

## DEMOCRATIZATION UNDER THE THREAT OF REVOLUTION: EVIDENCE FROM THE GREAT REFORM ACT OF 1832

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We examine the link between the threat of violence and democratization in the context of the Great Reform Act passed by the British Parliament in 1832. We geo-reference the so-called Swing riots, which occurred between the 1830 and 1831 parliamentary elections, and compute the number of these riots that happened within a 10 km radius of the 244 English constituencies. Our empirical analysis relates this constituency-specific measure of the threat perceptions held by the 344,000 voters in the Unreformed Parliament to the share of seats won in each constituency by pro-reform politicians in 1831. We find that the Swing riots induced voters to vote for pro-reform politicians after experiencing first-hand the violence of the riots.

KEYWORDS: Democratization, franchise extension, threat of revolution, Great Reform Act of 1832.

### 1. INTRODUCTION

WHY WOULD AUTOCRATIC ELITES ACCEPT democratic change and share power with segments of the population whose goals they oppose? To answer this question, Acemoglu and Robinson (2000, 2001) developed a theory that shows how democratic reforms, for example, the extension of the voting franchise, can be a rational response to a temporary threat of revolution.<sup>2</sup> This is because the autocratic elites can credibly commit to democratic reform while

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<sup>2</sup>While the idea that democratic reform is a response to a threat of revolution is implicit in many studies (e.g., Maehl (1967) and Tilly (1993, 2004)), Acemoglu and Robinson (2000, 2001) turned it into a theory of democratization which emphasizes the commitment value of democratic reform. Other aspects of the link between the threat of revolution and democratic reform were explored by, for example, Acemoglu and Robinson (2006), Boix (2003), Conley and Temimi (2001), Justman and Gradstein (1999), Ellis and Fender (2011), and Dorsch and Maarek (2015). For broader perspectives on democratization, see, for example, Congleton (2011), Robinson (2006), and Ziblatt (2006, 2008).

they are expected to renege on policy concessions once the threat has passed. In practice, those who seek to overthrow the status quo face many hurdles in mobilizing and coordinating popular support and they do not pose a permanent threat to the autocratic elites. Widespread protests and rioting, however, constitute a credible signal to the autocratic elites that these hurdles have been temporarily overcome: those who riot could, either on their own or in conjunction with members of other groups, trigger more costly unrest, and possibly even a full-fledged revolution. This, in turn, may motivate the autocratic elites to implement democratic reforms to preempt such an escalation. The “threat of revolution” theory, therefore, predicts a causal chain running from observed riots to the elites’ perceptions of the threat of revolution and the eventual adoption of democratic reform. An econometric test of this causal chain is challenging because it involves estimating the link between threat perceptions and the actions leading to democratic change induced by those perceptions. As a result, empirical research usually tests the theory indirectly, either by relating economic shocks to democratic change (Burke and Leigh (2010), Brücker and Ciccone (2011), Chaney (2013), Franck (2015), Aidt and Leon (2015)) or by assessing whether the immediate consequences of democratization can credibly result from a potential threat of revolution (Berlinski and Dewan (2011), Turner and Zhan (2012), Dasgupta and Ziblatt (2015)).

We examine the causal effect of the threat of revolution on democratic change by exploring the impact of plausibly exogenous variation in threat perceptions on constituency-level support for democratization in the context of the Great Reform Act of 1832. This act was the first of several reforms on Great Britain’s long journey to universal and equal suffrage. It was adopted by a House of Commons elected under the rules of the Unreformed Parliament which had not been modified in nearly two centuries before 1832. The act is often viewed as “a major turning point in English history” (Maehl (1967, p. 1)). It arguably triggered important economic reforms in the decades that followed, for example, the reform of municipal government in 1835, the personal income tax in 1842, and the repeal of the Corn Laws in 1846.

The Great Reform Act is particularly well suited for such a study. On the one hand, the last general election held under the rules of the Unreformed Parliament in April–June 1831 was effectively a referendum on the bill. This is because parliamentary reform had become a “party” question, with the Whigs and Radicals supporting the bill and the overwhelming majority of the Tories opposing it. The election resulted in a solid Whig majority in the House of Commons, which was necessary for the reform’s ultimate success. We can, therefore, evaluate the support for parliamentary reform among voters and patrons of the 244 English constituencies by measuring the electoral success of the Whigs and Radicals relative to previous elections. On the other hand, between August 1830 and the spring of 1831, there was a spike in social unrest across England known as the Swing riots (Hobsbawm and Rudé (1973),

Tilly (1995)). These riots were an uprising of agricultural laborers in the English countryside. The rural aspect of the riots matters because the typical parliamentary constituency before 1832 was a small market town or sometime simply a village embedded in the local agricultural economy. The Swing riots, therefore, gave the voters and patrons of the constituencies located in the affected areas first-hand experience with an uprising of unprecedented scale. We quantify the scale of local riots at the constituency level by geo-referencing the 2818 Swing riots recorded by Holland (2005) and assessing the number of riots within a 10 km radius of each constituency.

The timing and geography of the Swing riots suggest that they can be used to construct exogenous constituency-specific measures of threat perceptions. First, the peak of the Swing riots occurred between the July–September 1830 general election, which led to a change in government from the Tories to the Whigs, and the April–June 1831 general election.<sup>3</sup> We can, therefore, condition on the pre-existing level of support for the Whig party in each constituency. This potentially removes factors that would otherwise confound the relationship between the Swing riots in the vicinity of a constituency and the electoral success of the Whigs in the 1831 election. Second, Charlesworth's (1979) detailed analysis of the spatial and temporal distribution of the Swing riots establishes that they did not spread randomly after they broke out in Kent in August 1830. Instead, they spread systematically along a route that closely followed the pre-existing road network. This suggests that the riot dynamics was driven by local social interaction effects: agricultural workers willing to revolt were encouraged to do so by observing others engaged in rioting but with diminishing strength as the distance from the epicenter of the riots increased.<sup>4</sup> This feature suggests that we can use the travel-time distance between each constituency and Sevenoaks, the village in Kent where the first three Swing riots occurred on 3 August 1830, as an instrument for the number of riots that happened in the vicinity of each constituency. For this purpose, we construct an electronic map of the road network in England in 1830 to estimate travel-time distances.

Our empirical strategy relates the number of Swing riots that happened within a 10 km radius of each constituency to the share of seats won by the reform-friendly Whigs and Radicals in 1831, conditional on the share that they obtained in the previous election. We estimate this relationship with least squares, probit, matching, reduced form, and instrumental variable estimators. We complement these estimations with falsification tests that explore potential

<sup>3</sup>Unlike modern elections, polling did not take place on the same day in all constituencies, and within a constituency, polling was held over several days.

<sup>4</sup>Kuran (1989) provided a convincing theory as to why potential rioters are induced to riot. Individuals can hide their willingness to riot as long as they perceive that not many others are willing to join in. A slight surge in the apparent size of the group of rioters, caused by events which are insignificant in and of themselves, can trigger a riot cascade.

links between the Swing riots, the geography of other waves of riots, and the election results before and after the Great Reform Act.

The results suggest that voters and patrons who in previous elections had not supported the Whigs and Radicals were led to vote for the candidates of these parties and thus for parliamentary reform after experiencing first-hand the violence of the Swing riots. Our least squares estimates suggest that the Swing riots can explain about half of the actual increase between 1830 and 1831 in the average share of Whigs and Radicals elected to the House of Commons. The matching and instrumental variable estimates are two to five times as large as the least squares estimates. They can, therefore, explain the entire difference in the share of Whigs elected in 1830 and 1831. Moreover, we use the methodology of [Altonji, Taber, and Elder \(2005\)](#) to evaluate the extent of selection bias. We find that selection on unobservables would have to be two and a half times as important as selection on observables for our least squares results to be entirely attributed to a selection bias. Finally, our main falsification test shows that local Swing riots cannot predict the outcome of the 1830 election which was held before the peak of the riots. These results are consistent with the “threat of revolution” theory of democratic change.

The rest of the paper is organized as follows. Section 2 presents the relevant historical background. Section 3 presents our data. Section 4 discusses our empirical design and identification strategy. Section 5 presents our results and additional falsification tests and evaluates alternative explanations of the outcome of the 1831 election. Section 6 concludes. The Supplemental Material ([Aidt and Franck \(2015\)](#)) contains detailed information on the sources and coding of the data, supplementary tables (S1 to S28) providing robustness checks and falsification tests, as well as a detailed discussion of the alternative explanations of the Whig victory in 1831.

## 2. THE GREAT REFORM ACT OF 1832 AND THE SWING RIOTS

This section provides information on the political and economic situation in England before 1832, on the Great Reform Act, and on the Swing riots. It highlights aspects of the historical context that justify our empirical strategy and relieves potential endogeneity concerns.

### 2.1. *Constituencies, Parties, and Voters Under the Unreformed Parliament*

The rules of the Unreformed Parliament which governed the elections to the House of Commons until 1832 had not been modified in nearly two centuries. In England, there were 244 constituencies: 40 county, 202 borough, and two university constituencies. Each constituency returned, with a few exceptions,

two MPs, for a total of 489 English Members of Parliament (MPs).<sup>5</sup> The right to vote varied from constituency to constituency. It was usually limited and, at most, three percent of the English population could vote. Many borough constituencies had been established in the Middle Ages and had few inhabitants left by 1830. In the elections held before the Great Reform Act, patronage and electoral corruption were common. In the “close constituencies,” patrons—large landowners, wealthy commoners, or even the ruling government through its influence on the Treasury—effectively nominated the MPs and elections were uncontested. In the “open constituencies,” elections were more competitive and the voters could exhibit some political independence, but electoral corruption, fostered by the absence of secret voting, was widespread.<sup>6</sup>

To fully understand the logic behind our empirical strategy, three further facts about the Unreformed Parliament must be emphasized. First, the geography of political representation was strongly biased in favor of the countryside. With the exception of Liverpool, the large expanding industrial cities outside London were not represented. The voters in the county constituencies were mainly tenant farmers and many landowners extended a dominant influence through patronage. The typical borough constituency was a small market town which provided services to the surrounding agricultural economy. The borough voters were not just recruited from the local elites, but also from lower-middle class town dwellers. A useful metaphor is to think of the parliamentary boroughs as small islands scattered around the English countryside.<sup>7</sup>

Second, the agricultural economy in Great Britain around 1830 consisted of three social strata: the large landowners, the tenant farmers, and the agricultural laborers. The tenant farmers rented relatively large pieces of land on long-term contracts from the large landowners and employed agricultural laborers on wage contracts, typically of short duration. The agricultural laborers did not own or rent land and were, therefore, not self-sufficient farmers. They did not possess the right to vote before 1832 and did not obtain it with the Great Reform Act. It was to this social stratum of casual agricultural workers that the Swing rioters belonged and much of their anger was directed at the other two strata.

Third, our empirical strategy uses the party affiliation of the MP(s) elected in each constituency to measure the support for reform amongst the voters and patrons in that location. One concern with this strategy is that party affiliation may capture partisan divisions other than on the question of parliamentary reform, for example, on economic or religious issues. To evaluate this concern,

<sup>5</sup>Yorkshire and the City of London returned four MPs, while the boroughs of Abingdon, Banbury, Bewdley, Higham Ferrers, and Monmouth only returned one MP. Map S1 in the Supplemental Material shows the county borders in 1831.

<sup>6</sup>Voting became secret with the Secret Ballot Act in 1872.

<sup>7</sup>Map S2 in the Supplemental Material shows the approximate borders of the parliamentary boroughs of Colchester, Harwich, and Maldon in the county of Essex.

it is important to note that political parties in the modern sense did not exist in the Unreformed Parliament. There were, however, loose and shifting factions with parliamentary leaders, core followers, and occasional supporters, that bore the names of Whigs, Radicals, and Tories (Hill (1996), Jupp (1998)). The Whig and Tory MPs made up the dominant factions in the House of Commons and they overwhelmingly belonged to an oligarchy of landowners, businessmen, lawyers, and career soldiers. They, therefore, had nothing in common with the agricultural workers involved in the Swing riots.<sup>8</sup> Before and after 1832, the Tories were more supportive of the King's role in politics and more likely to defend the privileges of the Anglican Church than the Whigs and the Radicals (Gash (1965)). The lack of formal party structures, however, meant that political issues often cut across parties or united them. On the one hand, members of both factions were split on the questions of abolishing slavery and on giving political and civil rights to Catholics (known as Catholic relief). On the other hand, because of their common economic interests, Tories and Whigs agreed upon the necessity of protecting private property and the need for poor law reform (Halévy (1923, Vol. 3, Part 1, Chapter 2)). They also agreed that social unrest should be repressed by force, and this explains why the repression of the Swing riots carried out by the Whigs was tacitly supported by the Tories (Archer (2000), Stevenson (1992)).

However, the divisions on the question of parliamentary reform closely followed party lines. The Whigs, or at least Earl Grey and the other Whig leaders in Parliament, favored limited parliamentary reform. Long before the Swing riots, they were committed to give voting rights to the "respectable" sections of society, that is, property owners, and to give representation to the expanding industrial cities. They were motivated partly by ideology and partly by self-interest because they reasoned that these sections would naturally support the rule of the aristocratic Whigs.<sup>9</sup> The Whigs did not support far-reaching political reforms and regarded the Radicals, who advocated universal male suffrage and the secret ballot, as "dangerous doctrinaires" (Newbould (1990)). Although the six to ten Radical MPs in the 1830 and 1831 Parliaments favored more fundamental reform, they steadfastly voted with the Whigs on the reform bill. The Tory leadership, in contrast, did not support any measure of parliamentary reform. It is on this basis that we find it justified to use the party affiliation of the MP(s) elected in each constituency to measure the support for reform amongst the voters and patrons in that location.

<sup>8</sup>The Whigs never sought the support of the agricultural workers for electoral purposes or extra-parliamentary agitation, before and after the Great Reform Act. For instance when Henry Brougham, one of the Whig leaders, ran for election in Knaresborough and then in Yorkshire in 1830, his campaign sought the support of tradesmen (Hay (2005)).

<sup>9</sup>Parliamentary reform is one example of the Whigs' paternalistic and aristocratic mindset. It also led them to promote literacy among the masses because they thought that education was a necessary condition for a stable society based on private ownership that they would naturally govern. In so doing, they followed an idea then called, and often derided as, "the March of the Mind" (Mitchell (2005)).

## 2.2. *The Chronology of the Great Reform Act*

The Great Reform Act of 1832 was the first of several 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 two-fold. First, it redistributed parliamentary seats from the “rotten boroughs” to the large and fast growing industrial cities and to the counties. Second, it granted voting rights to resident householders with property valued at ten pounds or more per year in the boroughs and to fifty-pound renters in the counties.<sup>10</sup>

The political process that culminated in the Great Reform Act lasted two years and it could have failed at a number of hurdles (Butler (1914), Brock (1973), Cannon (1973)). The first hurdle was the formation of the Whig government under the leadership of Earl Grey in November 1830 after the general election in July–September of that year. This was the first reform-friendly government in 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 hurdle was the vote in the House of Commons on 22 March 1831. The Whigs and the Radicals supported the bill that was opposed by the Tories and eventually approved by the slightest of majorities: 302 in favor and 301 against (Aidt and Franck (2013, Table 3)).

Earl Grey was keenly aware that a one-vote majority 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 April–June 1831—the third hurdle—was effectively a referendum on parliamentary reform. Many Tories were not returned and the election result gave the Whigs the majority which they needed to pursue the reform. Figure 1 shows the shift in the support for the Whigs between 1830 and 1831 across the 244 English constituencies.

The fourth hurdle occurred in September 1831 when the House of Lords rejected the bill. This was followed by months of negotiations between Earl Grey’s government, the Lords, and the King. But without a solution, Earl Grey submitted his resignation as Prime Minister to the King in May 1832. This triggered a period of social unrest in the industrial and urban areas of the Midlands and of the North of England called the Days of May.<sup>11</sup> In the wake of

<sup>10</sup>It is difficult to estimate the size of the electorate before 1832. Cannon (1973) estimated it to be around 344,000 in 1831, but this estimate only counts people who actually voted. O’Gorman (1986) assumed that 20 to 30 percent of voters abstained and he estimated the electorate under the Unreformed Parliament to be between 410,000 and 450,000 individuals. The Great Reform Act increased the electorate to somewhere between 620,000 and 800,000. The effect of the reform, therefore, was to increase the size of the electorate from 2–3 percent to 4–6 percent of the total English population (14 millions).

<sup>11</sup>There is some evidence that the Days of May riots were supported, if not entirely organized, by organizations of middle-class activists in favor of parliamentary reform, such as the Birmingham Political Union led by Thomas Atwood, and organizations of urban artisans, such as Francis Place’s Chartist movement (Archer (2000)).

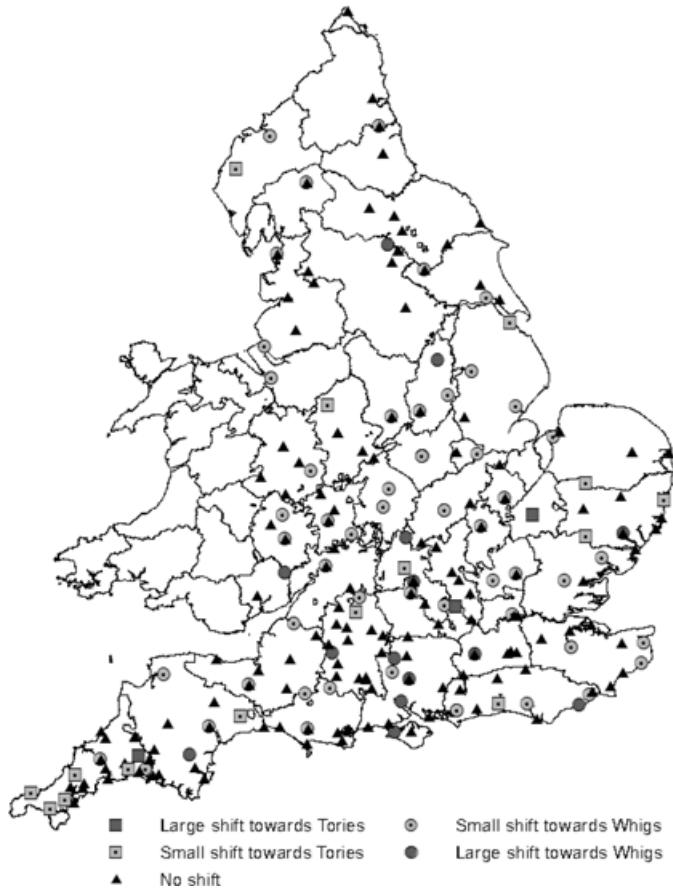


FIGURE 1.—The shift in the support for the Whigs and the Tories between 1830 and 1831 across the 244 English constituencies. *Source:* Aidt and Franck (2013).

these urban riots, King William IV aborted the formation of a new Tory government headed by the Duke of Wellington and promised Earl Grey to create, if necessary, new Lords to pass the bill. The bill then overcame the fifth and last hurdle since the Tory opposition did not wish to lose their majority in the House of Lords. The bill finally received royal assent on 7 June 1832, nearly two years after it was first introduced.

The reform bill could have failed at each of these five hurdles. Our data and empirical strategy, however, only allow us to study the 1831 general election. We use the electoral success of reform-friendly Whig and Radical politicians in that election relative to previous ones to evaluate (the change in) the reform attitude of voters and patrons in the 244 English constituencies.



### 2.3. *The Swing Riots*

The Swing riots 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 (many of whom were parliamentary voters or patrons).<sup>12</sup> The rioters were landless agricultural laborers and the riots were characterized by either a high degree of violence or a threat of violence. Common tactics included arson, the burning of farm houses and haystacks, the destruction of threshing machines owned by tenant farmers, as well as outright extortion of money and provisions from farmers, landowners, and parsons (Tilly (1995)).

Our empirical test is based on the assumption that the Swing riots were not an organized political movement with clear political aims related to parliamentary reform. This is important because this is what allows us to establish a link between local exposure to Swing riots and change in reform support at the constituency level. If, in contrast, the riots were systematically masterminded by national or local reform supporters, it would be harder to establish this link because riots would naturally tend to be concentrated in areas with strong underlying support for reform or in places where national political leaders would see a strategic advantage in organizing them. The available historical evidence supports our assumption. First, the uncoordinated violence of the Swing riots contrasted with the tactics of the emerging trade unions in urban areas that combined targeted aggressive actions with peaceful strikes (Stevenson (1992)). Second, the Swing riots were not fomented by urban artisans organized in the Chartist movement or by organizations of middle-class activists such as Thomas Attwood's Birmingham Political Union (Halévy (1923, Vol. 3)). Third, it is not even clear whether the meetings held by Radical politician William Cobbett in the Southeast of England or by several Radical orators in London after the outbreak of the Swing riots had any impact on the continuation of the violence (see, e.g., Halévy (1923, Vol. 3), Dyck (1992), Royle (2000, p. 85)).<sup>13</sup> Finally, there was no union of agricultural laborers at that time which could have taken an active role in organizing the riots.<sup>14</sup>

<sup>12</sup>Halévy (1923, Vol. 3) relied on William Cobbett's publication *The Political Register* (13 July 1833, 81, p. 72) to suggest that the word "Swing" refers to the swinging stick of the flail in agricultural parlance. Only a few authors of the Swing letters are known. Those who were identified include a straw plait manufacturer in Suffolk, a weaver in Norfolk, and a hostler in Hampshire, and they independently wrote letters signed as Captain Swing (Archer (2000, p. 17)).

<sup>13</sup>William Cobbett was brought to trial in July 1831, before the Swing riots began, for publishing a libel to incite laborers to violence, but was acquitted (Briggs (1967, Chapter 7)). However, the radical orator Richard Carlile was prosecuted for sedition and sentenced to two years in prison for issuing an "Address to the Insurgent Labourers" (Royle (2000, p. 85)).

<sup>14</sup>The parliamentary reforms in 1832, 1867, and 1884–1885 were all preceded by increased social agitation. By the 1860s, the organizational capacity of special interests seeking reform (e.g., reform unions and factions within the main political parties) had improved sufficiently to provide leadership to social protest. For example, the Hyde Park riots started as a political meeting orga-

It is, therefore, more likely, as suggested by Hobsbawm and Rudé (1973), Tilly (1995), Archer (2000), and Royle (2000), that the riots were caused by harsh rural socio-economic deprivation in general, and by the poor harvest in 1828–1829 in particular. By 1830, the generally deteriorating conditions of farm labor were compounded by agricultural mechanization and this explains why the rioters often destroyed threshing machines. This is not to deny that some rioters might have had political demands which they did not articulate very well, that local grassroots radicals in rural areas might have created awareness amongst the rioters about parliamentary reform and helped organize riots, and that urban radicals made attempts to capitalize on the rural unrest (Charlesworth (1983, p. 153), Royle (2000, p. 85)).<sup>15</sup> Even so, the historical facts do not suggest that the Swing riots were an organized movement with well-defined political aims and there is little evidence regarding the direct involvement of the rioters in political associations.<sup>16</sup>

The riots began with three disturbances in the village of Sevenoaks in Kent on 3 August 1830, peaked in intensity in November 1830, and ended in the spring of 1831 (Holland (2005)). We observe from Figure 2, which shows the monthly number of Swing riots, that the riots occurred after the polling period for the 1830 election and before the polling period for the 1831 election. Figure 3 shows the spatial distribution of the riots. They spread from Kent to other counties, such as Sussex, Hampshire, and Wiltshire in the South and West, and engulfed East Anglia as well as parts of the Midlands. The North (e.g., Yorkshire) and the most-Western part of the country (e.g., Cornwall and Devon) were relatively unaffected. Charlesworth (1979) documented how the riots spread in a systematic manner following the road network and provoked new riots along the way. It is, therefore, very unlikely that the Swing riots occurred primarily in areas where rioters expected to face little or no retaliation, especially given the historical patterns of riot repression in England (Bohstedt (2010), Stevenson (1992)).<sup>17</sup> It is much more likely that the spatial distribution was a result of local social interaction or imitation effects (Kuran (1989)).

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nized by the Reform League before turning into a major confrontation with the police. Similarly, the 1884 “Riots of the Field” were organized by Radical Party and Liberal Party leaders to put pressure on their own Whig party allies to radicalize (Hayes (1982)). Hence, unlike in 1830–1831, it is clear that the riots in 1867 and 1884 were encouraged by organized groups to influence the government’s actions.

<sup>15</sup>For instance, the radical-leaning newspaper, *The Examiner*, reported in its October 17th, 1830 issue that village radicals incited agricultural workers to riot.

<sup>16</sup>When the Royal Commission on the Poor Laws in 1834 investigated the causes of the Swing Riots, it attributed 300 of 410 cases to low wages and unemployment and the remaining 110 cases to agitators or to the presence of beer shops within the parish (Hobsbawm and Rudé (1973, pp. 57–59)).

<sup>17</sup>A local newspaper in Cornwall, *The Royal Cornwall Gazette*, published on December 18th, 1830 a plea from a curate to stop the riots, which nonetheless included some veiled threats of punishments, but this did not stop rioters. The Swing riots were eventually suppressed by troops under orders from the Parliament and not by local militia, which were overwhelmed by the violence (Archer (2000)).

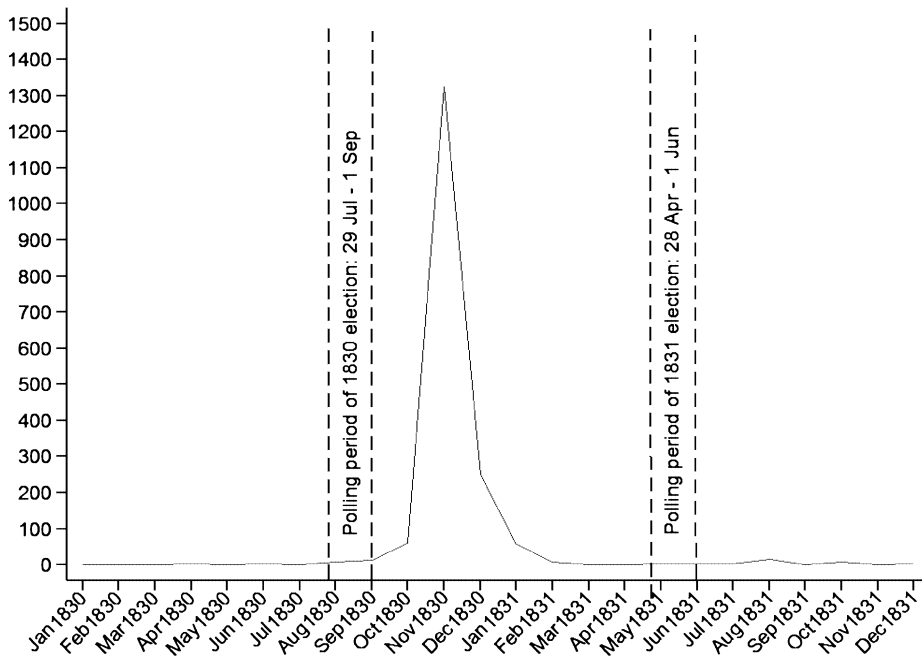


FIGURE 2.—The monthly number of Swing riots and the timing of the 1830 and 1831 general elections. *Source:* Holland (2005).

Given the location of the 244 English constituencies (see Figure 1), it is apparent that the voters and patrons in some constituencies were more exposed to Swing riots in the surrounding countryside than others. Our empirical strategy takes advantage of this feature to construct constituency-specific measures of riot exposure which we can relate to the constituency-specific measure of support for the Whigs and Radicals amongst voters and patrons in the 1831 election.

#### 2.4. *Fear of Revolution in England*

The Swing riots must be viewed in the larger British and European context. First, the members of the British elite had feared, since the 1789 French revolution, a “democratic” revolution that would overthrow the monarchy and the aristocracy. They dreaded the revolt of peasants and urban artisans which “Jacobins” and “demagogues” would take control of (see, e.g., Jupp (1998), Clark (2000), Mitchell (2005)).<sup>18</sup> In fact, the general mood of London in the

<sup>18</sup>Even in 1830–1832, the British aristocracy remembered the 1780 Gordon riots in London and the damage that crowds could inflict when led by a skillful orator (De Castro (1926)). The



FIGURE 3.—The spatial distribution of the Swing riots. *Note:* This map shows the intensity and geographic pattern of the Swing riots (August 1830–February 1831). The circles indicate the number of riots within a 10 km radius of each of the 244 English constituencies. *Source:* Holland (2005).

autumn of 1830, as reported in the London newspapers studied by Tilly (1995), reflected a widespread concern regarding a potential joint uprising of the farm laborers and of the workers from the expanding industrial cities that would be led by radical politicians or community leaders.<sup>19</sup> For instance, the Tory-leaning newspaper, *The Morning Post*, published on February 1st, 1831, an anonymous

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Lord Lieutenant of Kent articulated a common concern when he stated in a letter to the Home Office in 1830 that the Swing rioters were “those who wish Revolution in England, in order to create confusion.”

<sup>19</sup>The majority of the political newspapers and periodicals in England supported parliamentary reform (Jupp (1998, Chapter 8)). However, none seemed to have supported a revolution.

letter arguing that Captain Swing was “a Statesman of the first class” who would take advantage of the riots to implement the Radical agenda, and in particular, introduce universal suffrage. It is reasonable to assume that first-hand experience of Swing riots would stoke such fears. Voters and patrons might have thought that a limited reform of parliament could defuse the (perceived) threat to the established order by cutting any bond between radical leaders and the rural and urban poor.

Second, 1830 was a year of political upheavals in Europe. The Swing riots began on 3 August 1830 and it is unlikely that the rioters knew about the 27–29 July 1830 revolution in France that overthrew King Charles X (see Halévy (1935), Gash (1956), and Brock (1973)). Even at the peak of the Swing riots in November 1830, it is unclear whether the rioters had learned about the 1830 French revolution. However, by that time, the members of the informed public around England certainly knew about it and some, for example, in Preston, had seen tricolor flags (a symbol of Revolutionary France) paraded down the high street. As such, the news about the 1830 French revolution, and later on about the independence of Belgium from the Netherlands in November 1830, could have reinforced the impression that a revolution could also take place in England. It is against this background that we argue that first-hand exposure to the Swing riots influenced the voters’ and patrons’ perceptions of a revolutionary threat and that local riots, therefore, can be used as a proxy for “threat perceptions.”

### 3. DATA

This section presents our data. Table I reports descriptive statistics for the main variables. The definitions and sources of these variables are given in the Supplemental Material, which also presents the variables used in the falsification tests.

#### 3.1. *Support for Parliamentary Reform*

We construct two measures of the electoral success of the Whigs and Radicals (henceforth, we use the term “Whigs” to describe both groups). Our main dependent variable is the share of seats won by Whigs in each of the 244 English constituencies (*Whig share 1831*). Our secondary dependent variable is an indicator variable which takes the value 1 if a seat in a constituency, out of the 489 English seats in the House of Commons, was won by a Whig (*Whig elected 1831*).

TABLE I  
DESCRIPTIVE STATISTICS<sup>a</sup>

|   | Obs. | Mean  | Std. Dev. | Minimum | Maximum |
|---|------|-------|-----------|---------|---------|
| <i>Constituency-Level Variation</i>                       |      |       |           |         |         |
| <i>Support for parliamentary reform</i>                   |      |       |           |         |         |
| Whig share 1831   | 244  | 56.6  | 44.7      | 0       | 100     |
| Whig share 1830   | 244  | 42.9  | 37.9      | 0       | 100     |
| <i>Local Swing riots</i>                                  |      |       |           |         |         |
| Riots with 1 km   | 244  | 0.84  | 1.84      | 0       | 11      |
| Riots within 10 km  | 244  | 9.5   | 12.5      | 0       | 71      |
| Riots within 20 km  | 244  | 33.4  | 38.8      | 0       | 234     |
| Riots within 30 km  | 244  | 72.9  | 72.9      | 0       | 349     |
| Riots within 50 km  | 244  | 193   | 168       | 0       | 614     |
| Riots between 50 and 75 km                                | 244  | 199   | 153       | 0       | 562     |
| Riot treatment  | 244  | 0.71  | 0.45      | 0       | 1       |
| <i>Political controls</i>                                 |      |       |           |         |         |
| Whig share 1826   | 244  | 39.2  | 39.4      | 0       | 100     |
| (Whig share 1826) <sup>2</sup>                            | 244  | 3087  | 3892      | 0       | 10,000  |
| Reform support 1830                                       | 244  | -0.03 | 0.55      | -1      | 1       |
| <i>Institutional controls</i>                             |      |       |           |         |         |
| County constituency                                       | 244  | 0.16  | 0.37      | 0       | 1       |
| University constituency                                   | 244  | 0.008 | 0.090     | 0       | 1       |
| Narrow franchise  | 244  | 0.24  | 0.43      | 0       | 1       |
| Patronage index   | 244  | 1.00  | 0.68      | 0       | 2       |
| <i>Employment structure controls</i>                      |      |       |           |         |         |
| Emp. fract. index   | 244  | 0.76  | 0.073     | 0.24    | 0.86    |
| Agriculture (emp. share)                                  | 244  | 0.19  | 0.18      | 0       | 1       |
| Trade (emp. share)  | 244  | 0.37  | 0.13      | 0       | 0.87    |
| Professionals (emp. share)                                | 244  | 0.054 | 0.026     | 0       | 0.14    |
| <i>Demographic and economic controls</i>                  |      |       |           |         |         |
| Population (in 1000s)                                     | 244  | 62.8  | 175       | 0.097   | 1371    |
| Population density  | 244  | 5.56  | 0.83      | 3.92    | 9.79    |
| Thriving economy  | 244  | 0.17  | 0.38      | 0       | 1       |
| Declining economy   | 244  | 0.21  | 0.41      | 0       | 1       |
| <i>Spatial controls</i>                                   |      |       |           |         |         |
| Distance to urban center<br>(travel time in days by foot) | 244  | 30.9  | 15.2      | 0       | 72.8    |
| Connection to London                                      | 244  | 2.27  | 2.41      | 0.68    | 20.4    |
| Market integration  | 244  | 22.4  | 107.2     | 3.81    | 1424    |
| Cereal area   | 244  | 0.48  | 0.50      | 0       | 1       |
| Dairy area  | 244  | 0.30  | 0.46      | 0       | 1       |
| <i>Instrumental variable</i>                              |      |       |           |         |         |
| Distance to Sevenoaks<br>(travel time in days by foot)    | 244  | 7.7   | 4.2       | 0.30    | 19.4    |

(Continues)

TABLE I—*Continued*

|   | Obs. | Mean   | Std. Dev. | Minimum | Maximum |
|---|------|--------|-----------|---------|---------|
| <i>Variables for Placebo tests in Table VII</i> |      |        |           |         |         |
| Whig vote share 1835                            | 115  | 0.55   | 0.21      | 0       | 1       |
| Share of votes for 1867 Reform Bill             | 186  | 0.0026 | 0.027     | 0       | 0.34    |
| <i>Variables for tests in Table VIII</i>        |      |        |           |         |         |
| Petitions against reform                        | 244  | 0.26   | 0.60      | 0       | 4       |
| Petitions for reform                            | 244  | 1.69   | 2.50      | 0       | 31      |
| Petitions against slavery                       | 244  | 2.47   | 3.33      | 0       | 20      |
| Petitions against Catholic relief               | 244  | 1.30   | 2.16      | 0       | 18      |
| Petitions for Catholic relief                   | 244  | 0.62   | 1.14      | 0       | 8       |
| <i>County-Level Variation</i>                   |      |        |           |         |         |
| <i>Variables for Placebo tests in Table VII</i> |      |        |           |         |         |
| Food riots 1800–1818                            | 244  | 10.8   | 9.81      | 0       | 39      |
| Labor riots 1793–1822                           | 244  | 1.49   | 4.08      | 0       | 18      |
| <i>Variables for tests in Table VIII</i>        |      |        |           |         |         |
| Share of harsh sentences                        | 244  | 23.1   | 27.2      | 0       | 100     |
| Special commission                              | 244  | 0.21   | 0.41      | 0       | 1       |
| Growth in poor law expenses (%)                 | 241  | 706    | 323       | 258     | 1580    |
| <i>Seat-Level Variation</i>                     |      |        |           |         |         |
| <i>Support for parliamentary reform</i>         |      |        |           |         |         |
| Whig elected 1831                               | 489  | 0.56   | 0.50      | 0       | 1       |
| Whig elected 1830                               | 489  | 0.43   | 0.50      | 0       | 1       |

<sup>a</sup>For the variables with county-level variation, we attribute the county average to each constituency within that county.

### 3.2. Local Swing Riots

We geo-reference the location of the 2818 Swing riots listed by Holland (2005) and construct two types of measures of local riots.<sup>20</sup> First, the variables *Riots within R km* measure riots on the intensive margin. They record the number of Swing riots that happened within a radius of R kilometers from each constituency, where  $R \in \{1, 10, 20, 30, 50\}$ . In the baseline estimations, we employ the variable *Riots within 10 km* and use the other radiuses as robustness checks. The mean and median of *Riots within 10 km* are 9.4 and 5.5, respectively. The 10 km radius includes the countryside and villages immediately outside the parliamentary borough towns and county seats. It is reasonable to assume that information about riots within such a small neighborhood would reach voters and patrons, for example, via local markets and direct contact with individuals affected by the violence. Second, the indicator variable *Riot treatment* measures riots at the extensive margin. It is equal to 1 if strictly

<sup>20</sup>Holland (2005) added to the catalogue of 1475 riots originally compiled by Hobsbawm and Rudé (1973, Appendix III).

more than one riot occurred within 10 km of a constituency and zero otherwise. Since there were 35 constituencies which were not exposed to any riots within a radius of 10 km and another 35 which were exposed to just one, this cut-off implies that we, in practice, end up with 28 percent of the constituencies below the cut-off of one riot and 72 percent above.

### 3.3. *Characteristics of the Constituencies*

#### *Political Characteristics*

The Swing riots occurred between the 1830 and 1831 elections. To quantify the general support for the Whigs in a constituency prior to these events, we code the variable *Whig share 1826* which measures the share of seats won by a Whig MP in each constituency in the 1826 election.<sup>21</sup> We consider that this is a useful proxy for many unobserved constituency-specific determinants of support for the Whigs. Using a similar logic, we make use of the roll call data on Lord Russell's proposal to give direct representation to Birmingham, Leeds, and Manchester to control for the general attitude toward parliamentary reform in each constituency prior to the Swing riots. The bill was second read and defeated on 23 February 1830 in the House of Commons, almost half a year before the Swing riots started. We construct the variable *Reform support 1830* as the difference between the shares of the MPs from each constituency who supported and opposed the bill.

#### *Institutional Characteristics*

Under the Unreformed Parliament, the rules that defined who held the right to vote varied across the 244 English constituencies (see, e.g., Brock (1973)). The 40 county constituencies had a relatively broad electorate of tenant farmers based on a 40-shilling qualification (*County constituency*). The voters of the two university constituencies were restricted to the graduates from Oxford and Cambridge (*University constituency*). We use information from Philbin (1965) and Brock (1973, Table 2) on the six different voting franchises in the 202 borough constituencies to define the indicator variable *Narrow franchise*. It is equal to 1 for the boroughs with the smallest number of voters (often less than a dozen) and zero for those with a relatively large electorate (but still very small by modern standards).

To account for patronage and electoral corruption, we construct on a scale from zero to 2 the variable *Patronage index* with information from Philbin (1965) and Cannon (1973, Appendix 3). The index is equal to zero if the constituency was neither controlled by a patron nor singled out as one of the "rotten boroughs" to be disenfranchised by the reform bill and held at least

<sup>21</sup>We use the 1826 rather than the 1830 election to make sure that the Swing riots could not have affected the outcome of the election. In practice, it makes no difference.



one contested election between 1802 and 1831. It takes the value 1 if the constituency was wholly or partly controlled by a local patron or by the Treasury, or if it was one of the “rotten boroughs” to be disenfranchised, or if no contested election was held between 1802 and 1831. The index is equal to 2 if a constituency was both controlled by a patron and considered to be rotten.

### *Employment Structure*

We use information from the 1831 Population Census to estimate occupation shares.<sup>22</sup> We distinguish between three main categories of employment: *Agriculture (emp. share)* measures the share of tenant farmers and agricultural laborers; *Trade (emp. share)* measures the share of industry workers, those employed in trade and artisans; *Professionals (emp. share)* measures the share of doctors, lawyers, and other educated individuals, and can be interpreted as a proxy for the size of the middle class. The largest omitted category consists of domestic servants.

### *Demographic and Economic Characteristics*

We measure the size of each constituency by its population (in 1000s) and its population density by the number of inhabitants per house. Moreover, Philbin (1965) singled out the constituencies which were in decline in 1830 and those which were prospering. Based on this, admittedly incomplete, information on medium-term economic trends, we construct two indicator variables, *Thriving economy* and *Declining economy*, to control for the economic circumstances in each constituency. The omitted group is the constituencies which followed the general economic trend of the period.

## 4. EMPIRICAL FRAMEWORK

Our empirical strategy assesses whether spatial variation in the Swing riots that occurred in the neighborhood of each of the 244 English constituencies affected the electoral success of the Whigs in the 1831 election.

### 4.1. *Least Squares and Probit Estimators*

As a baseline, we estimate the following regression model with least squares:

$$(1) \quad (\textit{Whig share 1831})_i = \alpha_0 + f[(\textit{Whig share 1826})_i] \\ + \alpha_1(\textit{Riots with 10 km})_i + X_i\alpha_2 + \varepsilon_i,$$

<sup>22</sup>The Census reports these data at the constituency level for the counties, but not for the boroughs. For the latter, we use the closest geographical unit to approximate, which is usually the parish or the town.

where the dependent variable is the share of seats in constituency  $i$  won by Whigs in 1831, *Riots within 10 km* is the baseline measure of local Swing riots,  $X$  is a vector of observables, and  $\varepsilon$  is a disturbance term. We control flexibly for past Whig support in the constituency by including a polynomial function ( $f$ ) of *Whig share 1826*.<sup>23</sup> We report standard errors that correct for spatial correction in the error structure (Conley (1999)).

The main empirical challenge is to account for the possibility that numerous unobserved aspects of the economic and political environment in the constituencies could be correlated with factors which affected both the electoral success of the Whigs and the number of riots in specific areas. Even with the most comprehensive set of controls, “selection on observables” is likely to fail and the least squares estimate of  $\alpha_1$  in equation (1) will be biased. The direction of the bias is, however, not clear a priori. If riots were more widespread in places where voters and patrons were more willing to support the Whigs for reasons which we do not observe, then the bias would be positive, but if they were more widespread in Tory-leaning places, it would be negative. Moreover, attenuation bias drives the coefficient estimate toward zero. To assess the size of the omitted variable bias, we use the approach of Altonji, Taber, and Elder (2005).

As an alternative to the constituency level analysis in equation (1), we estimate the following binary choice model on the sample of the 489 English seats in the House of Commons with a probit estimator:

$$\begin{aligned}
 (2) \quad & \Pr((Whig\ elected\ 1831)_{ji} = 1 | (Riots\ within\ 10\ km)_i, \\
 & f[(Whig\ share\ 1826)_i], X_i) \\
 & = \Phi(\beta_0 + f[(Whig\ share\ 1826)_i] \\
 & \quad + \beta_1(Riots\ within\ 10\ km)_i + X_i\beta_2),
 \end{aligned}$$

where the dependent variable takes the value 1 if a Whig candidate was elected to seat  $j$  in constituency  $i$  in 1831 and  $\Phi$  is the cumulative distribution function of the standard normal distribution. We cluster the standard errors at the constituency level.

#### 4.2. Matching Estimators

Based on the coding of the variable *Riot treatment*, we divide the constituencies into 172 treated constituencies with strictly more than one riot within a radius of 10 km and 70 control constituencies with either no or one riot within

<sup>23</sup>A special case of equation (1) is a regression model where the dependent variable is the change in the Whig share between 1826 and 1831. Equation (1) is preferable because it is more flexible.

that radius.<sup>24</sup> The two subsamples are, on average, similar in terms of past Whig and reform support as well as in terms of their economic, demographic, and employment related characteristics. The only significant difference between the two groups is that *Patronage index* is higher in the control group (see Table S1 in the Supplemental Material). This is because many of the small, declining, corrupt constituencies established in the Middle Ages were in Cornwall, which was relatively unaffected by the Swing riots.

The aim of our matching strategy is to estimate the impact of the local Swing riots on Whig support amongst those constituencies which were exposed to riots, that is, the average effect of the treatment on the treated (ATT). This requires that two conditions are met. First, Whig support in the control constituencies is independent of the Swing riots given the vector of observable factors used to match treatment and control constituencies. Second, each treated constituency has a potential counterpart amongst the control constituencies with similar observable characteristics. The choice of these observable factors thus involves a trade-off: more variables make the “selection on observables” assumption more plausible, but make it harder to ensure common support. Since the observable factors from Section 3.3 are likely to simultaneously affect the exposure to the local Swing riots and Whig support while being predetermined at the time of the disturbances, we match on all rather than on a subset of these observable factors. We use the nearest-neighbor covariate matching estimators, developed by Abadie and Imbens (2006) and Abadie, Drukker, Leber Herr, and Imbens (2004), and the propensity score matching estimators, developed by Rosenbaum and Rubin (1983). The propensity score matching estimators are implemented with a variety of different matching algorithms (nearest-neighbor, radius, kernel, or stratification). These algorithms tackle the trade-off between the quality (bias) and the quantity (efficiency) of matches in different ways.

#### 4.3. Instrumental Variable Estimators

We also estimate equations (1) and (2) with instrumental variable estimators. Our instrument is motivated by Charlesworth’s (1979) study of the geography of the Swing riots. He shows that after the riots started in Kent in August 1830, they spread systematically along the main road network. This diffusion pattern suggests that the farm laborers who were ready to protest against poor economic conditions learned about rioting elsewhere through direct observation or verbal reports by individuals travelling through the affected areas, and not through newspapers or nationally orchestrated political agitation. This contagion dynamics is plausibly exogenous to the geographical pattern of reform

<sup>24</sup>The two university constituencies both belong to the treatment group. This makes it impossible to find a good match and we exclude them from the sample.

support because it is defined by physical geography and by the pre-existing road network.

We use the distance between the starting point of the riots and each constituency to instrument for the number of Swing riots that eventually occurred in the neighborhood of each constituency. We consider the starting point to be the village of Sevenoaks in Kent where the first three Swing riots are said to have taken place on 3 August 1830 (Holland (2005)).<sup>25</sup> To enable accurate calculations of travel-time distances along the road network between Sevenoaks and the centroid of each constituency, we digitalize the map series by John Cary of England in the early 1830s and measure our instrument *Distance to Sevenoaks* in days of travel by foot at the average speed of 30 km per day.<sup>26</sup>

To generate the required exogenous variation in local riots, the instrument should satisfy the independence condition and the exclusion restriction, and, in addition, it should be strongly correlated with *Riots within 10 km*. The independence condition is that the travel-time distance between a constituency and Sevenoaks cannot be a function of the Whig success (or lack thereof) in the 1831 general election in that constituency. This condition is likely to be satisfied because the road network, and hence the travel-time distance to Sevenoaks, cannot respond to the electoral success of specific candidates in particular places in the short run.<sup>27</sup>

The exclusion restriction requires that the instrument only operates through a single known causal channel, that is, the number of Swing riots in the neighborhood of a constituency. The condition may, however, fail for at least three reasons. First, *Distance to Sevenoaks* could be correlated with unobserved time-invariant aspects of the political geography that also determine shifts in the electoral success of the Whigs in 1831. Below we present placebo tests related to the 1830 election that suggest that this is unlikely to be the case.

Second, between August 1830 and April 1831, the English constituencies could have been affected by unobserved region-specific shocks. These shocks could have induced systematic shifts in the electoral support for the Whigs that were correlated with the spatial distribution of the Swing riots. While we cannot entirely rule out this possibility, we stress that we are unaware of any

<sup>25</sup>It does not matter for our results which location in Kent we use as the starting point for the Swing riots.

<sup>26</sup>John Cary published the first accurate map of England in 1793 (Cary (1793)). We use an updated version from the early 1830s.

<sup>27</sup>The Parliament had little direct influence on the allocation of funds for road construction. By 1820, England already had a dense network of roads that connected the 40 counties (Albert (1972)). The major turnpike roads were maintained by private trusts, while local roads were maintained by local government authorities (e.g., Aidt, Daunton, and Dutta (2010)). Moreover, the capacity of the central administration was limited by modern standards and there was no ministry or official agency in charge of public works (Jupp (1998)). It is, therefore, unlikely that the presence of roads may be confounded by political factors.

historical evidence suggesting the presence of such asymmetric shocks.<sup>28</sup> The two most likely transmission channels for asymmetric economic shocks are the geography of agricultural production and spatial differences in market integration. There were three different agricultural regions in England in the 1830s and the Swing riots were concentrated in the cereal-producing region while the regions with dairy and extensive farming were less affected. To control for these agricultural production patterns, we define two indicator variables that single out the cereal-producing Southeastern part of the country (*Cereal area*) and the dairy-producing Southwestern part (*Dairy area*), while the Northern part with extensive farming constitutes the omitted category. To proxy for spatial differences in market integration, we construct the variable *Market integration*. It measures the travel distance between any given constituency and the 243 other constituencies weighted by the population.

Third, the instrument could pick up a constituency's connection to London or to other major towns. In the early 1830s, London was England's largest urban center and information hub where the major daily national newspapers, as well as many weekly and quarterly newspapers, were published. The surrounding countryside was linked to London through trade and finance. The economic and informational connections to the capital were naturally stronger for the constituencies closer to London and in the constituencies where the circulation of local weekly newspapers would help disseminate information from London.<sup>29</sup> To control for such links, we build the constituency-specific variable *Connection to London* (see Supplemental Material Section S2). This variable isolates each constituency's geographic distance from Sevenoaks from its geographical, economic, and informational connections to London. Along similar lines, the relative proximity of a constituency to the regional urban centers could have potentially affected the electoral support of the Whigs, just as its connection to London might have. To control for this possibility, we identify

<sup>28</sup>For instance, it is unlikely that weather shocks and/or trade shocks only affected the areas hit by the Swing riots and which subsequently experienced the rise in the share of Whigs elected. This is because the English market was not segmented by internal trade barriers similar to the French *octroi* system (Franck, Johnson, and Nye (2014)). In fact, the historical evidence suggests that English goods and capital markets were integrated by 1820 (see, e.g., Buchinsky and Polak (1993), Szostak (1991)). Moreover, the historical evidence suggests that wheat prices were high in 1829, 1830, and 1831 throughout England, and not only in Kent where the riots started (Rostow (1948, pp. 124–125)). Furthermore, it does not appear that the economic depression of the late 1820s was harsher in the Southeast of England than in the North, or that the economic conditions in Cornwall in the Southwest were relatively better than those of Kent in the Southeast. Finally, to invalidate the exclusion restriction, the trade and/or weather shocks would have to be concentrated within the short time window between August 1830 and April 1831.

<sup>29</sup>Newspaper publishers had to pay stamp duties on each sheet of paper. This raised the price of a newspaper to a level that was outside the budget of most ordinary people who would read them, insofar as they could read, in coffee houses, in inns, or in public reading rooms. The accounts of the Stamp Office make it possible to roughly estimate the circulation of each newspaper, which we use to quantify informational links to London.

the 13 largest towns (by population) in 1831 and code the variable *Distance to urban center* as the distance in travel days from each constituency to the nearest of these towns.

## 5. RESULTS

### 5.1. *Least Squares and Probit Results*

Table II presents the baseline estimates of the effect of *Riots within 10 km* on the share of seats won by Whigs (panel A) and on the probability that a seat was won by a Whig (panel B) in the 1831 election. In panel A, we report least squares estimates with standard errors that correct for spatial correlation for a radius of 50 km (and for the sake of comparison, with White robust standard errors for *Riots within 10 km*); in panel B, we report probit estimates with robust standard errors clustered at the constituency level.<sup>30</sup> Column (1) shows a specification without any control variables and columns (2) to (5) progressively add more controls. Column (6) reports a specification which is tested down using a general-to-specific strategy. Across all the specifications in panel A, the point estimate on *Riots within 10 km* is statistically significant. The point estimate on *Riots within 10 km* decreases from 0.57 in column (1) to 0.37 in column (2) when we control flexibly for *Whig share 1826* and for *Reform support 1830*, and it reaches values between 0.42 and 0.47 as we add additional controls. Overall, the point estimate is remarkably stable. The university constituencies and those with a high *Patronage index* returned a lower share of Whigs, while the county constituencies returned a higher share (columns (5) and (6)). The two indicator variables accounting for the general economic trends are marginally significant, but only the negative correlation between the economic decline of a constituency and Whig support is robust to the general-to-specific strategy. The occupational variables are insignificant, both individually and as a group, as are *Population* and *Population density*. A similar pattern is observed in panel B.

Column (5) reports our preferred specification which includes all the control variables. The point estimate from panel A indicates that exposure to one additional riot within a radius of 10 km from a constituency increased the share of Whigs elected in that constituency by 0.47 percentage points relative to past Whig support. This implies that the share of Whigs elected in a constituency in the top quartile of the riots distribution (exposed to at least 12 riots) was 5.2 percentage points higher than in a constituency in the bottom quartile (exposed to at most one riot). To put these results into perspective, we observe that the shares of Whigs elected in 1830 and 1831 were 42.9 and 56.6 percent,

<sup>30</sup>The dependent variable in panel A is a share and we re-estimate equation (1) with the fractional estimator of Papke and Wooldridge (1996). Table S3 in the Supplemental Material shows that the results are qualitatively similar.

TABLE II  
 LOCAL SWING RIOTS AND THE OUTCOME OF THE 1831 ELECTION. BASELINE RESULTS<sup>a</sup>

|                                | (1)                         | (2)                        | (3)                          | (4)                          | (5)                          | (6)                          |
|--------------------------------|-----------------------------|----------------------------|------------------------------|------------------------------|------------------------------|------------------------------|
| <i>Panel A</i>                 |                             |                            |                              |                              |                              |                              |
|                                | <i>Whig Share 1831 (%)</i>  |                            |                              |                              |                              |                              |
|                                | Least Squares               |                            |                              |                              |                              |                              |
| Riots within 10 km             | 0.57<br>(0.32)*<br>[0.25]** | 0.37<br>(0.22)*<br>[0.19]* | 0.44<br>(0.18)**<br>[0.18]** | 0.47<br>(0.18)**<br>[0.18]** | 0.47<br>(0.18)**<br>[0.19]** | 0.44<br>(0.18)**<br>[0.18]** |
| Whig share 1826                |                             | 0.87<br>(0.19)***          | 0.32<br>(0.19)               | 0.35<br>(0.20)*              | 0.38<br>(0.20)*              | 0.38<br>(0.071)***           |
| (Whig share 1826) <sup>2</sup> |                             | -0.0045<br>(0.0019)**      | 0.00055<br>(0.0020)          | 0.00035<br>(0.0020)          | -6.8e-06<br>(0.0020)         |                              |
| Reform support 1830            |                             | 12.0<br>(5.60)**           | 12.1<br>(4.97)**             | 11.2<br>(5.09)**             | 12.1<br>(5.14)**             | 12.6<br>(4.77)**             |
| County constituency            |                             |                            | 33.0<br>(5.14)***            | 37.2<br>(6.50)***            | 35.2<br>(7.04)***            | 31.6<br>(4.68)***            |
| University constituency        |                             |                            | -60.8<br>(9.39)***           | -58.1<br>(10.7)***           | -58.1<br>(8.60)***           | -61.8<br>(10.50)***          |
| Narrow franchise               |                             |                            | -3.35<br>(5.62)              | -2.85<br>(5.39)              | -3.62<br>(5.26)              |                              |
| Patronage index                |                             |                            | -17.0<br>(3.42)***           | -13.5<br>(3.94)***           | -12.2<br>(3.86)***           | -15.3<br>(3.52)***           |
| Emp. fract. index              |                             |                            |                              | 7.52<br>(30.9)               | 7.83<br>(29.49)              |                              |
| Agriculture (emp. share)       |                             |                            |                              | -28.4<br>(27.5)              | -27.2<br>(27.0)              |                              |
| Trade (emp. share)             |                             |                            |                              | 11.4<br>(30.9)               | 14.0<br>(31.1)               |                              |
| Professionals (emp. share)     |                             |                            |                              | -143<br>(120)                | -119<br>(120)                |                              |
| Population                     |                             |                            |                              |                              | 0.00028<br>(0.009)           |                              |
| Population density             |                             |                            |                              |                              | 0.15<br>(2.68)               |                              |
| Thriving economy               |                             |                            |                              |                              | -10.1<br>(5.91)*             |                              |
| Declining economy              |                             |                            |                              |                              | -10.6<br>(5.86)*             | -10.3<br>(5.72)*             |
| Selection ratio                | N.A.                        | 0.67                       | 2.26                         | 2.54                         | 2.56                         | 2.59                         |
| Adjusted R <sup>2</sup>        | 0.021                       | 0.27                       | 0.44                         | 0.44                         | 0.45                         | 0.45                         |
| Obs. (constituencies)          | 244                         | 244                        | 244                          | 244                          | 244                          | 244                          |

(Continues)

TABLE II—*Continued*

|                    | (1)                      | (2)                  | (3)                  | (4)                  | (5)                  | (6)                  |
|--------------------|--------------------------|----------------------|----------------------|----------------------|----------------------|----------------------|
| <i>Panel B</i>     |                          |                      |                      |                      |                      |                      |
|                    | <i>Whig Elected 1831</i> |                      |                      |                      |                      |                      |
|                    | Probit                   |                      |                      |                      |                      |                      |
| Riots within 10 km | 0.0058<br>[0.0029]**     | 0.0056<br>[0.0028]** | 0.0062<br>[0.0029]** | 0.0068<br>[0.0029]** | 0.0056<br>[0.0027]** | 0.0065<br>[0.0029]** |
| Obs. (seats)       | 489                      | 489                  | 489                  | 489                  | 489                  | 489                  |

<sup>a</sup> Panel A reports least squares estimates associating local Swing riots to the outcome of the 1831 election (constant terms not shown). We report spatial (Conley (1999)) standard errors (50 km radius) in parentheses and White robust standard errors in brackets. The selection ratio (Altonji, Taber, and Elder (2005)) indicates how large the selection on unobserved factors must be relative to the selection on the observed factors included in each specification for the point estimate on *Riots within 10 km* to entirely result from an omitted variables bias. The regression in column (6) is tested down using a general-to-specific approach. Panel B reports probit results (marginal effects evaluated at the mean) associating local Swing riots to the likelihood that a Whig is elected to a seat in 1831. Each estimation includes the same control variables as the corresponding estimation in panel A, except that we cannot condition on *University constituency* because the two university constituencies elected Tories to all four seats. The full results are reported in Table S2 in the Supplemental Material. The standard errors in panel B are clustered at the constituency level.

\*\*\*, \*\*, and \* indicate statistical significance at the 1%, 5%, and 10% level, respectively.

respectively. The least squares estimate, therefore, suggests that the share of Whigs elected in the average constituency increased to 48.1 percent because of the riots. This explains about half of the actual increase between 1830 and 1831. Alternatively, we can use the marginal effects reported in panel B to quantify the effect of the Swing riots on the probability that a Whig won a seat. Given the probit estimate from column (5), the probability that a Whig won a seat in a constituency in the top quartile of the riot distribution is 6.1 percent higher than in a constituency in the bottom quartile of the distribution.

At the bottom of panel A, we report estimates of the selection bias obtained with the estimation strategy developed by Altonji, Taber, and Elder (2005). We find that the selection ratio, which is defined as the ratio of standardized selection on unobservables to observables under the assumption that the Swing riots did not influence the outcome of the 1831 election, varies between 0.67 in the specification in column (2) that only conditions on past Whig and reform support to 2.56 in our preferred specification in column (5). This means that the normalized shift in the distribution of the unobservable factors would have to be about two and half times as large as the shift in the observable factors to fully explain the effect of the Swing riots that is reported in Table II as a manifestation of selection bias.

Table III, panel A examines the robustness of the baseline results in two ways. First, we assess the impact of riots within a radius of 1, 10, 30, and 50 km of each constituency. Second, we report standard errors that allow for spatial correlation in the error structure for constituencies located within a radius of



TABLE III  
 LOCAL SWING RIOTS AND THE OUTCOME OF THE 1831 AND 1830 ELECTIONS  
 ALTERNATIVE MEASURES OF LOCAL SWING RIOTS AND SPATIAL CORRELATION<sup>a</sup>

|                                    | (1)                        | (2)       | (3)       | (4)       | (5)       | (6)     |
|------------------------------------|----------------------------|-----------|-----------|-----------|-----------|---------|
| <i>Panel A</i>                     |                            |           |           |           |           |         |
|                                    | <i>Whig Share 1831 (%)</i> |           |           |           |           |         |
|                                    | Least Squares              |           |           |           |           |         |
| Riots within 1 km                  | 2.76                       |           |           |           |           |         |
| Riots within 10 km                 |                            | 0.47      |           |           |           |         |
| Riots within 20 km                 |                            |           | 0.14      |           |           |         |
| Riots within 30 km                 |                            |           |           | 0.066     |           |         |
| Riots within 50 km                 |                            |           |           |           | 0.028     |         |
| Riots between 50 and 75 km         |                            |           |           |           |           | 0.021   |
| Beta coefficient                   | 0.11                       | 0.13      | 0.12      | 0.11      | 0.11      | 0.07    |
| Spatial std. errors, 20 km         | (0.99)***                  | (0.20)**  | (0.060)** | (0.030)** | (0.013)** | (0.016) |
| Spatial std. errors, 50 km         | (1.02)***                  | (0.18)**  | (0.058)** | (0.028)** | (0.013)** | (0.017) |
| Spatial std. errors, 100 km        | {1.13}**                   | {0.17}*** | {0.059}** | {0.029}** | {0.013}** | {0.019} |
| Spatial std. errors, 200 km        | [1.12]**                   | [0.17]*** | [0.061]** | [0.032]** | [0.014]** | [0.020] |
| White robust std. errors           | [0.97]***                  | [0.19]**  | [0.058]** | [0.030]** | [0.014]** | [0.017] |
| Adjusted R <sup>2</sup>            | 0.44                       | 0.45      | 0.44      | 0.44      | 0.44      | 0.43    |
| <i>Panel B (Placebo Test)</i>      |                            |           |           |           |           |         |
|                                    | <i>Whig Share 1830 (%)</i> |           |           |           |           |         |
|                                    | Least Squares              |           |           |           |           |         |
| Riots within 1 km                  | 0.59                       |           |           |           |           |         |
| Riots within 10 km                 |                            | 0.11      |           |           |           |         |
| Riots within 20 km                 |                            |           | 0.014     |           |           |         |
| Riots within 30 km                 |                            |           |           | -0.0010   |           |         |
| Riots within 50 km                 |                            |           |           |           | -0.0069   |         |
| Riots between 50 and 75 km         |                            |           |           |           |           | -0.011  |
| Beta coefficient                   | 0.03                       | 0.04      | 0.01      | -0.002    | -0.03     | -0.04   |
| Spatial std. errors, 50 km         | (1.02)                     | (0.11)    | (0.042)   | (0.025)   | (0.010)   | (0.011) |
| White robust std. errors           | [0.96]                     | [0.11]    | [0.038]   | [0.022]   | [0.010]   | [0.012] |
| Adjusted R <sup>2</sup>            | 0.56                       | 0.56      | 0.56      | 0.55      | 0.56      | 0.56    |
| Difference test ( <i>p</i> -value) | 0.06                       | 0.03      | 0.02      | 0.02      | 0.007     | N.A.    |
| Baseline controls included         | YES                        | YES       | YES       | YES       | YES       | YES     |
| Obs. (constituencies)              | 244                        | 244       | 244       | 244       | 244       | 244     |

<sup>a</sup>Panel A reports least squares estimates associating local Swing riots within various radiuses from the constituency to the outcome of the 1831 election. We report spatial (Conley (1999)) standard errors for four different radiuses (20 km, 50 km, 100 km, and 200 km) and White robust standard errors. Panel B reports the corresponding results for the placebo test on the outcome of the 1830 election. The difference test is a chi-squared test where the null hypothesis is that the coefficient on the *Riots within R km* variable in panel A is statistically different from the corresponding coefficient in panel B (Gelman and Stern (2006)). In both panels, the controls from column (5) in Table II are included (the coefficient in column (2) in panel A is thus the coefficient from column (5) in Table II). The beta coefficients show how many standard deviations the dependent variable will change per standard deviation increase of each of the *Riots within R km* variables.

\*\*\*, \*\*, and \* indicate statistical significance at the 1%, 5%, and 10% level, respectively.

20, 100, or 200 km of each other.<sup>31</sup> We observe that the Swing riots exerted a positive significant effect on the share of Whigs elected in 1831 for all radiuses between 1 and 50 km. The beta coefficients reported below the point estimates suggest that a one standard deviation increase in one of the *Riots within R km* variables raises *Whig share 1831* by 0.10–0.13 standard deviations. The largest effect is for *Riots within 10 km*, and we observe a slight decline as the radius is expanded beyond 10 km. Column (6) reports a regression that relates the number of riots that took place between 50 and 75 km from a constituency (*Riots between 50 and 75 km*) to *Whig share 1831*. Consistent with the hypothesis that the voters and patrons reacted to the local Swing riots and not to second-hand reports about riots far away from their constituency, we find that this variable is insignificant. Finally, it makes little difference to the size of the standard errors whether we allow for spatial correlation in the error structure within a radius of 20, 50, 100, or 200 km.

Table III, panel B reports the results of the main placebo test. It investigates whether the *Riots within R km* variables are related to the Whig electoral success in the 1830 election. This election took place before the Swing riots began and there should not, therefore, be any such relationship unless our measures of riots are correlated with unobserved constituency-specific determinants of Whig electoral success. It is reassuring to observe that there is not any significant relationship, irrespective of the radius which we use to assess local riots. Moreover, the difference between the significant coefficients of *Riots within R km* in panel A and the corresponding insignificant ones in panel B is statistically significant (Gelman and Stern (2006)).

## 5.2. Matching Estimates

Table IV, panel A reports a selection of covariate and propensity score matching estimates of the sample average effect of treatment on the treated (ATT) of the Swing riots on the outcome of the 1831 election.<sup>32</sup> The ATT estimates are corrected for the finite sample bias caused by nonexact matches. We restrict the sample to the constituencies that belong to the intersection of the propensity scores of the control and treated groups (two-sided common support).<sup>33</sup> This reduces the sample to 163 treated constituencies which were exposed to strictly more than one riot, and 65 control constituencies which were exposed to less than that.

<sup>31</sup>For the sake of comparison, Table III repeats the baseline results from Table II, column (5) with riots within a radius of 10 km and spatial correlation in the error structure for a radius of 50 km.

<sup>32</sup>The complete results are reported in Tables S5 to S10 in the Supplemental Material.

<sup>33</sup>This is a stronger assumption than what is needed for the estimation of the ATT. However, if we only impose a one-sided common support restriction (which limits the matches to be in the support of the propensity score for the treatment group), we include in the analysis many treated constituencies which are very unlikely to be untreated given their characteristics. This is not desirable since these constituencies are hard to match.

TABLE IV  
 LOCAL SWING RIOTS AND THE OUTCOME OF THE 1831 AND 1830 ELECTIONS  
 MATCHING ESTIMATES<sup>a</sup>

|                                    | (1)                        | (2)  | (3)                       | (4)                  | (5)                                | (6)                         |
|------------------------------------|----------------------------|--|---------------------------|----------------------|------------------------------------|-----------------------------|
|                                    | Covariate Matching         |  | Propensity Score Matching |                      |                                    |                             |
|                                    | Nearest-Neighbor           | Nearest-Neighbor<br>W. Over-Weighting <sup>b</sup> | Nearest-Neighbor          | Radius<br>(R = 0.05) | Kernel <sup>c</sup><br>(bw = 0.02) | Stratification <sup>d</sup> |
| <i>Panel A</i>                     |                            |  |                           |                      |                                    |                             |
|                                    | <i>Whig Share 1831 (%)</i> |  |                           |                      |                                    |                             |
| ATT (bias corrected)               | 11.1                       | 13.9   | 15.0                      | 16.0                 | 14.2                               | 14.2                        |
| Bootstrapped std. errors           |                            |  | (7.57)**                  | (7.12)**             | (6.92)**                           | (6.84)**                    |
| Analytic std. errors               | [5.35]**                   | [5.92]**   | [8.82]*                   | [6.97]**             | N.A.                               | [6.62]**                    |
| <i>Panel B (Placebo Test)</i>      |                            |  |                           |                      |                                    |                             |
|                                    | <i>Whig Share 1830 (%)</i> |  |                           |                      |                                    |                             |
| ATT (bias corrected)               | 1.8                        | 1.2  | 6.2                       | 4.4                  | 5.2                                | 1.6                         |
| Bootstrapped std. errors           |                            |  | (5.73)                    | (5.67)               | (4.73)                             | (5.25)                      |
| Analytic std. errors               | [4.28]                     | [4.03]   | [7.31]                    | [5.81]               | N.A.                               | [5.16]                      |
| Treated constituencies             | 163                        | 163  | 163                       | 163                  | 163                                | 163                         |
| Control constituencies             | 65                         | 65   | 43                        | 63                   | 65                                 | 65                          |
| Obs. (constituencies)              | 228                        | 228  | 206                       | 226                  | 228                                | 228                         |
| Common support                     | YES                        | YES  | YES                       | YES                  | YES                                | YES                         |
| Balancing test passed <sup>e</sup> | N.A.                       | N.A.   | YES                       | YES                  | YES                                | YES                         |

<sup>a</sup>Panel A reports the covariate and propensity score matching estimates of the sample average effect of treatment on the treated (ATT) for the outcome of the 1831 election. Panel B reports the corresponding placebo estimates for the outcome of the 1830 election. The treatment variable *Riot treatment* is a dummy variable equal to 1 if the number of riots within 10 km of the constituency is strictly greater than one and zero otherwise. The vector of matching variables includes the variables listed in column (5) of Table II, except that the two university constituencies are excluded since they both belong to the treatment group. The ATT estimates are corrected for the finite sample bias caused by nonexact matches. We report analytic heteroskedasticity-consistent standard errors in square brackets and bootstrapped standard errors in parentheses (1000 iterations). We apply the minima–maxima rule and restrict the sample to the overlap between the support of the propensity score of the treated and nontreated constituencies (two-sided common support). The range of overlap is [0.38; 0.92]. The propensity score and covariate matching (inverse variance method with replacement) estimations are implemented with the STATA procedures developed by Becker and Ichino (2002) and Abadie et al. (2004), respectively.

<sup>b</sup>We require perfect matches on *Whig share 1826* and *Reform support 1830*.

<sup>c</sup>The Epanechnikov kernel.

<sup>d</sup>The stratification method compares within five blocks where the covariates are balanced the average outcome for the treatment and control constituencies. Each block is weighted with the fraction of treatment constituencies within the block.

<sup>e</sup>The balancing test assesses whether the matching variables differ between the treated and control constituencies, within blocks where the propensity scores of the two groups of constituencies do not differ.

\*\*\*, \*\*, and \* indicate statistical significance at the 1%, 5%, and 10% level, respectively.

The covariate matching estimates reported in columns (1) and (2) are significant at the five percent level. The specification shown in column (2) requires exact matches on the two variables that summarize many of the unobserved determinants of support for the Whigs and for reform in each constituency (*Whig*

*share 1826 and Reform support 1830*). We observe that this increases the ATT estimate from 11.1 to 13.8. Columns (3) to (6) report propensity score matching estimates using the nearest-neighbor, the radius, the Epanechnikov kernel, or the stratification matching algorithm. We note that all the specifications pass the balancing test at the five percent level of significance. This indicates that the control and treated constituencies are, on average, similar in all their observable characteristics within the blocks where their propensity score does not differ (Dehejia and Wahba (2002)). The propensity score matching estimates lie in the range from 14.2 to 16.0 and are thus not sensitive to the choice of the matching algorithm. To evaluate if these matching estimates are contaminated by systematic, unobserved differences in the determinants of Whig electoral success in the control and treated constituencies, we report in Table IV, panel B a placebo test which investigates if the “riot treatment” can explain the outcome of the 1830 election. We observe that this is not the case.

Overall, the matching estimates suggest that the local Swing riots increased the share of Whigs elected in 1831 by about 14 percentage points in the constituencies which were “treated” to more than one riot. This corresponds to an 11 percentage point increase in the average share of Whigs returned in the 1831 election, that is, roughly three quarters of the difference between the share of Whigs elected in 1830 (42.9 percent) and 1831 (56.6 percent).

The lack of a “sharp” cut-off between the control and treated constituencies implies that these estimates should be understood as lower bound estimates for the treatment effect of the Swing riots. This is because the presence in the treated group of constituencies that were barely exposed to more than one riot within a neighborhood of 10 km attenuates the strength of the treatment. To gauge the importance of the choice for the cut-off, we carry out two series of robustness checks. First, we repeat all the matching estimations under the restriction that the treated group consists of the constituencies in the top 50 percentile of the spatial distribution of riots while we retain as the control group the bottom quartile. The constituencies in this new treated group were exposed to at least six riots within a radius of 10 km. Tables S7 and S10 in the Supplemental Material show that the covariate and propensity matching estimates are slightly larger than the estimates in Table IV and that they remain precisely estimated despite the smaller sample. Second, we implement a randomization inference procedure to account for potentially strong spatial patterns in the assignment to “treatment” in our data and the relatively small sample size (Barrios, Diamond, Imbens, and Kolesár (2012), Rosenbaum (2002, 2010)). The ATT estimates reported in Table S11 in the Supplemental Material are larger than in Table IV (close to 20) but similar to those reported in Tables S7 and S10 and they remain statistically significant at the five percent level.

### 5.3. *Reduced Form and Instrumental Variable Estimates*

Table V, columns (1) to (3) show our estimates of the reduced form relationship between *Distance to Sevenoaks* and the share of Whigs elected in 1831.

TABLE V  
 DISTANCE TO SEVENOAKS AND THE OUTCOME OF THE 1831 AND 1830 ELECTIONS  
 REDUCED FORM ESTIMATES<sup>a</sup>

|   | (1)                        | (2)       | (3)       | (4)                      |
|---|----------------------------|-----------|-----------|--------------------------|
| <i>Panel A</i>                          |                            |           |           |                          |
|   | <i>Whig Share 1831 (%)</i> |           |           | <i>Whig Elected 1831</i> |
|   | Least Squares              |           |           | Probit                   |
| Distance to Sevenoaks                   | -1.89                      | -2.60     | -2.60     | -0.036                   |
| Spatial std. errors <sup>b</sup>        | (0.84)**                   | (0.78)*** | (0.86)*** |                          |
| White robust std. errors                | [0.67]***                  | [0.81]*** | [0.87]*** |                          |
| Clustered std. errors <sup>c</sup>      |                            |           |           | {0.011}***               |
| Adjusted R <sup>2</sup>                 | 0.03                       | 0.44      | 0.43      |                          |
| Pseudo R <sup>2</sup>                   |                            |           |           | 0.41                     |
| <i>Panel B (Placebo Test)</i>           |                            |           |           |                          |
|   | <i>Whig Share 1830 (%)</i> |           |           | <i>Whig Elected 1830</i> |
|   | Least Squares              |           |           | Probit                   |
| Distance to Sevenoaks                   | -0.84                      | 0.39      | 0.46      | 0.013                    |
| Spatial std. errors <sup>b</sup>        | (0.60)                     | (0.75)    | (0.79)    |                          |
| White robust std. errors                | [0.57]                     | [0.75]    | [0.80]    |                          |
| Clustered std. errors <sup>c</sup>      |                            |           |           | {0.014}                  |
| Adjusted R <sup>2</sup>                 | 0.005                      | 0.55      | 0.55      |                          |
| Pseudo R <sup>2</sup>                   |                            |           |           | 0.45                     |
| Baseline controls included <sup>d</sup> | NO                         | YES       | YES       | YES                      |
| Spatial controls included <sup>e</sup>  | NO                         | YES       | YES       | YES                      |
| Kent included                           | YES                        | YES       | NO        | YES                      |
| Observations                            | 244                        | 244       | 235       | 489                      |

<sup>a</sup>Panel A reports reduced form least squares and Probit estimates for the effect of *Distance to Sevenoaks* (the village in Kent where the riots began) on the outcome of the 1831 election. Panel B reports the corresponding placebo estimates for the outcome of the 1830 election. In column (3), we exclude the constituencies in Kent. In column (4), the point estimate is the marginal effect which is evaluated at the mean of the explanatory variables.

<sup>b</sup>Spatial (Conley (1999)) standard errors (50 km radius).

<sup>c</sup>Clustered at the constituency level.

<sup>d</sup>The controls are those from column (5) in Table II. In column (4), *University constituency* is excluded because it predicts the outcome perfectly as the two university constituencies elected Tories to all four seats.

<sup>e</sup>The spatial controls are *Distance to urban center*, *Connection to London*, *Market integration*, *Cereal area*, and *Dairy area*.

\*\*\*, \*\*, and \* indicate statistical significance at the 1%, 5%, and 10% level, respectively.

We report least squares estimates with spatial standard errors (and White robust standard errors for comparison). Column (1) does not include any control variables, while column (2) includes the control variables from column (5) in Table II as well as the five spatial controls (*Connection to London*, *Distance to urban center*, *Market integration*, *Cereal area*, and *Dairy area*). Column (3), specifically, excludes all the constituencies in Kent—the county where the Swing riots started. All the results indicate that the share of Whigs elected in

1831 falls as the distance to Sevenoaks increases. In particular, the point estimate in column (2) suggests that one extra travel day from Sevenoaks reduces *Whig share 1831* by 2.60 percentage points. Since the average of *Distance to Sevenoaks* is 7.7 travel days, the average decrease in *Whig share 1831* is about 20 percentage points. A qualitatively similar result is reported in column (4), which reports the reduced form relationship between the probability that a seat was won by a Whig candidate and *Distance to Sevenoaks*.

Table VI reports a summary of the instrumental variable estimates (the full results are reported in Tables S13 to S16 in the Supplemental Material). The assumption underlying these estimates is that the effect of the distance to Sevenoaks on the outcome of the 1831 election operates only through its ef-

TABLE VI  
LOCAL SWING RIOTS AND THE OUTCOME OF THE 1831 AND 1830 ELECTIONS  
INSTRUMENTAL VARIABLE ESTIMATES<sup>a</sup>

|  | (1)  | (2)       | (3)       | (4)                      |
|--|--|-----------|-----------|--------------------------|
| <i>Panel A</i>                                       |  |           |           |                          |
|  | <i>Whig Share 1831 (%)</i>                             |           |           | <i>Whig Elected 1831</i> |
|  | Second Stage   |           |           |                          |
|  | 2SLS   | 2SLS      | 2SLS      | IV-Probit                |
| Riots within 10 km (instrumented)                    | 1.32   | 2.53      | 3.48      | 0.078                    |
| Spatial GMM std. errors <sup>b</sup>                 | (0.60)**   | (1.08)**  | (1.60)**  |                          |
| 2SLS robust std. errors                              | [0.46]***  | [0.87]*** | [1.32]*** |                          |
| Anderson-Rubin <i>p</i> -values <sup>g</sup>         | 0.006  | 0.002     | 0.003     |                          |
| Clustered std. errors <sup>c</sup>                   |  |           |           | {0.015}***               |
| <i>Panel B</i>                                       |  |           |           |                          |
|  | The Instrumented Variable Is <i>Riots Within 10 km</i> |           |           |                          |
|  | First Stage  |           |           |                          |
| Distance to Sevenoaks                                | -1.43  | -1.03     | -0.75     | -1.06                    |
| White robust std. error                              | (0.17)***  | (0.26)*** | (0.24)*** |                          |
| Clustered std. errors <sup>c</sup>                   |  |           |           | {0.26}***                |
| Partial <i>R</i> <sup>2</sup> on excluded instrument | 0.23   | 0.05      | 0.03      |                          |
| Kleibergen-Paap <i>F</i> -statistic                  | 74.3***  | 15.2***   | 9.9***    |                          |
| <i>Panel C</i>                                       |  |           |           |                          |
|  | <i>Whig Share 1831 (%)</i>                             |           |           | <i>Whig Elected 1831</i> |
|  | Least Squares  |           |           | Probit                   |
| Riots within 10 km                                   | 0.57   | 0.50      | 0.52      | 0.0069                   |
| Spatial std. errors <sup>d</sup>                     | (0.32)*  | (0.19)**  | (0.25)**  |                          |
| White robust std. errors                             | [0.25]**   | [0.21]**  | [0.29]*   |                          |
| Clustered std. errors <sup>c</sup>                   |  |           |           | {0.0031}**               |

(Continues)

TABLE VI—Continued

|  | (1)                        | (2)    | (3)    | (4)                      |
|--|----------------------------|--------|--------|--------------------------|
| <i>Panel D (Placebo Test)</i>                |                            |        |        |                          |
|  | <i>Whig Share 1830 (%)</i> |        |        | <i>Whig Elected 1830</i> |
|  | Second Stage               |        |        |                          |
|  | 2SLS                       | 2SLS   | 2SLS   | IV-Probit                |
| Riots within 10 km (instrumented)            | 0.59                       | −0.38  | −0.61  | −0.028                   |
| Spatial GMM std. errors <sup>b</sup>         | (0.43)                     | (0.75) | (1.11) |                          |
| 2SLS robust std. errors                      | [0.39]                     | [0.71] | [1.05] |                          |
| Anderson–Rubin <i>p</i> -values <sup>g</sup> | 0.14                       | 0.60   | 0.56   |                          |
| Clustered std. errors <sup>c</sup>           |                            |        |        | {0.028}                  |
| Baseline controls included <sup>e</sup>      | NO                         | YES    | YES    | YES                      |
| Spatial controls included <sup>f</sup>       | NO                         | YES    | YES    | YES                      |
| Kent included                                | YES                        | YES    | NO     | YES                      |
| Observations                                 | 244                        | 244    | 235    | 489                      |

<sup>a</sup>Panel A reports 2SLS and IV-probit estimates of the effect of local Swing riots on the outcome of the 1831 election. Panel B, columns (1) to (3) summarize the first stage estimates for the 2SLS procedure and column (4) summarizes the Maximum Likelihood estimates from the IV-probit procedure. Panel C reports the least squares estimates corresponding to the instrumental variable estimates in Panel A. Panel D reports the placebo second stage estimates related to the outcome of the 1830 election. The instrument is *Distance to Sevenoaks* (the village in Kent where the riots began). The point estimates in column (4) are marginal effects evaluated at the mean of the explanatory variables. The full sets of results are reported in Tables S13 to S16 in the Supplemental Material.

<sup>b</sup>Spatial (Conley (1999)) GMM standard errors (50 km radius).

<sup>c</sup>Clustered at the constituency level.

<sup>d</sup>Spatial (Conley (1999)) standard errors (50 km radius).

<sup>e</sup>The controls are those from column (5) in Table II. In column (4), *University constituency* is excluded because it predicts the outcome perfectly as the two university constituencies elected Tories to all four seats.

<sup>f</sup>The spatial controls are *Distance to urban center*, *Connection to London*, *Market integration*, *Cereal area*, and *Dairy area*.

<sup>g</sup>The Anderson–Rubin test of significance of *Riots within 10 km* is robust to weak instruments.

\*\*\*, \*\*, and \* indicate statistical significance at the 1%, 5%, and 10% level, respectively.

fect on the geography of the Swing riots. Panel A, columns (1) to (3) show two-stage least squares (2SLS) estimates of the (instrumented) impact of *Riots within 10 km* on *Whig share 1831*. We report GMM standard errors adjusted for spatial correlation (Conley (1999)) and for comparison, 2SLS robust standard errors. Column (4) shows the corresponding IV-probit estimates for the probability that a seat is won by a Whig. Panel B shows a summary of the first stage regression results, while panel C shows, for each column, the least squares or probit estimate corresponding to the instrumental variable estimate in panel A.<sup>34</sup>

<sup>34</sup>The least squares estimate of the coefficient on *Riots within 10 km* in the specification in column (2) which conditions on the five spatial controls is almost identical to the baseline estimate reported in Table II, column (5). None of the spatial controls are significant in the baseline models reported in Table II.

The first stage relationship between the instrumental variable *Distance to Sevenoaks* and *Riots within 10 km* is negative, as expected. The coefficient associated with *Distance to Sevenoaks* is significant at the one percent level in all the specifications. The Kleibergen–Paap  $F$ -statistic for weak identification is equal to 15.2 in our preferred specification in column (2) but just below 10 in the specification that excludes Kent. To remove any concern regarding weak identification, we report in panel A, below the 2SLS robust standard errors, the  $p$ -value associated with the Anderson–Rubin test of significance which is robust to weak instruments.

In the second stage, all the estimates of the effect of *Riots within 10 km* are significant at the five percent level when we correct the standard errors for spatial correlation and at the one percent level when we do not. The point estimate in the specification without any control variables (column (1)) is 1.32. Adding the baseline and spatial controls (column (2)) increases the point estimate to 2.53, while the point estimate obtained when Kent is excluded is 3.48 (column (3)). These 2SLS estimates are, therefore, about five times larger than the corresponding least squares estimates reported in panel C of Table VI. This suggests that the bias in the least squares estimate is negative. This could be because riots happened to be concentrated around constituencies where, for reasons unobserved to us, the voters and patrons favored the Tory party, or because attenuation bias is more important than any (positive) selection bias. However, these 2SLS estimates are not unreasonably large. The share of Whigs elected in a constituency in the top quartile of the riot distribution is about 28 percentage points higher than in a constituency in the bottom quartile. This estimate suggests that the Swing riots contributed to the creation of a solid Whig majority in the House of Commons.

The instrumental variable estimates can only be given a causal interpretation if *Distance to Sevenoaks* does not affect the electoral success of the Whigs through any other channel than the Swing riots that happened in the vicinity of a constituency. This exclusion restriction cannot be tested directly and it is a concern that the spatial instrument is correlated with distance to many other things than the riots. The five spatial controls included in the instrumental variable estimations aim at addressing this concern and are highly significant in the first stage regressions (see Table S14).<sup>35</sup> We can go one step further and assess

<sup>35</sup>Table S18 in the Supplemental Material presents a robustness check that reports IV and reduced form estimates where we include the latitude and longitude of each constituency as additional control variables. Unlike the other spatial control variables, latitude and longitude are not correlated with *Riots within 10 km* (or any of the other riot variables). However, they are highly correlated with *Distance to Sevenoaks*, and along with the other spatial controls, latitude and longitude can explain 84 percent of the variation in the instrument. As a result, *Distance to Sevenoaks* becomes a weak instrument for *Riots within 10 km* (the Kleibergen–Paap  $F$ -statistic is equal to 2.78) in specifications with these variables included. Inference must, therefore, be based on a weak instrument robust test statistic and we note that the  $p$ -value associated with the Anderson–Rubin (weak instrument robust) test of significance for *Riots within 10 km* is be-



the validity of the exclusion restriction indirectly through falsification tests. If *Distance to Sevenoaks* affected the support for the Whigs in different geographical locations through other spatial channels than the geography of the Swing riots, we would expect to find a significant reduced form relationship between *Distance to Sevenoaks* and the outcome of the 1830 parliamentary election, which took place before the peak of the Swing riots. Similarly, we would expect to find significant correlations between local Swing riots instrumented by *Distance to Sevenoaks* and the outcome of the 1830 election. We re-estimate the reduced form and instrumental variable models with the dependent variables *Whig share 1830* or *Whig elected 1830*. We find no statistically significant relationship in any of these placebo tests (see panel B of Table V and panel D of Table VI). We also investigate if *Distance to Sevenoaks* is correlated with the economic characteristics of the constituencies. Conditional on the five spatial controls, we find no correlation between *Distance to Sevenoaks* and demographics, occupational structure, and medium-run economic trends, except that constituencies closer to Sevenoaks tended to be more densely populated (see Table S17 in the Supplemental Material). All these tests are consistent with the identifying assumption that *Distance to Sevenoaks* did not affect the electoral support for the Whigs through other channels than the Swing riots.

#### 5.4. Further Falsification Tests

In this section, we discuss additional falsification tests aimed at alleviating concerns about confounding factors.<sup>36</sup> The first of these tests extends the placebo test related to the outcome of the 1830 election discussed above and investigates if the local Swing riots can predict other election outcomes before and after the Great Reform Act. If local Swing riots observed directly by voters and patrons truly caused the shift in the electoral fortunes of the Whigs, there should be no such connection. Table VII, columns (1) and (2) show that *Riots within 10 km* is uncorrelated with the election outcome in 1826 and in 1835. Along similar lines, there should not be any systematic relationship between local Swing riots in 1830–1831 and constituency-level support for the Second Reform Act in 1867. We test this with data from Moser and Reeves (2014) on the roll call votes associated with the Second Reform Act. Table VII, column (3) shows that there is no relationship between Swing riots in the neighborhood of a constituency (*Riots with 10 km*) and the voting behavior of the MPs elected in that constituency.

Another way to look for evidence of confounding factors is to test if riots that pre-date the Swing riots can predict the outcome of the 1831 election. If

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low 0.01. The problem of weak identification can be alleviated by considering a larger radius around each constituency. In specifications with *Riots within 20 km* and *Riots within 30 km*, the Kleibergen–Paap *F*-statistic is equal to 6.05 and 10.9, respectively, and in both cases, the second stage estimate is statistically significant irrespective of how the standard errors are calculated.

<sup>36</sup>We discuss these tests in detail in Supplemental Material Section S3.

TABLE VII  
 LOCAL SWING RIOTS AND THE OUTCOME OF THE 1831 ELECTION  
 ADDITIONAL PLACEBO TESTS<sup>a</sup>

|                                    | (1)                            | (2)                                 | (3)  | (4)                            | (5)                               | (6)                               |
|------------------------------------|--------------------------------|-------------------------------------|--|--------------------------------|-----------------------------------|-----------------------------------|
|                                    | <i>Whig<br/>Share<br/>1826</i> | <i>Whig Vote<br/>Share<br/>1835</i> | <i>Share of Votes<br/>for 1867<br/>Reform Bill</i> | <i>Whig<br/>Share<br/>1831</i> | <i>Riots<br/>Within<br/>10 km</i> | <i>Riots<br/>Within<br/>10 km</i> |
| Riots within 10 km                 | 0.16                           | 0.00044                             | -0.00013   |                                |                                   |                                   |
| Spatial std. errors <sup>b</sup>   | (0.12)                         | (0.001)                             | (0.00014)  |                                |                                   |                                   |
| White robust std. errors           | [0.12]                         | [0.001]                             | [0.00015]  |                                |                                   |                                   |
| Food Riots 1800–1818               |                                |                                     |  | 0.082                          | -0.30                             | 0.039                             |
| Clustered std. errors <sup>c</sup> |                                |                                     |  | [0.21]                         | [0.097]***                        | [0.088]                           |
| White robust std. errors           |                                |                                     |  | (0.24)                         | (0.048)***                        | (0.052)                           |
| Labor Riots 1793–1822              |                                |                                     |  | -0.25                          | 0.38                              | -0.14                             |
| Clustered std. errors <sup>c</sup> |                                |                                     |  | [0.49]                         | [0.22]*                           | [0.20]                            |
| White robust std. errors           |                                |                                     |  | (0.43)                         | (0.15)**                          | (0.17)                            |
| Baseline controls                  | YES <sup>d</sup>               | YES <sup>e</sup>                    | YES <sup>e</sup>                                   | YES <sup>d</sup>               | NO                                | NO                                |
| Spatial controls <sup>f</sup>      | NO                             | NO                                  | NO   | NO                             | NO                                | YES                               |
| Distance to Sevenoaks              | NO                             | NO                                  | NO   | NO                             | NO                                | YES                               |
| Obs. (constituencies)              | 244                            | 97                                  | 164  | 244                            | 244                               | 244                               |

<sup>a</sup>Table VII, columns (1) and (2) report the least squares results associating local Swing riots in 1830–1831 to the share of Whig MPs elected in the 1826 election and to the share of votes obtained by Whig candidates in the 1835 election, respectively. Supplemental Material Tables S19 and S23 report the full results. We also investigate the relationship between *Riots within 10 km* and the outcome of all the other elections between 1802 and 1865 and find no significant correlation except for the 1802 election. Column (3) reports the least squares results associating local Swing riots in 1830–1831 to the cumulated number of votes in support of Disraeli's 1867 (successful) reform bill at the committee stage from the MPs elected in a given constituency as a proportion of the total votes that could have been cast by MPs from that constituency. Supplemental Material Table S24 reports the full results. Column (4) reports the least squares results associating labor and food riots between 1793 and 1882 (recorded at the county level) to the outcome of the 1831 election. Supplemental Material Table S20 reports the full results. Columns (5) and (6) report least squares results associating labor and food riots between 1793 and 1822 (recorded at the county level) to *Riots within 10 km*. Supplemental Material Table S21 reports the full results.

<sup>b</sup>Spatial (Conley (1999)) standard errors (50 km radius).

<sup>c</sup>Clustered at the county level.

<sup>d</sup>In columns (1) and (4), the controls are those from column (5) in Table II.

<sup>e</sup>In columns (2) and (3), we control for *County constituency*, *Population*, *Population density*, employment shares, and *Emp. fract. index*. In column (3), the demographic controls are from the 1861 instead of the 1831 Census and we control for *Liberal seat share* and for *Conservative seat share*. In column (2), we control for the Whig vote share in the 1832 election.

<sup>f</sup>The spatial controls are *Distance to urban center*, *Connection to London*, *Market integration*, *Cereal area*, and *Dairy area*.

\*\*\*, \*\*, and \* indicate statistical significance at the 1%, 5%, and 10% level, respectively.

they can, it would suggest that it was not the Swing riots per se and the fear that they instilled in voters and patrons that caused the Whig victory in 1831, but something else related to the general propensity for social unrest in an area. To conduct this test, we collect information on the spatial patterns of food and rural labor riots between 1793 and 1822 from Charlesworth (1983) and Bohstedt (2010). We re-estimate equation (1) with these measures of pre-Swing riots in

place of *Riots within 10 km*. Table VII, column (4) shows that they cannot predict the 1831 election result. We also use the pre-Swing riots data to test for a link between the spatial distribution of riots in the past and the Swing riots. Such a link might be seen as evidence that riots tend to happen around specific constituencies and might, therefore, be correlated with constituency-specific confounding factors. Table VII, column (5) shows that the raw correlations between pre-Swing food and labor riots and *Riots within 10 km* are significant, but that these relationships disappear once we control for observable spatial factors such as agricultural areas (column (6)).<sup>37</sup>

Overall, these tests are inconsistent with the view that our results are driven by unobserved constituency-specific characteristics.

### 5.5. *Alternative Explanations of the Whig Victory in 1831*

Our empirical analysis suggests that the Whig victory in 1831 was a consequence of local Swing riots. The historical literature, however, points to alternative explanations that it is important to evaluate explicitly. To do this, we collect quantitative data related to each of them and augment equation (1) with additional control variables. This allows us to test the relevance of the alternatives directly and to evaluate if the effect of local Swing riots is diminished in the augmented regression models.<sup>38</sup> Table VIII reports the main results.

The first alternative explanation for the Whig victory in 1831 is that administrative reforms in the pre-reform years weakened the Tories' ability to control seats (Morrison (2011)). While it is true that Lord Liverpool undertook reforms after 1815 to make the administration more efficient and less corrupt (see, e.g., Jupp (1998, Chapter 1), Mokyr (2009, pp. 425–426)), this had little effect on the number of seats in the House of Commons that were actually controlled by the Treasury (and thus the Tory governments that ruled between 1807 and 1830). It stayed in the range from 25 to 30 between 1750 and 1832 (Namier and Brooke (1964, Vol. 1, pp. 54–57), Thorne (1986, Vol. 1, pp. 46–56), Philbin (1965)). A simple test of Morrison's (2011) conjecture is to assume that the Tories lost hold of the 14 English constituencies (28 seats) which were controlled by the Treasury in 1830 and then to re-estimate equation (1) on the reduced sample that excludes these constituencies. If the conjecture is true and we confound the effect of the administrative reforms with that of the Swing riots, the coefficient on *Riots within 10 km* should become insignificant. Table VIII, column (1) shows that this is not the case.

The second alternative explanation is that the Whig victory in 1831 was caused by mobilization of religious networks, of anti-slavery advocates, or of

<sup>37</sup>Using these data on pre-Swing riots, we also test if riots that occurred in the neighborhood of the constituencies in the lead-up to each of the seven elections between 1802 and 1826 had an effect on the share of Whigs elected in those elections. We find no evidence of this (see Table S22 in the Supplemental Material).

<sup>38</sup>We discuss the alternatives in detail in Supplemental Material Section S4.

TABLE VIII  
 LOCAL SWING RIOTS AND THE OUTCOME OF THE 1831 ELECTION  
 ALTERNATIVE EXPLANATIONS FOR THE WHIG VICTORY<sup>a</sup>

|   | (1)                         | (2)                          | (3)                          | (4)                          | (5)                           |
|---|-----------------------------|------------------------------|------------------------------|------------------------------|-------------------------------|
|   | <i>Whig Share 1831</i>      |                              |                              |                              |                               |
|   | Least Squares               |                              |                              |                              |                               |
| Riots within 10 km                          | 0.37<br>(0.18)**<br>[0.19]* | 0.46<br>(0.18)**<br>[0.19]** | 0.52<br>(0.17)**<br>[0.18]** | 0.49<br>{0.19}**<br>(0.17)** | 0.51<br>{0.17}***<br>(0.16)** |
| Petitions against Catholic relief           |                             | 1.44<br>(1.24)               | 2.15<br>(1.18)*              | 2.19<br>{1.31}               | 2.14<br>{1.33}                |
| Petitions for Catholic relief               |                             | 0.82<br>(2.21)               | 2.35<br>(2.29)               | 2.85<br>{2.52}               | 2.26<br>{2.33}                |
| Petitions against slavery                   |                             | 0.27<br>(0.93)               | 0.61<br>(0.92)               | 0.62<br>{1.00}               | 0.64<br>{1.00}                |
| Petitions against reform                    |                             |                              | -8.02<br>(3.37)**            | -7.76<br>{4.50}*             | -7.83<br>{4.39}*              |
| Petitions for reform                        |                             |                              | -1.30<br>(0.95)              | -0.32<br>{1.84}              | -1.30<br>{1.03}               |
| Growth in poor law expenses                 |                             |                              |                              | 0.0081<br>{0.0050}           |                               |
| Special commission                          |                             |                              |                              |                              | -11.5<br>{5.27}**             |
| Share of harsh sentences                    |                             |                              |                              |                              | 0.18<br>{0.068}**             |
| <i>F</i> -test for added group <sup>b</sup> | N.A.                        | 0.87                         | 4.00**                       | N.A.                         | 4.35**                        |
| Baseline controls included <sup>c</sup>     | YES                         | YES                          | YES                          | YES                          | YES                           |
| With treasury boroughs                      | NO                          | YES                          | YES                          | YES                          | YES                           |
| Adjusted <i>R</i> <sup>2</sup>              | 0.44                        | 0.45                         | 0.46                         | 0.47                         | 0.46                          |
| Obs. (constituencies)                       | 230                         | 244                          | 244                          | 241                          | 244                           |

<sup>a</sup>Table VIII reports alternative estimates associating local Swing riots to the outcome of the 1831 election to account for the presence of Treasury Boroughs, the petitions for and against Catholic relief, petitions against slavery, petitions for and against parliamentary reform, growth in poor law expenses, and the level of repression. For *Riots within 10 km*, we report in columns (1) to (3) spatial (Conley (1999)) standard errors (50 km radius) in parentheses and White robust standard errors in square brackets; in columns (4) and (5) where *Growth in poor law expenses*, *Share of harsh sentences*, and *Special commission* are measured at the county level, the standard errors in curly brackets are clustered at the county level while those in parentheses are spatial (Conley (1999)) standard errors (50 km radius). In column (1), we exclude the 14 boroughs (28 seats) that were controlled by the Treasury around 1830 according to Philbin (1965). In column (4), three constituencies (London, Monmouth, and Monmouthshire) are excluded because Gonner (1912, Appendix B) does not report their poor law expenses.

<sup>b</sup>The null hypothesis is that the coefficients of the variables added in the column are all zero.

<sup>c</sup>The baseline control variables are those reported in Table II, column (5).

\*\*\*, \*\*, and \* indicate statistical significance at the 1%, 5%, and 10% level, respectively.

pro-reform political associations. The basis for this conjecture is that pressure groups became more and more effective at organizing successful campaigns to influence the House of Commons in the years after 1815. For example, the mobilization of dissenters and Catholics in the 1820s contributed to the adoption of the Catholic Relief Act in 1829, which gave the members of these religious minorities substantial political rights (Machin (1964), Jupp (1998, pp. 366–376)). Likewise, the London Society for Mitigating and Gradually Abolishing the State of Slavery Throughout the British Dominions and its 230 local groups had a large impact on the anti-slavery debate in the late 1820s. A common tactic of these pressure groups was to send petitions to Parliament calling for abolition of slavery or rights for Catholics. These petitions are recorded in the Journal of the House of Commons. Based on word searches for the names of the constituencies, we can quantify how many petitions related to religious issues or to the anti-slavery campaign originated from each of them between 1828 and 1831 (Journal of the House of Commons, vol. 83–86). We use these counts as proxies for civic mobilization at the constituency level. Table VIII, column (2) reports a regression where we add three constituency-specific measures of petitions: *Petitions for Catholic relief*, *Petition against Catholic relief*, and *Petition against slavery*. These three variables are insignificant, individually and jointly, and the point estimate on *Riots within 10 km* is similar to the baseline estimate in Table II, column (5). This result is, perhaps, to be expected since Catholic relief and the abolition of slavery were issues which cut across and not between political factions. Variations in local mobilization on these questions are, therefore, unlikely causes of the Whig victory in 1831.<sup>39</sup> In contrast, the attitude to parliamentary reform closely followed party lines, as discussed in Section 2.1, and it is, therefore, possible that the mobilization of political associations could account for the Whig victory in 1831. We use constituency-level petition data related to the reform question to test this conjecture. Table VIII, column (3) shows that the number of petitions against reform is negatively correlated with *Whig share 1831*, while the number of petitions in favor of reform is insignificant. The point estimate on *Riots within 10 km* is a little larger than in the baseline and significant at the one percent level. This, on the one hand, suggests that anti-reform mobilization was correlated with Tory electoral success. On the other hand, the effect of local Swing riots is larger and more precisely estimated than in the baseline. This is evidence against the conjecture that local reform agitators played a systematic role in organizing Swing riots.

<sup>39</sup>In addition to the petition data related to Catholic relief, we also use the 1851 Census of Religious Worship to construct alternative constituency-specific proxies for the density of religious networks. We match these data to the constituencies of the unreformed Parliament by attributing the return for the census district with the name of the constituency to that constituency. In Table S25, column (2) in the Supplemental Material, we find a positive correlation between the presence of a Catholic Church and the share of Whigs elected in 1831 and note that the point estimate on *Riots within 10 km* is larger and more precisely estimated than in the baseline.

The third alternative explanation is that local variations in poor law expenditures simultaneously determined the intensity of the Swing riots and the Whig success in 1831. This could have been the case, since responding to systematic underemployment and preventing disorder were among the objectives of the poor law system (see, e.g., [Blaug \(1963\)](#), [Boyer \(1993\)](#), and the related discussion in [Greif and Iyigun \(2013\)](#)). To test the hypothesis that higher poor law expenses might be systematically correlated with the ideological leaning of the voters and patrons in the constituencies, we use data from [Gonner \(1912, Appendix B\)](#) on the growth in poor law expenses between 1750 and 1813 in each county. Table VIII, column (4) shows that the variable *Growth in poor law expenditures* is insignificant and that there is no substantial change in the size or significance of the point estimate on *Riots within 10 km*.

The fourth alternative explanation is that the Whig victory was a consequence of the Whig government's effective repression of the Swing riots: it convinced voters and patrons that the Whigs could be trusted to protect private property rights and preserve social order in spite of their commitment to parliamentary reform. To investigate this possibility, we take advantage of variations in the harshness with which the disturbances were repressed in different counties ([Hobsbawm and Rudé \(1973, Appendix II\)](#)). Table VIII, column (5) shows that these differences have an ambiguous impact on the share of Whigs elected in 1831. On the one hand, the share of harsh sentences handed to convicted rioters has a positive and significant impact on the Whig success. On the other hand, the establishment of a special commission in a county to swiftly try rioters has a negative and significant effect. Importantly, however, controlling for these variables does not affect the size or significance of the point estimate on *Riots within 10 km*.

Overall, these results show that some of the alternatives have explanatory power for the Whig electoral success in 1831 but none of them weakens the effect of the local Swing riots.

## 6. CONCLUSION

This paper examines the “threat of revolution” theory of democratic change in the context of the Great Reform Act of 1832. Its focus on a specific democratic reform avoids many of the well-known pitfalls associated with cross-national comparisons (e.g., [Acemoglu, Johnson, Robinson, and Yared \(2008\)](#), [Przeworski \(2009\)](#), [Aidt and Jensen \(2014\)](#), [Campante and Chor \(2012\)](#)), but this obviously comes at the cost of external validity.

The results show that the reform-friendly Whigs would not have obtained a majority of seats in the House of Commons in the 1831 election had it not been for the violence of the Swing riots. Without such a majority, the reform process would almost surely have come to a stop. These results are robust to different identification strategies and their credibility is strengthened through falsification tests.

We acknowledge that there are several possible interpretations of the shift of the voters and the patrons toward the Whigs in 1831 other than the local Swing riots. As discussed in Section 5.5, these interpretations are, however, not fully compelling. Our interpretation of the results is that the voters and patrons shifted their allegiance toward the Whigs in the wake of the Swing riots that they had observed first-hand in the immediate neighborhood of their constituency because they feared that the rioters could become useful allies for the segments of the population that supported far-reaching political changes. Such fears could have been stoked by the fact that Radical orators took up the Swing riots in their lectures in London (Royle (2000, p. 85)). By returning a Whig (rather than a Tory) in 1831, the voters and patrons knew that they were helping the Whig leadership build the required majority to pass the reform bill. Although it was clear that the rioters themselves would not obtain voting rights if the bill succeeded, the limited extension of the franchise sought by the Whig leadership would be sufficient to appease upper-class and middle-class dissidents who might take advantage of agricultural riots to initiate a revolution. In short, we argue that the local Swing riots observed first-hand by voters and patrons made the prospect of revolution tangible in their minds and convinced them that the extension of the franchise to a limited section of the population was necessary; they reacted by backing reform-friendly Whig candidates in the 1831 election; this gave the Whig leaders, who were already committed to parliamentary reform prior to the Swing riots, the majority they needed to take the reform process forward. Taken together, the evidence from the Great Reform Act is, therefore, consistent with the “threat of revolution” theory.

It is, however, important to put our study in its historical context. Our paper suggests that the “threat of revolution” played a role in *one* of the five critical hurdles that the reform bill had to pass on its way to the statute book. This leaves ample space for other factors to have played a role, including rivalry within the elite between the Whigs and the Tories as well as constitutional bargaining between King and Parliament. We, therefore, stress that we are not claiming that the “threat of revolution” was the only causal factor at play. In fact, since the reform process was already underway by the time of the 1831 general election, our study does not explain why the reform process began in the first place and why the Whig leadership was committed to reform. Moreover, our study should not be interpreted as proof that the Unreformed Parliament would never have been reformed without the shadow of the threat of revolution induced by the Swing riots. Most surely, it would have been, but not in 1832.

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