Did the Extension of the Franchise Increase the Liberal Vote in Victorian Britain? Evidence from the Second Reform Act

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Abstract. We use evidence from the Second Reform Act, introduced in the United Kingdom in 1867, to analyze the impact of changes in rules concerning eligibility to vote on electoral outcomes. By exploiting the constituency level variation in the effect of the change in the franchise, we separate the effect of the reform from that of underlying constituency level traits correlated with the voting population. We analyze three measures of Liberal strength- the proportion of constituency level seats contested by the Liberals, the proportion of constituency level seats won by the Liberals, and the Liberal share of the constituency vote, amongst other things. Controlling for a trend in Liberal support, we find no evidence that relates Liberal strength in 1868 to changes in the franchise rules. We show that our results are robust to various possible sources of endogenity.[3]

1. Overview and Introduction

Several studies shed light on the relationship between franchise extension and political outcomes. Theoretical models provide plausible connections between changes in the rules governing the eligibility to vote and incentives of policy makers, leading to greater redistribution (Acemoglu and Robinson, 2000) or expansion of local public goods (Lizzeri and Persico, 2004). Empirical studies exploit variation in electoral rules across countries (Lindert, 1997, 2004), and US states (Husted and Kenny, 1997) to explore the effects of franchise extension on redistribution and provide evidence for a positive relationship between these variables. In this paper we explore the political mechanism that links changes in the franchise to outcomes. In principle policy changes correlated with franchise extension could arise due to several factors,*inter alia*, the voting behavior of newly enfranchised citizens, differences in party competition, candidacy and incumbency effects, or differences in agenda-setting and voting behavior of political elites.

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Analysis of specific franchise extensions can help isolate these different effects. An important test case is the Second Reform Act in the United Kingdom which greatly increased the franchise in England, Scotland, and Wales in 1867, with an increase in the franchise of 97%. To explore our questions we analyze the impact of franchise extension on political outcomes in the United Kingdom during this period by exploiting variation in the constituency level voting population that arose from the reform. There are few studies that analyze the impact of reform at such a local level.[4] Doing so allows us to understand the political mechanisms at work, and can also can provide new insights into rival hypothesis about the underlying causes and effects of the reforms.

Historians of the period have been puzzled by the exact nature of the reforms and most have seen extension of the franchise as related to competition between the Liberal and Conservative parties, and tensions between their two great leaders, Disraeli and Gladstone. There are, however, several reasons why franchise extension is unlikely to be related to inter-party political calculations. The Second Reform Act of 1867 lead to the election of the first ever unequivocally Liberal administration in 1868, and ushered in a period of reform associated with a decrease in inequality. The Reform Act was more radical than a Liberal measure that had failed to pass in the Commons in the previous year. The Act was, however, introduced by a minority Conservative government, whose main constituency was the rural voters. Finally, and critically, the Reform Act of 1867 increased the voting population in precisely those urban areas likely to be sympathetic to the Liberals.

The argument is nicely summarized by Acemoglu and Robinson (2000): "as the result of the split over the Corn Laws, support for the Conservative party was essentially concentrated in rural areas, with Tory landowners exerting substantial control over the electorate in the absence of a secret ballot. The reform measure passed under Disraeli increased the voting population by only 45 percent in counties, compared to 145 percent in the boroughs, effectively ensuring a Conservative defeat in the following elections."

The outcome of the 1868 election- a landslide victory for the Liberals- can then be considered as evidence against the view that franchise extension arose due to such internal party political considerations and suggests instead that franchise extension arose as a response to popular pressure for reform. This claim would be stronger, if it could be shown that the extension of the franchise, initiated by the Conservative government, is causally, rather than accidentally, related to the election of a Liberal government with a reforming agenda. Although the extension of the franchise was correlated with Liberal success, and hence the introduction of their radical agenda, it is as yet unproven that the change in rules concerning voter

[4]An exception is (Aidt, Daunton, and Dutta, 2008) who look at the impact of changes in the local government franchise in the UK in the 19th century, showing a non-monotonic relationship between inequality and the provision of local goods.

eligibility contributed to that success. Whilst we cannot directly observe the counterfactual - the electoral outcome had the electoral rules established in 1832 remained in place- a careful identification strategy provides a second best solution that allows us to understand the impact change in the voting mechanism.

Three features of the Second Reform Act allows us to address the impact of the change in the rules concerning eligibility to vote on the political outcomes of subsequent elections. Firstly, the extent and impact of the Second Reform Act was largely unanticipated - Disraeli's strategy was famously referred to by Lord Cranbourne as a "leap in the dark". Secondly, unlike previous and later reforms, the extension of the franchise in 1867 was uncorrelated with other constitutional changes that would confound any attempt to isolate the causal impact of reform. Thirdly, the most immediate impact of franchise extension is observed in the constituency level vote shares for the candidates of the main parties and, as we show, there is a large amount of variability with regard to the effect of the change in the electoral rules on the eligible voting population at the constituency level.

By exploiting the constituency level variation in the impact of franchise reform - to separate its effect from that of underlying constituency level traits correlated with the voting population - and controlling for a national swing, we can isolate the effect on Liberal support that is due to the impact of the extension of the franchise from that of other factors that may have lead to the Liberal victory in 1868. We thus ask whether the constituency level expansion of the franchise due to the new electoral law introduced in 1867 can account for the success of Liberal MPs.

We first explore whether the electoral outcomes of the election reflected the differential effects of franchise extension at the constituency level. Surprisingly we find no evidence suggesting that the differences in the Liberal share of votes, or the percentage of seats won by Liberal candidates, can be explained by changes to the rules governing the eligibility to vote. Neither do we find any effect of the new franchise on the difference in constituency level turnout.

Next we consider whether the differences in the constituency level franchise were related to change in the structure of party competition between the Liberals and Conservatives. Before the Second Reform Act many seats were uncontested. In these constituencies the parties agreed the share of seats between them and no ballots were cast. A possible source of Liberal advantage is that, under the new franchise rules, they could contest a larger share of constituency level seats. In fact we find that although the number of uncontested seats declines sharply this is unrelated to the constituency level impact of reform. Moreover, we we find no evidence that the Liberal share of candidates increased in areas most affected by the reform.

Finally we consider the impact that the extension of the franchise had on the nature of candidate competition at the constituency level. Were parties more likely to place new challengers or deselect existing incumbents in those areas where the effects of reform were largest? We find no evidence that this was the case.

Extending our analysis, we explore possible causes for our null findings. The first involves measurement error which may come in one of two guises. The most immediate-classical measurement error- is that which arises due to imprecision in the way the data is recorded. A potentially more serious issue arises because we do not directly observe the relevant population (those eligible to vote under the new rules), but rather a subset of the eligible voting population who registered to vote. Historians of the period have highlighted the difficulties involved in voter registration. If, as seems plausible, such difficulties reflected differences in the size of the eligible voting population at the constituency level, then our estimates of the impact of changes in the voting population at the constituency level could be biased.

A second cause of our null findings relates to reverse causality that may bias downwards our estimates of a marginal increases in the voting franchise in a given constituency. This problem arises if, as some historians have claimed, Disraeli anticipated how the constituency level Liberal vote would respond to local changes in the franchise. For example, some historians have claimed that Disraeli was prepared to abandon boroughs that would return Liberal candidates under any voting rule and concentrate instead on shoring up the Conservative vote in the counties.

To deal with all these issues we explore a number of strategies: we instrument the change in the level of enfranchised voters, we study pre-trends in the outcomes of interest and we look at a restricted sample of constituencies where there are no boundary changes between 1865 and 1868. Doing so yields no systematic change in our results.

In sum we find no evidence to support the view that the Liberals were stronger in areas most affected by electoral reform and our results thus show that Liberal candidates were not directly advantaged by increases in the voting population at the constituency level. Instead, the Liberal victory in 1868 appears to be due to a nationwide swing toward the Liberals.

We begin our analysis in the following section by providing an overview of the details of the Second Reform Act and its immediate effects. We then look at why the nature of the reform has puzzled historians of the period as well as contemporary political economists. In section 4 we describe our data . In section 5 we present our identification strategy. In section 6 we present our main estimates and section 7 looks at how robust they are. Section 8 explores some possible medium run effects of the reform act. Finally section 9 concludes.

2. Facts and Details on the Second Reform Act 1867

Elections in Britain in the Victorian period under investigation took place under the first-past-the-post voting system that is still in place. Most constituency elections were contested by candidates who aligned with one of two major parties, the Conservatives and the Liberals. The Liberals brought together a loose coalition of Whigs, Radicals, and Peelites (a faction that had broken from the Conservatives) and by 1860 formed a cohesive parliamentary block. Following Lord Palmerston's death in 1865 the Liberals were lead by William Gladstone. For the immediate period preceding the elections of our investigation, Liberals had held they key ministries of government, but between 1865 and 1868 the Conservatives formed a minority government, first under Lord Derby and then Benjamin Disraeli.

The Representation of the Peoples Act, otherwise known as the Second Reform Act, was passed by Parliament in 1867. As its name suggests it was the second major voting reform bill that transformed the political landscape in the Great Britain. The first major extension of the franchise in the UK took place in 1832. The Great Reform Act of that year introduced several measures that mitigated malaportionment: increasing representation in the industrialized cities, and taking away seats from the so-called "rotten boroughs" with small voting populations. The act also increased the male franchise to around 650,000.

On August 15, 1867 the second Reform Act became law in England and Wales. It extended the franchise in the Boroughs to all males over the age of 21 who were inhabitant occupiers, whether house-owners or tenants, (from a previous threshold of 10) and to male lodgers whose rent was at least 10. A residence of at least one year in the borough was required and women were still unable to vote. In counties, the franchise was extended to holders of life interests, copyholds and leases of sixty years and more worth £5 per annum (from a previous threshold of £10) and to tenants occupying land worth £12 (from a previous threshold of £50 per annum). The Reform Acts for Scotland and Ireland were delivered in 1868. Unlike in Scotland, where the effect of the reform was similar to that in England, the impact of reform in Ireland was marginal.

Table 1 presents the number of registered electors in 1859, 1865, 1868 and 1874 for England, Wales, and Scotland, (omitting Ireland), in both boroughs and counties. It is clear that the Reform Act lead to an increase in the franchise which was not witnessed before or after the 1868 election. From 1859 to 1865 the registered electorate in England, Scotland and Wales increased by 7% and from 1868 to 1874 by 12%; this compares with a 97% increase from 1865 to 1868. Overall the franchise included more than 1,000,000 newly registered electors. The increase was more marked in the more densely populated urban Boroughs where the franchise increased by 153% with respect to a 46% increase in Counties. Indeed historians

have noted that the most striking contemporaries feature of the Second Reform Act was the unexpectedly wide extension of the franchise in the boroughs.

The Reform Act brought into the franchise voters from previously unenfranchised income brackets. Mackenzie (1921) and Bowley (1937) estimate the income of the head of the household at median, quartile and lowest decile of the income distribution in 1860. Mackenzie (1921) also provides estimates of household budgets for a typical family (man, wife, and 3 schoolchildren) which include the amount paid for rent. We present this information in Table 2. The annual income of a head of household in the upper quartile of the income distribution was more than 70 pounds; typically he was a semi-skilled worker (e.g, a brick-layer) and paid an annual rent in excess of 10 pounds.[5] The annual income of the head of household in the lower quartile was around 60 percent of that in the upper quartile; this would typically be the income of an unskilled worker (e.g., brick-layer labourer) and paid an annual rent in excess of 6 pounds. At the median of the income distribution the rent paid was close to 8 pounds. Although the calculations are obviously rough, given the data limitations, they help us to illustrate the type of households that gained the vote under the new franchise. It is clear that the extension of the franchise gave the vote to urban unskilled workers.[6]

3. Interpretations of the Second Reform Act

The Second Reform Act of 1867 was introduced by the Conservative government lead by Lord Derby, though most historians view Disraeli as its prime architect. Electoral reform had been considered for some time before the passing of the 1867 reform. Indeed a reform bill proposed by the Liberals lead by Earl Russell had been defeated by a coalition of Conservatives and Liberals opposed to reform in 1866. The fact that the reform bill of 1867 was introduced by a minority Conservative government and supported by backbench Conservatives, and that the reforms lead to a far greater increase in the franchise than would have been possible had the original Liberal reforms passed, has puzzled historians and commentators of the time.

One can take different views on why Disraeli pursued this course. The most obvious is that he believed that the Conservative Party could reap rewards. 19th century commentators such as Bagehot explained the reform as part of Disraeli's vision of a 'Tory Democracy' that would appeal to the conservative instincts of the British working classes, or at least the more highly skilled elements of the working class who received the vote (Shanon, 1992). So Disraeli's strategy can be seen as part of his grander vision of one-nation conservatism- an attempt to build a majority Conservative party that appealed to different elements of British

[5]At the time 1 pound = 20 shellings and 1 shelling = 12 cents.
[6]Those in the lowest decile of the income distribution (the agricultural labourers) were only enfranchised in 1884.

society. A related view is that Disraeli hoped to secure an electoral advantage by exploiting divisions within the Liberal party over the issue. Further, in outsmarting his erstwhile rival Gladstone by passing a more radical bill than the Liberals had been able to, he hoped to reveal perceived deficiencies in Gladstone as leader and parliamentarian Jenkins (1996).

An opposite view is that the extension of the franchise is related to external threats to the established political order, rather than inter-party disputes. Acemoglu and Robinson (2000) analyze the political economy of franchise extension and offer a compelling account of how franchise extension relates to fiscal redistribution. In their model, which is largely motivated by the Second Reform Act, an elite, which faces the threat of social revolution, has incentives to raise taxes to levels desired under a democracy. The ability of workers to extract concessions is transitory and arises only in periods of economic growth. An elite is thus unable to credibly commit to a redistributory tax policy when growth is stochastic and so are unable to assuage unrest. Extension of the franchise allows for a durable compromise in which the wealthy can make credible commitments of moderate redistribution that would dampen agitation for more radical economic reform. According to this view, events such as the Hyde park riots in 1866 and 1867 -in which supporters of the Reform League were involved in violent clashes with the police- were critical in shaping political incentives during this period, and forced an elite sceptical about reform to nevertheless embark upon the path of enfranchising the working classes.

Acemoglu and Robinson's theory brings together several disparate parts of this Victorian puzzle- the fact that the reform bill was passed by a Conservative government, and that the election was won by a reforming Liberal party, who arguably went on to transform British society with a series of radical measures. The critical piece of the puzzle, is the the fact that the reforms were designed to increase the vote share disproportionally in urban boroughs relative to the counties, despite the fact that the latter provided the bedrock for Conservative support. This feature of the reform lends support to the view that the reforms could *only* have benefited the Liberals and so can not plausibly be connected to electorally motivated considerations by the architects of the reform. Moreover, an immediate glance at the data provided in Table 1, that highlights the predominant impact of the reforms in the boroughs, tends to support the hunch that Liberal success was likely attributable to the precise nature of the reforms.

However, there are many mechanisms that can link franchise extension to change in political outcomes. The Liberal success in 1868 may have been due to the incorporation into the franchise of low skilled workers with an average income lower than that found in the pre-existing franchise. Thus, fixing all aspects of competition between the Liberals and Conservatives, the inclusion of a new block of voters may, in and of itself, account for the immediate political

outcomes. But of course, the parties responded to the new situation: the Liberal party may have been attracted by the prospect of competing in constituencies where previously the Conservatives had run unopposed; or increasing its share of constituency level candidates. Another avenue by which the reforms might impact on political outcomes is in providing incentives for parties to put forward different types of candidates, as changes in candidacy provides a mechanism by which the parties could credibly appeal to their new electorate.

Whilst there are different mechanisms that might link franchise reform to the immediate political outcomes of the 1868 election, another view should also be considered - namely that the reforms had little effect on those outcomes. Whilst Disraeli did not wish to be seen to stand in the way of reform, he may well have believed that the reforms themselves would not damage the Conservatives. Indeed Disraeli's vision of "one-nation" conservatism suggests his belief in the inherently conservative credentials of the new working class voters. Moreover, although members of his government were horrified at the thought of expanding the Liberal voting base, it was not clear at the time that the extension of the franchise would have this effect. Although some historians believe that the sympathies of the newly enfranchised workers lay with the Liberals (see, for example, Whitfield (2001),p239), the voting behavior of Victorian Britons, and the link between their behavior and the actions of their representatives, was poorly understood as testified by contemporary social historians.

The preceding discussion presents many unanswered questions. Acemoglu and Robinson present a compelling argument, largely based on the outcome of a Liberal victory in 1868, that reform is unrelated to party political consideration. Yet it is unclear to what extent the different parts of the puzzle are in fact causally linked. In particular, as yet, there is little evidence that suggests that the extension of the franchise under the Second Reform Act did in fact lead to an increase in the Liberal vote in 1868. Answering this question requires a careful investigation of the electoral data.

4. Data and descriptive statistics

Our data is gathered from Craig, F.W.S (1989), *British Parliamentary Election Results, 1832-1885*, 2nd edition, which provides, for all national elections, information on: Number of seats per constituency, Boundary changes in the constituency, Registered voters by constituency, Name of candidates running, Party of candidates, and Votes per candidate. We also make use of the national population census 1861 and 1871 as reported in J. Vincent and M. Stenton (1971), "McCalmont's Parliamentary Poll Book", 8th Edition, Harvester Press: Sussex.

In order to identify the impact of the franchise extension we must be able to isolate its effect from other possibly confounding institutional factors. Whilst the extension of the franchise in 1867 coincided with a level of redistricting -some constituencies which previously had two Members of Parliament were reduced to one, or increased to three, some constituencies were eradicated, whilst others merged- unlike in 1832, in most constituencies the only major district level change was the increase in the franchise. Our focus on this period thus allows us to separate the effect of franchise extension on political behavior from other possibly confounding effects at the constituency level, in a way that an analysis of the earlier and posterior Reform Acts cannot.

Although the Reform Act of 1867 was a major political factor, it was not the only one that may have influenced the outcome of the 1868 election. Some constituencies introduced boundary changes in the 1867 reform. Of particular importance is the role of the Boundary commission, appointed by the government with a remit to explore how franchise extension would affect existing boroughs and counties and to recommend proposals to Parliament. As noted by Cowling, Disraeli used his influence over appointments and on the commission, to ensure that, as far as possible, the rural character of the counties would not be diminished Cowling (1986),p 231.

Table 3 shows that, by and large, there was no major overhaul of the constituency level landscape due to redistricting, providing details of the total number of constituencies in 1865,1868, and 1874 and changes that arise during this period of reform. Between the 1865 and 1868 elections, 4 new borough constituencies and 15 county constituencies were created. Overall 304 constituencies appear in all three elections. Although some constituencies changed in district magnitude, a total of 261 seats maintained the same number of elected candidates between 1865 and 1874. A sum of 173 constituencies were were left untouched by any form of redistricting - they maintained the same boundaries and number of seats. Our results are robust to restricting our sample to those constituencies.

In this paper we focus on those constituencies that have no changes in MPs from the 1832 electoral reform until the 1874 reform. Thus, constituencies who were de-franchised during this period, that change the number of MPs, or were newly created, are eliminated. This leaves a sample of 164 boroughs and 97 Counties. We show that our results are robust to the inclusion of constituencies where the number of MPs changed in the 1867 reform.

Figure 1 plots the density function in the boroughs and counties with the difference in the voting population expressed in logs. The upper panel plots the density function for the logged difference in the registered voters between 1865 and 1865 by constituency. The lower panel depicts the density function in the level of registered voters by constituency in 1868. An immediate and important observation is the wide variance in the effect of franchise extension

in the constituencies, which is nicely illustrated in particular in the boroughs. Although the largest effects of reform were felt in the boroughs, there were marked differences in the effect of franchise extension within the boroughs. In some boroughs the change in the voting rules had little discernible impact on the number of eligible voters, whereas in others the size of the (registered) electorate increased considerably. Adopting the language of the experimental literature- we can view the extension of the franchise as a 'treatment' that varies in intensity, ranging from from (just below) 0 to (just over) 2 with an average around 1.

Table 4 provides further information on the characteristics that determined larger changes in electoral registration. We first examine how the franchise extension in 1867 relates to the (logarithm of) registered electores in 1832. In column 1, we observe that the growth in voter registration is negatively and strongly correlated with the level of registered voters in 1832. In column 2 we add changes in the population size from 1871 to 1861 and find similar results. This is true both for the boroughs (Panel A) and the counties (Panel B). In columns (3) and (4) we look at the effect of the log of the level of population in 1861. The result in this case is also negative but less strong. In columns (5) and (6) we include the level of the electorate, the level of the population, and an interaction. We find that both variables are positively related with the change in the franchise, their interaction is negatively correlated with this change, and that these results are consistent in boroughs and counties.

In our main analysis we concentrate on the constituency level impact of reform on electoral outcomes. First we analyze turnout. Then we look at two indicators of Liberal strength that are directly related to the behavior of the voting population: the proportion of the constituency level seats won by Liberals which ranges from 0 to 1; and the Liberal vote share in a given constituency, which ranges from 0 to 1.

Of course, voting behavior at the constituency level is affected by the menu of choices on offer, that in turn reflects the strategic calculations made by parties. In the 1868 election some constituencies were single member districts, some had two members, and a few retained 3 members. An immediate political measure that we observe is the number of contested seats. In 1865 and 1868 it was still the practice that, in some constituencies, Liberal or Conservative candidates would run unopposed. And in some multi-member districts the division of seats was agreed between the major parties before hand. As our results show this aspect of political collusion decreased sharply with the passing of the Second Reform Act. We investigate the extent to which this feature, which provides an indirect mechanism by which the extension of the franchise can shape the electoral outcomes, is causally related to the franchise extension.

In addition, we look at the proportion of those candidates who did run in a constituency, and did so under the Liberal party label: this measure ranges from 1 in an uncontested

constituency with only Liberal candidates, to 0 in an uncontested constituency with only Conservative candidates. We also assess the ratio of candidates running in a given constituency to the number of seats in the constituency. Finally, and at the level of candidate, we explore the proportion of incumbents who run in 1868, and, disaggregating further, we look at the proportion of Liberal incumbents who run again in the same electoral constituency. Following the same logic, we also look at the share of candidates who lose in 1865 but who stand once again for election in the same constituency.

Because in the counties most seats were uncontested our main analysis will focus on the boroughs where the franchise reform was also more radical. As we show later, however, our results are similar for the sample of counties. Table 5 provides summary statistics on our main outcomes for our sample of boroughs during four national elections 1859, 1865, 1868 and 1874.Our main measures, the average proportion of candidates who are Liberal and, the average constituency level Liberal vote share do not change between 1865 and 1868. However, in 1865 the proportion of the constituency level seats won by Liberals was 72%, but this increased to 77% in 1868. Turning to the competitiveness of the constituency races-in 1865 40% of constituencies returned candidates without any votes being vast, whereas in 1868 the percentage of uncontested seats falls sharply to 20%. The candidate to seat ratio also increased across the period, and in particular from 1.49 to 1.7 in the elections immediately either side of the Second Reform Act. However, the Liberal share of candidates is steady across this period. The share of incumbents who run increases from 57% to 66% and this increase is also true of Liberal incumbents. Whereas in 1865 42% of Liberal candidates were sitting incumbents this increases to 52% in 1868 following the reform. As expected the rate of retention of losing candidates is much lower: only 6% of candidates in 1865 had lost the same seat in the previous election and this increases to 8% in 1868, with a similar rate of increase seen amongst losing Liberal candidates.

5. Identifying the Impact of the Second Reform Act

To understand how political outcomes are causally related to the change in rules governing the eligibility to vote, we need to isolate the effect of a change in the franchise from other possibly confounding factors. In particular we want to ensure that the conditional expectation of our estimated Liberal strength is the same across constituencies in all aspects other than the eligible voting populations. Although franchise extension was applied nationally and simultaneously in all constituencies, the magnitude of the change at the constituency level reflects local conditions. In particular the local impact of a change in the electoral law is related to the constituency level distribution of income and housing in 1867. In estimating the causal effect of franchise expansion on political outcomes we face the problem that the

change in franchise is systematically related to a set of constituency level characteristics that are likely to have an independent effect on political outcomes. Without controlling for these local characteristics, our estimates are likely to be biased due to omitted variables.

A second factor we need to consider when estimating the impact of franchise extension is the national swing toward the Liberals in 1868. If were to ignore this effect then some portion of our estimate of an increase in the franchise at the constituency level may in fact be due to factors uncorrelated with the impact of this institutional change. To avoid the possibility of a spurious correlation that may arise, we need to control for the underlying trend in Liberal support that is the same across constituencies.

As long as this differential impact of the reform on franchise levels is driven by community characteristics that are fixed overtime (or that vary slowly), we can measure the impact of franchise extension by comparing the differences in outcomes between communities where franchise levels vary by different amounts.

We estimate the following benchmark model for constituency j at time t:

$$\triangle Y_{jt} = \alpha_0 + \beta_1 \triangle \text{Log}(R_{jt}) + \epsilon_{jt} \tag{1}$$

where Y_{jt} is one of our outcomes of interest and $\triangle Y_{ijt}$ represents the difference between 1865 and 1868 in this indicator; $\triangle \text{Log}(R_{jt})$ is the difference in the log of constituency level registered voters between 1865 and 1868; and, finally, ϵ_i is a random error term.[7]

If the franchise level is the ratio of registered voters to the relevant constituency population then, provided that the population remains fixed or its change is uncorrelated with changes in R_{jt}, equation (1) is similar to regressing $\triangle Y_{jt}$ on the change in franchise level. Because this may not be the case we also present estimates where we control for the change in the local population by including the (logged) difference between the the population in 1865 and 1868, via the term $\triangle \text{Log}(P_{jt})$. Including this term we then estimate

$$\triangle Y_{jt} = \alpha_0 + \beta_1 \triangle \text{Log}(R_{jt}) + \gamma \triangle Log(P_{jt}) + \epsilon_{jt}. \tag{2}$$

The parameter of interest in equation (1) and (2) is β, the causal effect of changes in the franchise on electoral outcomes. Our estimate of β is the average increase in Liberal strength as a (linear) function of the difference in the log of the enfranchised population. Our estimate separates this effect from a constituency specific constant, and a trend that is constant across all constituencies.

[7]This model in first-differences is equivalent to one in levels with constituency fixed effects and a time dummy.

Our empirical strategy goes a long way towards controlling for potential confounders in the relationship between franchise extension and political outcomes. It seems unlikely that, given the abrupt change to the constituency franchise, local trends in population, income, or wealth, could be systematically correlated with the expansion in the franchise. However, our identification strategy may still suffer from concerns about the exogeneity of our measure of franchise expansion.

The first issue concerns reverse causality. If, and as some historians have claimed, the reforms were designed to maximize the Conservative vote share, or at least minimize their losses, then the difference in the constituency level voting population should reflect the expected vote shares in the constituency. For example, some historians have claimed that Disraeli was prepared to abandon the boroughs that would return Liberal candidates under any voting rule, and concentrated on shoring up the Conservative vote in the boroughs. Although the first differences eliminate the constituency fixed effect, and so assuages this concern to a large part, the largest change in the electorate may have occurred in areas where support for the Liberals grew relatively faster. If this is so then our estimates will be biased downwards. Of course, we can test this hypothesis by looking at the difference in electoral outcomes between 1865 and 1859 and its correlation with the change in the local franchise.

A second concern is measurement error. In particular, when using registered electors as a measured of enfranchised voters we do not capture all citizens who are eligible to vote. In particular,

$$R_{jt} = r_{jt} E_{jt}$$

where E_{jt} is elegible voters and r_{jt} is the registration rate. If the registration rate is constant over time across constituencies, $r_{jt} = r_j$, then first differencing the logarithm of this expression eliminates the common error. However, if this error varies over time, we are subject to the traditional downwards bias even when measurement error is uncorrelated with E_{jt}.

In order to assess this problem we follow two strategies. Some of the biases created by the need to register large increases in eligible voters in a short time period are eliminated when looking at a longer time frame. Thus we also present results for the period 1874-1865.

An alternative strategy to deal with potential biases is to identify pre-existing constituency characteristics that explain the differential growth in the electorate. We instrument the change in electorates using the level of the electorate in 1832 and the population size in 1861.[8] These variables can then be used as instruments under the assumption that they are

[8]This is equivalent in the fixed effects strategy to instrument using the interaction between the year dummy and the log (electorate 1832) and log (population 1861).

correlated with changes in the electorate but not directly correlated with subsequent changes in the outcomes. Clearly, if the instruments are valid they could also help us evaluate the claim of reverse causality.

6. Estimating the Effect of the Second Reform Act

We begin by exploring whether the immediate electoral outcomes in 1868 can be related to the extension of the franchise. We present our analysis in Table 6 which shows estimates for equation 1 when exploring the effect of the franchise extension on constituency level turnout, the share of seats won by the Liberals, and the Liberal share of the vote.

The first column estimates the impact of the change in the constituency level franchise on the proportion of voters in the constituency who took part in the landmark election. Our estimates suggest that the difference in turnout between 1865 and 1868 is somewhat lower in areas where the impact of franchise extension was largest, and this is also true when, as in column 2, we add a covariate for the difference in the size of the local population. But, when taking into account the standard error of this estimate, we fail to reject the null hypothesis that the extension of the franchise had no significant effect on turnout: it appears to have been the same in areas where the voting population increased by a large amount as in areas where the voting population was relatively stable.

The third and fourth columns record the impact of the difference in the relative constituency level voting populations on the share of constituency seats won by the Liberals. Recall that this measures ranges from 0 in constituencies where liberals won no seats, to 1 where Liberals won 100 percent of the seats. Surprisingly the estimated effect of the differences in the relative constituency level voting population is negative. Once again, however, when considering the standard error we would fail to reject the hypothesis that the franchise extension had no impact on the Liberal seat share at the constituency level, once we account for national and constituency level factors.

Columns 5 and 6 analyze whether there is any evidence relating the change in the constituency level franchise to the change in the Liberal share of the constituency level vote. The estimates are positive as expected, but again the magnitude of the effect is small. For example, in model 6 that includes population controls, a 100% increase in the (logged) difference in the voting population the leads to a 0.07% increase in the Liberal share of the vote. However, even this result, indicating a very small effect, fails to pass standard tests of statistical significance and so we fail to reject the null hypothesis that the increase in the franchise had no effect on the constituency level Liberal vote share.

Of course, we are unable yet to conclude that the extension of the franchise had no affect based on electoral outcomes. This is because the choices that voters make depends upon the menu of options available to them. And this, in turn, reflects the strategic choices made by parties as to where to run their candidates (and where not to run them). We explore this question in Table 7 where we present estimates of models that look at the share of seats that were uncontested in each constituency, the ratio of candidates to seats, and, critically, the share of candidates at the constituency level who were Liberal.

In columns 1 and 2 of Table 7 we study the effect of franchise extension on the share of uncontested seats. We show that, although between 1865 and 1868 the percentage of uncontested seats declines sharply, the share of uncontested seats is unrelated to the impact of the reform at the constituency level. More precisely, although our estimate of the change in the electorate on the share of uncontested seats is negative as expected, we fail to reject the null hypothesis of no relationship between these variables. Similarly, whilst our estimate is that the ratio of candidates to seats is positive we once again fail to reject the null hypothesis. Finally, and critically, we turn to a key indicator of liberal strength, namely the share of constituency level candidates who represent the Liberal party. Our estimate suggests a small increase in this share, but when taking into account the standard error of this estimate we cannot conclude there was in fact a significant impact of franchise extension on the share of Liberal candidates who stood for election in 1868.

Finally, in Table 8, we look at whether the extension of the franchise is causally related to candidate competition at the constituency level. Our estimates suggest that incumbents were less likely to run again in a constituency where the impact of the reform on the voting population was large, and that this is also true of Liberal candidates, but once again we fail to reject the null hypothesis of no causal impact of the reform on these outcomes. Similarly there is no evidence that suggests that the decision to place Liberal candidates in constituencies where they had been beaten at the previous election was affected by the impact of the voting reforms.

In sum, and for the period 1865-1868, once we account for a trend toward the Liberals that affects all constituencies, as well as local demographics, and when considering a variety of different political mechanisms that might translate electoral reform into a Liberal advantage in 1868 we are left with a stark conclusion: whilst there are many plausible links between the extension of the franchise and the Liberal success in 1868, there is no independent effect of changing the franchise that we can causally link to their victory.

7. Robustness checks

Our estimates fail to show any conclusive evidence that the outcome of the election in 168- a victory for the Liberals- was in any way related to the change in the rules governing the eligibility to vote in this election. In this section we assess how robust are our null findings.

First, we explore whether our results change when we consider different constituency samples. Whereas till now our estimates have been from borough elections, in panel A of Table 9, we analyze the returns from county elections and show that the null findings from the boroughs carry over to the counties.

A further issue concerns the redrawing of constituency boundaries that accompanied the reform. Redistricting is a politically charged issue, the more so in light of a major change in the vote eligibility requirements. And as we noted earlier, Disraeli had through his role on the Boundary commission, established under the Second Reform Act to establish the boundaries for boroughs and counties, done his best to enhance the rural character of the counties. In panel B of Table 9 we restrict our analysis to those boroughs where the boundaries were left unchanged. Our main findings remain unchanged when we consider this sample but their are subtle differences in the outcomes. For example we now find a statistically significant *decrease* in turnout where the impact of reform on the voting population was largest. And we find a statistically significant, though small, increase in the share of Liberal candidates: an effect of around 1% for a 100% increase in the franchise. We fail, however, to reject the null hypothesis of no relationship between increase in the constituency level voting population and the Liberal share of votes or the share of elected Liberal candidates.

Finally, in In panel C of Table 9, we explore whether our results change when using a less restrictive sample. Here we include all boroughs, irrespective of whether there are changes in the number of seats or not, and discover no new evidence of an impact of the reforms.

As discussed earlier, a possible problem with our analysis occurs if the constituency level voting population, is causally related to the previous Liberal vote share in those constituencies. If this were so then then we could not reasonably claim that the treatment is exogenously assigned; correspondingly, estimates derived from an analysis of equation 1 would be biased.

To analyze the validity of our assumption, we analyze the correlation between our key measure, difference in the log of the relative voting populations in 1865 and 1868, and the difference in outcomes between the 1865 election and the 1859 election. If the franchise would have grown more strongly (weakly) in places where the liberals were stronger (weaker) we would expect a positive (negative) association between the difference in outcomes in 1865-1859 and the difference in log electorates in 1868-1865. In Table 10 we present the result

of our analysis. Reassuringly, for each of our measures, we are unable to reject the null hypothesis of no association between these variables.

We turn now to probe deeper into the reasons why, contrary to expectation, we were unable to find a causal link between the extension of the franchise in 1867 and the Liberal victory in 1868. As discussed earlier, possible reasons for our null findings relate to measurement error and endogeneity that arises because the impact of the franchise extension at the constituency level is not randomly assigned, but rather reflects political calculations made by the implementers of the reform.

An empirical strategy that can plausibly deal with these issues involves the use of instrumental variables. An alternative strategy to check for biases is to identify pre-existing constituency characteristics that explain the differential growth in the electorate. We instrument the change in electorates using the level of the electorate in 1832 and the population size in 1861. These variables can then be used as instruments under the assumption that they are correlated with changes in the electorate but not directly correlated with subsequent changes in the outcomes.

The first-stage was presented in Table 4. Table 11 replicates the analysis of Table 5 using our instruments which pass standard overidentification tests. Most of our results remain unchanged with regard to the election in 1868. There are two caveats. We find that a 100 percent increase in the franchise reduces those constituencies that were uncontested by around 5%. Moroever, we now find some evidence of an effect of franchise extension on the Liberal share of the vote. When we do not control for the change in the constituency population, then a 100% increase in the franchise leads to an increase in the Liberal vote share of around 2%. However, the effect is smaller, and no longer statistically significant, when we add this control. Overall our findings are largely unchanged: we fail to reject the hypothesis that there is no causal link between the constituency level change in the franchise and the strength of Liberal candidates.

8. What Was the Effect of Franchise Extension?

Thus far then we have failed to find a direct impact of the electoral reform, at least upon the outcomes of the 1868 election. Pushing further, we consider whether the impact of the reform, whilst not felt immediately in 1868, affected the outcome of the next election in 1874 won by the Conservatives. We explore this issue in Table 12 where we analyze the outcomes of that election and their relations to the constituency level difference in the voting population. We find no effect, even in the longer term, of franchise extension on the Liberal share of the vote or of constituency seats. However, whilst we are unable to find a direct

effect on our key indicators of Liberal strength, here we do find evidence that the change in the electoral rules did affect party and candidate competition at the constituency level.

In particular, there is an associated decrease in the number of constituency races were a candidate runs unopposed. A substantive interpretation of these results are that a 100% increase in the franchise leads to a 14% reduction in contested seats at the constituency level and a 17.7% increase in the ratio of candidates to seats. However, despite the increase in the number of contested seats, and the fact that this effect was more pronounced in constituencies most affected by reform, this is not due to greater contestation by Liberal candidates. In fact we observe that the share of Liberal candidates is decreasing in the areas where the impact of franchise reform is largest. A 100% increase in the local franchise corresponds with a 7% decrease in the share of Liberal candidates This suggests that the reforms did not benefit the Liberals in 1874 as they were unable to adapt their organization to take advantage of this opportunity. Instead the evidence supports the view that it was the Conservatives who took the advantage.

Finally, Table 12 also shows a statistically significant relationship between franchise extension and incumbency effects in the 1874 election. The share of candidates who were sitting incumbents decreased and more sharply in areas most affected by reform. A 100% increase in the franchise corresponds with a rough 17% decrease in the share of incumbents running in a given constituency. Moreover, almost of all of this effect is picked up by the fact that Liberal incumbents were less likely to run.

One argument that support the findings in Table 12 is that the Conservative Party presented an organizational means by which the impacts of reform could be neutralized. As noted by St John (2006), in his biography of the Conservative leader, Disraeli realized that extension of the franchise placed an onus on effective party organization. He hired John Gorst to act as electoral agent of the party and as head of the new Conservative Central Office which provided a central register from which local Conservative associations could select candidates. In addition local conservative associations were encouraged and formed under the umbrella of a National Conservative Union. The net result was that in the 1874 election the Conservatives were able to contest 63 previously uncontested Liberal seats.

Whilst the evidence suggests that neither party benefitted directly through the incorporation of new voting groups, the key factor that links franchise extension to the electoral success of the Conservatives in 1874 is the ability of the Conservatives to adapt their party organization to fighting previously uncontested seats. Somewhat surprisingly then, it was the Tory party, champion of traditional institutions, and allied to the interests of the landowning class, which was quickest to adapt to the requirements of the constitutional reform. Our result could be seen as evidence that, despite fundamental democratic reform, political elites in

the Conservative Party were able to adapt their organization to minimize any detrimental impact of reform. However, in the case of 1874, and unlike that of 1868, we cannot be sure that there is a direct causal relationship between franchise extension and political outcomes: in 1872 the secret ballot was introduced and so this affected the circumstances of the election in 1874.

9. Conclusion

The Second Reform Act of 1867 is a major turning point in British history and is associated with the election of the first unequivocally Liberal government on a reforming agenda. The causes and consequences of the Reform Act remain disputed by historians and political economists to this day, with some viewing it as a result of a combination of ingredients related to inter- party competition, whilst others highlight the effect of factors external to inter-party competition at the time, namely popular agitation for reform. The immediate effect of the Act, the election of a reforming Liberal government under Gladstone, can be used as evidence against the view that reform was was related to strategic political calculation. We have explored a number of possible political mechanisms that can plausibly relate the change in the electoral rules to the outcomes of the election in 1868. However, and despite the significance of the Act, when controlling for a trend toward the Liberals that affects all constituencies, as well as local demographics, we find no evidence that electoral reform had a causal effect on the outcome of the 1868 election.

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Figure 1: A timeline of Major Electoral Reforms in the UK

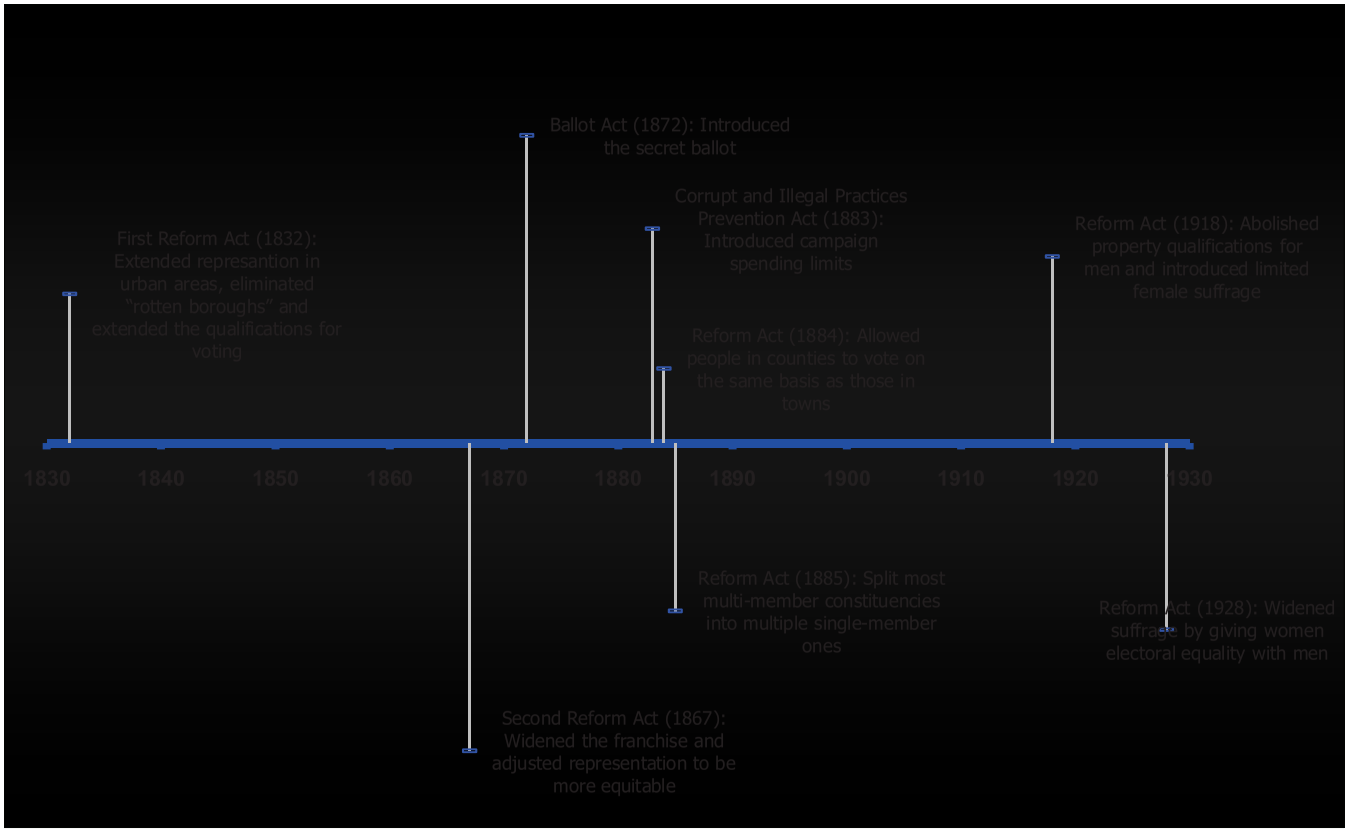
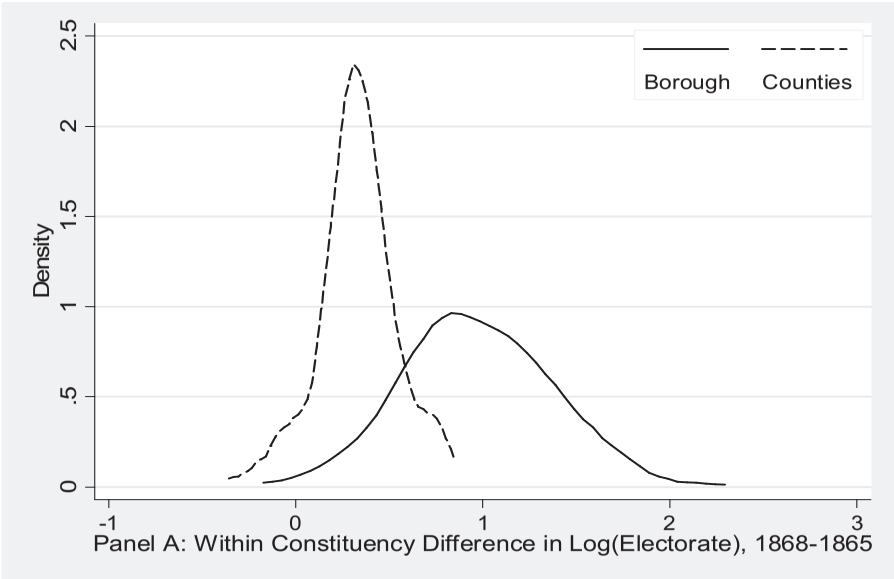


Figure 2


Borough
Counties
Density
0
.5
1
1.5
2
2.5
-1
0
1
2
3
Panel A: Within Constituency Difference in Log(Electorate), 1868-1865

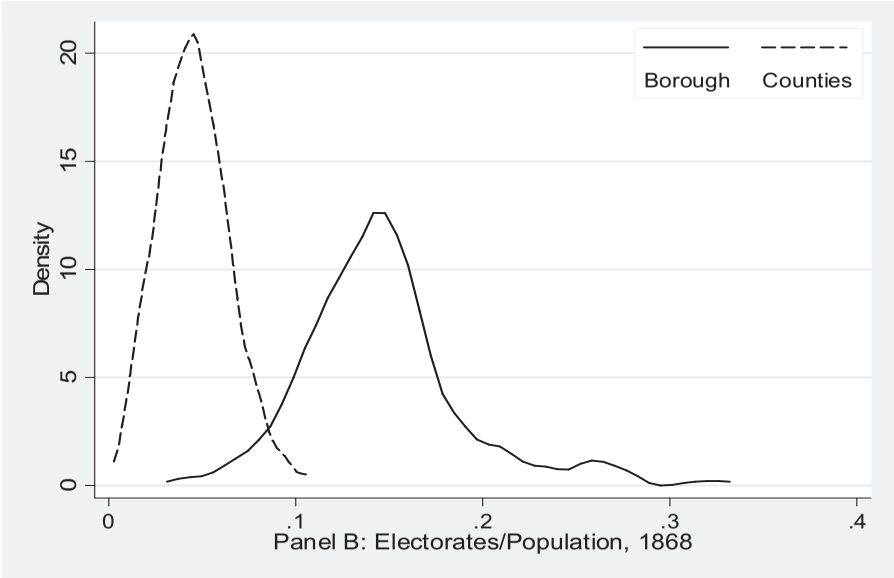

Borough
Counties
Density
0
5
10
15
20
0
.1
.2
.3
.4
Panel B: Electorates/Population, 1868

Table 1: Registered Electorate

	Country			Location	
Year	England	Scotland	Wales	Boroughs	Counties
1859	908,273	105,608	56,033	491,402	578,512
1865	979,375	105,069	61,656	544,661	601,439
1868	1,891,136	237,509	127,385	1,377,124	878,906
1874	2,109,916	280,877	137,143	1,606,454	921,482

Source: Own calculations based on data from Craig (1989).

Table 2: Head of Households Estimated Annual Income, Skills and Rent Paid in the UK at different points of the Income Distribution in 1860

	Annual Income of the Head of the Household	Typical Occupation of the Head of the Household	Rent Paid by a Typical Family
Lowest decile	33 pounds 16 shellings	Average agriculatural laborer	3 pounds 18 shellings
Lower quartile	40 pounds 6 shellings	Bottom of unskilled	6 pounds 10 shellings
Median	53 pounds 6 shellings	Top of unskilled	7 pounds 16 shellings
Upper quartile	71 pounds 10 shellings	Ordinary semi-skilled	10 pounds 8 shellings

Source: Own calculations based on Mackenzie (1921) and Bowley (1937).

Table 3: Number of constituencies 1865-1874

	Boroughs	Counties
Total number of constituencies in 1865	221	114
Total number of constituencies in 1868	225	129
Total number of constituencies in 1874	223	129
Constituencies that appear in 1865, 1868 & 1874	207	97
Constituencies without changes in seats, 1865-1874	164	97
Constituencies without changes in seats and no boundary changes, 1865-1874	113	60

Source: Own calculations based on data from Craig (1989).

Table 4: Explaining the change in Electorates, 1868-1865

	Dependent variable: Log(Electorate 1868) - Log(Electorate 1865)					
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Boroughs						
Log (Electorate 1832)	-0.153***	-0.147***			0.458**	0.391*
	[0.029]	[0.028]			[0.194]	[0.204]
Log (Population 1861)			-0.045	-0.057*	0.753***	0.654***
			[0.034]	[0.034]	[0.150]	[0.158]
Log (Electorate 1832) x Log (Population 1861)					-0.076***	-0.067***
					[0.019]	[0.020]
Observations	164	164	164	164	164	164
R-Squared	0.127	0.209	0.012	0.113	0.277	0.311
Panel B: Counties						
Log (Electorate 1832)	-0.139***	-0.128***			0.134	0.068
	[0.018]	[0.017]			[0.292]	[0.242]
Log (Population 1861)			-0.090***	-0.069**	0.296	0.272
			[0.033]	[0.033]	[0.209]	[0.184]
Log (Electorate 1832) x Log (Population 1861)					-0.029	-0.023
					[0.026]	[0.022]
Observations	97	97	97	97	97	97
R-Squared	0.385	0.416	0.127	0.173	0.423	0.471
Control Population Change?	No	Yes	No	Yes	No	Yes

Source: Craig (1989) and Vincent and Stenton (1971).
Notes: OLS regressions. *** indicates statistical significance at 1%, ** at 5%, and * at 10%. Robust standard errors are in parentheses.

Table 5: Descriptive Statistics

Year:	1859			1865			1868			1874		
	N	Mean	SD	N	Mean	SD	N	Mean	SD	N	Mean	SD
Electorate	164	2373.18	4163.94	164	2591.31	4699.28	164	5766.94	6818.66	164	6684.71	7673.58
Share Liberal Seats	164	0.70	0.41	164	0.72	0.40	164	0.77	0.38	164	0.58	0.44
Voter Turnout	83	0.73	0.13	99	0.76	0.24	130	0.76	0.25	145	0.75	0.14
Share Liberal Vote	83	0.66	0.21	99	0.63	0.19	130	0.64	0.17	145	0.54	0.17
Share Uncontested Seats	164	0.49	0.50	164	0.40	0.49	164	0.21	0.41	164	0.12	0.32
Candidate Seat Ratio	164	1.40	0.43	164	1.49	0.46	164	1.70	0.46	164	1.88	0.45
Share Liberal Candidate	164	0.65	0.34	164	0.66	0.30	164	0.66	0.23	164	0.57	0.21
Share Incumbent				164	0.57	0.44	164	0.66	0.42	164	0.61	0.42
Share Liberal Incumbent				164	0.42	0.44	164	0.52	0.44	164	0.45	0.43
Share Losers				164	0.06	0.15	164	0.08	0.16	164	0.09	0.16
Share Liberal Losers				164	0.03	0.11	164	0.05	0.13	164	0.03	0.10

Source: Own calcualtion based on data from Craig (1989) and Vincent and Stenton (1971).

Table 6: Electoral Outcomes Under the New Franchise (1868-1865)

	Dependent variable:					
	Change Share of Liberal Seats		Change Voter Turnout		Change Share Liberal Vote	
	(1)	(2)	(3)	(4)	(5)	(6)
Change Log(Electorate)	-0.057	-0.097	-0.085	-0.085	0.021	0.008
	[0.077]	[0.085]	[0.067]	[0.062]	[0.049]	[0.046]
Observations	164	164	88	88	88	88
R-Squared	0.003	0.017	0.017	0.017	0.003	0.056
Control Population Change?	No	Yes	No	Yes	No	Yes

Source: Craig (1989) and Vincent and Stenton (1971).
Notes: OLS regressions. *** indicates statistical significance at 1%, ** at 5%, and * at 10%. Robust standard errors are in parentheses.

Table 7: Party Competition Under the New Franchise (1868-1865)

	Dependent variable:					
	Change Share Uncontested Seats		Change Candidate Seat Ratio		Change Share Liberal Candidates	
	(1)	(2)	(3)	(4)	(5)	(6)
Change Log(Electorate)	-0.018	-0.059	0.046	0.084	0.039	0.049
	[0.122]	[0.117]	[0.132]	[0.132]	[0.054]	[0.055]
Observations	164	164	164	164	164	164
R-Squared	0.001	0.009	0.001	0.008	0.004	0.006
Control Population Change?	No	Yes	No	Yes	No	Yes

Source: Craig (1989) and Vincent and Stenton (1971).
Notes: OLS regressions. *** indicates statistical significance at 1%, ** at 5%, and * at 10%. Robust standard errors are in parentheses.

Table 8: Candidate Competition Under the New Franchise (1868-1865)

	Dependent variable:							
	Change Share Incumbents		Change Share Liberal Incumbents		Change Share Losers		Change Share Liberal Losers	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Change Log(Electorate)	-0.15	-0.141	-0.098	-0.105	0.03	0.047	-0.007	-0.009
	[0.117]	[0.129]	[0.104]	[0.110]	[0.039]	[0.038]	[0.032]	[0.032]
Observations	164	164	164	164	164	164	164	164
R-Squared	0.01	0.01	0.005	0.006	0.003	0.014	0.001	0.001
Control Population Change?	No	Yes	No	Yes	No	Yes	No	Yes

Source: Craig (1989) and Vincent and Stenton (1971).
Notes: OLS regressions. *** indicates statistical significance at 1%, ** at 5%, and * at 10%. Robust standard errors are in parentheses.

Table 9: Electorate Outcomes, Party and Candidate Competition Under the New Franchise in Different Samples (1868-1865).

	Dependent variable: Change in the ...									
	Share Seats Liberal	Voter Turnout	Share Liberal Vote	Share Seats Uncontested	Candidate Seat Ratio	Liberal Candidate	Share Incumbent	Share Liberal Incumbents	Share Losers	Share Liberal Losers
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Panel A: Counties										
Change Log(Electorate)	0.109	0.012	0.096	-0.085	0.025	0.025	-0.264	-0.176	-0.087	-0.082
	[0.252]	[0.126]	[0.150]	[0.357]	[0.320]	[0.174]	[0.224]	[0.155]	[0.089]	[0.090]
Observations	97	21	21	97	97	97	97	97	97	97
R-Squared	0.011	0.052	0.026	0.004	0.001	0.004	0.014	0.008	0.016	0.02
Panel B: Boroughs without boundary changes										
Change Log(Electorate)	-0.047	-0.175*	0.056	-0.169	0.239	0.113*	-0.255	-0.177	-0.020	-0.045
	[0.115]	[0.104]	[0.065]	[0.146]	[0.151]	[0.064]	[0.170]	[0.139]	[0.044]	[0.039]
Observations	113	63	63	113	113	113	113	113	113	113
R-Squared	0.009	0.045	0.071	0.023	0.032	0.025	0.023	0.014	0.018	0.009
Panel C: Boroughs with change of seats										
Change Log(Electorate)	-0.073	-0.081	0.004	-0.125	0.142	0.036	-0.176	-0.094	0.032	-0.008
	[0.075]	[0.057]	[0.047]	[0.112]	[0.120]	[0.050]	[0.115]	[0.099]	[0.033]	[0.028]
Observations	207	111	111	207	207	207	207	207	207	207
R-Squared	0.009	0.087	0.015	0.016	0.019	0.016	0.031	0.016	0.008	0.001
Control Population Change?	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Source: Craig (1989) and Vincent and Stenton (1971).
Notes: OLS regressions. *** indicates statistical significance at 1%, ** at 5%, and * at 10%. Robust standard errors are in parentheses.

Table 10: Is the Change in Electorate (1868-1865) Correlated with Previous Changes in Outcomes (1865-1859)?

	Dependent variable: Change (1865-1859) in the ...									
	Share Seats Liberal	Voter Turnout	Share Liberal Vote	Share Seats Uncontested	Candidate Seat Ratio	Liberal Candidate	Share Incumbent	Share Liberal Incumbents	Share Losers	Share Liberal Losers
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Change Log(Electorate, 1865-1865)	0.028	0.035	0.006	0.047	-0.042	0.026				
	[0.068]	[0.043]	[0.051]	[0.106]	[0.100]	[0.037]				
Observations	164	66	66	164	164	164				
R-Squared	0.004	0.053	0.004	0.015	0.02	0.008				
Control Population Change?	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Source: Craig (1989) and Vincent and Stenton (1971).
Notes: OLS regressions. *** indicates statistical significance at 1%, ** at 5%, and * at 10%. Robust standard errors are in parentheses.

Table 11: Electorate Outcomes, Party and Candidate Competition Under the New Franchise: Instrumental Variables Estimates

	Dependent variable: Change in the ...									
	Share Seats Liberal	Voter Turnout	Share Liberal Vote	Share Seats Uncontested	Candidate Seat Ratio	Liberal Candidate	Share Incumbent	Share Liberal Incumbents	Share Losers	Share Liberal Losers
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Change Log(Electorate)	0.157	-0.039	0.154	-0.31	0.167	0.043	-0.517**	-0.380*	-0.017	-0.034
	[0.140]	[0.119]	[0.121]	[0.227]	[0.259]	[0.105]	[0.239]	[0.221]	[0.070]	[0.057]
Observations	164	88	88	164	164	164	164	164	164	164
Overidentification test (p-value)	0.2234	0.0734	0.3686	0.4711	0.5672	0.2806	0.062	0.4378	0.2712	0.3431
Control Population Change?	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Source: Craig (1989) and Vincent and Stenton (1971).
Notes: Instrumental Varibles regressions. Instruments are: Log (Electorate, 1832), Log (Population, 1861), and their interaction. *** indicates statistical significance at 1%, ** at 5%, and * at 10%. Robust standard errors are in parentheses.

Table 12: Electorate Outcomes, Party and Candidate Competition Under the New Franchise (1874-1865)

	Dependent variable: Change in the ...									
	Share Seats Liberal	Voter Turnout	Share Liberal Vote	Share Seats Uncontested	Candidate Seat Ratio	Liberal Candidate	Share Incumbent	Share Liberal Incumbents	Share Losers	Share Liberal Losers
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Change Log(Electorate)	0.047	0.015	0.006	-0.201**	0.249**	-0.105**	-0.237**	-0.221**	0.051	-0.004
	[0.087]	[0.026]	[0.039]	[0.095]	[0.124]	[0.047]	[0.102]	[0.110]	[0.036]	[0.025]
Observations	164	93	93	164	164	164	164	164	164	164
R-Squared	0.004	0.01	0.002	0.026	0.027	0.05	0.026	0.029	0.019	0.024
Control Population Change?	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Source: Craig (1989) and Vincent and Stenton (1971).
Notes: OLS regressions. *** indicates statistical significance at 1%, ** at 5%, and * at 10%. Robust standard errors are in parentheses.