Ballot or Bullet: The Impact of the UK's Representation of the People Act on Peace and Prosperity

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Abstract

Abstract. Does democracy curb domestic political violence? To study this, we focus on the United Kingdom's Representation of the People Act of 1867 – which is a critical juncture in the history of democratization. Constructing a novel borough ("urban center") level dataset on social conflict events and economic performance around the 1868 Elections (the first elections where newly enfranchised citizens could vote), we exploit arguably exogenous variation in enfranchisement intensity. We find a strong and robust peace-promoting effect of franchise extension and identify as a major channel of transmission the increase of the population's political influence (voice) and local economic growth.

Short title: Ballot or Bullet

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Keywords: Social Violence, Social Conflict, Riots, Democracy, Enfranchisement, Franchise Extension, Voting, Elections, Voice, Growth, Prosperity, Development.

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"This is why I say it's the ballot or the bullet. It's liberty or it's death. It's freedom for everybody or freedom for nobody." (Malcolm X)

"The Reform Act of 1867 was one of the decisive events, perhaps the decisive event, in modern English history. It was this act that transformed England into a democracy (...)." (Gertrude Himmelfarb)

1 Introduction

The extension of voting rights has often been used by elites as a way to stave off civil unrest. This phenomenon is well-documented in political economics research (see Acemoglu and Robinson (2000) and related work).[1] However, as later explained, empirical evidence in this domain is lacunary at best. Most of the existing literature explaining the relationship between voting rights and conflict lacks exogenous variation and is confined to correlational and cross-country evidence, and risks being biased by various confounders. Recent advances in Optical Character Recognition (OCR) and Natural Language Processing (NLP) combined with the availability of comprehensive digital newspaper archives now make it possible to carry out fine-grained within-country investigations, allowing our research to address the caveats of past findings and uncover causality.

In an attempt to address this literature gap, the current paper focuses on England's particularly decisive "Age of Reform" in the 19th century Victorian era. Suffrage extensions from this period have not only shaped modern Britain but have also inspired the rise of liberal democracies around the world. Among the series of notable reforms, the 1867 UK's Representation of the People Act, also referred to as Second Reform Act, has resulted in the largest relative surge in enfranchised voters – roughly doubling the electorate at the time. In addition to being of great historical importance, this reform act implemented a countrywide property value threshold

[1] One strand of the literature sees democracy as commitment device, with democratization leading to redistribution in favor of the population, thereby reducing reasons for revolt (see Acemoglu and Robinson (2001), Acemoglu and Robinson (2005), Fearon (2011), and Bidner et al. (2014)). In contrast, in another strand of the literature, democracy reduces asymmetric information and through this channel curbs the risk of conflict (Laurent-Lucchetti et al. (2019)). Both types of settings yield the prediction that democratization reduces the likelihood of conflict.

to determine voter eligibility. Our research exploits this arguably exogenous threshold in the identification strategy. In a nutshell, the turbulence of this key historical period, together with the exogenous variation available and the newly assembled data, offers an opportunity for novel evidence on the aforementioned classic research question.

To study the impact of this reform act on social conflict, we have built a novel panel dataset of borough level monthly data which includes new measures of social conflict, local economic growth, and several other control variables. Prior to 1867, voting rights were related to residential property values - a clear proxy for income and wealth. In particular, men owning a property with a rental value of at least ten pounds were eligible for voting. The reform extended suffrage to all men, irrespective of the rental value of their property or whether they owned property but excluded the very poor, defined as those not paying taxes (i.e. the so called "poor rates"). We leverage information on the pre-reform distribution of property rental values in each borough to predict the increase in eligible voters. More specifically, we draw on the arguably exogenous idiosyncratic variation around the previous voting threshold of a housing rent value of ten pounds. Instrumenting the extent of franchise extension using this idiosyncratic variation, we estimate how enfranchisement mattered for the risk of social conflict. We detect a strong and significant pacifying effect from suffrage extension. Quantitatively, the average increase produced by the reform (i.e. roughly doubling the number of electors) reduced likelihood of social conflict by over 12 percentage points, which amounts to about half of the baseline conflict risk. Expressed in terms of standard deviations, an enfranchisement change of one standard deviation (roughly 40% more voters) resulted in a decreased risk of social conflict of about 0.13 standard deviations. Interestingly, we find that it is not at the moment of the *de jure* passing of the act that it deploys its effects but only when the *de facto* change in representation has materialized. This is quite intuitive, as registration offices where struggling to register all new voters (Davis and Tanner (1996)), hence making it natural for newly eligible voters to have residual doubts on the process before the elections actually took place.

When investigating the specific mechanisms at work, we find that enfranchisement has led to a substantial rise in the population's political influence (which we will refer to as *voice*) and state accountability. In particular, we find that the participation of new voters in the 1868 elections

has made UK politics more competitive and has increased elected representative turnover. This strengthened civic engagement was also reflected by a substantial increase in the number of petitions received by the House of Commons. Importantly, drawing on novel data from job advertisements, we also detect a strong and significant impact of franchise extension on boosting local economic growth, in particular in areas with large market potential. In terms of magnitude, increasing the number of electors by 89% (the average increase produced by the reform) is associated with an increase in economic activity of around 15.5%. Our findings indicate that more inclusive, pluralistic political institutions may foster economic growth, coinciding with recent findings of Acemoglu et al. (2019) and Abeberesey et al. (2020), which in turn provides a fertile breeding ground for peace (in line with e.g. Miguel et al. (2004); Dell et al. (2014); König et al. (2017)).

One caveat of the analysis is that it is inherently short-run, which makes it difficult to study mechanisms and channels of transmissions that take substantial time to materialize. In particular, our results are unable to detect any increase in state capacity and public spending linked to increased enfranchisement: this is unsurprising given that such changes are only likely to materialize in the long run.

The current paper is related to several different areas of research. First, an empirical literature establishes correlations between democracy and social conflict at the country level. These studies fail to plausibly demonstrate causal links as they do not exploit exogenous variation in democratization. In addition, these papers find overall contradictory results. In particular, Fearon and Laitin (2003) find no significant effect of democracy, while Besley and Persson (2011a) conclude that the conflict-fuelling effect of negative shocks is muted by cohesive institutions. There is also evidence that the relationship between democracy and conflict is non-linear: Hegre et al. (2001) conclude that full democracies and full dictatorships are associated with a lower conflict risk than intermediate regimes, Collier and Rohner (2008) find that democracy is linked to a lower conflict risk in rich countries while there is no beneficial effect in poor countries and Cervellati and Sunde (2013) detect a peace-promoting impact of "third wave" democratization mostly when transitions are non-violent. Furthermore, Fetzer and Kyburz (2018) conclude that local elections in Nigeria lead to more cohesive institutions that limit distributional conflicts

between groups. In terms of potential pitfalls, Esteban et al. (2015) stress that while consolidated democracy may be associated with fewer mass killings, initial democratization can induce an increased risk of violence. This finding is consistent with evidence from case studies documenting how ill-managed democratic transition instigates spikes of nationalist conflict (Snyder (2000), Mann (2005)). Likewise, Fergusson et al. (2020) find that narrow elections of previously excluded left-wing parties in Colombia leads to backlash from right-wing paramilitaries. Also related to this type of literature, Collier and Vicente (2014), Cederman et al. (2013) and Dercon and Gutiérrez-Romero (2012) study episodes of (post-)electoral violence.

Another relevant branch of literature studies the drivers of franchise extension, including namely Acemoglu and Robinson (2000), Lizzeri and Persico (2004), Llavador and Oxoby (2005), Przeworski (2009), Doepke and Tertilt (2009), Aidt and Jensen (2014), Aidt and Franck (2015).[2] These works closely relate to more theoretical writings analysing how and why democracy reduces the scope for conflict. The democracy-peace nexus can be explained by the following mechanisms: the ability of the population to select tax rates and potentially enhance redistribution (see e.g. Acemoglu and Robinson (2001) and the literature on democracy as commitment device), the capacity of democracy to reduce asymmetric information (Laurent-Lucchetti et al. (2019)), or strengthened state accountability under democracy (Collier and Rohner (2008)) – which may in turn improve state governance and boost the economy, thereby reducing grievances and increasing the opportunity cost of rebellion. Yet, democracy may also provoke adverse effects, by exacerbating electoral competition (see discussion above on the dangerous transition to democracy and on (post-)electoral violence), as well as galvanizing subversive uprisings through freedom of assembly.

In light of these potential channels and mechanisms, it is also important to mention the series of papers studying other implications of franchise extension and democracy, i.e on public finances (Aidt et al. (2006), Aidt et al. (2010)), on economic growth (Acemoglu et al. (2019), Abeberesey et al. (2020)), or on health outcomes (Besley and Kudamatsu (2006), Kudamatsu (2012)).

[2]Our study of the effects of a massive, universal franchise extension for all ethnic groups (almost doubling in our case the number of eligible voters) is also complementary to the research on the effects of more specific laws or measures improving the representation of given minority groups (see Fujiwara (2015), Mueller and Rohner (2018), Facchini et al. (2020), Lacroix (2020)).

Furthermore, the current contribution is also part of the e conomic history literature studying Victorian England (Aidt et al. (2010), Berlinski et al. (2011), Berlinski et al. (2014), Aidt and Franck (2015), Aidt and Franck (2019), Chapman et al. (2020)).

Lastly, this paper is part of the emerging literature analysing how institutions and policies are able to reduce the scope of conflict, using arguably exogenous policy variations (see the recent literature survey of Rohner (2022)). Other contributions in this line of research have focused for example on the impact of food aid (Nunn and Qian (2014)), education (Rohner and Saia (2019)), grand coalitions (Mueller and Rohner (2018)) or reconciliation ceremonies (Cilliers et al. (2016)).

In a nutshell, the novel contribution of this paper is twofold: First, moving beyond correlational evidence at the country-level, it studies the impact of a very large-scale and arguably exogenous franchise extension on social conflict, drawing on newly constructed fine-grained social conflict data. Second, this more granular, newly assembled data allows us to investigate the mechanisms at work that link enfranchisement to civic peace.

The remainder of this paper is structured as follows. Section 2 describes the historical context of the UK's Second Reform Act. Section 3 describes the data used, Section 4 lays out the identification strategy and Section 5 presents the baseline results and a series of robustness tests. Section 6 is dedicated to a discussion of the main mechanisms at work. Finally, section 7 concludes. An extensive (Online) Appendix contains further detailed explanations and additional results.

2 Historical Context

Britain's 19th century has often been referred to as the "Age of Reform", during which several franchise extensions have made politics increasingly inclusive.[3] A first milestone was the 1832 "First Reform Act", which extended suffrage to the middle class. This reform act was introduced

[3]The following description draws on the accounts of Himmelfarb (1966), Smellie (1968), Fraser (1976), Davis and Tanner (1996), Zimmerman (2003), Schlager (2004), Lizzeri and Persico (2004), Saunders (2007), Lawrence (2009), Aidt et al. (2010), Berlinski et al. (2011), Turner and Zhan (2012), Berlinski et al. (2014), Aidt and Franck (2015), Chapman et al. (2018), Aidt and Franck (2019), Chapman et al. (2020), www.parliament.uk, the "Encyclopedia Britannica" and the "St. James Encyclopedia of Labor History Worldwide".

to appease (violent) popular demands for voting rights (with the 1831 "Queen Square" riots in Bristol being a famous example of such popular unrest). However, as the popular elite wanted to maintain political power, the electoral law only allowed men owning (housing) property worth at least £10 to vote, which precluded the working class from enfranchisement.

Thus, political protest continued, mostly fuelled by the urban and unfranchised working class (Harrison (1962)). After additional attempts to restore peace with insufficient reforms, the next major advance in enfranchisement was the "Second Reform Act" (known formally as the Representation of the People Act of 1867) granting voting rights to the urban male labour class in England and Wales for the first time. While introduced by the Conservative government under Prime Minister Benjamin Disraeli, it benefitted from support beyond party lines. It received Royal Assent by the British Crown on August 15, 1867. The bill was to be implemented in progressive stages over the following years as decided by the UK Parliament. The population's newly found political participation culminated in the 1868 elections, where incumbent Conservatives led by Prime Minister Benjamin Disraeli were defeated by the Liberals, paving the way for the Liberal William Gladstone to become the new Prime Minister.

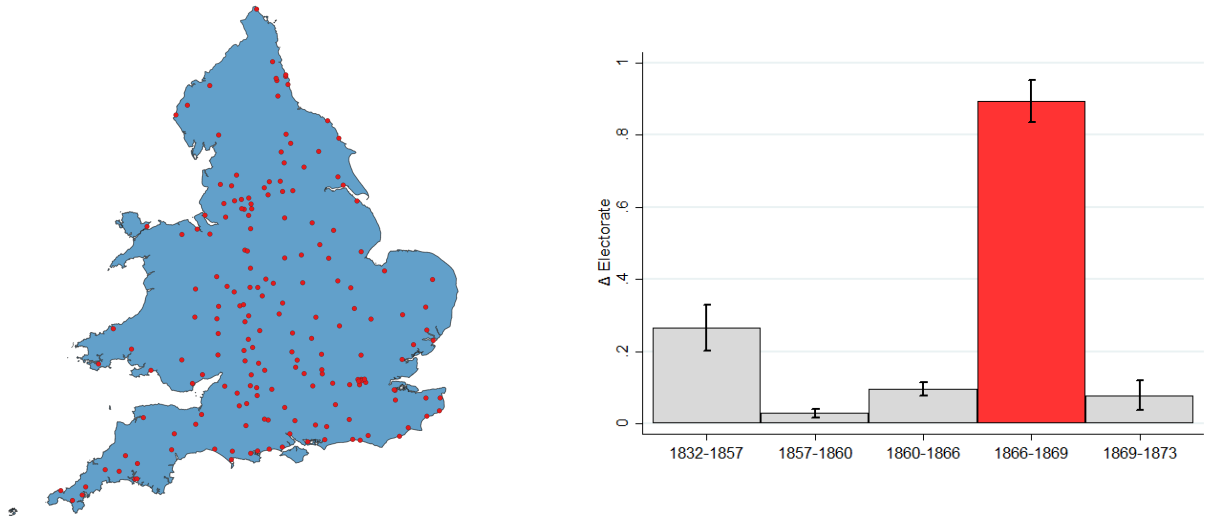
The 1867 reform bill abolished the £10 per year threshold and granted voting rights to all house owners or occupiers (householders) in English and Welsh boroughs that had resided during the last 12 months and paid their taxes (i.e. the so-called "poor rates"). While there is no clear-cut rent threshold for householders being subject to paying the poor rates tax, the typical threshold may lie somewhere around £4.[4]

This 1867 reform took place in a climate of intense debate between Progressives, who wanted to extend suffrage to the greater population, and Conservatives, who were afraid of any radical extensions, that would confer too much political power to the working class that they perceived as untrustworthy (Zimmerman (2003)). In the end, the Representation of the People Act of 1867 was one of the most decisive reforms in English history (Himmelfarb (1966)) and resulted in a path-breaking electoral reform: men who occupied or owned a house in urban areas and who paid taxes (the so called "poor rates") were enfranchised, which roughly doubled the electorate

[4]Additional details on the Reform and on the rationale for using £4 as threshold for taxation (and hence voting rights) in the empirical analysis are provided in the Appendix A.

in England and Wales from one to two million men. Panel B in Figure 1 emphasizes that the 1867 reform was a particularly important milestone in the history of UK electoral reform as it massively increased the numbers of newly enfranchised voters.[5] This paper exploits the 1867 "Second Reform Act" to study the effect of franchise extension on the likelihood of political unrest.[6]

Figure 1: British Boroughs and Electorate growth over 19th Century



(A) Boroughs included in the sample (B) Electorate growth

NOTE: Panel A: Map of England and Wales with each dot representing a given borough included in the sample. Panel B: Each column displays the growth rate in the number of electors in British boroughs over different elections. Black lines indicate the 95% confidence interval. Electoral data are taken from official accounts of the parliamentary papers (House of Commons (1857), House of Commons (1866a), House of Commons (1869b) and House of Commons (1874b)). The timing of measurement of the electorate is not evenly spaced (hence, over longer time spans population growth could affect electorate growth) and does not always correspond to election years.

3 Data

We build a novel panel dataset at the borough [urban area] and month level. The baseline monthly dataset covers 184 boroughs of England and Wales over the period 1868-1869 (24 months around the 1868 elections, the first elections after the passage of the Second Reform Act).[7] In our analysis we focus on boroughs that did not experience changes in their enfranchisement

[5] One relevant question to ask is why there was not more backlash of members of the elite against this reform. One can think of several reasons. First of all, the Second Reform Act still contained some minimum wealth requirements for being able to vote. Hence, the poorest parts of the population were still excluded from politics, attenuating the elite's fear of radical redistribution. Further, opposing openly a new act that had been voted in Parliament and had the support of both major parties (liberals and conservatives), as well as of the Prime Minister and the Queen would have been a risky endeavour.

[6] In the years after our sample period the UK government also tightened rules on firearms (e.g. with the 1870 Gun License Act and the 1903 Pistols Act).

[7] In Panel A of Figure 1 are mapped all boroughs included in the sample.

status, i.e. those that were enfranchised both before and after the reform and for which we exploit the "intensive margin" of numbers of individuals able to vote.[8] For robustness checks we also construct an extended longer time-span dataset, as well as a more fine-grained dataset at the weekly frequency.

3.1 Social Conflict Data

The dependent variable of interest we study is social conflict outcomes. In particular, in the baseline specifications we focus on the extensive margin of social conflict events, relying on a dummy variable taking a value of 1 in a given borough and month where at least one social conflict event took place and 0 otherwise. In the robustness checks, we also investigate the intensive margin and construct a variable of social conflict intensity at the borough and month level.

To construct our novel social conflict data, we start from the British newspaper archive and perform data scraping of a sample of over 300,000 newspaper articles containing at least one social conflict-related keyword.[9] This sample of newspaper articles stems from pieces published over our sample period by over 500 national or local newspapers. Our algorithm codes as social conflict observation news reports containing social conflict-related keywords linked to a given borough location. For illustration, the five newspapers with widest national cover of social conflict events for our sample period where London Evening Standard, The Sun, The Scotsman, Morning Post and London Daily News. The full list of newspaper sources is reported in Appendix F.[10]

[8]Importantly, applying this criterion implies that 87 percent of the boroughs are included in the sample (184 out of 211). The few missing ones are boroughs that either before or after the reform were disenfranchised (e.g. as sanction for mismanagement). The main reason that led to our choice is the fact that we are unable to retrieve the number of electors for the disenfranchised boroughs after the passage of the act and the number of electors for the enfranchised boroughs before the passage of the act. This is due to the fact that in those cases information on the numbers of electors is provided at a different level of aggregation over time. For example, for a disenfranchised borough, after the passage of the act the number of electors is only available at the level of the county constituency.

[9]The list of keywords used in the baseline analysis are Disturbance, Disturbances, Unrest, Riot, Riots, Rioters, Rioting, Tumult, Tumults, Disorder, Disorders. For more details see Appendix D.21.

[10]To provide –for the purpose of illustration– a few examples of social conflict-related events covered by our data, consider e.g. riots in Ashton-under-Lyne, Stalyridge, Bristol, Cardiff, Bolton, North Shields, among many others. See also some examples of newspaper articles in Appendix Figure A2.

One potential issue with social conflict data generated using historical newspaper articles is that factors such as media pressure (i.e. originating from other timely topics needing coverage) could affect reporting (see e.g. Jetter (2017); Durante and Zhuravskaya (2018)). To address this, in the Appendix B.3 we show that social conflict data generated from our historical newspapers correlates strongly with data from an established existing social conflict dataset provided from an external source (Holland (2005)), and recently used e.g. by Caprettini and Voth (2020) (see Appendix Table A3). Potential reporting bias could also arise if media owners have vested interests, as far as enfranchisement is concerned. Thankfully this concern is attenuated by the very short sample period over which media ownership is unlikely to change.

3.2 Electoral Data

The main explanatory variable is the number of enfranchised citizens. The first election taking place after the passing of the Second Reform Act in 1867 is the 1868 United Kingdom general election [17 November – 7 December 1868]. The number of voters in a given borough is time-varying over our sample period, corresponding to the pre-reform number of voters until October 1868, and the post-reform voter numbers from November 1868 onwards. The number of enfranchised electors before and after the reform are taken from official accounts of the parliamentary papers (House of Commons (1866a) and House of Commons (1869b)).[11] Note that in a robustness analysis we move the analysis from the monthly to the weekly level (Appendix D.14), which allows to take into account that the elections only started in mid-November. We have also assembled additional voting statistics and electoral data from House of Commons (1866b), House of Commons (1869a), House of Commons (1874a) and Craig (1977). Finally, we have also collected data for other elections before and after 1868 (drawing on elections 1865 and elections 1874 data from House of Commons (1857), House of Commons (1866a) and House of Commons (1874b)), which we use for a placebo analysis.[12]

[11] In particular, we use information on the "total number of voters on register of each borough" available in the various Parliamentary Papers. This value corresponds to "the number of electors on the register in force at the time of the election" available in the book "British Parliamentary Election Results 1832-1885" (Craig (1977)).

[12] Our main focus is on the 1868 United Kingdom general election [17 November – 7 December 1868], that are the first election after the passing of the Second Reform Act (August 1867). In the placebo analysis we use the 1865 general election [11–24 July 1865] and the 1874 general election [31 January – 17 February 1874].

3.3 Instrumental Variable

As discussed in more detail below, we instrument for the number of newly enfranchised voters after the reform by exploiting idiosyncratic variation at the borough level in the structure of rents paid by householders. As later argued in further detail, when controlling for the average rents and inequality of rents, any remaining variation around the £10 threshold can be seen as quasi-exogenous. Put differently, if two boroughs have the same average rents and rent inequality, but one has for some reason a higher share of rents below the pre-reform £10 cut-off (close to the threshold), this borough will (exogenously) experience a larger increase in new voters. To construct this instrument and the corresponding control variables, we draw on the fine-grained rents distribution data from House of Commons (1866a) (see Appendix B.2 for further information on the rents data).

3.4 Other Data

A series of control variables are included in the baseline and robustness specifications, namely average gross estimated rents and Gini index of rents, both computed using data from House of Commons (1866a), as well as population variables from House of Commons (1866a). Additionally, other borough-level employment-based variables (i.e. the share of working population, the share of elementary occupations, and the gender-ratio) have been constructed using the 1861 Population Census conducted by the Secretary of State of the United Kingdom (IPUMS (2020)). We have also constructed novel time-varying data on job ads at the borough level (as described in detail in Appendix B.4).

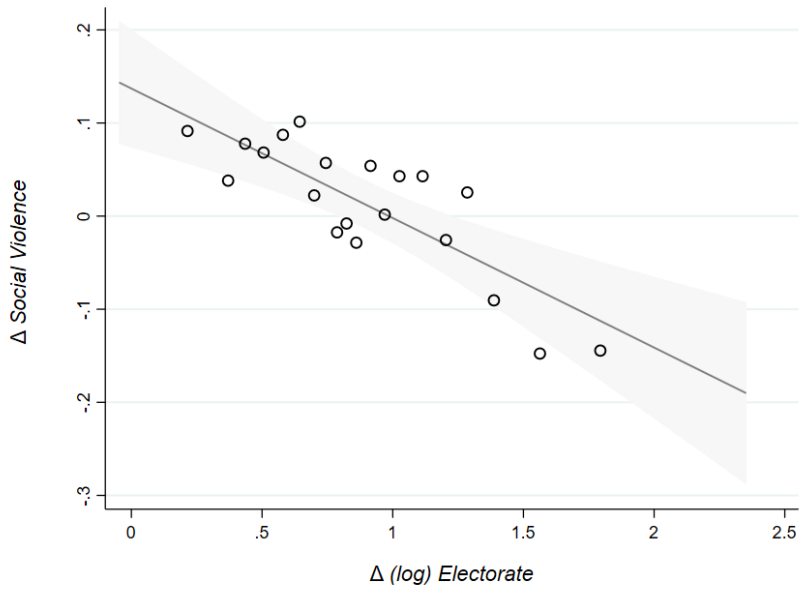
3.5 Descriptive Summary Statistics and Raw Association

The summary descriptive statistics are displayed in Appendix B. In particular, Table A2 depicts the key moments of the main variables of the analysis. Among the 4,416 borough-months in our sample roughly 23 percent experienced social conflict. As far as the increase in the electoral base is concerned, during our Second Reform Act period of interest (1866 to 1869) the number of eligible voters almost doubled, while over the other periods concerned (before and after) average franchise extension was in the order of magnitude of between 3 to 27 percent. The

summary statistics of various further variables reveal additional interesting patterns, e.g. that public spending did not increase between 1868 and 1869, which is in line with the discussion of mechanisms below. Moreover, Appendix B.2 provides further information on rents, such as the distribution of rents in Figure A1. Table A64 lists average rents, rent inequality (measured using the Gini Index), as well as the number of householders in total and per relevant rent bracket for each borough.

Figure 2 below provides a bin scatter illustration of the raw association between enfranchisement and social conflict reduction. While this negative correlation between the changes in electors and social conflict is visually striking, it could be partially driven by confounders. Hence, in the remainder of the paper, we study in much more depth how franchise extension affected social conflict, drawing on arguably exogenous variation in the level of enfranchisement.

Figure 2: Raw association between enfranchisement and change in social conflict



NOTE: Bin scatter graph with each bin summarizing values of several boroughs. The red line represents the linear prediction plot based on the underlying data. The x-axis depicts the Δ in the (log) electorate pre and after the Second Reform Act observed in a given borough, while the y-axis plots the difference in (average) social conflict between the period before and after November 1868.

4 Model Specification and Identification

4.1 OLS Specification

In the goal of identifying the impact of franchise extension on social conflict, we perform a difference-in-differences analysis exploiting the variation of the total number of electors over time and across boroughs. We start with the following specification for the OLS regressions:

$$Social\ Violence_{it} = \beta_0 + \beta_1\ (log)\ Electorate_{it} + FE_i + FE_t + \epsilon_{it}, \tag{1}$$

where the variable $Social\ Violence_{it}$ is a dummy that takes a value of 1 if a violent event was observed in a borough i and during month t. $Electorate_{it}$ represents the number of registered electors at the time of the election observed in borough i during period t.[13]

Our identifying variation originates from the fact that the intensity of new electors enfranchised in the Elections of 1868 varies widely across boroughs. The specification features borough fixed effects (which filter out time-invariant borough characteristics such as e.g. elevation, sea access, longitude and latitude) and monthly time dummies (which control for country wide shocks, such as e.g. major political and economic nationwide shocks). The standard errors are clustered at the level of the 184 boroughs in all regressions (unless indicated otherwise).

4.2 Instrumentation

The above specification has the advantage of filtering out time-invariant borough characteristics and nationwide shocks, but one may worry that borough-specific shocks and trends could confound with the coefficient of interest of franchise extension. In particular, it could be that poorer boroughs experience a bigger franchise extension and at the same time are subject to shocks or trends that foster "gentrification" and tackle social unrest. Any pacifying effect attributed to enfranchisement could hence be spuriously driven by borough-specific socio-economic changes and policies.

Another potential identification threat is related to the fact that our variable of interest

[13]Note that given that we control for borough fixed effects, the estimates would be identical if we had our explanatory variable based on the share of the population registered as electors instead of the absolute number of registered electors.

corresponds to registered voters. Given the burden of registration procedures and the role of potential partisan and non-partisan interests in the registration process (see Davis and Tanner (1996)), it could be that the increase in enfranchisement may be affected by borough-level social dynamics. For example, one could imagine that in boroughs where poorer citizens have more political momentum, there is more registration (and hence enfranchisement) and more redistributive policies, reducing the scope for social unrest. Further, in more conservative boroughs with anti-enfranchisement views registration could be made deliberately complicated, hence limiting the number of new voters, while more liberal areas may embrace franchise extension wholeheartedly. Hence, a correlation in OLS between franchise extension and peace could be spurious and driven by the underlying momentum of liberal forces in a given borough.

To address such concerns, we run two-stage least square (2SLS) regressions where we instrument for the scope of enfranchisement by exploiting idiosyncratic –arguably as good as random– variation in the number of newly enfranchised householders for each borough. As discussed above, the reform led to the removal of the previous administrative threshold of £10 rental value. If there are two boroughs, A and B, with exactly the same average rents and same rent inequality, but for some idiosyncratic reason in borough A there is a slightly higher mass of citizens with rental value right below the previous £10 threshold, borough A will, for quasi-random reasons, experience greater enfranchisement than borough B. We control for average rents and rents variance to filter out potentially confounding effects of prosperity and inequality. Hence, in our instrumental variable strategy, the remaining identifying variation in mass around the £10 threshold can arguably be seen as good as random.

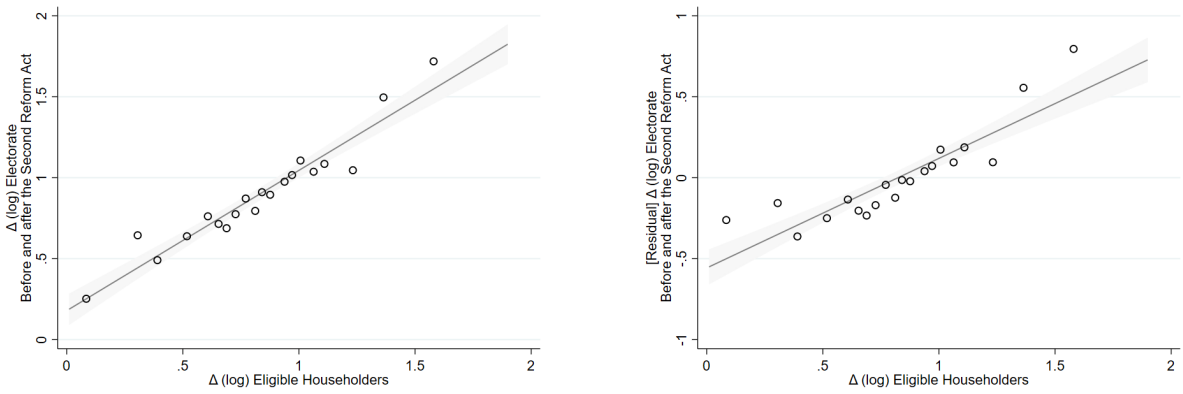
In particular, our instrumental variable (IV) is labelled *Eligible Householders*$_{it}$ and corresponds to the eligible population in each period. In particular, in the period before the Election of 1868, the value of our variable *Eligible Householders* is equal to number of householders above £10, while in the period after the election it is equal to the number of householders living in a house with rental value above £4 (i.e., those living in a house with rental value between £4 and £10 who were previously banned from voting but enfranchised by the Second Reform Act plus the number of householders with rental value above £10 who could already vote before the

reform).[14]

Figure 3 illustrates graphically the identifying variation of the first stage of our 2SLS estimation. We see that our instrument is strongly correlated to the increase in the number of electors in a given borough. Note that the raw correlation (Panel A) also holds for the residual correlation when controlling for average rents and inequality in rents (Panel B).[15]

As far as the exclusion restriction of the IV is concerned, the instrument is valid under the assumption that the share of householders newly eligible to be enfranchised by the Reform only affects the social conflict risk through the increase in the electorate and not through some other channel. The plausibility of this assumption is supported by the fact that we control for both the average level of rents and for the inequality of rents. Hence, given that these controls both account for the general level of wealth, prices and inequality of a given borough, the remaining variation in rents amounts to quasi-random idiosyncratic variation around the previous voting threshold of £10.

Figure 3: Δ (log) Electorate pre and after the Second Reform Act and Δ (log) householders below threshold



(A) Unconditional (B) With Controls

NOTE: Panel A displays the values of Δ (log) Electorate pre and after the Second Reform Act and Δ (log) householders below threshold in 184 English boroughs along two axes. Panel B displays the values of the residual of Δ (log) Electorate pre and after the Second Reform Act when control for average rents and inequality in rents and Δ (log) householders below threshold in 184 English boroughs along two axes.

[14]Note that in terms of terminology we use the expressions "householders above £10" and "rent value > £10" interchangeably (and analogously for other thresholds).

[15]In Appendix C we present the bin scatter illustration of the raw association between our instrument and social conflict reduction.

5 Main Results

Table 1 displays the main results. In column 1, we start with the OLS specification where we directly regress social conflict incidence on the number of electors. We include borough fixed effects and time dummies, which makes this specification a classic difference-in-differences setting. We find a sizeable effect of enfranchisement reducing social conflict which is statistically significant at the 1 percent level. Quantitatively, we find that increasing the number of electors by the average increase produced by the reform (89%), reduces the social conflict likelihood by over 12 percentage points, which amounts to about half of the baseline conflict risk. Expressed in terms of standard deviations, a one standard deviation change in enfranchisement (roughly 40% more voters) results in a decreased risk of social conflict risk of about 0.13 standard deviations.

In column 2, we add average rents interacted with monthly time dummies, and, in column 3, we further include the interaction of rent inequality with monthly time dummies. It turns out that the coefficient magnitude remains very stable and the statistical significance high when controlling for the average and distribution of rents. Hence, while conceptually important to control for average prosperity and inequality, it turns out that for our sample the results are virtually unchanged when these controls are added.

In columns 4-6 we estimate the reduced-form impact of our instrument on social conflict incidence. We find that the increase in eligible voter householders significantly reduces the potential for social conflict. This result holds in a specification controlling for the same batteries of fixed effects, averages, and inequality of rents as in the first three columns.

Table 1: Democracy and Social Violence: Main Table

Dep. Variable: Social Violence$_{it}$	(1) OLS	(2) OLS	(3) OLS	(4) RF	(5) RF	(6) RF	(7) 2SLS	(8) 2SLS	(9) 2SLS
(log) Electorate$_{it}$	-0.139***	-0.139***	-0.141***				-0.169***	-0.176***	-0.175***
	(0.0421)	(0.0446)	(0.0457)				(0.0562)	(0.0627)	(0.0620)
(log) Eligible Householders$_{it}$				-0.146***	-0.158***	-0.158***			
				(0.0483)	(0.0550)	(0.0549)			
Observations	4,416	4,416	4,416	4,416	4,416	4,416	4,416	4,416	4,416
R-squared	0.389	0.392	0.397	0.389	0.392	0.396	-	-	-
1st stage F-Stat	-	-	-	-	-	-	232	194	184
Borough FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Time FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Average Rents$_i$ * *Time FEs*	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes
Rent Inequality$_i$ * *Time FEs*	No	No	Yes	No	No	Yes	No	No	Yes
Sample Mean	.237	.237	.237	.237	.237	.237	.237	.237	.237

NOTE: The unit of observation is borough i and month t. The sample covers 184 boroughs over the period 1868-1869. LPM (2SLS) estimates are reported in columns 1-6 [7-9]. The dependent variable is a dummy that takes a value of 1 if a violent event was observed in borough i and month t. The social violence data was constructed using national or local newspapers available on the *British Newspaper Archive*, following the approach described in Section 3.1. The variable *(log) Electorate*$_{it}$ corresponds to the electorate pre and after the Elections of 1868 in a borough i. The variable *(log) Eligible Householders*$_{it}$ corresponds to the number of householders with rental value above £10 for the period before the Elections of 1868 and the number of householders living in a house with rental value above £10 and between £4 and £10 (who were previously banned from voting but enfranchised in the Second Reform Act) for the period after the Elections of 1868. Borough-level rent-based variables have been computed using data from House of Commons (1866a). Robust standard errors clustered at the borough level are reported in parenthesis. Statistical significance is represented by * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Finally, in columns 7-9 we perform a two-stage least square (2SLS) estimation where our IV of the number of eligible householders is in the first stage used to instrument for the number of electors and in the second stage the estimated electors number is used as regressor on the dependent variable of social conflict incidence. We find that our instrument is a strong predictor of the number of electors (as pointed out by the F-stats that are substantially above the conventional threshold of 10) and that an increase in electors statistically significantly drives down the risk of social conflict.[16] It is equally important to note that the coefficient size of the 2SLS estimation is of a similar order of magnitude as in the OLS estimation, and is very stable across all 2SLS results (i.e. across columns 7-9). This may be consistent with the view that most *de jure* eligible new voters actually ended up complying (registering), and that registration biases and other endogenous confounders were rather limited.

5.1 Robustness Analysis

In this subsection we briefly list the main robustness tests performed. In the interest of space, they have been relegated to the Appendix.

Additional Controls The first set of robustness checks focus on adding further control variables. In particular, for the purpose of addressing concerns on potential confounders, in Appendix D.1 a battery of additional socio-demographic control variables from the 1861 census are included (interacted with time fixed effects), while in the Appendix D.2, we include flexible functional form controls for rents brackets interacted with monthly time dummies, respectively. Appendix D.2 also shows that our rent bracket of interest between £4 and £10 is the only rent bracket that is a statistically significant predictor of conflict. Next, Appendix D.3 controls for fixed effects at the spatial NUTS3 times month level and Appendix D.4 accounts for electoral changes over the previous elections. Finally, Appendix D.5 includes alternative inequality measures. Our results prove robust to all these additional controls.

Alternative Instruments In a second set of robustness checks we investigate whether our

[16]The strength of our instrument is also confirmed by the Olea-Pflueger robust test for weak instruments (Olea and Pflueger (2013)). The Effective F-statistic for column 7 is equal to 223 (and the critical value for the null hypothesis that the 2SLS bias exceeds 5 percent of the OLS bias is equal to 37.418).

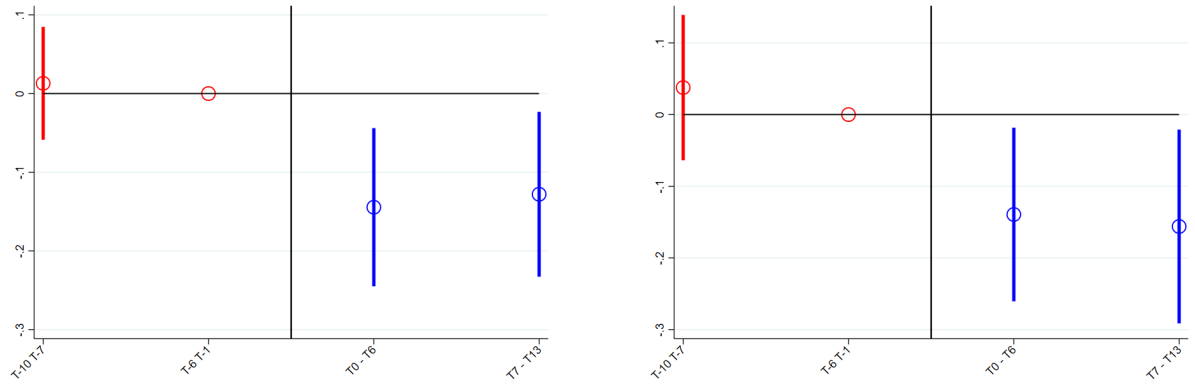
findings are sensitive to the exact way of constructing our instrument. While in Appendix D.6 an alternative property threshold of £0 is considered (i.e. we include in the instrument householders for any rent band), Appendix D.7 reproduces our baseline results but drawing on earlier rent data (addressing concerns on rent manipulation). Our results remain very similar for all these specifications.

Alternative Dependent Variable To assess the scope of our findings, in Appendix D.8 we display the results for an alternative dependent variable, focusing not only on the extensive margin, but also on the intensity of social conflict incidence. We find that enfranchisement does not only affect the likelihood of social conflict, but also its intensity (e.g. measured by the number of social conflict events).

Estimation methods and inference The next set of robustness checks investigate whether our findings hinge on the exact statistical methodology applied, or hold across a broader range of methodological approaches. Our main dependent variable being a binary 0-1 dummy, we replicate our results using logit in Appendix D.9. Next, we focus on the issue of a common pre-trend. In Figure 4 below, we perform an event study. In particular, we split our sample period into 4 sub-periods in order to assess whether enfranchisement observed in the elections correlates with differences in social conflict behavior prior to the reform. We do not detect any pre-trend and the effect of enfranchisement only kicks in after the 1868 election. Similarly, Appendix D.10 contains an event-study analysis for boroughs with below- versus above-median enfranchisement. Going further, in Appendix D.11 we rely on the Synthetic Control Method (SCM), recently applied e.g. by Abadie and Gardeazabal (2003), Billmeier and Nannicini (2013), and Saia (2017), to guarantee –by construction– an identical pre-trend before the enfranchisement reform. These sets of methodological sensitivity checks leave the conclusion from the baseline analysis unchanged: Franchise extension has curbed social conflict. Finally, in Appendix D.12 we perform alternative (two-way, spatial and more granular) clustering of standard errors, which allows to account for complex correlation patterns of standard errors. The statistical inference remains very similar.

Outliers and different time units In this next set of sensitivity checks we focus on the

Figure 4: Democracy and Social Violence: Leads and Lags



(A) Electorate (B) Eligible Householders

NOTE: The figure displays the coefficients of estimates of leads and lags of the variables *(log) Electorate Post*$_{it}$ (i.e. the (log) number of electors post-reform) in *Panel A* and *(log) Eligible Householders Post*$_{it}$ (i.e. the (log) number of eligible householders post-reform) in *Panel B*. In each of the two specifications we also control for *(log) Electorate Pre*$_{it}$ [*(log) Eligible Householders Pre*$_{it}$] (i.e. the number of electors [eligible householders] pre-reform) in *Panel A* [*Panel B*]. These variables are interacted with time dummies taking value 1 for each time-window. Time-windows are displayed on the horizontal axis (omitted period is [T-6:T-1]). Estimates are obtained including interactions of average rents and rent inequality with monthly time dummies. In this way we obtain leads and lags specifications comparable with columns 1 and 4 of Table 1. The dependent variable is *Social Violence*$_{it}$.

sample composition. In particular, we assess robustness to the presence of potential outliers and to different time units. More specifically, in Appendix D.13 investigates whether the findings are driven by outliers. For this purpose, the regressions are re-run when boroughs, newspapers and random days are dropped from the sample, revealing that the results are very stable across specifications. Further, Appendix D.14 depicts the findings when the temporal unit of observation is either the week or the pre-post reform period. In all cases, our results prove robust to these sensitivity tests.

Time trends and extended sample period As mentioned above, a key identifying assumption is the existence of a common pre-trend. A complementary approach to the exercises already discussed above (and displayed in Figure 4 and in Appendices D.10 and D.11), is to control for a borough-level time trend and to extend the sample in order to allow for longer pre- and post-treatment periods. The corresponding results are presented in Table A25 in Appendix D.15. We find that our baseline results continue to hold when we include a borough-specific linear trend, and when we extend the panel length up to two years before and after our baseline sample.[17] The extended sample allows us also to investigate the role of *de jure* enfranchisement

[17]Note that the extended sample comes at a price, as for longer sample durations there is a higher risk of unobserved heterogeneity and confounding factors, such as institutional changes that have occurred (e.g, the Ballot Act and several public health bills introduced during the 1860s and 1870s). These concerns make it crucial to include time trends when analysing the extended sample. Still, despite these, the longer sample offers overall

(at the passing of the act) versus *de facto* political representation (at the first election where the newly enfranchised electoral body was called to vote). Similarly to the exercise presented in Figure 4 above, we divide the sample into several sub-periods to identify any differential effect after the elections versus after the passage of the act. The corresponding results are presented in the Appendix D.15. It is shown that the *de jure* passing of the law did not suffice to curb social conflict, and we also do not observe any change in social conflict events between the passing of the act and the first election thereafter in 1868. In contrast, it is found that after the 1868 election the level of social conflict decreases. This finding is not entirely surprising: given that voters had to register and that registration offices were overwhelmed and struggling to register all new voters (Davis and Tanner (1996)), it made sense for people at the moment of the passing of the act to have residual doubts on the process and on their ability to participate in the next vote. However, in 1868 when the elections actually took place, the promises of the reform were realized and confidence in the process and representation were consolidated.

Placebo analysis The next set of robustness tests address concerns about our identification strategy "mechanically" picking up something else, e.g. due to measurement error or reporting bias. We start off in Appendix D.16 with an assessment of whether our instrumental variable also affected social conflict in other time periods, where the reform did not apply and where accordingly we would not expect any effect. We find that for both pre-post 1865 and pre-post 1874 elections, reassuringly no effects of 1868 reforms were detected. In a similar vein, in Appendix D.17 the main analysis is replicated by using sport events instead of social conflict events as the dependent variable. As expected, the reform only affects the latter, and not the former. This attenuates concerns about reporting bias from newspaper reports affecting our results, as any mechanical bias should also affect reports on sport and not just politics. Moreover, in Appendix D.18 we randomly assign treatment in 1,000 placebo datasets with the same average social conflict likelihood as the "true" data, finding reassuringly that it would be extremely unlikely that our results were found "by chance". Last but not least, in Appendix D.19 we randomly permutate the rents paid by householders to investigate the validity of our instrument. Reassuringly, no effects are found for this fake data.

a less compelling identification strategy, which is why we focus in the baseline estimations on the shorter, more homogenous sample (24 months).

Data construction Finally, we also carry out a series of robustness checks with respect to the algorithm used to detect and geo-code social conflict events (see Appendix D.20) and the keywords used (see Appendix D.21). Importantly, as major robustness check, in Appendix D.22 we also re-construct the social conflict measure relying –instead of the bag-of-words approach (which could be sensitive to the exact terms included)– on machine learning techniques, using a lasso model. In particular, we hand-code 1,000 strings (sentences or sentence parts) as indicating the presence of social conflict or not. Of these, 900 strings are then used to train the machine learning algorithm that is next applied out-of-sample on the 100 remaining hand-coded strings (yielding an out-of-sample accuracy of 93%). We then apply this algorithm to the full set of strings of all newspaper articles to construct an alternative lasso-based dependent variable. Strikingly, the sample mean of the resulting social conflict measure is extremely close to that of our baseline variable, and when replicating our baseline regressions using this very different alternative data construction approach, we find very similar results (see Appendix Table A37). Further, in Appendix D.23 we apply a Natural Language Processing (NLP) approach to construct our social conflict variable. This sophisticated method not only takes into account keywords but also makes use of sentence structures. Our baseline results prove robust to all these various sensitivity tests on data construction. Finally, another major worry could be that our variable of social violence may not only capture important events of social conflict, but may also occasionally pick up smaller, very local events (e.g. bar rows). In order to address this concern, in Appendix D.24 we restrict the social violence variable construction to include only events that are so large-scale that they are also reported in newspapers with headquarters in boroughs far away from where a given event took place. We find that our results are –if anything– stronger when focusing on such bigger events.

6 Channels

After having scrutinized our results for a broad range of robustness checks in the previous section, we now study the underlying mechanisms and channels at work. In particular, we focus on the following three potential mechanisms through which enfranchisement potentially drives down the risk of social conflict: i) increased participation in the political arena [voice],

ii) increase in economic activity, and iii) increased state capacity. We assess them successively. The main results are reported below, and supplementary robustness results on the channels and mechanisms are relegated to the Appendix E. Of course, a limit to our analysis is that these outcome variables and social conflict can be codetermined and there can be complex bi-directional causality links between e.g. social conflict and economic activity.

6.1 Voice: Increased Participation in the Political Arena and Beyond

A mechanism through which enfranchisement could deploy effects on social conflict outcomes is a higher state accountability, as greater political participation and inclusion may result in larger scrutiny. The key role of state accountability for reducing fighting has been stressed in Collier and Rohner (2008). To start with, one may expect –almost mechanically– that greater *de jure* eligibility for voting may result in a higher number of *de facto* voters casting their votes, which may affect the competitiveness of elections and political turnover. Beyond these effects, also general, non-electoral forms of civic engagement, such as petitions, may be affected. Interestingly, voting and petitions could be either complements or substitutes, as investigated below.

Number of Voters A determining pre-condition for this channel is that the newly enfranchised voters actually made use of their new-found powers and went voting, thereby increasing political openness and competition. In Table 2 we investigate this, running the following specification:

$$(log)\, Voters_{it} = \beta_0 + \beta_1 (log)\, Electors_{it} + FE_i + FE_t + \epsilon_{it} \tag{2}$$

We find that boroughs with more new eligible electors did indeed see a larger increase in the number of voters participating to the November 1868 elections.[18][19]

[18] In Appendix E.1 we provide further evidence of the non-linear effect of enfranchisement on voters participation. Results provided in Table A43 suggest that boroughs with higher intensity of enfranchisement experienced a larger increase in the number of voters.

[19] One related question is whether the new voters did not only participate to elections, but on top of that were more active than previously-enfranchised citizens, thereby pushing upward the whole overall turnout. As reported in Appendix Table A44, the results on this are not very conclusive. The coefficient of interest is always positive but never statistically significant.

Table 2: Voice - Democracy and Participation in the Political Arena

Dep. Variable: (log) Voters$_{it}$	(1) OLS	(2) OLS	(3) RF	(4) RF	(5) 2SLS	(6) 2SLS
(log) Electorate$_{it}$	1.102***	1.043***			1.226***	1.180***
	(0.105)	(0.0722)			(0.194)	(0.178)
(log) Eligible Householders$_{it}$			0.851***	0.826***		
			(0.152)	(0.150)		
Observations	216	216	216	216	216	216
R-squared	0.977	0.978	0.966	0.968	-	-
1st stage F-Stat	-	-	-	-	87	57
Borough FEs	Yes	Yes	Yes	Yes	Yes	Yes
Month FEs	Yes	Yes	Yes	Yes	Yes	Yes
*Average Rents$_i$ * Time FEs*	No	Yes	No	Yes	No	Yes
*Rent Inequality$_i$ * Time FEs*	No	Yes	No	Yes	No	Yes

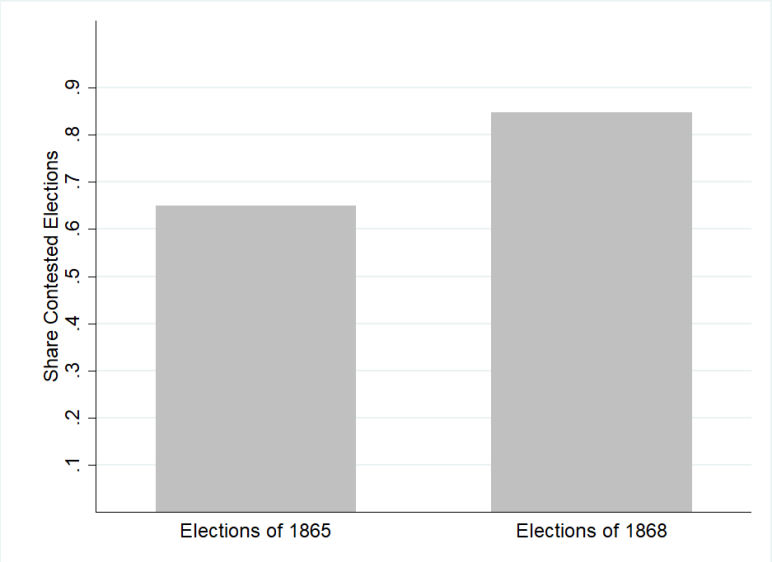
NOTE: The unit of observation is borough i and election t (where t corresponds to the elections of 1865 and 1868). OLS (2SLS) estimates are reported in columns 1-4 [5-6]. The dependent variable is the *(log) Voters$_{it}$* correspond to the electorate in a borough i observed in election t. The variable *(log) Electorate$_{it}$* corresponds to the electorate pre and after the Elections of 1868 in a borough i. The variable *(log) Eligible Householders$_{it}$* corresponds to the number of householders with rental value above £10 for the period before the Elections of 1868 and the number of householders living in a house with rental value above £10 and between £4 and £10 (who were previously banned from voting but enfranchised in the Second Reform Act) for the period after the Elections of 1868. Borough-level rent-based variables have been computed using data from House of Commons (1866a). Robust standard error are reported in parenthesis. Statistical significance is represented by * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Contested Elections For more voters to trigger greater political accountability, a key question is whether this influx of new political actors has actually made the political arena more competitive. It turns out that this was indeed the case. As documented in Figure 5, in the elections of 1868 there has been a surge in political competition which is reflected as an increase in the number of contested elections.[20] Our findings are in line with the results of Berlinski et al. (2011) that highlight a rise in electoral competition following the reform act.

Political Turnover After investigating the notion of contested elections, we now question whether challengers have a real chance of defeating incumbents in the polls and if a healthy level of political turnover takes place in reality. This is an important pre-condition for effective citizen representation and would typically be violated if an established political elite holds a *de facto* monopoly over political power and access to political office. A priori, the Second Reform Act offered a well-needed opportunity to "stir things up", allowing the entry of new faces into

[20] Complementary to these findings, we also report in the Appendix (see Appendix Table A45) that new voters forming a plurality decreases significantly social violence and that the more newly enfranchised voters there are (and hence the more "voice" they have), the more social conflicts decrease.

Figure 5: Voice - Share Contested Election 1865-1868



NOTE: The two bars indicate the share of borough with contested elections in the elections of 1865 and 1868, respectively. Election data is obtained from House of Commons (1866a) and House of Commons (1869b).

the political landscape. Below we investigate to what extent the enfranchisement has indeed favored political turnover and paved the way for new candidates to win elections.

In effect, we digitalise new data from the British Parliamentary Election Results book, containing information about the name of all the candidates and elected politicians for the years 1832-1885. With the help of this data, we construct two new dependent variables: (i) a dummy equal to 1 if at least one incumbent candidate gets re-elected and (ii) the share of seats for which the incumbent party got re-elected. We estimate a difference-in-differences specification, regressing these dependent variables on our main variable of interest (log electorate), as well as borough and year fixed effects.[21] Our findings are displayed in Table 3. In the odd columns we display the results for the dependent variable (i), while the even columns refer to the dependent variable (ii). We find that suffrage expansion gave rise to an effective change in the political arena, reducing the likelihood that incumbent candidates and parties got re-elected.

Petitions One key element for accountable democratic societies is the relationship between the people and their politicians, and a vibrant civic culture typically gives birth to regular interaction that is not only limited to periodic elections. In particular, public petitions matter and are a

[21]For dependent variable (i) we also control for the number of seats in the town (which changed for some boroughs between the two elections), while for dependent variable (ii) there is no need to add this control, as we already express it as share of the number of seats.

Table 3: Voice - Democracy and Effects for Incumbents

Dep. Variable:	(1) OLS *Incumbent Re-elected$_{it}$*	(2) OLS *Share Seats Inc. Party$_{it}$*	(3) RF *Incumbent Re-elected$_{it}$*	(4) RF *Share Seats Inc. Party$_{it}$*	(5) 2SLS *Incumbent Re-elected$_{it}$*	(6) 2SLS *Share Seats Inc. Party$_{it}$*
(log) Electorate$_{it}$	-0.351**	-0.105*			-0.327*	-0.190*
	(0.137)	(0.0623)			(0.186)	(0.106)
(log) Eligible Householders$_{it}$			-0.282*	-0.171*		
			(0.162)	(0.0934)		
Observations	366	366	366	366	366	366
R-squared	0.573	0.595	0.563	0.598	-	-
1st stage F-Stat	-	-	-	-	176	177
Controls	Yes	Yes	Yes	Yes	Yes	Yes

NOTE: The unit of observation is borough i and election year t. The sample covers 183 boroughs for the two elections in years 1865 and 1868. LPM (2SLS) estimates are reported in columns 1-4 [5-6]. The dependent variable in odd columns [1-3-5] is a dummy that takes value 1 if at least one incumbent gets re-elected in a given election t. The dependent variable in even columns [2-4-6] is the proportion of incumbent parties that get re-elected in a borough (i.e. $\frac{\#parties \quad re-elected}{\#seats}$) in a given election t. These dependent variables were constructed using data collected by the authors from Craig (1977). The variable *(log) Electorate$_{it}$* corresponds to the electorate pre and after the Elections of 1868 in borough i. The variable *(log) Eligible Householders$_{it}$* corresponds to the number of householders with rental value above £10 for the period before the Elections of 1868 and the number of householders living in a house with rental value above £10 and between £4 and £10 (who were previously banned from voting but enfranchised in the Second Reform Act) for the period after the Elections of 1868. Borough-level rent-based variables have been computed using data from House of Commons (1866a). All columns include Borough and Year of election, Average Rents by year and Gini Rents by year FEs. In odd columns [1-3-5] we also control for the number of seats in the town for each election (in even columns [2-4-6] our dependent variable is already divided by the number of seats). Robust standard errors clustered at the borough level are reported in parenthesis. Statistical significance is represented by * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

powerful indicator of the extent to which "the people" have a "voice" in a given political system. There may be a complementarity between inclusive elections and further means of expression such as petitions – one could expect that enfranchisement and better representation of citizens may also lead to more vibrant civic exchanges between electors and the authorities. The right of the people of the United Kingdom to present petitions to the House of Commons dates to 1669, and the Nineteenth century is considered a "golden age" of parliamentary petitioning. In 1843, prime minister Benjamin Disraeli said, “I believe that at this moment the right of petition [...] is a more important and efficient right than has ever been enjoyed by the people of England in this respect” (Parliamentary Debates, 3rd Series, Vol 101, c673, 30 August 1843).

We use newly digitized archival records of the Select Committee on Public Petitions (see Parliament) to retrieve all public petitions received by the House of Commons from 1868 to 1869. For each petition, we know where the petitions originated, the number of signatures related to the petition and the corresponding topic. Petitions over the period were related to a variety of topics. There were several petitions related to educational topics (i.e., petitions

related to ragged schools), religious issues (i.e., petitions related to the Irish Church Bill) and various legal questions (i.e., petitions related to the property rights of married women or related to the sale of liquor). Examples are displayed in Appendix Figures A19(A) and A19(B).

Information on petitions is available at the monthly level but not all months in our sample report petitions (i.e. our data is exhaustive but in some months no petitions were submitted). Overall, we have 3 months where we observe petitions before the elections and 7 months after the elections. The results are displayed in Table 4. To reflect the count data structure (see Appendix Figure A20), we rely on a Poisson model. The dependent variable of interest is the number of petitions received in month t from borough i. It is striking how an increase in the number of electors triggered a substantial boost in petitions, highlighting the multi-dimensional surge in civic engagement in the aftermath of the Second Reform Act.[22]

Finally, note that further results on mechanisms linked to voice are presented in Appendix E.4.

Table 4: Voice - Democracy and Petitions

Dep. Variable: Number of petitions submitted$_{it}$	(1)	(2)	(3)	(4)
(log) Electorate$_{it}$	3.274***	11.25***		
	(0.848)	(2.884)		
(log) Eligible Householders$_{it}$			1.805***	10.03***
			(0.531)	(1.439)
Observations	1,840	1,840	1,840	1,840
Borough FEs	Yes	Yes	Yes	Yes
Time FEs	Yes	Yes	Yes	Yes
Average Rents$_i$ * *Time FEs*	No	Yes	No	Yes
Rent Inequality$_i$ * *Time FEs*	No	Yes	No	Yes

NOTE: The unit of observation is borough i and month t. The sample covers the period 1868-1869. Poisson estimates are reported in columns 1-4. The dependent variable is the number of petitions submitted in borough i and month t. The petition data was constructed using archival records of the Selected Committee on Public Petitions (see Parliament). The variable *(log) Electorate*$_{it}$ corresponds to the electorate pre and after the Elections of 1868 in borough i. The variable *(log) Eligible Householders*$_{it}$ corresponds to the number of householders with rental value above £10 for the period before the Elections of 1868 and the number of householders living in a house with rental value above £10 and between £4 and £10 (who were previously banned from voting but enfranchised in the Second Reform Act) for the period after the Elections of 1868. Borough-level rent-based variables have been computed using data from House of Commons (1866a). Robust standard errors clustered at the borough level are reported in parenthesis. Statistical significance is represented by * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

[22]Given the scarcity of data points on petitions it is also useful to replicate this analysis for the extended sample. In particular, in Appendix Table A46 we report results with the extended sample (using the years 1866-1871) and including a linear trend (this corresponds to the specification of our extended sample specification of Table A25).

6.2 Increase in economic activity

A second channel that we investigate is that enfranchisement could have boosted economic activity consequently providing a fertile breeding ground for peace. In particular, more inclusive, pluralistic political institutions may create the conditions for more inclusive economic institutions and greater and more sustained economic growth (see Acemoglu et al. (2019) and Abeberesey et al. (2020), which suggest that democracy does favor economic growth). In turn, various papers (see e.g. Miguel et al. (2004); Dell et al. (2014); König et al. (2017)) have found that peace is more easily achieved under favorable economic conditions.

Two factors that may make it more difficult in our context to detect large-scale effects on economic activity are that (i) our analysis is inherently short-run (and full-scale effects on economic activity may take time), and that (ii) we study a change in representation in the *national* parliament, while several –yet not all– relevant economic policy measures may be taken at the *local* level. Still, it is important to keep in mind that there could be anticipation effects (i.e. despite the short sample length, one may detect already a surge in economic activity early on if investment today is fuelled by the future prospects for peace and stability).

In our analysis, we use the number of times boroughs were mentioned in job advertisements in newspapers as a proxy of economic activity.[23] We show in Appendix B.4 that job advertisements correlate strongly with other proxies for economic activity from very different, existing sources.

The results are displayed in Table 5. Throughout all specifications, we find that greater enfranchisement was associated with an increase in our proxy of economic growth. The effect is of substantial magnitude: increasing the number of eligible electors by 89% (i.e. the average increase triggered by the reform) is associated with a rise in advertisements (our proxy for economic activity) by around 15.5%. Expressed in terms of standard deviations, a one standard deviation increase in enfranchisement is associated with greater economic activity of 0.16 standard deviations.

In the Appendix E.3 we further show that the strong association between the Second Act

[23] We identify pages of job advertisements as those pages classified as advertisement which contain the word "*wanted*".

Reform and economic activity carries over to an alternative specification using a Poisson model (Appendix Table A47), and that our results are not driven by general media penetration (Appendix Table A48). We then demonstrate that the *predicted* level of job advertisements is indeed a strong determinant of lower social conflict levels (Appendix Table A49). Furthermore, we find that our results are robust to alternative definitions of our newspapers-based index of economic activity. These results are confined to Appendix E.4.

Finally, changes in economic activity do not only represent a channel of transmission through which enfranchisement reduces social conflict, but it is simultaneously an outcome of interest of its own, that may equally depend on the level of social conflict, i.e. there may also be an impact of protests (and the expectation of protests) on economic activity. Put differently, enfranchisement may at the same time boost economic growth and reduce conflict and these two beneficial effects may mutually reinforce each other (a better economic environment fosters peace and social stability fuels economic activity). The structure of our data would make it difficult to disentangle to what extent the impact of the economy on social peace is larger than the inverse impact of social peace on economic activity.[24]

Table 5: Democracy and Economic Growth - Newspaper Ads

Dep. Variable: (log) Borough Mentions in Newsp. Ads$_{it}$	(1) OLS	(2) OLS	(3) OLS	(4) RF	(5) RF	(6) RF	(7) 2SLS	(8) 2SLS	(9) 2SLS
(log) Electorate$_{it}$	0.175***	0.167***	0.186***				0.248***	0.252***	0.241***
	(0.0446)	(0.0484)	(0.0458)				(0.0535)	(0.0604)	(0.0582)
(log) Eligible Householders$_{it}$				0.215***	0.226***	0.217***			
				(0.0481)	(0.0554)	(0.0526)			
Observations	4,416	4,416	4,416	4,416	4,416	4,416	4,416	4,416	4,416
R-squared	0.952	0.952	0.954	0.952	0.952	0.954	-	-	-
1st stage F-Stat	-	-	-	-	-	-	232	194	184
Borough FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Time FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
*Average Rents$_i$ * Time FEs*	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes
*Rent Inequality$_i$ * Time FEs*	No	No	Yes	No	No	Yes	No	No	Yes
Sample Mean	3.974	3.974	3.974	3.974	3.974	3.974	3.974	3.974	3.974

Note: The unit of observation is borough i and month t. The sample covers 184 boroughs over the period 1868-1869. OLS (2SLS) estimates are reported in columns 1-6 [7-9]. The dependent variable is (log+1) of number of mentions of borough i in pages of job advertisements containing the word "*wanted*" in month t using national or local newspapers available on the *British Newspaper Archive*. The variable *(log) Electorate$_{it}$* corresponds to the electorate pre and after the Elections of 1868 in borough i. The variable *(log) Eligible Householders$_{it}$* corresponds to the number of householders with rental value above £10 for the period before the Elections of 1868 and the number of householders living in a house with rental value above £10 and between £4 and £10 (who were previously banned from voting but enfranchised in the Second Reform Act) for the period after the Elections of 1868. Borough-level rent-based variables have been computed using data from House of Commons (1866a). Robust standard errors clustered at the borough level are reported in parenthesis. Statistical significance is represented by * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

[24]For a theoretical framework featuring such virtuous cycles of business and peace mutually reinforcing each other, see e.g. Rohner et al. (2013).

6.3 Increase in State Capacity

A third potential channel of transmission could be an increase in state capacity. As suggested by the 18th century political slogan "No taxation without representation", one may expect a quid-pro-quo with enfranchisement going along with an extension of the activities of the state. As argued, among others, by Fearon (2005); Collier et al. (2009); Besley and Persson (2011b), weak state capacity can be a major cause for social conflict, hence a reason for the decline in social violence after franchise extension could be through greater state power.

Nevertheless, there are several reasons why it may be difficult to detect mechanisms linked to state capacity with our research design/data. First, one reason for scepticism on this potential channel is that it is well known among historians that this time period has not been characterized by high levels of public spending (see Aidt et al. (2010), Chapman et al. (2018), Chapman et al. (2020)). Second, any potential proxies for state capacity for this period are quite unsatisfactory, which would typically result in substantial measurement error and attenuation bias. Third, our identification strategy focuses on the impact of *sharp* changes in the electorate on *sharp* short-run changes in state capacity. To the extent that building up powerful state infrastructure and enacting public spending takes many years, our identification strategy may miss out on such medium- and long-run effects. This is likely, for example, to apply to educational spending and school construction, which may deploy effects only several years down the road. Fourth, a factor that could lengthen delays in potential state capacity effects is the fact that this paper deals with elections for the national parliament and not for the local executive. While for the latter type of local executive elections any effects could be quite rapid and direct (i.e. a newly elected mayor may want to swiftly implement electoral promises), in our case of the former type of national legislative elections any effects would be indirect and could take longer (i.e. local constituents who are better represented at the national level are likely to benefit from more "pork-barrel" spending down the road some years later).[25] Altogether, these caveats imply that it would –if anything– be surprising to find state capacity related mechanisms over a short time

[25]As discussed e.g. in Chapman et al. (2018) and Webster (2018), local councils were traditionally relatively important for public goods provision, but from the 1860s onwards the central government started to gain more and more importance in terms of financing. Importantly, Members of Parliament (MPs) played non-negligible roles for the financing of their local constituencies.

horizon.

Nonetheless, in Appendix E.5 we study the impact of the Second Reform Act on (short-run) changes in public spending and deficits. As expected in the light of the aforementioned caveats, we are not able to detect such effects in the short-run.

6.4 Heterogeneous Effects

In the objective of further substantiating the notion that the Second Reform Act succeeded in attenuating social tensions by fostering economic opportunities, we study heterogeneous effects of the above results, by distinguishing between areas with big versus little economic potential. To be concise, all detailed information on the exact specification and all tables have been relegated to Appendix E.6. The most relevant results are that the pacifying and growth-promoting effects of enfranchisement are magnified in towns that have a high market potential (i.e. that are located close to large numbers of potential consumers). This is consistent with the notion that the growth-promoting effect of democratization may be one of the prime mechanisms at work that explains the drop in violence after the Second Reform Act.

A further heterogeneous effect makes use of detailed information on the demographic composition of the population. As discussed in depth in Appendix E.7, one dimension of social tensions in the 1860s were conflicts between the Anglican population and Catholic immigrant workers from Ireland, giving rise e.g. to the so-called "Murphy riots" (Arnstein (1975)). We expect greater inter-group tensions in towns with a greater level of ethnic polarization (i.e. with a few large groups facing each other; e.g. in a borough with close to half of the population being English and the other half Irish). This is indeed what we detect in Appendix E.7 (which also contains all methodological details and exact variable definitions). These findings are in line with the notion that areas with higher initial social tensions experience a grater pacifying potential from political reform.

Finally, in Appendix E.8 we investigate what types of social violence are affected by enfranchisement. As explained in detail in Appendix E.8, we expect an (almost mechanical) decrease in *political* violence linked to claims for representation (as the enfranchisement has addressed

various points of pre-reform demands), and also a reduction in *ethno-religious* violence due to better representation of all major ethnic groups in society. Finally, given the growth-promoting effect of the reform (see Section 6.2), one may expect a higher opportunity cost of social unrest, which could attenuate the risk of all types of social conflict – not only the those mentioned above, but also *economic* social conflicts. The results presented in Appendix E.8 show indeed that franchise extension tends to reduce all these three types of social conflict.

7 Conclusion

In this research paper, we examine the impact of enfranchisement on peace and prosperity, drawing on a milestone electoral reform of the UK's Victorian epoch: the Representation of the People Act of 1867. Building a novel panel dataset at the borough and month level for the period around the reform, and collecting novel data on social conflict, local economic growth and a battery of controls, this paper exploits arguably exogenous variation in the extent of enfranchisement across UK boroughs. Our results show a strong and significant pacifying effect of franchise extension that is reinforced by many successful robustness checks. While there is substantial evidence that a relevant channel of transmission is a surge in the population's political voice and competition in UK politics, we also find evidence for another mechanism at work which is that democratization fuels local economic growth.

On a more general level, our findings support the notion that civil peace and economic development are intertwined; it is difficult to cultivate peace in a context where economic growth is neglected, and vice-versa. This point of inter-dependence has been recently stressed by Rohner and Thoenig (2021) who talk about a *macro-complementarity* between promoting peace and fostering development. Our current paper highlights the complementary effect of a *political* reform not only achieving the *political* goal of reducing unrest, but on top of that boosting local *economic* growth. These complementarities of peace and prosperity call for an integrated approach of tackling unrest and under-development *at the same time*, rather than leaving these inter-linked problems to two distinct policy communities. Further research on this, as well as an in-depth analysis of the effects of specific institutional rules, is strongly encouraged.

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