great part of England. It was fortunate that they had risen in isolation, but not inconceivable that they might rise again in conjunction with the much more readily mobilized movements of factory and city.”

Hilton (2006, p. 211-16) goes one step further and argues that the riots contributed to convince tenant farmers, who constituted an important fraction of the electorate in many counties, of the need for parliamentary reform. It, therefore, seems plausible that direct exposure to the riots would have invoked a real sense of fear and affected directly how electors, patrons and MPs would assess the threat of revolution and that this in turn might have affected their attitude towards reform. The general mood of London in the autumn of 1830 certainly reflected a widespread concern of what could happen if the farm labourers were to rise in conjunction with the workers from the big and expanding cities. The rest of the paper seeks to establish whether

there was a causal connection between the riots (and social unrest generally) and the success of the reform bill, as suggested by the “threat of revolution” hypothesis.

### 3. Quantifying the Threat of Revolution

To disentangle the threat of revolution from the other factors that might have contributed to the success of the Great Reform Act, we rely on the quasi-natural experiment induced by the Swing Riots as well as on the peculiar features of the unreformed parliament. Our starting point is that the MPs who voted on the bill were elected in geographically distinct parliamentary districts spread out unevenly across the country according to the rules of the unreformed parliament. Insofar as different constituencies were exposed to different degrees of political violence, the local political decision-makers, i.e., the electors, the MPs returned to Parliament, and the local patrons, would form different expectations about the likelihood of revolution which might have affected their attitude towards reform. This logic, in principle, allows us to assess the influence of the threat of revolution on the success or failure of suffrage reform by studying the choices made by these decision makers.

As documented in the previous section, the Swing Riots offer two advantages compared to the entire catalogue of contentious gatherings compiled by Tilly (1995). First, their geographical impact was very uneven (Table 2). This creates the spatial variation in exposure to violence that we rely upon to estimate the impact of the threat on constituency-level decisions. Second, they constituted a major spike in the overall social contention during the reform period (Figures 2 and 3). They therefore stand out as a discrete event which we can use to capture over-time variation in social unrest. However, we also make some use of the broader catalog of contentious gatherings, but our main focus remains the Swing Riots.

More specifically, our geographic identification strategy is to use information about the riots that happened in the “neighborhood” of a constituency to proxy for the threat of revolution as perceived by political decision makers in that constituency. We consider that the relevant “neighborhood” is the county where the constituency is located. For each constituency, we let the variable *threat* count the number of Swing Riots (or contentious gatherings) occurring in its county from 1828 until the critical juncture under consideration.

These data allow us to assess the role that the threat of revolution might have played at two of the five critical junctures identified above (see Figure 1): the first roll-call vote held on

March 22<sup>nd</sup> 1831 and the 1831 general election.<sup>20</sup> At these two junctures, we observe decisions made by MPs, patrons, and parliamentary electors that were directly related to the success or failure of the reform – something we do not at the other three junctures. Since different decision-makers were involved at the two junctures, studying both the roll-call vote in March 1831 and the subsequent general election enables us to separate the two potential channels through which the threat of revolution could have affected the fate of the Reform Act. On the one hand, our study of the vote behavior of the MPs in March 1831 captures the impact of the threat of revolution induced by the Swing Riots independently of how the threat might have affected the electors and the patrons in the constituencies that elected those MPs. This is because these MPs were elected in 1830 *before* the riots began in earnest. On the other hand, our study of the 1831 general election captures the impact of the threat on the behavior of the electors and local patrons – the local decision makers who selected the MPs returned in 1831 to the House of Commons. Our studies of these two junctures thus complement each other and speak, not only to the general effect of the threat of revolution on democratization but also to the channels through which the threat might operate.

In the next sections, we discuss the research designs used to analyze these two decisions in more detail and present the results we obtain. We start with the 1831 general election followed by the March 22 1831 roll-call vote.

#### **4. The support for reform in the 1831 general election**

The single most important issue of the 1831 general election was the reform question. Given that virtually all Whigs were pro-reform, the electoral success of the Whigs and the Radicals<sup>21</sup> in each constituency is an accurate measure of a constituency's attitude to reform.<sup>22</sup> We use this measure of the reform support in three different empirical designs. The designs vary with respect to the source of variation in the threat of revolution which we draw upon for identification.

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<sup>20</sup> See Aidt and Franck (2008) for details about how these data are obtained.

<sup>21</sup> Very few Radicals were elected (ten in 1831), but those who were, firmly supported reform.

<sup>22</sup> The Whigs and Tories were not political parties in the modern sense, but loose and shifting factions (see Hill, 1996). In the cross section analysis we have included the few Whig MPs whom we know *ex post* to have voted against the Bill in July 1831 amongst the Tory opposition under the assumption that their stance would have been known to patrons and electors *ex ante*.

#### 4.1. *The impact of the threat of revolution on the outcome of the 1831 general election*

##### 4.1.1. Empirical methodology: Cross-section estimates

Our first design studies the cross-section of 489 parliamentary seats in England and thus explores cross-sectional variation in the threat level only. We use a probit model to estimate the probability that a particular seat is won by a reformer, i.e., a Whig or a Radical, in the 1831 general election as a function of the perceived threat of revolution, as captured by the variable *threat*.<sup>23</sup> The design only identifies a causal effect if *threat* is truly uncorrelated with all unobserved determinants of reform attitude or general support for the Whigs and Radicals in a constituency. One particular concern is that socio-economic deprivation both generates anti-reform attitudes and the sparks that trigger social unrest. Another is that the politics of constituencies located within the low-wage areas most exposed to the Swing Riots happened to represent the most reform friendly electors and patrons. We address the possibility of an omitted variable bias by including a large range of constituency level control variables.

Most importantly, we know if a particular seat was won by a Whig or a Radical in the general election of 1826. We can use this to control for the underlying support for the Whigs and the Radicals in a constituency in the 1831 election. This should alleviate some of the concerns about omitted factors.

Patronage was a major feature of the unreformed parliament: powerful aristocrats, big landowners, or wealthy commoners controlled the electorate, either fully or partially, in many boroughs and counties. In the close constituencies, patrons effectively nominated the MPs and elections were uncontested. In the open constituencies, on the other hand, election did take place and the electors could exert some independent influence on the choice of MP. Based on the detailed description of each parliamentary constituency in Philbin (1965) and on the data on contested elections reported by Cannon (1973, Appendix 3), we construct the dummy variable *close constituency*. It takes the value one if Philbin (1965) explicitly states that it is wholly or partly controlled by a local patron or by the Treasury through employment and other patronage or if Cannon (1973) records that there were no contested elections between 1802 and 1832. We expect that support for reform is lower in close constituencies simply because the patrons controlling these constituencies stood to lose a valuable asset.

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<sup>23</sup>We exclude Wales, Scotland and Ireland in the analysis because neither Hobsbawm and Rudé (1973) nor Tilly (1995) records any Swing Riots in these regions.

Under the unreformed parliament there were six different types of parliamentary franchises in operation (see, e.g., Brock, 1973). These had important implications for the number of voters in each constituency and for the cost of “buying” a seat. This may, in turn, have affected the reform attitude in the constituency. Based on the information about the different suffrage rules and the scattered and incomplete information on the approximate size of the electorate reported in Philbin (1965) and in Brock (1973, Table 2), we construct a dummy variable, *narrow franchise*, that is equal to one for burgh and corporation boroughs. These boroughs had very narrow franchises, which limited the number of voters, and were thus relatively easy to ‘buy’. In the burgh boroughs, for example, only the owners of a property with an old form of tenure, called the burgh, could vote. These were often limited to plots of land that had formed the borough when it was first laid out and could be owned by a single person. In the corporation boroughs, only members of the local town council, called the corporation, could vote. In other boroughs with scot and lot, potwalloper, or freeman franchises, the electorate tended to be more sizable. That was also true in the county constituencies which had a freeholder franchise based on a 40-shilling qualification. We include the dummy variable *county* to pick this up, as well as any additional differences between the counties and the borough and university constituencies. We expect more support from the large county and borough constituencies than from the boroughs with a very narrow franchise.

Another but related feature of the unreformed parliament was the so-called rotten boroughs. In these boroughs electoral corruption was ripe and one of the demands of the reformers was their disenfranchisement. It is natural to suppose that the patrons who controlled these rotten boroughs did not wish to return reform-minded MPs. We include the dummy variable *rotten borough* to control for this effect.

Information flows between London and the constituencies in the provinces might also have affected the attitude of electors and patrons to reform ideas (and other events happening elsewhere in the country, including the Swing Riots). The information hub was London where the major daily national newspapers, many weekly newspapers as well as the quarterlies were written and published. In the era before the expansion of the rail network, which happened in the mid-1830s, and of the telegraph, which was introduced in the 1840s, news travelled slowly from London to the provinces by horseback and postal mail (Brown, 1992). The reading public outside London were exposed to national news through the 130 or so weekly local and regional

newspapers that picked up the news from London (and mixed them with local and regional news) or directly by London papers circulated to the provinces (Barker, 2000, chapter 2 and 3). Newspapers could be mailed free of charge and it was common practice for MPs to mail papers back to their friends and family so that all constituencies had some connection to the hub. However, it is reasonable to presume that the connection was stronger for the constituencies closer to London and in constituencies where the circulation of local weekly newspapers was large. In addition, Stamp duties had to be paid on each sheet of paper. While this raised the price of a newspaper to a level that was outside the budget of most ordinary people, who would read them in coffee houses, in inns or at public libraries, if at all, the accounts of the Stamp Office make it possible to estimate roughly the circulation of each newspaper.<sup>24</sup> Based on such estimates and the distance to London, we constructed a constituency-specific *news exposure index*.<sup>25</sup> Since both pro- and anti-reform agitation would flow from London to the constituencies around the country, the impact of the index on reform support is a priori unclear but the variable can help clarify if the press played an important role, either way, in the reform process.

Quantitative information on the economic circumstances of each constituency is not available. Philbin (1965), however, singles out the constituencies which were in decline in 1830 and those which were prospering. Based on this, admittedly incomplete, information on long term economic trends, we include two indicator variables, *thriving economy* and *declining economy*, to control for these broad patterns of decline and expansion.

The final control variables use information from the 1831 census to estimate the occupation structure of each constituency. The census reports these data at the constituency-level for the counties, but not for the boroughs, where we use the closest geographical unit to approximate. We can distinguish between six different categories of employment, landowners employing workers, *large landowner*, landowners who do not employ workers, *small landowners*, *agricultural workers*, *industry workers*, *artisans*, and *professionals*. These are measured as shares of the workforce of the geographical unit to which they belong.

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<sup>24</sup> Circulation in general was much lower than readership. It is argued by, e.g., Westminster Review (1830), that each London Newspaper would be read by 16 people. For a general discussion of the politics of stamp duties in the 1830s, see e.g., Asquith (1978).

<sup>25</sup> Appendix A provides the details of how this index is constructed.



#### 4.1.2. Results

The main results are reported in table 3.<sup>26</sup> The first two of the four specifications focus on the effect of the Swing Riots on reform support and make use of the two catalogues of riots provided by Hobsbawm and Rudé (1973) and Tilly (1995), respectively. The estimates suggest that the Swing Riots in both cases had a positive effect on the probability that a “reformer” was returned to parliament in 1831. The other two specifications make use of Tilly (1995)’s broader measures of social contention. We observe that both violent contention (which include some of the Swing Riots as well as violent incidents happening in the industrial centers and in London) and general contention (which include peaceful meetings, delegation, orderly demonstration, etc.) correlate positively with the probability of returning a “reformer” to the 1831 parliament. All in all, this suggests that the Swing Riots, as well as social contention more generally, conveyed information about the threat of revolution to electors and patrons who lived in the affected areas and had an influence on their decisions in the 1831 general election. This is consistent with the “threat of revolution” hypothesis.

Inspection of table 3 shows that the many of the observable characteristics of the constituencies affected the probability that a “reformer” was returned. We find that seats that were won by a Whig or a Radical in 1826 were much more likely to be won by a Whig or a Radical in 1831. As argued above, this variable picks up a general leaning in the constituency towards the Whigs or Radicals. As one would expect, excluding this variable increases both the magnitude and the statistical significance of the threat of revolution proxies [not reported]. We also note that seats in close constituencies were less likely to be won by reformers, while the opposite was true in the county constituencies, which with their freeholder franchise had relatively large electorates of tenant farmers. On the other hand, Whigs and Radicals were less likely to be elected in the rotten boroughs and in the constituencies which Philbin (1965) singles out as being in economic decline. The occupational structure had little influence on the win probability. The size of the electorate within the boroughs did not play much of an independent role either. The press might have played a pro-reformer role in the average constituency, but this effect is only significant in some of the specifications.<sup>27</sup>

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<sup>26</sup> Since each constituency (except for a handful) returned two (or in a few cases four MPs), we cluster the standard errors at the constituency level.

<sup>27</sup> If *news exposure index* is dropped from the two specifications (where it is insignificant) that make use of violent and general contention to proxy for the threat, *threat* becomes significant at the five percent level [not reported].

We have undertaken several robustness checks which demonstrate the reliability of these findings [not reported]. Most importantly, we run a falsification test which examines whether the measures of the threat of revolution based on the Swing Riots are correlated with Whig and Radical electoral success in the 1826 general election. Since the Swing Riots had not yet happened there should be no such correlation. It is reassuring that this is indeed the case. A similar result can be found for the 1830 general election which took place just as the riots were about to begin in earnest.

[Table 3: The probability that seat won by Whig/Radical in the 1831 general election, the Swing Riots and Contentious Gatherings (CGs).]

## 4.2. Shifts in the threat of revolution between the 1826 and 1831 elections

### 4.2.1. Empirical methodology: Panel data estimates

Although we control for a large number of observable constituency-specific factors in the cross section estimates reported above, it is clear that they have two major shortcomings. Firstly, the threat of revolution could be correlated with unobserved constituency characteristics that also determine their reform stance. Secondly, the proxies for the threat could be picking up a general shift in the support for the Whigs in 1831 unrelated to any perceptions of the threat of revolution or aggregate shifts in economic conditions. The aim of our panel design is to address these two concerns and to evaluate if the results reported in table 3 are robust to controlling for unobserved constituency-specific effects and aggregate time shocks.

For this purpose, we construct an “index of reform support” for the two elections in 1826 and 1831. It is calculated as the share of seats won by a Whig or a Radical, which we denote by  $S_{it}$ , where  $i$  is an index for constituency, and  $t$  is an index for (election) year, for each of the 244 English borough, university, and county constituencies. The general idea is to compare this index of reform support before and after the peak of the Swing Riots while still accounting for the spatially differentiated effect of the Swing Riots in each constituency. Formally, we assume that the index of reform support at time  $t$  is a linear function of the threat of revolution

$$S_{it} = \alpha_i + \beta_{rt} + \gamma threat_{it} + \delta news_{it} + \mu_{it}, \quad (1)$$

where  $\alpha_i$  represents unobserved constituency fixed effects,  $threat_{it}$  is the proxy for the threat and  $\mu_{it}$  represents unobserved time-varying factors.  $\beta_{rt}$  represents common shocks. The index  $r$  indicate that we allow these shocks to vary across the 10 major regions of England to take into

account that common time shocks, e.g., political preference shocks such as the “rise in the popularity of the Whigs”, may have a different impact in regions with different economic and social structures.<sup>28</sup> Moreover, in the wake of the 1821-1823 depression, the mid-1820s witnessed somewhat higher agricultural prices while during poor harvest due to weather conditions created a depression after 1828. The impact of these general economic movements would have been different in the various parts of England, and so would their potential impact on the stance towards reform. This provides another justification for allowing the common shocks to vary by region. While all the time-invariant, observable factors included in the cross section gets absorbed by constituency specific effects, it is reasonable to suppose that the information flows regarding the *change* in the situation around the country in general as well as about the reform debate in particular between 1826 and 1831 might have affected the constituencies differently depending on how connected there were to the London news market. To capture this effect and to isolate the impact of the Swing Riots happening in the immediate neighborhood of each constituency from the impact of information shocks, including information about riots elsewhere in the country, that come through this channel, we include the variable *news* in the model. The variable is equal to the *news exposure index* interacted with a common time effect.

We are interested in estimating  $\gamma$  from within-constituency variation in the perceived threat of revolution. This requires that we observe some time variation in the threat proxies at the time of the 1826 and 1831 parliamentary elections. As noted above the Swing Riots took place between 1830 and 1831 with a peak in November 1830. Tilly (1995) records no Swing Riots between the start of his sample period in 1828 and the late summer of 1830. Similarly Hobsbawm and Rudé (1973) date the first Swing event to February 2 1830. Although one can find accounts of isolated incidents similar to those of the Swing rebellion itself before 1830, it is safe to assume that Swing-type riots in 1826 were rare, small scale, and localized in their impact and that the perception of the threat of revolution induced by such events was minimum throughout England. This allows us to code all the threat measures based on the Swing Riots as zero in 1826 so that the threat proxies for 1831 effectively measure the increase in the threat

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<sup>28</sup> If we do not allow the common shocks to differ by region, then we effectively assume that the shift in support was uniform across the country. This seems wholly implausible. On the other hand, we might argue that the shocks would operate differently at the county level. This is not implausible but we do not have variation to estimate these county-specific time effects.

level as well as the level of threat as perceived in that year in each constituency.<sup>29</sup> As noted above, patronage played a key role under the unreformed parliament. Since the degree of patronage in a particular constituency is likely to have been constant between 1826 and 1831 this will be absorbed by the constituency fixed effect.<sup>30</sup> However, it is natural to conjecture that the impact of the increase in the perceived threat level could work differently in close than in open constituencies. We allow for this possibility by including an interaction term between *close constituency* and the threat proxy in some of the specifications.

Identification of  $\gamma$  requires that the differential exposure to the riots was uncorrelated with constituency level time-varying factors that also caused a change in reform attitude. This is a much weaker requirement than that needed for identification in the cross-section. In practice, we estimate  $\gamma$  using a (linear) fixed effects estimator with panel-corrected standard errors, as recommended by Beck and Katz (1995).<sup>31</sup>

#### 4.2.2. Results

The results are reported in table 4. In the first two columns, we report regressions with constituency fixed effects, but without aggregate time effects. We observe that in both cases the exposure to the threat of revolution in a constituency had a significant effect on the increase in its reform support. This suggests that unobserved determinants of reform support which are fixed (between 1826 and 1831) cannot account for the correlation between *threat* and reform support. In the following two columns, we include regional specific time effects. Consistent with the historical narrative, these are highly significant capturing the common component of the swing between the two elections towards the Whigs as well as other region specific economic shocks. While the point estimate on *threat* has a p-value of 0.056 in the specification that relies upon Tilly (1995)'s catalogue of riots, it is no longer significant in the specification using Hobsbawm and Rudé (1973)'s data. This suggests that *threat* is partly picking up the general shift in reform

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<sup>29</sup> We prefer to use the 1826 general election rather than the 1830 general election because some of the riots took place before and during the polling period in June-July 1830. There was little turnover in the party affiliation of the elected MPs between 1826 and 1830 and so this choice makes little difference for our results but makes it more plausible that we can think of the accumulated Swing Riots as the increase in the threat level.

<sup>30</sup> In any case, we only have one observation from Philbin (1965) describing the degree of patronage around 1830.

<sup>31</sup> A similar approach is taken by Berlinski and Dewan (2010). Alternatively, we could interpret the seat share as a discrete indicator of the latent support for reform. This variable is ordered in a natural way and one could apply an ordered probit model. However, this would not allow us to control for unobserved fixed constituency fixed effects and, for this reason, we decided not to pursue that approach.

support: it is therefore hard to separate the effect of the threat of revolution from this general swing towards the Whigs throughout England. The two specifications reported in the last columns of the table show, however, that this can partly be attributed to the different impact of the Swing Riots on the reform support in the open and close constituencies. In the open constituencies, *threat* has a highly significant positive effect on the reform stance of the electors; in the close constituencies, we cannot reject the null hypothesis that the Swing Riots left no mark on the reform attitude of the patrons in control. They were unapologetically against reform. It was, therefore, amongst the relatively independent electors in the open constituencies that the threat of revolution made a long-lasting impression. Finally, looking across table 4, we see that *news* is highly significant in all specifications and that the point estimate is positive. This result suggests that exposure to national information flows, overall, triggered a reform friendly response and thus that the press played a pro-reform role.

[Table 4: Constituency level index of reform support in the 1826 and 1831 general election and the threat of revolution]

#### 4.3. *The threat of revolution and the geographic spread of the Swing Riots*

##### 4.3.1. Empirical methodology: Instrumental Variable Estimates

The panel estimates reported in table 4 go a long way towards isolating the causal effect of the threat of revolution on reform support, but could be contaminated by constituency specific over-time shifts in the support for reform. To isolate the truly exogenous variation in exposure to the threat of revolution, we rerun our panel data regressions with an instrument. Our choice of instrument builds upon Charlesworth (1979)'s work on the geography of the Swing Riots. This study clearly shows that the riots spread in a systematic manner: the rising spread along the major roads to London and gradually made its way from Kent to East Anglia moving clock-wise around London through the Midlands. The riots in a given county were therefore directly related to the riots in neighbouring counties and spread from county to county in a systematic manner. This suggests that we can use information on riots in other counties to instrument for the number of riots in a particular county. Specifically, we propose to use the number of riots in other counties weighted by the inverse distance in miles between the county seat of the county in which the given constituency resides and the county in which the riots took place as an instrument for *threat*. While this instrument, which we call *riots, outside county*, is relevant in

the sense that it can explain the number of riots in each county, and thus, the constituency specific exposure to the threat of revolution, the issue of validity requires elaboration.

For *riots, outside county* to be a valid instrument, it should, conditional on constituency fixed effects, (regional) time effects, and *news*, be independent of all unobserved time varying constituency-specific determinants of reform support. It is useful to consider two categories of such unobserved factors. The first category is reform-support shocks unrelated to the threat of revolution. While such shocks could well be correlated with the likelihood of riots happening in a particular place, they are unlikely to be correlated with riots in other counties. Accordingly, if this was the only concern, *riots, outside county* would be valid. However, there is another category of unobserved factors that could invalidate the instrument. The logic behind our empirical strategy is that decision makers at the constituency level observe riots in their neighbourhood, which we define as the county, and base their assessment of the threat of revolution on that. The logic, however, does not rule out that they might also learn from riots outside their own county. This means that riots outside a county can affect the assessment of decision makers inside a county about the threat of revolution, not only through the effect they have on riots inside that county – call this the “contagion effect” – but also through this “learning effect”. To circumvent this problem, we need to condition on the “learning effect”. We do this by including *news* multiplied by the number of Swing events reported in the London newspapers. Recall that *news* captures the mechanism through which news, including information on social unrest, float from one county to another via the London news market. In other words, this variable controls for the “learning effect” and conditional on that we argue that *riots, outside county* is a valid instrument for *threat*. This identification strategy clearly only makes sense for the Swing Riots recorded by Hobsbawn and Rudé (1973) since they are based on the actual spread of the riots as opposite to the news about the riots (in London newspapers) recorded by Tilly (1995).

#### 4.3.2. Results

The 2SLS estimates are reported in Table 5.<sup>32</sup> We consider two specifications, with and without the interaction effect between the threat of revolution and the dummy variable distinguishing between close and open constituencies. In column one, we report the second stage

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<sup>32</sup> The reported standard errors are robust to both arbitrary heteroskedasticity and arbitrary autocorrelation.

estimation for the specification without the interaction effect. The instrumented impact of the threat of revolution is significant at the five percent level. The first stage regression shown in column two performs well and confirms that *riots, outside county* is a relevant instrument. The second stage regression from the specification with the interaction effect is shown in column three. Again, we observe that the threat of revolution increased reform support in the open constituencies. Unlike the fixed effects regressions shown in Table 4, we cannot reject at the ten percent level that the threat of revolution has a small positive effect on the support for reform also in the close constituencies.<sup>33</sup> In addition, the first stage regressions reported in the last two columns of table 5 perform fairly well, although we note that the F statistics reported in column four is below the critical value for the weak instrument test suggested by Stock and Yogo (2005).<sup>34</sup>

Our estimates suggest that the threat of revolution had a large impact on the reform process. One way to see this is to use the point estimate of 0.0038 from the estimation in column one in table 5 and counterfactually ask what would have happened on average across all the English constituencies to the share of Whigs/Radicals elected to the House of Commons if the Swing Riots had not happened. In 1831, the Whigs/Radicals controlled 134 out of the 244 seats in the English constituencies, i.e., 55% of the English seats. Increasing *threat* from zero to its mean value (56.5) adds 21 percentage point to the Whigs/Radicals' share of English seats. Therefore, had the Swing Riots not happened, our estimates suggest that the Whigs/Radicals would only have obtained 34 percent, i.e., less than the majority, of the English seats. This is a large and politically significant effect which accounts almost entirely for the rise in the support for reform between the two elections.

[Table 5: Constituency level index of reform support in the 1826 and 1831 general election and the Swing Riots, instrumental variables estimates.]

To summarize, the three different approaches used to estimate the impact of constituency specific exposure to the Swing Riots on indicators of reform support in the 1831 general election all point in the same direction. The threat did affect the reform stance of the electors and patrons and contributed to the Whig victory which was essential to move the reform agenda forward. The effect was clearly strongest in the open constituencies, but in our instrumental variable design,

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<sup>33</sup> The p-value on a test of the null hypothesis that the sum of the two coefficients is zero is 0.06.

<sup>34</sup> We also estimated the models with a LIML estimator and the results are virtually the same.

we also find some evidence that the threat mattered, but to a lesser extent, in the close constituencies controlled by patrons. These estimation results lend substantial support to the view that the threat of revolution did, in fact, play a causal role for the reform success by helping the reform bill through at least one of the critical juncture.

## 5. The Roll-Call model

The other critical juncture that we study is the 22 March 1831 roll-call vote on Lord Grey's initial draft of the bill. The 461 English MPs who voted on that day were elected in June-July 1830, i.e., before the Swing Riots begun in earnest. We seek to determine whether the Swing Riots, as well as the broader measures of social unrest, exerted any influence on the way they voted. For this purpose, we estimate a probit model where the dependent variable equals one if a MP voted in favor of the reform and zero if he opposed it. We exclude from the analysis the MPs who did not take part in the vote held on 22 March 1831 in the House of Commons.

The vote of each MP might, of course, have been governed by many other considerations than perceptions about the threat of revolution. We thus included two groups of observable factors in the estimations to minimize the risk of an omitted variable bias. The first group pertains to the characteristics of the constituency where the MP is elected. It includes whether the constituency is close or open (*close constituency*), whether the franchise of the MP's constituency is broad or narrow (*narrow franchise*), whether his borough is considered rotten and thus likely to be disenfranchised (*rotten borough*), whether he is elected in a borough or a county constituency (*county*), and whether the constituency, according to Philbin (1965), is thriving or declining economically (*thriving economic* and *declining economic*).<sup>35</sup> The second group of control variables relates to the circumstances of the individual MPs. They are included because personal factors might have influenced the reform stance of each MP. We know from table 1 that party affiliation is a strong predictor of reform support and therefore include a dummy variable, called *Whig or Radical*, which is equal to one if the MP is listed as a Whig or a Radical (on the division list) and zero otherwise. We also collect information on the occupations and the social background of the MPs elected in 1830 from Namier and Brooke (1964), Stookes Smith (1973),

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<sup>35</sup> We also experimented with the set of variables that measures the occupation structure of the constituency but they are never significant and do not affect the evidence on the threat of revolution. For the sake of clarity, we decided not to add them to the estimations which we report. The *news exposure index* is also omitted from the reported models because it is not significant but also because it would be fair to assume that the MPs themselves, as opposite to the electors and patrons back home in their constituencies, would be fully integrated in the London news market.



Thorne (1986), and various editions of Dod's Parliamentary Companions. Many MPs were army officers, lawyers or jurists, bankers, industrialists, merchants, landowners or "dynasty heirs", i.e., the MPs who were immediately returned to Parliament when they finished their education and/or reached majority.<sup>36</sup> Finally, we account for the fact that many MPs also had a relative who previously sat in Parliament (*Relatives in parliament*).

[Table 6: The probability of a pro-reform vote in the roll-call vote on March 22 1831 and social contention]

Table 6 reports the results. We find that the Swing Riots did not influence the way the MPs elected in 1830 voted on 22 March 1831. However, we notice in column three that their exposure to contentious gatherings more generally correlates positively with the likelihood of a pro-reform vote. In columns four and five, where we split the contentious gatherings into those which involved violence and those which did not, such as demonstrations, petitions, or meetings, we observe that exposure to violent social unrest in the home county is at best marginally significant at the ten percent level whereas exposure to non-violent contention is significant. This suggests that the MPs were largely unmoved by the rural rebellion of the previous year and by other violent events reported in the London newspapers, but that they paid attention to non-violent contentious gatherings occurring in the neighborhood of their home constituency. This is in contrast to the electors and patrons, who, as shown in the previous section, paid close attention to what had occurred during the Swing Riots in their neighborhood when electing (or nominating) MPs to the 1831 parliament. In other words, the threat of revolution, as captured by the Swing Riots and other violent social unrest, mainly played a role in relation to the 1831 election. However, more orderly social contention, in the form of demonstrations and petitions, affected the votes of some MPs on 22 March 1831.

Additional results from table 6 are also worth highlighting. As expected, party affiliation is a highly significant predictor of the vote pattern, with Whigs and Radicals being much more likely to support the reform than to oppose it. The anti-reform votes, on the other hand, were more likely to come from MPs elected in close and rotten boroughs, while MPs elected in to county seats were more likely to vote in favor of the bill. The occupation and other personal circumstances of the MPs, on the other hand, mattered little for their reform stance.

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<sup>36</sup> These categories are not mutually exclusive and many MPs are therefore recorded in more than one category, e.g., as an army officer and as a landowner. We use landowners as the control group.

## 6. The French and Belgian Revolutions

The analysis above establishes that the exposure to local social unrest and riots is related to reform support in a systematic and causal way. British electors, patrons and MPs might, however, have looked to the revolutionary events unfolding on the other side of Channel in July 1830 when making judgments about the likelihood of revolution in Britain.<sup>37</sup> It was not so much because anyone thought the revolution in France, which took place between 27 and 29 July 1830, and the revolution in Belgium, which started on 25 August 1830, would spread directly to England. Rather, the events showed to the ruling British elites that popular riots could oust them from power and gave inspiration to the British Radicals who clearly used the examples of France and Belgium to paint a picture of an eminent threat in Britain. Going one step further, Halevy (1935) argues that the revolution in France had a direct and immediate impact on the 1830 general election which set the reform process in motion. Others, in particular Gosh (1956) and Brock (1973), refute the existence of a link. They argue that the timing was too tight because information on the events in France did not appear in the London newspapers until the polls had closed in most constituencies.

Our data allow us to shed some light on this question by comparing the outcome of the 1826 and 1830 general election. Using a fixed effects panel data approach similar to the one used above in Section 4.2 but comparing the outcomes of the 1826 and 1830 elections, we test whether the change in Whig support between the two elections can be attributed to an aggregate shock capturing, among other factors, news about the French and Belgian revolutions.

Our results suggest that aggregate time shock did not matter, whether or not our specifications include constituency-specific fixed effects [not reported]. This makes it unlikely that the election result in 1830 was influenced by the events on the continent. However, this does not rule out the possibility that the revolutions in France and Belgium affected the beliefs of the decision-makers in the constituencies and in Parliament about the threat of revolution at other junctures during the reform crisis. We can, however, conclusively assert that if the French and Belgian revolutions played a role, it was not because they influenced the outcome of the 1830 general election.

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<sup>37</sup> See Tilly (1993, 2004) for a discussion of the role of revolution in European political history.

## 7. Conclusion

This paper proposes a new perspective on the economic origins of democracy by providing a detailed study of one key reform, the Great Reform Act of 1832, unlike past work which has focused on cross-national comparisons.<sup>38</sup> In so doing, it provides a robust test of one of the leading hypotheses in the democratization literature, namely the “threat of revolution” hypothesis. Economists, political scientists and historians have over the years argued whether the riots which occurred in the years prior to the passing of the bill had an effect on the Members of Parliament, or whether their votes were motivated by a mix of ideology and personal interest, since they were (correctly) hoping to remain in power after the reform. Our results provide strong evidence that electors and patrons were more likely to elect a pro-reform MP in 1831 if they originated from constituency which had been exposed to riots and other forms of political violence in the period leading up to the election. As such, our findings suggest that acts of violence might have been decisive for the ultimate success of the reform and lend strong support to the “threat of revolution” hypothesis. A fruitful direction for future research is to determine whether the threat of revolution played a similar role in other episodes of suffrage reforms.

## Appendices

### *Appendix A. The news exposure index*

This appendix explains how we constructed the *news exposure index*. This variable aims at measuring the exposure to news from outside the immediate neighbourhood of a constituency. The historical narrative makes it clear that London was the information hub and that information flowed from the centre to province through a number of channels (e.g., Barker, 2000; Brown, 1992 or Aspinall, 1973). This suggests that we can quantify the exposure of decision makers in a particular constituency (the electors and the patrons) to news from outside their own county by a combination of three factors.

Firstly, if the constituency had a newspaper it would be a natural local information hub, even if, as it is commonly the case, the market for the newspaper included a far larger area. Local editors would pick up on and report national news from London, but would also spread the news informally in coffee houses and inns. Thus it is safe to assume that London papers would reach

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<sup>38</sup> See e.g., Przeworski et al. (2000), Acemoglu et al. (2008), Barro (1999), and Gundlach and Paldam (2009).

the locality and be read by more than the editor of the local newspaper. Secondly, if the constituency is located in a county with a large number of newspapers and a large circulation, it is natural to suppose that national news would reach the relevant decision-makers more easily than in counties with few newspapers and low circulation. Thirdly, although it is reasonable to presume that the electors and patrons in all constituencies had some exposure to news from London, it is also natural to suppose that this exposure was not uniform and that it varied with the distance to London. If nothing else, the frequency with which the local MP would go to London is likely to be affected by the distance.

Our proxy for the learning effect is then based on measures of these three factors. We rely upon information from two Returns to the House of Commons in 1833 regarding the number of stamps issues to each newspaper published in London and in the provinces.<sup>39</sup> Each page published required a stamp so that these numbers can be converted into an estimate of the extent of circulation.<sup>40</sup> Outside London, all 130 local or regional newspapers were weeklies; in London there were 12 dailies, with the Times being by far the largest, as well as 7 newspapers published three times a week, 1 twice a week and 37 weeklies. To make London comparable to the provinces, we estimate circulation numbers as the total number of papers published in a year and allocate these circulation figures directly to a constituency, if the name of the newspaper allows us to do that, or to the county in which it is published if not. Based on this information, we compute our constituency specific *news exposure index*, as the sum of

- 1) The yearly circulation in the county (of the constituency) excluding newspapers published in the borough (if applicable) divided by the county population in 1831.
- 2) The yearly circulation of newspapers published in the borough divided by borough population in 1831. For the county constituencies, this is always zero by construction.

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<sup>39</sup> The source of this information is two returns to Parliament in 1833 about the number of stamps issued for all London and all provincial newspapers (House of Commons (1833a) and House of Commons (1833b)). The stamp issue returns are notoriously inaccurate with respect to individual newspapers, in particular in London, but the totals should give a fair picture of the total circulation of newspapers in London in the year.

<sup>40</sup> We follow Wadsworth (1955) and use the following conversion factors. For weekly newspapers, 50000 stamps early correspond to 1000 weekly newspapers. For daily newspapers, 3.2 million stamps per year correspond to 10000 copies per day. We convert the thrice and twice dailies into dailies and use the conversion factor for the dailies to estimate the number of copies per day. The weekly circulation numbers are converted into yearly total by assuming 42 weeks per year and the daily circulation numbers are converted into yearly numbers by assuming 42 six-day weeks.

- 3) The yearly circulation of newspapers published in London divided by the total population of England (and Wales) (equal to 3.6) times the inverse distance between the county seat of the county in which the constituency is located and London. To take into account that the counties around London and Middlesex (which is counted geographically as London) – Surrey and Hertfordshire in particular – are likely to be fully integrated in the London news market, we allow for a radius of 20 miles (the distance to the county seat nearest to London) before applying the distance discounting.

The idea behind (1) and (2) is that all constituencies in a county get exposed to the average circulation of news in the county, but that a borough constituency inside the county which hosts a newspaper gets extra exposure. This is captured by normalising the circulation of that newspaper hosted by the borough by the borough population rather than the county population. A borough constituency without a newspaper (which are the vast majority) gets the same exposure as the county as a whole. The idea behind (3) is that the London newspapers were, in a sense, national newspapers. As such each citizen should get the average exposure (calculated as the average circulation per year per capita) but that this is discounted as one moves further and further away from London. This does not mean that places far from London automatically get a low exposure rate – in actual fact Newcastle upon Tyne is highly exposed according to this index because of a vibrant local press – but it does mean that on average being close to London entails more direct exposure to the London newspapers.

The above calculation of the *news exposure index* applies to all constituencies with four exceptions. Firstly, for London and Middlesex themselves, we assume that the exposure is greater than the national average and divided the total London circulation by the total population of these two counties rather than by the national population. This means that the exposure index is largest for London. Secondly, for the two University constituencies in Oxford and Cambridge, where the voters are graduates living elsewhere, many in London, we simply assume that they are exposed to the national circulation average for London without any distance discounting (3.6). This makes them among the most exposed outside London. The resulting index, which varies by constituency, range from 0.55 in County Durham to just over 16 in London and Middlesex.

#### *Appendix B: Descriptive statistics*

[Tables A1 and A2: Descriptive statistics]

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Table 1. The Vote Counts for the Four Roll-Call Votes in the House of Commons.

<b>Date of the vote</b>	<b>Bill</b>	<b>Parliament</b>	<b>Votes for</b>	<b>Votes against</b>
22 March 1830	Second reading of the first Reform Bill.	1830 Parliament	Whig: 248 Radical: 6 Tory: 48 Total: 302 (235)	Whig: 8 Radical: 0 Tory: 293 Total: 301 (249)
6 July 1831	Second reading of the second Reform Bill.	1831 Parliament	Whig: 316 Radical: 10 Tory: 41 Total: 367 (290)	Whig: 5 Radical: 0 Tory: 227 Total: 232 (208)
21 September 1831	Third reading of the second Reform Bill.	1831 Parliament	Whig: 302 Radical: 10 Tory: 34 Total: 346 (282)	Whig: 5 Radical: 0 Tory: 232 Total: 235 (184)
17 December 1831	Second reading of the third Reform Bill.	1831 Parliament	Whig: 285 Radical: 4 Tory: 35 Total: 324 (275)	Whig: 4 Radical: 0 Tory: 158 Total: 162 (140)

Note: The votes for and against on the Reform Bills refer to all MPs, including those representing Scotland and Ireland. The votes in the bracket refer to the MPs from England and Wales.

Source: Parliamentary Debates, various years.

Table 2: The spatial distribution of the swing riots, contentious gatherings, and violent contentious gatherings, 1828-1832.

County	H&R	Tilly	Tilly	Tilly
	Swing riots	Swing riots	All CGs	All violent CGs
Hampshire	208	54	117	46
Wiltshire	208	41	74	44
Berkshire	165	22	68	27
Kent	154	42	163	59
Sussex	145	52	119	51
Norfolk	88	12	55	20
Essex	46	5	61	9
Dorset	42	12	62	19
Suffolk	40	5	32	6
Buckinghamshire	39	13	39	14
Gloucestershire	37	4	62	13
Lincolnshire	30	0	42	10
Oxfordshire	30	4	26	10
Surrey	29	5	144	51
Huntingdonshire	25	3	11	4
Northamptonshire	19	4	47	6
Devon	18	0	66	1
Cambridgeshire	17	2	26	4
Bedfordshire	16	1	11	4
Somerset	15	3	34	3
Yorkshire	13	0	164	26
Middlesex	11	0	914	353
Worcestershire	10	0	36	5
Cumberland	9	0	26	8
Lancashire	9	0	242	43
Cornwall	7	1	51	3
Leicestershire	7	0	28	2
Derbyshire	6	0	29	2
Hertfordshire	6	0	11	2
Nottinghamshire	5	0	29	3
Staffordshire	5	0	31	3
Cheshire	4	0	40	15
Herefordshire	4	0	7	0
Salop	4	0	9	1
London	2	0	914	353
Warwickshire	2	0	69	12
Durham	0	0	16	1
Northumberland	0	0	41	3
Rutland	0	0	2	0
Westmorland	0	0	2	0
Monmouthshire	0	0	15	2
Total	1475	285	3935	1238

Note: Tilly (1995) record evens in London and Middlesex combined.

Table 3: The probability that seat won by Whig/Radical in the 1831 general election, the Swing Riots and Contentious Gatherings (CGs).

	1	2	3	4
	H&R	Tilly	Tilly	Tilly
	Swing	Swing	Violent CGs	All CGs
Threat	0.002*	0.010**	0.007*	0.002*
	[1.66]	[2.10]	[1.87]	[1.83]
Whig elected in 1826	1.393***	1.398***	1.398***	1.406***
	[7.84]	[7.88]	[7.83]	[7.85]
News exposure index	0.075**	0.072**	-0.063	-0.03
	[2.04]	[2.02]	[0.70]	[0.39]
Close constituency	-0.589***	-0.593***	-0.602***	-0.602***
	[2.72]	[2.73]	[2.83]	[2.83]
Narrow franchise	0.053	0.058	0.018	0.03
	[0.28]	[0.31]	[0.09]	[0.16]
County	1.387***	1.396***	1.415***	1.396***
	[4.17]	[4.16]	[4.26]	[4.18]
Rotten borough	-0.520*	-0.537*	-0.579**	-0.589**
	[1.88]	[1.95]	[2.06]	[2.08]
Large landowners	-6.617	-6.734	-6.984	-7.53
	[1.14]	[1.13]	[1.23]	[1.34]
Small landowners	-0.649	-0.249	-2.57	-3.09
	[0.12]	[0.04]	[0.46]	[0.56]
Agricultural workers	-0.184	-0.112	-0.159	-0.055
	[0.15]	[0.09]	[0.13]	[0.05]
Industry workers	0.466	0.582	-0.074	-0.338
	[0.48]	[0.59]	[0.07]	[0.32]
Artisans	0.748	0.855	0.563	0.353
	[0.72]	[0.82]	[0.53]	[0.33]
Professionals	-2.817*	-2.73	-3.465	-4.07
	[1.71]	[1.60]	[1.35]	[1.30]
Thriving economy	-0.304	-0.289	-0.354	-0.372*
	[1.40]	[1.32]	[1.61]	[1.69]
Declining economy	-0.461**	-0.464**	-0.441**	-0.430**
	[2.07]	[2.08]	[2.01]	[1.97]
Constant	-0.042	-0.099	0.341	0.377
	[0.07]	[0.16]	[0.54]	[0.59]
Observations	489	489	489	489

Note: Robust z-statistics clustered at the constituency level in brackets; \* significant at 10%; \*\* significant at 5%; \*\*\* significant at 1%.

Table 4: Constituency level index of reform support in the 1826 and 1831 general election and the threat of revolution, fixed effects estimates.

	1	2	3	4	5	6
	H&R	Tilly	H&R	Tilly	H&R	Tilly
	Swing	Swing	Swing	Swing	Swing	Swing
Threat	0.00057**	0.00239**	0.00030	0.00285*	0.0013***	0.0071***
	[2.32]	[2.50]	[0.82]	[1.91]	[2.80]	[3.66]
Threat*close constituency					-0.0012***	-0.0051***
					[-2.67]	[-2.84]
News	0.00016***	0.00016***	0.00027**	0.00030***	0.00025**	0.00028***
	[4.59]	[4.74]	[2.51]	[2.71]	[2.32]	[2.58]
Observations	488	488	488	488	488	488
Constituencies	244	244	244	244	244	244
Regional time fixed effects (RTE)	NO	NO	YES	YES	YES	YES
Constituency fixed effects	YES	YES	YES	YES	YES	YES
R <sup>2</sup>	0.750	0.750	0.770	0.771	0.773	0.774
Chi <sup>2</sup> test for RTE=0			45.90	47.62	45.29	47.55
Prob>chi <sup>2</sup>			0.00	0.00	0.00	0.00

Note: Robust (PCSE) z-statistics in brackets. \* significant at 10%; \*\* significant at 5%; \*\*\* significant at 1%.

Table 5: Constituency level index of reform support in the 1826 and 1831 general election and the Swing Riots, instrumental variables estimates.

	1	2	3	4	5
	2nd	1 <sup>st</sup>	2 <sup>nd</sup>	1 <sup>st</sup>	1 <sup>st</sup>
	Support index	(threat)	Support index	(threat)	(threat*close)
Threat, Instrumented	0.00382**	2.361***	0.00542**	2.11**	-0.472
	[2.07]	[3.38]	[2.57]	[2.77]	[-0.63]
Threat*close constituency, Instrumented			-0.00211**	0.3435	3.13***
			[-2.28]	[0.82]	[7.64]
News*number of Swing events recorded by Tilly	0.00030**	-0.0229	0.00026**	-0.018	-0.004
	[2.30]	[-1.03]	[2.00]	[-0.86]	[-.20]
Observations	488	488	488	488	488
Constituencies	244	244	244	244	244
Regional time fixed effects (RTE)	YES	YES	YES	YES	YES
Constituency fixed effects	YES	YES	YES	YES	YES
R <sup>2</sup>	0.73	0.82	0.73	0.82	0.8
F-test for relevance of instrument		11.39		6.03	32.6
Shea partial R <sup>2</sup>		0.05		0.09	0.29

Note: HAC Robust z-statistics in brackets; \* significant at 10%; \*\* significant at 5%; \*\*\* significant at 1%; *Threat* is based on Hobsbawn and Rudé (1973).

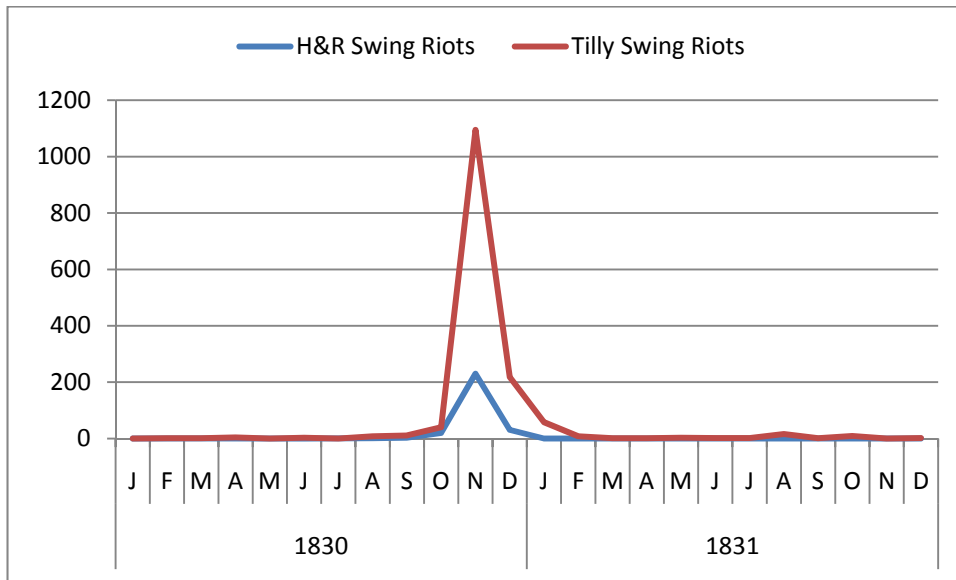
Table 6: The probability of a pro-reform vote in the roll-call vote on March 22 1831 and social contention

	1	2	3	4	5
	H&R	Tilly	Tilly	Tilly	Tilly
	Swing	Swing	All GCs	Violent	Non-violent
Threat	-0.001	-0.005	0.001**	0.002	0.002**
	[0.91]	[1.19]	[2.41]	[1.58]	[2.50]
Close constituency	1.629***	1.629***	1.638***	1.628***	1.644***
	[10.72]	[10.70]	[10.65]	[10.68]	[10.64]
Narrow franchise	-0.585***	-0.586***	-0.560***	-0.566***	-0.559***
	[3.03]	[3.04]	[2.85]	[2.86]	[2.86]
County	0.12	0.119	0.069	0.077	0.07
	[0.63]	[0.62]	[0.37]	[0.41]	[0.37]
Rotten borough	0.428**	0.420**	0.472**	0.455**	0.481**
	[2.32]	[2.29]	[2.53]	[2.44]	[2.57]
Thriving economy	-0.364*	-0.357*	-0.387**	-0.390**	-0.383**
	[1.95]	[1.90]	[2.06]	[2.09]	[2.02]
Declining economy	0.116	0.103	0.124	0.152	0.098
	[0.46]	[0.41]	[0.50]	[0.62]	[0.39]
Whig or Radical	0.198	0.197	0.216	0.22	0.211
	[1.02]	[1.02]	[1.14]	[1.17]	[1.11]
Army officers	-0.355	-0.364	-0.31	-0.324	-0.306
	[1.57]	[1.61]	[1.38]	[1.43]	[1.36]
Bankers	-0.367	-0.366	-0.422	-0.404	-0.423
	[1.34]	[1.34]	[1.54]	[1.48]	[1.55]
Dynasty heir	-0.007	-0.007	0.007	0.001	0.009
	[0.04]	[0.04]	[0.04]	[0.01]	[0.05]
Industrialist	-0.061	-0.068	-0.123	-0.114	-0.12
	[0.17]	[0.19]	[0.35]	[0.32]	[0.34]
Lawyers or Jurists	-0.059	-0.072	-0.001	-0.007	-0.005
	[0.24]	[0.29]	[0.00]	[0.03]	[0.02]
Merchants	0.122	0.122	0.149	0.135	0.156
	[0.46]	[0.46]	[0.57]	[0.51]	[0.59]
Relatives in parliament	-0.127	-0.126	-0.121	-0.12	-0.12
	[0.76]	[0.75]	[0.72]	[0.72]	[0.72]
Constant	-0.245	-0.234	-0.447	-0.412	-0.471
	[0.75]	[0.73]	[1.44]	[1.32]	[1.51]
Observations	461	461	461	461	461

Notes: Robust z-statistics clustered at the constituency level in brackets; \* significant at 10%; \*\* significant at 5%; \*\*\* significant at 1%.

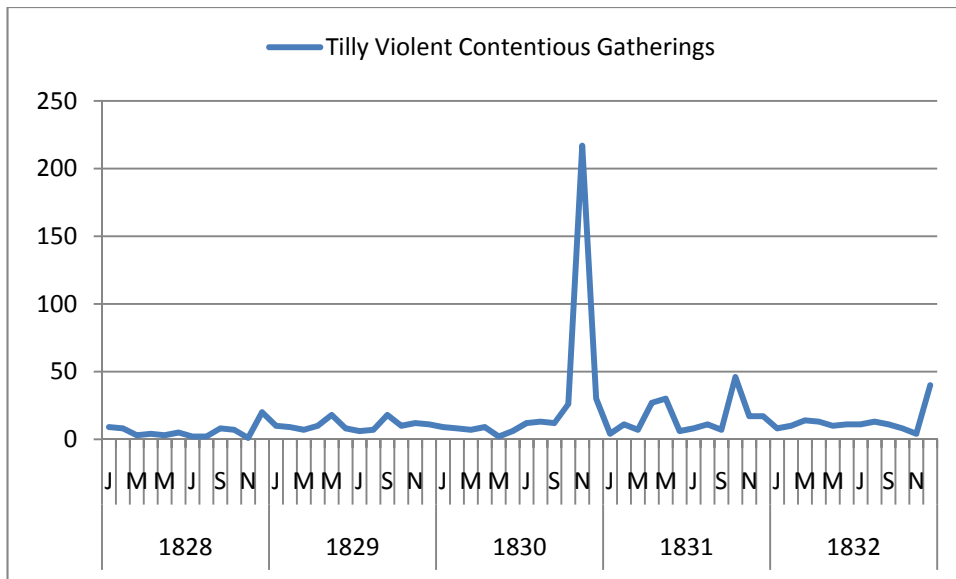


Figure 2: The distribution of the Swing Riots over time, 1830-1831.



Source: Hobsbawn and Rudé (1973) and Tilly (1995).

Figure 3: The distribution of violent contentious gatherings, 1828-1832.



Source: Tilly (1995).

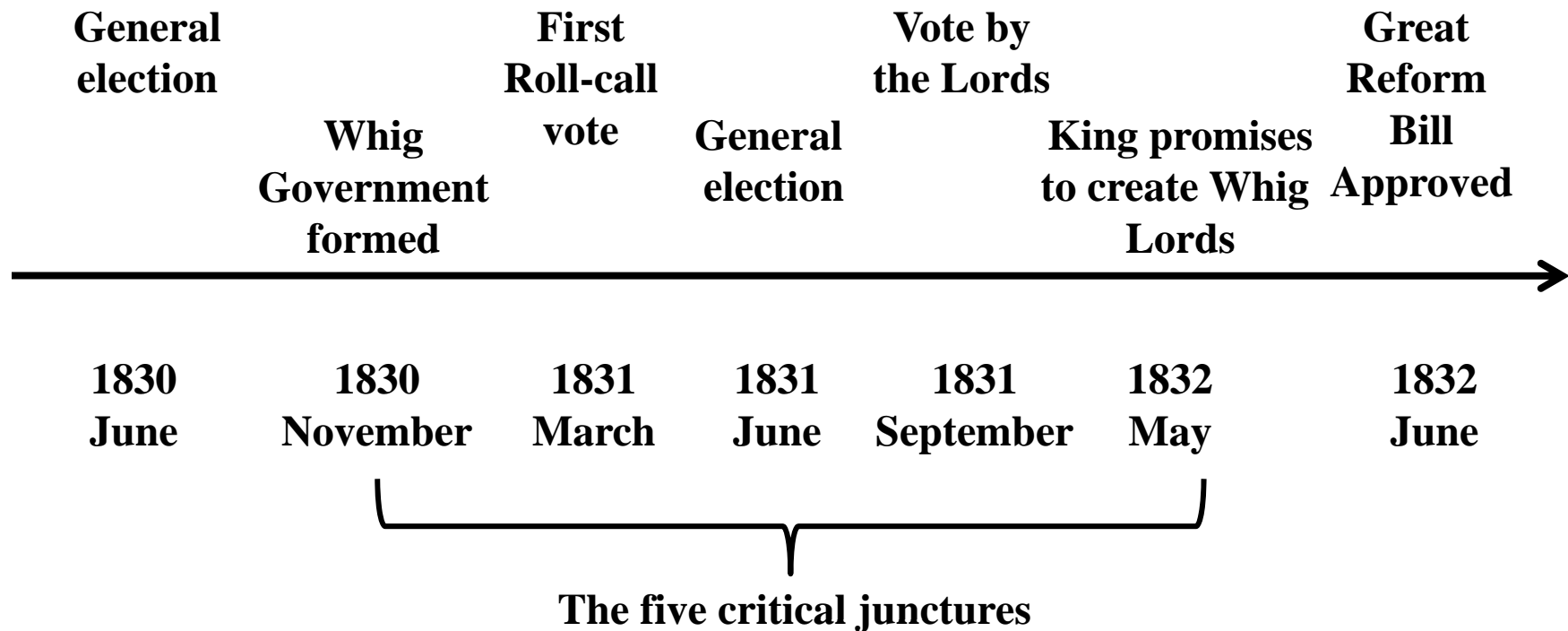


Table A1: Descriptive statistics for the study of the 1831 general election

<b>Name</b>	<b>Observation</b>	<b>Mean</b>	<b>St. dev</b>	<b>Minimum</b>	<b>Maximum</b>
<i>Cross section</i>					
Reformer	489	0.54	0.50	0	1
Threat, Swing H&R	489	56.22	71.10	0	208
Threat, Swing Tilly	489	12.50	18.86	0	54
Threat, Violent CGs, Tilly	489	26.62	50.93	0	353
Threat, Total CGs, Tilly	489	91.97	130.63	2	914
Whig elected in 1826	489	0.37	0.48	0	1
News exposure index	489	1.95	2.24	0.56	16.50
Close constituency	489	0.76	0.43	0	1
Narrow franchise	489	0.24	0.43	0	1
County	489	0.17	0.38	0	1
Rotten borough	489	0.23	0.42	0	1
Large landowners	489	0.025	0.030	0	0.25
Small landowners	489	0.014	0.020	0	0.14
Agricultural workers	489	0.15	0.15	0	0.77
Industry workers	489	0.043	0.092	0	0.50
Artisans	489	0.33	0.11	0	0.87
Professionals	489	0.057	0.051	0	0.73
Thriving economy	489	0.18	0.38	0	1
Declining economy	489	0.21	0.41	0	1
<i>Panel data</i>					
Index of reform support, 1826	244	0.37	0.38	0	1
Index of reform support, 1830	244	0.38	0.38	0	1
Index of reform support, 1831	244	.55	0.45	0	1
Threat (H&R)	244	56.22	71.10	0	208
Threat (H&R)*close constituency	244	45.54	68.81	0	208
Threat (Tilly)	244	12.50	18.86	0	54
Threat (Tilly)*close constituency	244	10.25	17.92	0	54
News	244	540	581	160	4702
<i>IV estimates</i>					
Riots, outside county	244	17.15	7.57	6.08	48.81

Table A1: Descriptive statistics for the study of the March 22 1831 roll call vote (the English seats, only).

<b>Name</b>	<b>Observation</b>	<b>Mean</b>	<b>St. dev</b>	<b>Minimum</b>	<b>Maximum</b>
Majority	461	0.48	0.50	0	1
Threat, Swing H&R	461	57.33	71.96	0	208
Threat, Swing Tilly	461	12.72	19.07	0	54
Threat, Total CGs, Tilly	461	82.43	120.66	1	823
Threat, Violent CGs, Tilly	461	26.11	50.23	0	338
Threat, Violent CGs, Tilly	461	56.32	73.74	1	485
News exposure index	461	1.95	2.24	0.56	16.50
Close constituency	461	0.76	0.42	0	1
Narrow franchise	461	0.23	0.42	0	1
County	461	0.18	0.38	0	1
Rotten borough	461	0.22	0.41	0	1
Whig or Radical	461	0.38	0.49	0	1
Army officers	461	0.19	0.39	0	1
Bankers	461	0.061	0.24	0	1
Dynasty heir	461	0.42	0.49	0	1
Industrialist	461	0.054	0.23	0	1
Lawyers or Jurists	461	0.12	0.32	0	1
Merchants	461	0.093	0.29	0	1
Relatives in parliament	461	0.60	0.49	0	1
Thriving economy	461	0.18	0.39	0	1
Declining economy	461	0.20	0.40	0	1



**Figure 1: The timeline of the main events as the bill passes through the political process.**