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ORIGINS OF PARTY ALIGNMENT IN ENGLAND - EMPIRICAL STUDY OF VICTORIAN ELECTIONS

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Abstract

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Abstract

In this study we empirically examine the development of party oriented electorate in 19th century England by utilizing a set of recently digitized voting records, known as poll books. Poll books provide us with unique individual-level data on voting behavior that allows us to avoid the pitfalls of making inferences using aggregated data. We use a difference-in-difference setup to study voting behavior between working and middle class voters. We observe a statistically significant drop in levels of split voting, and increase in liberal candidate's vote shares, in 1865 general election and following elections. This reduction in split voting and increased amount of votes for liberal candidates is significantly stronger among the working class voters. Our results provide new evidence for the claim that in England the electorate became party oriented before the major expansion of the franchise, Second Reform Act of 1867.

Key words	party alignment, differences-in-differences, poll books, voting
Other	

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1. Introduction

In this study we examine the political development of England during the 19th century. We focus on the period between the First and the Second Reform Act, and examine the development from a clientelistic system of the early 1800's to the policy and party oriented system of the late 1800's. This shift occurred rapidly during the 40 year period from 1832 to 1872. Our attempt is to empirically analyze the shift from candidate centric system to a party centric system by analyzing old voting records, known as poll books. Poll books offer us individual-level longitudinal data on voting behavior, which allows us to potentially follow the same voter across multiple elections and observe possible changes in voting behavior. This kind of data is extremely valuable for voting research, since it doesn't rely on individual's own account of their vote, like mass surveys, such as, European Election Study (EES), European Social Survey (ESS) or American National Election Study (ANES), which serve as the basis of most modern empirical studies on voting behavior. Individual-level data also allows us to apply modern econometric methodology, which allows us to make causal inferences about the underlying changes in voting behavior. Individual-level data is also not susceptible to statistical fallacies born from ecological inference, such as ecological fallacy, because we are able to observe who actually voted for who.

Dewan et al. (2020) was the firsts study to our knowledge, to use a digitized individual-level poll book data for their analysis. Their findings corroborate the seminal works of Cox (1984a, 1986, 1987), which observed the decrease in levels of non-partisan voting that happened from 1857 to 1865. Dewan et al. provide causal evidence for this finding, by analyzing poll books from three constituencies. Dewan et al. also analyze class differences in the non-partisan voting and find that the decrease was mostly driven by the alignment of the English working class with the Liberal party. The analysis of class differences in voting is important, since we know that different socioeconomic factors are a large driver of voting participation (Martikainen et al., 2005; Dee, 2004) and party choice (Gelman et al., 2007; Brooks and Manza, 1997) to this day.

The findings of Dewan et al. are important since they are the first to apply modern econometric methods on the individual-level data, which allow the authors to make causal claims about the development of the party oriented system. Thus, Dewan et al. (2020) is the first study which provided causal evidence that British parties emerged to resolve the issues of effective legislating as the power of an individual MP diminished. Formation of the organized political parties cannot be attributed to be the mere consequence of electoral reforms. Party oriented electorate followed, as a result of new programmatic appeals made by these newly organized parties. We replicate and expand the results of Dewan et al. by using a larger set of poll books, which have been digitized only in recent years. The novel analysis of a larger and more representative sample of voting records is the main contribution of this study.

Our findings mostly align with those of Dewan et al. (2020), but we find meaningful differences in the effect sizes, especially when accounting for various fixed effects. Like Dewan et al., we observe a statistically significant reduction in split voting rates among working class voters in elections during and after 1865. However, Dewan et al. observe this reduction to be 4.5 p.p., whereas we observe an effect which is only one third of

that, 1.5 p.p. We also observe a statistical significant alignment of the working class with the Liberal party, in elections during and after 1865. However, this result cannot be interpreted as causal, as we show that voting behavior for Liberals among the middle and working class has not evolved along the same trend. We also find that working class voters have always voted more for the Liberal candidates compared to the middle class, even in elections before 1865. This result is in opposition to the findings of Dewan et al., who find an opposite result.

To us, our results suggest that the original sample used by Dewan et al., was not representative of the English electorate. The original sample is heavily driven by the constituency of Sandwich, which represented over 62% of the total observations. This is, why we believe the original results to overestimate the effects of changes in electoral behavior. However, these slight differences in results do not affect the interpretations of the underlying electoral behavior made by Dewan et al.

In Chapter 2, we give a brief overview of the important electoral reforms which affected the development of the party oriented system. We start form the First and Second Reform Acts and the events and debates leading to expansion of the franchise. We discuss the Secret Ballot Act of 1872, which ended public voting, and public voting records, in Britain.

In Chapter 3, we discuss the related literature on political development. We cover multiple theoretical and empirical studies that discuss how countries develop from a clientelistic system to policy oriented ones. At the end of the chapter, we go over the procedural changes which occurred in House of Commons in the 19th century, and which led to the development of party discipline and political parties as the wielders of political power. We also discuss the changes which affected the development of the party oriented electorate.

In Chapter 4, we discuss ecological fallacy, which is a statistical fallacy that occurs when making inferences about individual-level effects based on aggregated data. It is important to use individual-level data from the poll books, because we show that constituency-level aggregate data, leads to upwardly biased results in the final estimation. Individual-level data also allows us to observe what groups were the most prevalent split voters and who ultimately ended up changing their voting behavior. We also extensively discuss the poll book data and provide descriptive statistics of the voting data and electorate. We also discuss the problems with the matching of the poll book data to the census data, and show that the matched data is ultimately unreliable for any causal inference. We also briefly discuss the theoretical aspects of voting in a election with two votes.

In Chapter 5, we go over our empirical strategy. We use a Difference-in-Differences (DID) estimation, because we can divide our voters into middle and working-class based on their occupations. With this divide, we can use DID estimation to study the change in split voting rates and rates of liberal candidate's vote shares, which we use as dependent variables. We discuss the history and famous applications of DID and then move on to our application of DID, which follows closely that of Dewan et al. (2020). We also discuss recent developments in the two-way fixed effects literature related to multiple treatment periods and why we need not be concerned about this problem in our setting.

In Chapter 6, we go over our results for the DID estimation. We first replicate the results of Dewan et al. (2020) using our own classification of the occupations. Our replication results strongly agree with the results of Dewan et al. We then go over the constituency-level results for the constituencies which had the occupational information recorded in the poll books, after which we cover the results for those constituencies which

did not have the occupational information in the poll books, and required matching from census data. Finally, we go over the results for the pooled data, which we consider to be our main results. Chapter 7 concludes.

2. Institutional setting and Poll books

In this section we give an overview of the institutional background of 19th century England. We cover the major franchise expansions that happened during our period of interest, which is 1832-1872. We also discuss the mechanisms behind the emergence of party discipline and party oriented electorate. The goal of this section is to give better contextual understanding of our research setting before jumping into the poll book data and empirical analysis.

2.1 The First Reform Act of 1832

We start our analysis of the political climate of the first half of 19th century, by analysing the First Reform Act (also sometimes called the Great Reform Act) of 1832. By the beginning of the 1830's, the problems of England's electoral system had became apparent to almost everyone. At the beginning of the century, the English working class had started to slowly form in the industrialized cities across the nation. Many of these newly industrialized cities, such as Manchester and Birmingham, had no seats in the parliament, while some smaller constituencies had multiple seats. These smaller constituencies became known as *"rotten boroughs"*, due to their disproportionate representation and political corruption. Before the time of secret ballots, it was common for political candidates to buy votes. Vote buying was especially pronounced in the rotten boroughs, due to their smaller size, which made it relatively cheap to buy a seat in the parliament. (Phillips and Wetherell, 1995; Kam, 2017).

In 1830 a new Whig administration stepped in to power, led by the prime minister Charles Grey. In 1831 Lord Grey's government introduced the Reform Bill in the parliament for the first time. The Bill was eventually struck down, which led Lord Grey to ask the King to dissolve the parliament. A new general election was held, where Grey ran with the reform platform, which turned out to be overwhelmingly popular across the nation. After Grey's administration returned to power, with even greater majority in the Commons, the Reform Bill was successfully passed. However, it was struck down by the Tory controlled House of Lords. The defeat of the Bill in the House of Lords, prompted riots and demonstrations in large cities across the nation, which further exacerbated the fear of a revolution. A modified version of the Bill passed again the House of Commons, but was still defeated in the House of Lords. Eventually, the King reluctantly had to make threats to pack the House of Lords if the Bill would not be passed. This finally forced the House of Lords to pass the Bill that then became known as the Reform Act of 1832 (Phillips and Wetherell, 1995).

The Reform Act of 1832 increased the English electorate from approximately 400 000 electors to more than 650 000. It also disenfranchised over 80 rotten boroughs and enfranchised 67 new constituencies, that now included many of the industrialized cities across the nation. The Act allowed all men who payed over 10 £in annual rent to vote.

2.2 The Second Reform Act of 1867

The second franchise expansion came in 1867, nearly 35 years after the first major reform. The Act doubled the number of enfranchised electors, from approximately one million to two million in England, Scotland and Wales (Berlinski and Dewan, 2011).

The Reform Act of 1832 was a major step towards the modern electoral system in England, but it still left a lot to be desired. The First Reform Act highlighted the problems of representation and political corruption, which were still largely unresolved. By the end of 1830's a movement called *Chartism* had started to gain prominence. Chartists were mostly radicals that pushed for universal manhood suffrage and secret ballot voting, among other progressive policies. Chartists presented three petitions in the parliament in the years 1839, 1842 and 1848, with each petition being rejected.

By the beginning of 1850's the Chartists were no longer the driving force for reform, as politicians had begun to understand that another reform would soon be necessary. In the early 1850's, the largest push for reform came from the remnants of the Anti-Corn Law League. Anti-Corn Law League was a political movement that opposed protectionist Corn Laws, which banned or inhibited the importation of foreign corn (all grain was called colloquially 'corn', in the 19th century England), to England. The reason for the Corn Laws was to protect local agricultural interests, but it ultimately ended up worsening the economic conditions for every one else, through higher prices and lower availability of food, especially in the industrial cities. Corn Laws were successfully repealed in 1846, which led to the dissolution of the movement, but the more fundamental battle between the landed aristocracy and the new industrial powers remained.

This juxtaposition between the rural and urban England, was at the center of the first drafts of the Second Reform Act. Both, conservative and liberal parties, supported some form of electoral reform, but there was fierce debate about the specific provisions of the bill. For the purposes of this study, it is enough to know that conservatives wanted to expand the rural franchise for the purpose of protecting the interests of landed aristocracy and other land owners, meanwhile the liberal parties wanted to expand the urban franchise. In a technical sense, this meant that conservatives pushed to include land ownership as a qualification for the vote, whereas liberals tried to lower the household rental rate, to include much of the urban population.

By 1866, the Reform Act had mostly reached it's final form. It still could not pass the House of Commons, despite a Liberal majority, due to a split in the liberal party. This split was caused by the emergence of so called *Adullamites*, a more conservative faction within the liberals, who were mostly concerned with the balance of the classes within the liberal coalition. Adullamites demanded certainty about the new voters and constituencies that would be formed and brought under the new franchise, and since this certainty could not be guaranteed, the Adullamites joined in the dissenting vote with the conservatives. This led to the resignation of Russell's liberal government.

The fear that the newly enfranchised voters would increase the vote for the opposing side, is one of the main themes that emerges from all debates concerning the second Reform Act, and seems to be one of the main reasons for why it was ultimately such a difficult bill to pass, despite the fact that both political sides had a desire for another reform. In 1867, Benjamin Disraeli's conservative government took charge of passing the reform bill, in hopes of shaping the borough franchise, an issue dear to liberals, to their liking. Conservatives knew about the upcoming general election next year, and

wanted to pass the bill before it, to gain a possible electoral advantage. Finally in August of 1867, Reform Act was passed, after a debate spanning multiple months, about the "compounding"-system, which ultimately was abolished. Ironically, the general election held in 1868 was overwhelmingly won by the liberals, despite conservatives' attempts to shape the franchise to their advantage.

2.3 The Secret Ballot Act 1872

The secret ballot was of the main electoral reforms being pushed in England since 1832. Secret ballot was in many ways the natural and required extension for expanded suffrage. Voting rights could not be truly secured in the presence of immense outside pressures, enabled by open ballots. The first attempt to legislate secret ballots, came with the First Reform Act of 1832. However, it was ultimately dropped from the bill as a result of compromises that were required to pass the broader voting reform. After the passing of the First Reform Act, there was still political momentum behind the issue of secret ballots. Secret voting was one of the main issues pushed by the Chartist movement in the 1830's (Jaggard, 2019).

British elections before the introduction of the secret ballot were large and lengthy public events. Crook and Crook (2003) compares British and French elections in the 1800's, both countries having a system of public voting, but very different cultures related to elections. Crook and Crook state that British elections were held in large public gatherings and were notorious for their "exuberance, not to mention violence and drunkenness". Voters could be called one by one in front to prove their eligibility to vote and be questioned by the election officials and made swear oaths relating to their credibility, bribery and allegiance to the monarch. It was common for the voters to receive a range of reactions from the gathered crowd, ranging from cheers to beatings. Crook and Crook account that in Boston's 1830 elections, the crowd reacted to each vote that was cast by shouting. This should paint a picture of various forms of pressure being applied to each voter at the time.

Attempts to reform the voting system received a lot of push back. Defendants of public voting argued that secret voting would not prevent corruption and bribery. Some were even not sure that corruption in the elections was a problem worthy of addressing. Public voting was also seen as honorable. Secret voting was seen as un-English, because Englishmen were claimed to be proud of their vote. One account from 1869 states the following: "*The liberty cherished by Englishmen must be of vigorous growth, and no sickly exotic creeping into corners unable to bear the light*" (Crook and Crook, 2003).

Finally, in 1872 the Secret Ballot Act was passed, but not without a lenghty debate. The House of Lords managed to add critical changes in the last stages of bill, such as scrutinization of voters and an expiry date, which was set to 1880. Secret ballot was thus supposed to be only a temporary experiment. The new ballot was neither fully anonymous, since each voter still had to sign their registration number upon voting. This made it possible to identify each voter in case of suspected fraud. The Ballot Act also introduced written nominations for parliamentary elections, thus getting rid of the tradition of public nominations, which were also communal events. More emphasis was also put in orderliness in elections post Ballot Act. Polling stations were under supervision and voters were reminded to keep their votes secret. However, it seems that voters quickly

became accustomed to the new system, and the Secret Ballot Act was renewed, without major political battles in 1880. The practice of secret voting was also extended to smaller elections, such as county and district councils by the end of the century (Crook and Crook, 2003).

3. Related literature on political development

In this section we go over previous literature on political development and clientelism. Our aim is to provide answers to the question of why and how nations develop from a clientelistic systmem to a programmatic one.

Clientelism is a political system defined by direct or indirect exchange of goods and votes between political candidates and electorate. Clientelism differs from programmatic platforms, where political candidates promise to enact their policies, by giving direct transfers to voters, either in form of money or gifts. Clientelistic systems have become increasingly rare with the almost universal adaption of secret voting, especially in the developed world. Clientilistic systems with secret voting do still exist in the developed world (Wantchekon, 2003; Fujiwara and Wantchekon, 2013). Secret voting introduces the problem of credibility to clientelistic systems, that makes the equilibrium less stable. 19th Century elections in England were highly clientelistic in nature (Camp et al., 2014; Lizzeri and Persico, 2004).

Camp et al. (2014) construct a theoretical model of competing political parties to examine the clientelistic equilibrium. In the model two political parties compete and make a decision to, either distribute uniform benefits to all voters (programmatic strategy), or to use agents to target the benefits to a specific group of voters (clientelistic strategy). Camp et al. find that despite the use of agents being costly, it is also effective. This leads the parties to a bad equilibrium, similar to the prisoners dilemma, where programmatic approach would be less costly, but none of the parties want's to choose this, if the other party is using agents. The way out of this bad equilibrium is some exogenous change, which in Britain was the increased demand for public goods, wrought by industrialization and urbanization. Increased demand for public goods made clientelistic approach less effective, and together with legislation, such as the Secret Ballot Act of 1872 and the Corrupt and Illegal Practices Act of 1883, pushed Britain out of the clientelistc equilibrium.

Lizzeri and Persico (2004) study the question of, why political elites would want to expand the voting franchise, giving up some of their own political power? Lizzeri and Persico construct a theoretical model of a sequential game with two parties. The first party makes a decision between public good provision (programmatic strategy) or targeted benefits (clientelistic strategy). The second party observes the strategy of the first party and then decides it's own strategy (same two options as for the first party). Finally, voters observe promises made by each party and vote for the one that maximizes their utility. Lizzeri and Persico solve this game for restricted and universal suffrage, and find that there exists a solution, where under the universal suffrage elites are better off, when public goods are provided. The real world application of this model can be seen in the increase in public spending by local governments in England, during the 19th century. The spending of local governments was mainly directed towards public goods, especially in areas that were relevant for public health, such as plumbing. Disease epidemics, such as cholera and typhoid, were common during the 19th century, especially in the urban areas with high population densities. In these urban areas, not even the elites were able to totally avoid these epidemics, which led to increased demand for public health investment across all classes.

Multiple modern empirical studies on clientelism have come out of field experiments

in Benin Wantchekon (2003); Fujiwara and Wantchekon (2013); Adida et al. (2020). In Wantchekon (2003), a field experiment was conducted in the first round of 2001 Benin presidential elections, where 20 villages across 10 districts were matched based on the ethnic makeup of the village. Four presidential candidates were given two districts each, where they targeted one village with a clientelistic message and one with a public policy oriented message. Results of the experiment found that clientelistic message gave an advantage to all candidates in the villages, where clientelistic message was used. Clientelism was particularly effective for incumbent and regional candidates.

In Fujiwara and Wantchekon (2013), the authors conduct another field experiment in 2006 Benin presidential elections. Compared to Wantchekon (2003), the authors wanted to implement more specific policy platforms as alternatives to clientelistic ones, and include more public involvement from the voters in the form of town hall meetings with public debates. Three leading presidential candidates were involved in the experiment and they were given randomized treatment and control villages. In the treatment villages, researchers conducted town hall meetings that served as a substitute to the standard clientelistic rallies. In the town hall meetings, representatives of the presidential candidates gave a 15-minute speech on the policy platform of that specific candidate and then the floor was open for public debate, during which representatives could contextualize the policy platforms and amend them based on the public debate. In the control villages, standard clientelistic rallies were held, with no limitation on donations or gifts that the candidates gave to the voters. The authors find that the treatment had a negative effect on the most dominant candidate in the village and a positive effect on the candidate with the public policy platform, if they were not the most dominant candidate. Treatment had also a negative effect on the public policy platform candidate if they were the most dominant candidate in the village. No negative effect on the turnout was observed in the treatment villages. The authors suggest that candidates should adopt a policy oriented strategy in the villages, where they are not the largest candidate, and continue with clientelistic practices in their political strongholds. According to the authors, in the long run this could lead to the collapse of clientelistic equilibrium, if policy oriented platforms lead to voters becoming more informed, and thus less swayed by clientelistic appeals.

Adida et al. (2020) conducted an information campaign in Benin before the 2015 National Assembly elections. The authors collaborated with local nongovernmental organizations to spread information on the incumbent candidate's legislative performance. Information treatment was varied to capture the effect of legislative performance salience and collaboration within the constituency. The authors find that voters who got the information treatment rewarded good legislative performance and punished bad performance only when performance was salient and the information was widely spread across the constituency. This suggest that voters need to have a clear understanding of what is good legislative performance and need to know that others have received the same information, for them to alter their voting behavior. This interpretation is strengthened by the result that access to only positive legislative performance information, meaning no salience treatment or information about how many other voters received the information, actually lowered the vote share of the incumbent party.

Acemoglu and Robinson (2000) study why economic development was treated differently across Europe during the industrial revolution. Acemoglu and Robinson suggest that the opposition towards economic and technological development cannot be purely explained by economic interest, old industries protecting their old methods of production

by blocking entry of new technologies to the market (economic-losers hypothesis). This is because attempts to block new technologies, have always in the end proven unsuccessful. Acemoglu and Robinson hypothesize instead, that the reason for different economic development and levels of industrialization, can be explained by the *political-losers hypothesis*. Economic development during the industrial revolution, not only affected the distribution of economic power, but also that of the political power. In the study, the authors develop a theoretical model to explain this phenomenon and relate it to four real world examples, Britain, Germany, Austria-Hungary and Russia. In Britain and Germany, political elites were more favorable towards industrialization, and thus economic development, due to the fact that they had established institutions and coalitions that protected their political power, such as the House of Lords.

Boix (2001) study the effects of economic growth on the size of the public sector. Boix develops a theoretical model where individuals optimize for the tax rate and level of public investment. The model is then empirically tested against a macro economic data set, where public revenue as a percentage of GDP is the dependent variable, and various factors of development, such as, democracy, per capita income and level of turnout is used as independent variables. Boix finds two mechanisms that explain the growth of the public sector as the economy grows. First, as the economy grows, the government needs to step in to regulate market failures, provide public goods and set up institutions that boost private investment. Second, as economic development leads to structural changes in the economy, redistributive mechanisms mut be put in place. As the economy changes from an agrarian to an industrialized state, informal care, such as extended family taking care of the elderly and sick, goes down. Thus, government needs to set up institutions that provide some basic level of economic security to all individuals unable to participate in the economy. Boix also finds that in democratic regimes, turn out is positively linked to the size of the public sector.

Berlinski and Dewan (2011) study the effects of the Second Reform Act on 1868 general election results. As previously discussed, the Second Reform Act enfranchised more than 1 000 000 new voters in England, Scotland and Wales, most of whom came from urbanized areas, which traditionally voted more in favor of the Liberals. The 1868 general election was won by the Liberal party led by Gladstone. Despite this coincidence, Berlinski and Dewan show that the Liberal victory cannot be purely explained by the new franchise. In other words, the newly enfranchised voters, did not systematically vote more in favor of Liberals than Conservatives. However, the electoral reforms did significantly increase political competition at the constituency level, which Liberals were better positioned to take advantage of.

In another study of the Second Reform Act, Berlinski et al. (2014) study the effects of enfranchisement on the political power of the British aristocracy. Applying similar methods and exploiting the same variation in franchise expansion as Berlinski and Dewan (2011), the authors study the background characteristics of Cabinet members and elected candidates, before and after the Second and Third Reform Acts. The authors find no significant variation in background characteristics, confirming previous hypothesis and findings that state that expanding the franchise had no effect on the types of candidates elected to the House of Commons. Thus, even after the franchise reforms, the political power of the aristocracy remained the same.

In this section we have covered theoretical models of clientelism (Camp et al., 2014; Lizzeri and Persico, 2004), and found out that clientelistic equilibrium is hard to break out

of, since it requires some exogenous shock to change voters preferences. In the 19th century Britain, this shock came in the form of increased demand for public goods, especially investments related public health, as urbanization and industrialization took place.

Empirical evidence on clientelism (Wantchekon, 2003; Fujiwara and Wantchekon, 2013; Adida et al., 2020) seems to also suggest that clientelism is truly a benefitial strategy for winning elections, and breaking out from clientelism requires a more educated voter base. Political elite's response to economic growth seems to also be dependent on the institutional structures that are in place, which secure political elite's power during the transition from a developing state to a developed state (Acemoglu and Robinson, 2000). Economic development is also linked to expansion of the public sector, as the state needs to intervene to address market failures and provide more income transfers to address economic inequalities (Boix, 2001).

We also briefly covered the effects of franchise expansion in 19th century England. There is no evidence that supports the claim, that the Second Reform Act affected electoral outcomes by enfranchising disproportionately Liberal voters (Berlinski and Dewan, 2011). Rather, the Second Reform act affected electoral outcomes through redistricting. The Second Reform Act also had no immediate effect on the background characteristics of political representatives (Berlinski et al., 2014). This further supports the story, that the old political elites were largely unaffected by the expansion of the franchise.

3.1 The emergence of a party discipline

During the 19th century, the legislative process of the English parliament went trough a transformation. In the early decades of 1800's, individual MPs had a lot of power in the introduction of new bills to the parliament. This was mainly because the cabinet was seen as exclusively executive branch of the English government, not a legislative one. This meant that nearly all legislation, besides funding for the existing programs, was introduced by individual MPs. What made this possible, was the alignment of interest and incentives, by both, individual MPs and the cabinet. Everyone in the House of Commons enjoyed broad parliamentary rights and benefits, for example, every MP had the right to adjourn the House whenever they so wished (Cox, 1987, p. 46).

This system started to change when so called *Order days* were introduced in the House. The Order days were introduced in 1811, partly as a result of additional House seats added in Acts of Union 1800, when the Irish parliament was merged together with the UK parliament. Additional MPs meant that more bills were introduced, which meant that less time was available for debate and discussion relating to individual bills. Since MPs had a lot of individual power, anyone could filibuster any bill they wanted and for long as they wanted. Thus Order days became the days when the House went trough all the bills that were proposed beforehand, which were listed in an Order book, before they could proceed to the so called "Orders of the Day". The Order days were Monday, Wednesday and Friday. Tuesdays and and Thursdays were left for the backbench MPs. The days reserved for the private MP business also quickly got a bad reputation, due to the fact that each MP could use their aforementioned parliamentary powers as they wish, which led to chaotic sessions. The cabinet also had little incentive to attend the House sessions on Tuesdays and Thursdays, because their legislative endeavors were scheduled for the remaining days. This ultimately lowered the backbencher MP's attendance as

well. This arrangement had solidified by the 1830's, and thus were born the so called "government days" and "private member days" (Cox, 1987, p. 47).

The increased constrains on time and pressure to pass more legislation, a point which we will discuss more later, led to two important outcomes regarding party discipline. First, as the legislative power of the cabinet increased with the Order days, it was increasingly hard for an individual MP to propose any legislation. This meant that connectedness and good reputation within one's party became increasingly important. This also meant that the ruling party, which was in charge of appointing cabinet positions, had now increased importance in pushing legislation. Second, as the proposed bills became longer and more complex by the latter half of the century, MPs didn't have as much time to study each bill. The result of this was the fact that, MPs ultimately became less informed of the bills that they had to vote on. This, together with the increased importance of the cabinet, is the main reason for why party discipline was born. MPs did not have enough time to get to know each bill intimately, thus they started to use the party's stance on any issue as a heuristic for their individual vote. This was a good strategy, since it increased the reputation of an MP within their party and solved the information problems brought by time concerns.

The last point we will discuss in this section is the question, why was there an increasing pressure to pass more legislation at the same time as there was less and less time to do politics? The reason for increased pressure to legislate, comes as a result of the previously discussed voting reforms. The Reform Acts of 1832 and 1867 enfranchised a substantial amount of new voters from newly formed constituencies, that were born around many of the industrial cities around England. These new constituencies, together with the newly enfranchised, meant that there were a lot more voters to please, to gain a seat in the House of Commons. Before the Reform Act of 1832, in many of the smaller boroughs the candidates could bribe enough voters, and thus essentially buy their way to the parliament. This strategy, however, was not a feasible one, because it became unreasonably expensive, after the Reform Acts (Cox, 1987, p. 56). This argument also has theoretical as Boix (2001) has shown that economic development increases the demand for public goods. Lizzeri and Persico (2004) have also shown that the demand for these public goods doesn't only come from the working class, but political elites as well.

This meant that, the MPs who came from these new large constituencies had to actually show that they could pass legislation that was important to their voters, to secure re-election. Cox (1987, p. 58) shows correlational evidence for the claim that MPs with large constituencies spoke more in parliament. Between May and August of 1859, 66.2 % of the MPs from the boroughs that had more than 2000 electors spoke in the parliament, whereas only 42.2 % of the MPs spoke who came from constituencies that had less than 2000 electors.

3.2 The emergence of a party oriented electorate

Now as we have a general understanding of the changes in parliamentary procedures which lead to the emergence of party discipline and parties as the main wielders of political power, we can start to examine the theory behind the main research question of this study; why did voters become party oriented?

The most extensive analysis of the development of party oriented electorate in England

is found in the works Gary Cox, mainly in Cox (1987) and Cox (1986). In Cox (1986), Cox observes the shift in split voting rates (SV) and rates of non-partisan plumping (NPP), by analyzing over 1000 ballot counts in 20 general elections, spanning from 1832 to 1918. Cox observes a clear decrease in SV rates and rates of NPP from 1857 to 1865, which precedes the Second Reform Act of 1867 and the later major reforms, such as the Secret Ballot Act of 1872, the Corrupt and Illegal Practices Act of 1883 and the Third Reform Act of 1884. After 1865, Cox finds no meaningful change in SV rates and rates of NPP, until 1910's.

Before the seminal works of Cox, two branches of quantitative research on the development of party oriented electorate can be identified. The first analyses the public voting records, known as poll books, to study the changes in voting behavior. However, these early studies of poll books mainly focus on short time series of poll books from individual constituencies, thus making broad nation wide inferences hard. Statistical analysis of these early studies is also rather elementary. The Second branch studies double-member districts and uses constituency-level election results as the statistical unit. This use of aggregate-level data is insufficient for good inference, because it is susceptible to a well understood statistical fallacy, known as Ecological fallacy, which we discuss more in Section 5.3. Cox (1986) was thus the first study to analyze a representative sample of ballot counts from a nearly 100 year time period.

After establishing that there indeed was a change in the observed levels of non-partisan voting, Cox explores two plausible explanations for the phenomenon. Cox first examines how voting in the two-member constituencies differs from single-member districts. We cover the theory of voting in a election with two votes and three candidates in the Section 4.3. For the purposes of this section, it is only important to know, that the theory predicts that the reduction in non-partisan voting can be either explained by 1) Conservatives and Liberals becoming more similar in terms of policy platforms, or 2) the party affiliation started to become more valuable to voters. This is because, voters are more likely to split the vote if they perceive a significant difference between the candidates of the same party. By significant difference, we mean that the two candidates from the preferred party might be so far apart, in terms of voters preferred policy, that the third candidate from the other party is closer to the voter's own preference than the less preferred candidate from voter's favorite party. Hence, any shift that reduces the differences between candidates of the same party also reduces split voting.

The first theoretical explanation for the voting behavior does not stand significant scrutiny. According to Cox, we observe party polarization coming up to the passing of the Second Reform Act. The next time we observe Conservatives and Liberals becoming more homogeneous is in the 1880's over the crisis of union with Ireland. This increased homogeneity in the 1880's, thus cannot explain the decrease in rates of non-partisan voting in the late 1850's.

The second explanation is much more credible. There are multiple explanations of why party labels became more important to voters. As we already covered in the previous section on the emergence of party discipline, the political power of an individual MP decreased significantly in the time between the First and Second Reform Acts. The political power that was lost by the MPs, was then transferred to the party, as parties became more organized, and eventually the well oiled political machines we know. Expansions in the electorate also made it harder, especially in the larger constituencies, for the candidates to promote their individual characteristics. This made candidates more reliant on newspaper

coverage, which were more focused on the party label and platform (Cox, 1987, p. 129), rather than individual aspects of the candidate. Cox promotes the arguments of Vincent (1966), who argued that the removal of the Stamp Tax in 1855 on newspapers, created a new form of newspapers, known as the 'penny press'. (Cox, 1987, p. 120-121) argues that the penny press was less politically independent, since it tried to increase it's sales by promoting partisan news. Close (1969) also argues that the major newspapers displayed partisan alignment, already in the 1830's. Thus, it seems like a reasonable explanation that the press played a major role in emphasizing the importance of the party label in voting, instead of characteristics of an individual candidate.

Another factor that affected the electorates knowledge about the parties and their platforms were local party organizations. (Cox, 1987, p. 119-120) discusses the varying levels of Conservative's party organization between the two Reform Acts. In the 1830's Conservatives had Tory clubs in multiple constituencies, which were one of the earliest forms of Conservative organizing. Many of these dissolved as the Conservative party went through a phase of disarray, as a result of disputes over the Corn laws in late 1840's and early 1850's. By late 1850's and early 1860's, however, the Conservative party was more organized than ever before. (Cox, 1987, p. 120) states that in 1852, Benjamin Disraeli, a politician who would become the leader of the Conservatives and the prime minister first in late 1860's and again in the 1870's, started rebuilding the nation wide information networks necessary for organizing. This development led to the establishment of the national registration association in 1866 and Conservative Central Office in 1870. All this to say, these local organizations served as another information spreading mechanism to the electorate, which emphasized the importance of the party over the candidate.

Theoretical works also suggest that economic development increases the demand for public goods, especially for public health investments (Boix, 2001; Lizzeri and Persico, 2004). Lizzeri and Persico (2004) emphasize that public health investments were popular among all societal classes due to common phenomenon of pandemics, that transcended barriers of class. Public health concerns, according to Lizzeri and Persico, drove even the elites further away from special interest politics, since bribery and patronage could not address the demand for public goods. Economic development and effects of industrialization were also more apparent in the newly enfranchised industrial cities. In these new constituencies, with large electorates, old ways of getting elected, these being bribery and patronage, were not as feasible because these became excessively expensive (Cox, 1987, p. 56). Cox shows that MPs from large constituencies participated more in the legislative procedure, because it was the only feasible way they could increase chances of re-election.

To summarize, no single explanation completely explains the sudden alignment of the electorate with the parties. The most likely explanation for the alignment is the combination of politicized newspapers together with the local party organizations, which emphasized parties and their platforms over individual candidates. Another factor is industrialization and the expansion of the franchise, which reduced the effectiveness of patronage and clientelistic practices. Together all these factors, with the developments in the parliament, provide a coherent story of the development of a party oriented electorate.

4. Poll book data

In this section we discuss the voting records, known as poll books, that we use as the basis of our empirical analysis. The practice of keeping poll books began in the late 17th century and ended in 1872 as a result of the Secret Ballot Act. Previous analyses of voting behavior, rely on the aggregated results reported in the poll books or ballot counts reported in the newspapers. These aggregated results provide useful information about the trends in voting, but are ultimately unsuitable for causal inference, due to a well known statistical fallacy, known as the ecological fallacy. We discuss ecological fallacy at length in Section 5.3.

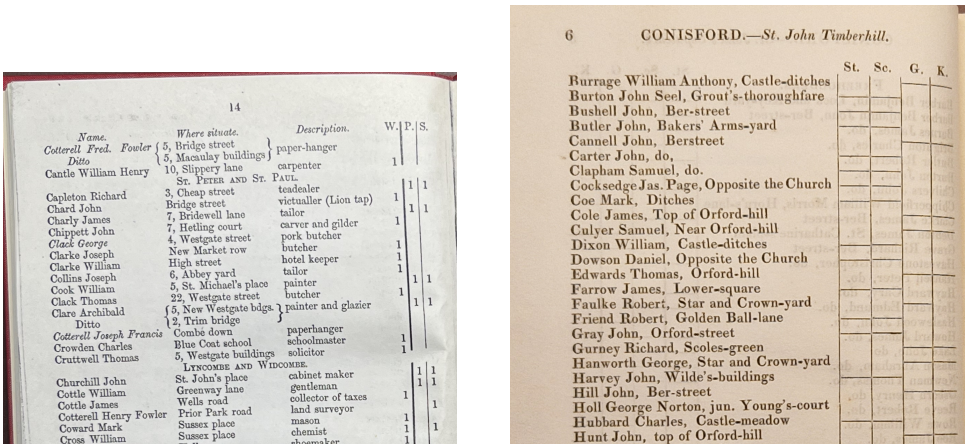
Our poll book analysis builds on previous works, such as Cox (1984a, 1986, 1987), but mainly on Dewan et al. (2020). Dewan et al. was the first study to our knowledge, that used individual-level data from poll books of three constituencies, Ashford, Guildford and Sandwich. We use the same poll book data from these constituencies, but we also have recently digitized data from Aylesbury, Bath, Cambridge, Lancaster, Beverly, Barnstaple, Great Yarmouth, Norwich, Totnes, York, Boston, Canterbury and Derby. In total we have 16 constituencies in our data set. These 13 new constituencies, have not been previously analyzed, so our contribution in this front is novel. However, this is only a subsection of the total amount of digitized poll books we have at hand. These 13 previously unanalyzed constituencies were selected on the basis that they had a long enough time series of four elections, or longer, and at least one election during or after 1865, which were the requirements for our DID analysis.

The production of poll books was never properly standardized, so there is heterogeneity in the informational content, especially between different constituencies. This introduces additional challenge for us, since we need to be able to link an individual voter to a class, based on their occupation. Occupational information is recorded in the three constituencies included in the original article and in Aylesbury, Bath, Cambridge, Lancaster, Beverly and Barnstaple. Great Yarmouth, Norwich, Totnes, York, Boston, Canterbury and Derby, do not have any occupational information, or they have occupational information for only one election year. For these constituencies, we have to match the occupational information from census data from years 1851 and 1861.

Fig. 4.1: Approximate locations of the constituencies. Red dots represent the new poll books with occupation and the purple ones represent the ones without occupation. Blue dots represent the original three constituencies.

Figure 4.1. shows a map with the approximate location of each constituency in our data. The blue dots represent the constituencies analyzed by Dewan et al. (2020), which are located in the South Eastern England. Red dots represent the new constituencies, which have occupational information in the poll books, and blue dots represent those new constituencies, which don't have the occupational information recorded in the poll books. As we see, our new sample is also geographically more representative.

Excerpts from poll books of Bath 1852 and Norwich 1832 are shown in the Figure 4.2. These give a picture of the variation in the information content between poll books. In the poll book of Bath 1852 (left), voters name, street address and occupation are recorded, whereas in Norwich 1832 (right) only the name and general location of residence is recorded.



14

Name.	*Where situate.*	*Description.*	W.	P.	S.
Cotterell Fred. Fowler	5, Bridge street	paper-hanger			
Ditto	5, Macaulay buildings		1		
Cantle William Henry	10, Slippery lane	carpenter			
	St. Peter and St. Paul.				
Capleton Richard	3, Cheap street	teadealer		1	1
Chard John	Bridge street	victualler (Lion tap)	1		
Charly James	7, Bridewell lane	tailor		1	1
Chippett John	7, Hetling court	carver and gilder	1		
Clack George	4, Westgate street	pork butcher			
Clarke Joseph	New Market row	butcher	1		
Clarke William	High street	hotel keeper	1		
Collins Joseph	6, Abbey yard	tailor	1		
Cook William	5, St. Michael's place	painter		1	1
Clack Thomas	22, Westgate street	butcher	1		
Clare Archibald	5, New Westgate bdgs.	painter and glazier		1	1
Ditto	2, Trim bridge				
Cotterell Joseph Francis	Combe down	paperhanger	1		
Crowden Charles	Blue Coat school	schoolmaster	1		
Cruttwell Thomas	5, Westgate buildings	solicitor			
	Lyncombe and Widcombe.				
Churchill John	St. John's place	cabinet maker		1	1
Cottle William	Greenway lane	gentleman		1	1
Cottle James	Wells road	collector of taxes	1		
Cotterell Henry Fowler	Prior Park road	land surveyor			1
Coward Mark	Sussex place	mason	1		
Cross William	Sussex place	chemist	1		1

6 CONISFORD.—*St. John Timberhill.*

	St.	Sc.	G.	K.
Burrage William Anthony, Castle-ditches				
Burton John Seel, Grout's-thoroughfare				
Bushell John, Ber-street				
Butler John, Bakers' Arms-yard				
Cannell John, Berstreet				
Carter John, do,				
Clapham Samuel, do.				
Cocksedge Jas. Page, Opposite the Church				
Coe Mark, Ditches				
Cole James, Top of Orford-hill				
Culyer Samuel, Near Orford-hill				
Dixon William, Castle-ditches				
Dowson Daniel, Opposite the Church				
Edwards Thomas, Orford-hill				
Farrow James, Lower-square				
Faulke Robert, Star and Crown-yard				
Friend Robert, Golden Ball-lane				
Gray John, Orford-street				
Gurney Richard, Scoles-green				
Hanworth George, Star and Crown-yard				
Harvey John, Wilde's-buildings				
Hill John, Ber-street				
Holl George Norton, jun. Young's-court				
Hubbard Charles, Castle-meadow				
Hunt John, top of Orford-hill				

Fig. 4.2: Excerpts from poll books of Bath 1852 and Norwich 1832.

4.1 Ecological Fallacy

Prior to the study by Dewan et al. (2020) and our continuation of their work, only aggregate-level data of the voting records has been broadly available. For example in the Eggers and Spirling database (Eggers, 2025). Inferences made from aggregated data are called ecological inferences, and are thus susceptible to ecological fallacies. With our individual-level data, we need not fear the ecological fallacy, but it is still worthy to spend some time understanding this phenomenon. To our knowledge, our individual-level poll book data is in this respect unique with the aforementioned exception of Dewan et al. (2020).

Ecological fallacy refers to the well studied statistical fallacy that states that inferences about individual-level behavior cannot be made using aggregated data. First discussed in Robinson (1950), the ecological fallacy was found to be present in many of the important studies of the time. Robinson defines ecological correlation as a correlation where the statistical unit is a group of persons, and gives an example of illiteracy rates and percentage of the black population in a state. However, Robinson noticed that the correlation between illiteracy and the share of black population, was highly dependent on the size of the ecological group. The initial correlation that Robinson found, was 0.946, but when the size of the group was only one individual (individual-level data), the correlation was only 0.203. Robinson also looks at illiteracy rates and percentage of foreign born population, expecting to find a positive correlation, but to his surprise, only the individual level correlation was positive (0.118) and the ecological correlation was highly negative (-0.619).

After Robinson's seminal paper, multiple follow-up studies were published. One of the most thorough ones being Firebaugh (1978), where he goes over the general problem of ecological inference, reproduces Robinson's original results, and re-examines them, and finally proposes his own $\bar{X}$-rule, a set of assumptions under which aggregate data does provide unbiased estimates of individual-level effects. Firebaugh's $\bar{X}$-rule states that aggregate data does produce unbiased results of individual-level effects if the means of the independent variables are independent from the dependent variable, net of the effects of independent variables. In other words, the estimates are unbiased if the aggregation process is independent of the dependent variable. A concrete example of this can be found in the estimations of Robinson, further refined by Firebaugh. One source of ecological bias in Robinson's original analysis came from the aggregation of illiteracy and ratio of black population by state. Individual states have a lot of state-specific characteristics that cannot be observed after aggregation, and which affect the dependent and the independent variables, a phenomenon known as confounding in modern econometrics.

One of the most convincing articles related to problems arising from the use of aggregate data for making individual-level inferences, comes from the field of Epidemiology by Piantadosi et al. (1988). The authors present first a mathematical proof of the ecological fallacy, followed by an empirical example confirming that aggregation leads to varying levels of correlation, both in terms of sign and statistical significance.

Next, we will go over the condensed form of the proof provided by Piantadosi et al. The authors define sums of squares and cross-products of different levels of aggregation in the following manner; T, represents the total sample, W, represents the within group samples and finally, E, represents the ecological, or between group samples. The total sum of squares of X is denoted as, T_{XX}, and the cross-product of X and Y is similarly T_{XY}.

The authors define regression coefficients as,

$$\beta = \frac{T_{XY}}{T_{XX}}, \quad \beta_e = \frac{E_{XY}}{E_{XX}}, \quad \beta_w = \frac{W_{XY}}{W_{XX}}.$$

Now we can, based on the definitions, show that,

$$\beta = \frac{E_{XX}}{T_{XX}}\beta_e + \frac{W_{XX}}{T_{XX}}\beta_w,$$

and this can be further shown as,

$$\beta_e = \beta_w + \frac{T_{XX}}{E_{XX}}(\beta - \beta_w).$$

Here we clearly see that $\beta_e \neq \beta_w$, which is the correct coefficient, if group effects are present in the data. It can also be shown that,

$$\beta_e = \beta + \frac{W_{XX}}{E_{XX}}(\beta - \beta_w),$$

from which we see that $\beta_e \neq \beta$, where β is the coefficient of the individual-level data. Thus, it is clear, that we can assume $\beta = \beta_w = \beta_e$ only in some special cases, outside of which, β_e, is a biased estimate of the individual-level effects.

4.1.1 Ecological fallacy in voting studies

Most of the modern voting studies have to grabble with the problems of ecological inference. Since secret voting is nowadays almost universally adopted, researchers have to make ecological inferences to answer the most relevant question in almost all voting studies; who voted for who?

Fallacious ecological inference in voting studies usually occurs, when researches look at the correlation of some demographic variable (e.g. share of working class voters) and election results. We can imagine a hypothetical situation in which some particular election has been won by a left-wing party. Additionally, let's say that we observe a positive correlation between the working class share of the population in a voting district and the amount of votes gained by the left-wing party in the most recent election. The natural inclination is to infer that the increased vote share in these districts came from the working class voters, since left-wing parties mostly attempt to appeal to working class voters. This is the most standard form of ecological fallacy in voting studies. In reality, since we cannot link a resident of the voting district to their cast vote, we cannot say where the gained vote share has come from. It could also be the case that, the gained vote share came from middle class voters, since most of the working class voters have always voted for the left-wing party and the gained vote share is so big that it has to include mostly middle class voters who have changed their vote.

This kind of analysis of the changing vote shares has been done, but without the fallacious inference, by King et al. (2008). King et al. study the elections in Weimar Republic which led to the accession of the Nazi party. Since Weimar Republic used a secret voting, the authors have to make ecological inferences based on the aggregate election results. King et al. observed a national trend in the votes for the Nazi party in multiple elections. After this they used census data to estimate upper and lower bounds for who changed their vote between the election.

To tie this all together with the classic examples of ecological fallacies, such as Robinson (1950), let's express the example of illiteracy and foreign born population in the similar fashion as we did with our voting example. Robinson observed that the ecological fallacy is dependent in the size of the statistical unit. This really just means that since we used individual U.S. states as the smallest statistical unit, we cannot observe any variation within these units, which might explain these results. Robinson's results can be entirely explained by the fact that immigrants moved to the most developed states where illiteracy rates are already low. If instead of states, we could observe individual counties or smaller administrative units, the magnitude of the ecological fallacy would go down. In similar fashion, as we get more granular voting data, the most granular form being individual-level, the better we can control for the variation within the larger units, such as voting districts or constituencies.

4.1.2 Ecological fallacy in our analysis

For our poll book data, ecological fallacy may rise when constituency-level aggregate data is used for making inferences about individual voters' behavior. Previous analyses of the poll book data and voting behavior, such as Cox (1986, 1987), have used aggregated data points from constituencies to construct their arguments about how voting behavior developed to a party oriented system, from a candidate centric one. These works have provided very valuable information about the more macro-level changes in voting behavior, but are ultimately too coarse to make any inferences about micro-level voting behavior, let alone causal claims.

Ecological fallacy is not a concern for us, if we can prove that there are no unobserved differences between the characteristics of individual constituencies. In our DID estimations we control for constituency fixed-effects, which should not affect our estimated results, if constituencies don't have any unobserved characteristics. In Section 6, we go over all our results for individual constituencies, as well as, the pooled data, which combines all constituencies. Tables 7, 8 and 9 show the pooled results for old constituencies, all constituencies and new constituencies, respectively. In all the Tables specifications (3) and (7) show the estimates, where year and constituency fixed-effects are accounted for. We clearly see that the estimates of our main variable, $1[Workingclass]_{it} \times 1[Year \geq 1865]_t$, for split voting rates, are sensitive to constituency fixed-effects. This suggests that there are some unobserved characteristics between constituencies, that lead to upwardly biased results, if not accounted for. However, the estimates of the same variable for liberal vote shares only slightly change when we control for constituency fixed-effects.

To summarize, based on our pooled results, there seems to be some unobserved characteristics between the constituencies that lead to an upwardly bias in our estimations, if left unaccounted for. This seems to be especially true for the results of split voting rates. Thus, making inferences about split voting rates by only using aggregated data

from constituencies, would lead to upwardly biased estimates.

4.2 New Poll Books with Occupational Information

In our new data set, we have six new constituencies that have occupational information recorded in the poll books. Three of these constituencies have perfect occupational information, meaning, occupational information is recorded for majority of the observations each election year. These constituencies are Barnstaple, Cambridge and Lancaster. The other three constituencies have occupational information for most of the observations, but have some election years, where no occupational information was recorded in the poll books. These constituencies are Aylesbury, Bath and Beverley.

For the constituencies that have missing occupational information for some years, we have matched the occupation from previous or later elections. However, this matching was not done the same way as for the constituencies that had no observations with occupational information for any election years. For constituencies with occupational information recorded in some election years, but not all, we have identified voters from previous or following elections. For example, if we have occupational information for the 1859 and 1868 election, but not for the 1865 election, we attempt to identify the common voters from all these elections and inherit the occupational information for voters in 1865 elections.

For example, in Aylesbury we have missing occupational information for the years 1857, 1859 and 1868. Here the nearest election with occupational information is 1852. We combined these four elections into same data set and then created the *voterid* variable based on the name of the voter. Then we created four dummy variables, for each election year, that indicate if the *voterid* is observed in that specific election. If a specific *voterid* is observed only in elections after 1852, it is dropped from the data. Only those *voterid* are kept that are observed in 1852 and some of the subsequent elections.

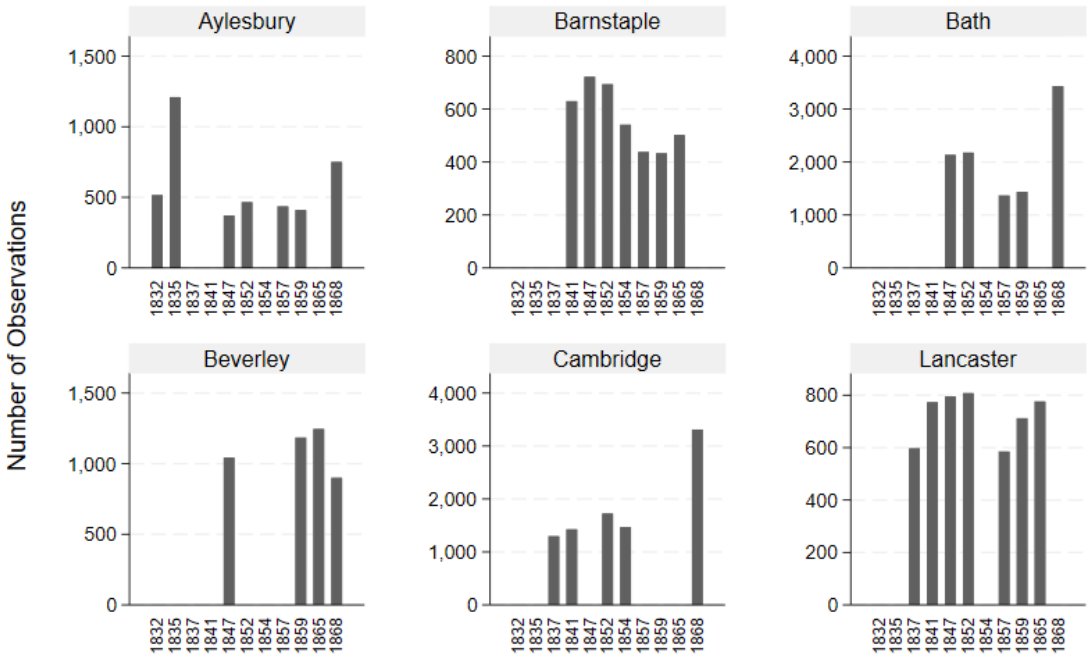


Fig. 4.3: Number of observations by election year and number of elections observed per constituency.

After having only *voterid*'s that are observed in multiple elections and one of them being 1852, we check if there are any *voterid* that don't have occupational information for any of the observed years. If occupation is missing for all years, these observations are dropped from the data. Then we form a new variable that collects the most common occupation per each *voterid*. The most common occupation is then inherited to all the observations of that *voterid*, if the occupation is missing. This means that, no observations from 1852 that already have an occupation, is altered.

This method of matching is not perfect, but is much less distortive than the census matching, that we use for constituencies that have no occupational records. This means that our data remains high quality, even after this matching procedure, and the results can be trusted. This can be seen in the Figure 4.4, where we have plotted the vote distributions for Aylesbury, Bath and Beverley. We see that no meaningful change in the shapes of the distributions has happened due to matching. Only in Aylesbury we see a slight decrease in the share of conservative votes (from 28.6% to 21.3%) and a minor increase in the share of liberal votes (from 41.1% to 44.5%), but this change won't affect the analysis when all data from the constituencies is pooled together.

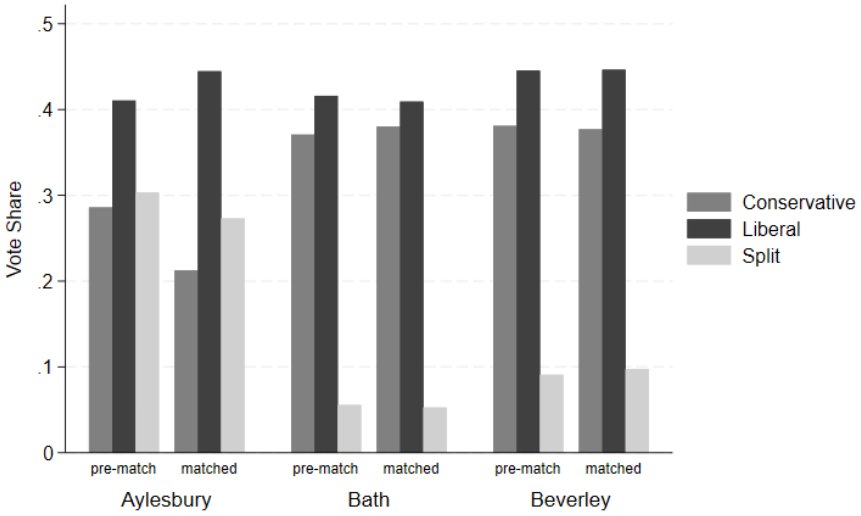


Fig. 4.4: Distribution of vote shares pre and post matching.

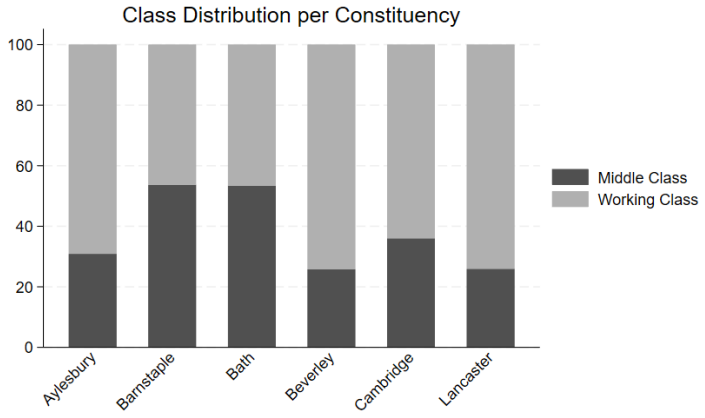


Fig. 4.5: Distribution of voters' class by constituency.

We observe rather heterogeneous levels of total observations, working and middle class voters, and liberal/conservative/split votes. Figure 4.3 shows how many elections are observed per each constituency and how many observations there are per election year. Here we see a rather large variance between constituencies, e.g. Barnstaple and Lancaster both only have less than 800 observations for single elections, whereas Bath and Cambridge have over 3000 observations for the 1868 election. 1868 Election is the first election after the Second Reform Act, which is why we typically have more observations for this election, than the previous ones. We also observe different number of elections for each constituency. For Aylesbury, Barnstaple and Lancaster, we have data for 7 elections, whereas Bath, Beverley and Cambridge only have 5 or four elections. These missing elections result from not having access to all the original poll books. However, we have at least four observed election year for all the included constituencies and at least one of these has to be the 1865 or 1868 election.

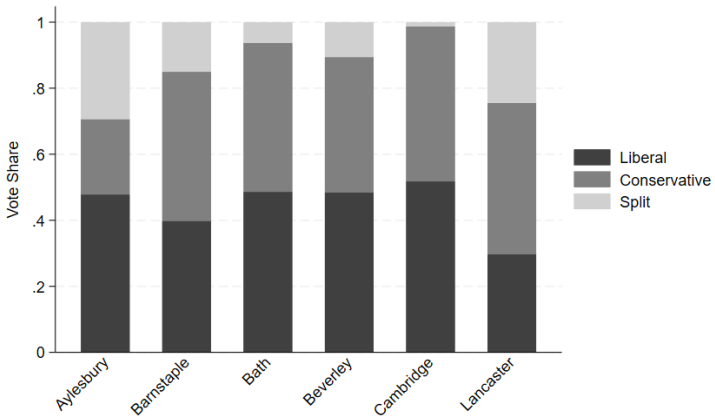


Fig. 4.6: Distribution of voter composition by constituency.

Figure 4.5 shows the class distribution for each constituency. Here we see that working class voters are the majority in most constituencies. Exceptions to this are Barnstaple and Bath where the share of middle class voters is slightly above 50%. In the pooled data set of the six new constituencies, the class distribution ends up being 39.66% middle and 60.34% working class.

The distribution of votes is depicted in Figure 4.6. Here we see meaningful heterogeneity in the levels of split voting, which is one of the main dependent variables in our analysis, especially in Cambridge, where we observe a very low rate (1.3%) of split voting. In the pooled data set, split voting rate is 10.68%.

4.3 Matching Poll Books to Census

In our data set we have 7 constituencies, which have not recorded the occupational information in the poll books. These constituencies are Boston, Canterbury, Derby, Great Yarmouth, Norwich, Totnes and York. To account for the lack of occupation data, we have matched the names found in the poll books to census data from years 1851 and 1861. Census records have information about persons age, sex, marital status, living conditions and occupation among many other factors. This data allows us to match the voters recorded in poll books, who have no occupational information, to persons recorded in the census. However, this is no trivial task.

Our method of matching voters from poll books to persons in the census records, is done by comparing the names between data sets. First, we narrow the census data to the specific county in which our constituency is located. Then we do additional narrowing based on the districts that are included in the poll books and the census. Since the voting areas do not perfectly map on to the administrative districts, it is possible that this introduces some persons to the data who lived outside the constituency and would not have been able to vote. After narrowing the districts as close to the constituencies as possible, we drop all the women, persons under 21 and persons, who have no occupational information in the census, from the data.

Matching the names from poll books and census records is done in Stata using a fuzzy matching function called *matchit*. Matchit compares the similarity of two strings using a bigram-algorithm, which is a widely used method for comparing the similarity of two

strings. Matchit compares each name variable combination and determines a similarity score for every pair. In most cases we find a good subsample, of the original poll book, with a perfect match, which corresponds to a similarity score 1. In most cases we use similarity score of 0.95 as a cut-off point for dropping the bad matches. This cut-off was chosen by observing the matches, and based on our consideration, the matches with a similarity score in the low .90's and below, start to include some potential bad matches. For example in the Boston constituency, the names *Isaac Millson* and *Isaac Willson* get a similarity score of .88. These two names do not obviously, in our opinion, refer to the same person. Similarity score of .95 seems to allow most non critical one character differences in the names, such as typos (*William* vs. *Wiliam*) and possible abbreviated middle names (*Thomas Brown* vs. *Thomas J. Brown*).

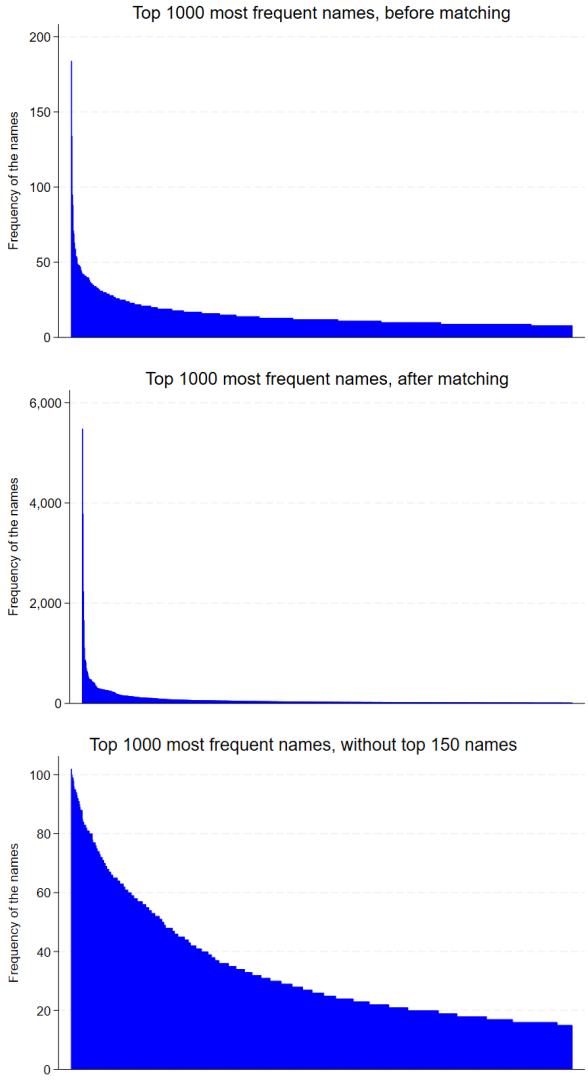


Fig. 4.7: Distribution of the 1000 most common names before and after matching. Also shown is the post-matching distribution, where the top 150 most common names are removed.

In our pooled data of the seven new constituencies, before matching we have 32 695 unique names and 75 425 observations in total. After matching, we have only 12 775 unique names and 118 370 total observations. This means that even though, our number of unique names is almost one third of the original after matching, we have over 50% more total observations. These additional observations are a byproduct of matching procedure. To account for the distortions caused by matching, we have also tried to remove the top 150 most common names after matching. If we do this, our number of unique names drops to 12 625 and our total observations drop to a mere 69 715. This means that the top 150 most common names account for 48 655 observations, which is 41.1% of the sample after matching.

Figure 4.7. shows the distortions caused by matching. Before matching, we observe the most common names about 180 times. After matching, the same names are observed almost 5500 times.

4.3.1 Problems with the matching

Matching persons from poll books to persons in census, solely on the basis of name, is not a perfect method by any means. Ideally we would like to also use the address information to our advantage, but unfortunately this information is not accurate or standardized enough to help us. Poll books and the census data have a wide range of address information, which can be hard to combine. For example, a poll book might report just the part of the city that the voter resides in, whereas the census might report the street name. In most cases the poll books and the census include both street names and parts of the city. This makes it impossible to, for example, combine the *name* variable and *address* variable to a new *name-address* variable, that could be matched with Matchit. Thus, to use the address information to our advantage, would require a more sophisticated algorithm, which we do not currently have. It could be possible in the future, to geocode the address information, in the poll books and the census, in some standardized way, which would allow us to help with the matching, but currently this is beyond the scope of this study.

Another meaningful obstacle with the matching is the fact that we have a lot of observations with the same name. As stated previously, Matchit compares all the name combinations in the data and then determines the similarity score, which we use to drop bad matches. Comparing all the combinations is necessary, but also problematic for the following reason. Let's say, we have three persons in the poll book that have the name *George Brown* and similarly three persons with the same name in the census. After the matching, we have all the possible combinations of these three *George Brown*'s in our data, which means $3 \times 3 = 9$ observations. Thus, by matching, we have created 6 additional *George Brown*'s to our data. This is problematic for obvious reasons. One way to alleviate this problem, is to exclude the most common names from our data. The most severe distortion is caused by these so-called *John Smith*-cases, by which we mean, names that have, over or close to, ten observations in each data set. These are by far, the most distortive data points in our sample. Other names, that are not as common, but still might be observed multiple times, we have no silver bullet for.

Our current hope is that, most of the names observed in our sample are observed 2-5 times. This would lead to most of the sample to be distorted, but in a similar proportion. This is something we can test by plotting descriptive statistics and graphs of the data.

4.3.2 Placebo matching

The *John Smith* -problem introduces a credibility problem for our results that are derived from the matched poll book data. Matching introduces additional observations, which can be problematic if the underlying distribution of liberal/conservative/split votes and middle/working class observations changes. Since matching introduces additional observations based on how common the name is in the data, the underlying properties of the voting and class distributions might remain the same, if the matching process behaves like a continuous transformation for the distributions.

For the constituencies with no occupational information, we can compare the voting distributions before and after matching, but not the class distributions, since we only have the occupational information, from which we derive the class information, after the matching. However, to test how matching affects the class distribution, we can use the constituencies which have the occupational information recorded in the poll books, and compare the class distributions before and after. We do this "placebo matching" for three constituencies, Barnstaple, Cambridge and Lancaster. These constituencies were chosen on the basis that they have occupational information recorded for each election year. We apply the same matching procedure for these three constituencies that we have used for all the constituencies which don't have occupational information.

Constituency	Vote	Mean (Matched)	Mean (Unmatched)	t-stat	p-value
Barnstaple	Liberal	0.389	0.397	-0.702	0.483
Barnstaple	Conservative	0.463	0.450	1.061	0.289
Barnstaple	Split	0.145	0.150	-0.611	0.541
Cambridge	Liberal	0.497	0.505	-1.005	0.315
Cambridge	Conservative	0.491	0.458	4.335	0.000
Cambridge	Split	0.012	0.013	-0.676	0.499
Lancaster	Liberal	0.284	0.278	0.780	0.435
Lancaster	Conservative	0.388	0.429	-4.529	0.000
Lancaster	Split	0.265	0.229	4.572	0.000
Constituency	Class	Mean (Matched)	Mean (Unmatched)	t-stat	p-value
Barnstaple	Middle class	0.378	0.537	-13.806	0.000
Barnstaple	Working class	0.622	0.463	13.806	0.000
Cambridge	Middle class	0.299	0.360	-9.314	0.000
Cambridge	Working class	0.701	0.640	9.314	0.000
Lancaster	Middle class	0.191	0.259	-10.058	0.000
Lancaster	Working class	0.809	0.741	10.058	0.000

Tab. 4.1: Results for the t-test comparison of the means for vote shares and class by constituency.

If the distributions of votes and class truly remain similar after matching, we should see the same levels of voting and class. We compute the means of matched and unmatched vote and class shares. After this, we perform a simple t-test, with the assumption that both, matched and unmatched means, are from the same distribution. Thus, any statistically significant results can be interpreted to break this assumption and lead ud to believe that the means are not computed from the same distribution.

The results of the t-test comparison are found in Table 4.1. We see that some of the vote distributions, mainly for Barnstaple, could have been from the same distribution, but overall the differences between the means are statistically significant. For the class means, this result is even stronger, as we see that the differences between matched and unmatched class shares are highly statistically significant. Table 4.2 gives the t-test results for the dataset where the top 150 most common names from the matched data are removed. This is done to remove the *John Smith*-names, which are over represented in the data after matching. Unfortunately, we still see that most of the distributions are statistically different from the original distributions.

Table 4.3 shows the results for the frequency weighted t-test of the means. Here we have done a weighted t-test using Stata's *pweight* command using a inverse frequency variable, which sums up to 1 for each unique name. For example, if we have 10 John Smith's in the data, each will get a weight of $\frac{1}{10}$. We see that by doing this, the two vote share distributions are no longer statistically significantly different. Only exception to this is Lancaster, where we do observe significant differences in the means. However, this method still doesn't fix the problems in the class distributions, where the means are still statistically significantly different. This is problematic, because the aim of the matching procedure is to reproduce the original class distribution.

To us, these results suggest that the matching process isn't a mere continuous transformation for the data, and that the additional data points created by the *John Smith*-problem, do distort the data in a meaningful way. It also seems that removing the most common names, which reduces the *John Smith* -observations, yields similar results. This means that matching significantly alters the distribution of the data, which makes the matched data ultimately inappropriate for any causal inference of the underlying voting behavior.

Constituency	Vote	Mean (Matched)	Mean (Unmatched)	t-stat	p-value
Barnstaple	Liberal	0.389	0.397	-0.702	0.483
Barnstaple	Conservative	0.463	0.450	1.061	0.289
Barnstaple	Split	0.145	0.150	-0.611	0.541
Cambridge	Liberal	0.497	0.505	-1.005	0.315
Cambridge	Conservative	0.491	0.458	4.335	0.000
Cambridge	Split	0.012	0.013	-0.676	0.499
Lancaster	Liberal	0.284	0.278	0.780	0.435
Lancaster	Conservative	0.388	0.429	-4.529	0.000
Lancaster	Split	0.265	0.229	4.572	0.000
Constituency	Class	Mean (Matched)	Mean (Unmatched)	t-stat	p-value
Barnstaple	Middle class	0.399	0.537	-11.237	0.000
Barnstaple	Working class	0.601	0.463	11.237	0.000
Cambridge	Middle class	0.314	0.360	-6.346	0.000
Cambridge	Working class	0.686	0.640	6.346	0.000
Lancaster	Middle class	0.235	0.259	-3.066	0.002
Lancaster	Working class	0.765	0.741	3.066	0.002

Tab. 4.2: Results for the t-test comparison of the vote shares and class by constituency, but 150 most common names removed.

Constituency	Vote	Mean (Matched)	Mean (Unmatched)	t-stat	p-value
Barnstaple	Liberal	0.409	0.403	0.382	0.702
Barnstaple	Conservative	0.434	0.441	-0.453	0.651
Barnstaple	Split	0.152	0.153	-0.077	0.939
Cambridge	Liberal	0.518	0.510	0.828	0.408
Cambridge	Conservative	0.468	0.452	1.642	0.101
Cambridge	Split	0.013	0.013	0.064	0.949
Lancaster	Liberal	0.320	0.295	2.120	0.034
Lancaster	Conservative	0.362	0.420	-4.930	0.000
Lancaster	Split	0.318	0.285	3.275	0.001
Constituency	Class	Mean (Matched)	Mean (Unmatched)	t-stat	p-value
Barnstaple	Middle class	0.444	0.581	-8.575	0.000
Barnstaple	Working class	0.556	0.419	8.575	0.000
Cambridge	Middle class	0.312	0.352	-4.178	0.000
Cambridge	Working class	0.688	0.648	4.178	0.000
Lancaster	Middle class	0.275	0.256	1.768	0.077
Lancaster	Working class	0.725	0.744	-1.768	0.077

Tab. 4.3: Results for the frequency weighted t-test of vote shares and class by constituency.

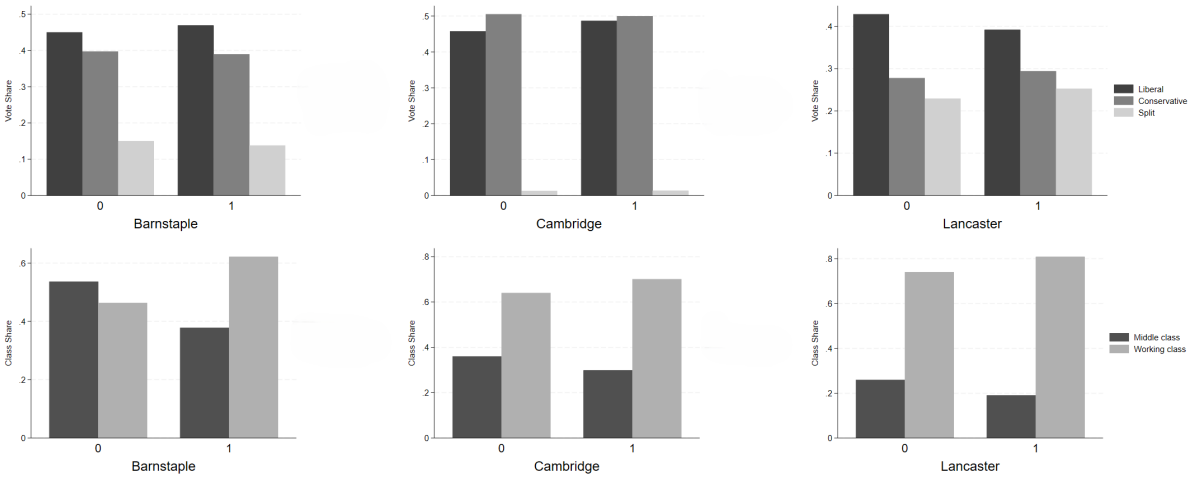


Fig. 4.8: Distribution of votes and classes before (0) and after (1) matching.

4.4 New Poll Books without Occupational Information

In this section we go over the poll books which don't have the occupational information recorded in them. Since we have already given an extensive explanation of the matching

procedure and all the problems related to it, we will focus on the descriptive statistics of the seven constituencies.

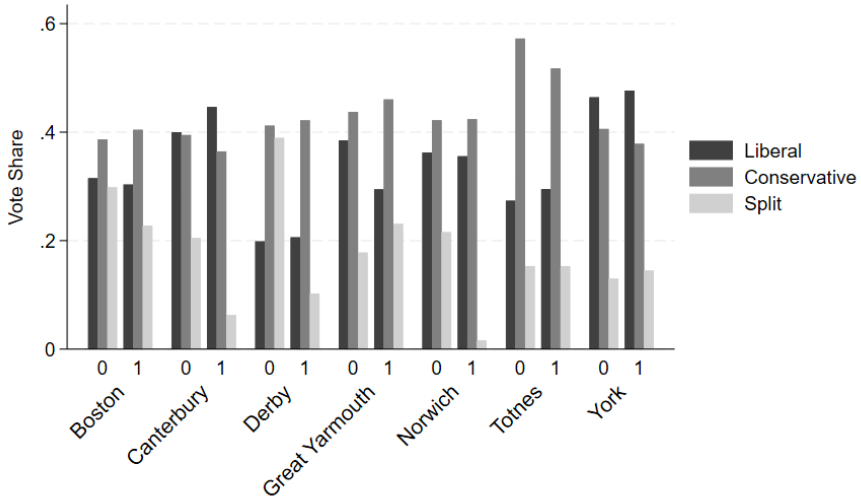


Fig. 4.9: Distribution of votes before (0) and after (1) matching.

In Figure 4.9., we have plotted the vote shares of Liberal, Conservatives and split votes, before and after matching. As we see, in most constituencies, especially Canterbury, Derby and Norwich, the distribution of votes changes by a significant amount. We also observe meaningful level differences in Liberal and Conservative vote shares across the constituencies.

Figure 4.10., shows the distribution of working and middle class voters after matching. The distribution of classes before matching is unknown to us, since we don't have the occupation information recorded in the poll books. Here we see a pretty uniform level of working class voters across the constituencies, which is about 25% to 30%. Only exception to this is Totnes, where approximately 50% of the observations are working class.

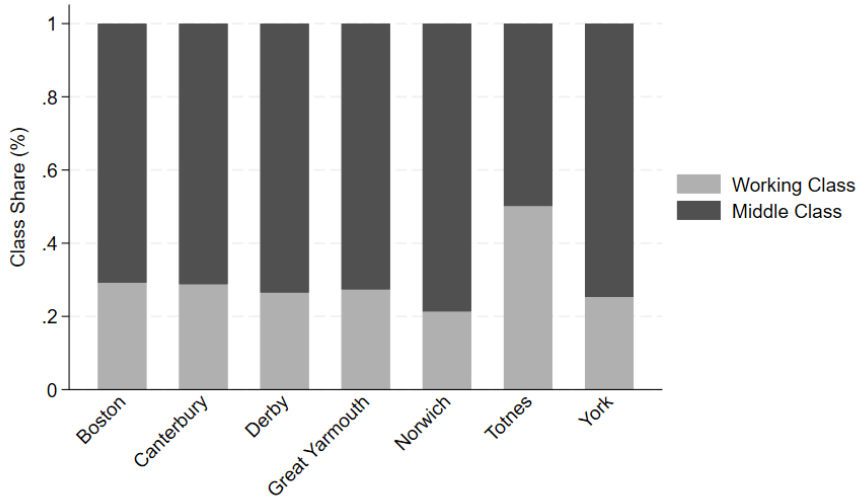


Fig. 4.10: Distribution of classes after matching.

4.5 Voting in double-member districts

The main research question of this study revolves around the question of split voting rates. However, the phenomenon of split voting is unintuitive in many ways since electors in modern elections are used to casting only one vote and have relatively strong party preferences. The addition of a second vote introduces additional complexity to electors decision making, which we attempt to clarify in the following section. The theoretical framework for voting in a double-member district, which we go over, is based on the works of Cox but especially Cox (1984b).

The theoretical framework for voting behavior in a multi-member districts, can get quite complex. For this reason, and also for the relevancy to this thesis, we limit ourselves to the situation of double-member district with three candidates running. Our three candidates shall be named 1, 2 and 3, and our elector has the following preference relation for the possible outcomes: $(1,2) \succ (1,3) \succ (2,3)$. Now, clearly our voter prefers candidate 1 above the other two, so they are faced with a problem, should they cast ballot $\{1\}$ or $\{1,2\}$? The decision of the voter is dependent on their belief of other voters behavior, or the expected result of the election. If candidate 1 has a clear lead in the voter's opinion, it is the best strategy to cast $\{1,2\}$, because it is most likely to lead to the best outcome of $(1,2)$. However, it could also be the case that candidate number 3 is the clear front runner and candidates 1 and 2 are close to each other. In this situation it is clearly the best strategy to plump and cast only $\{1\}$, since using the second vote for candidate 2 could end up putting 2 ahead of 1. Thus, our electors behavior is tied to their beliefs about the state of the world.

We can further formalize these beliefs by introducing additional notation. Let T_j be the number of votes received by candidate j, without our elector's vote. Because candidate 1 was our most preferred, we will define $M = min(T_2, T_3)$, to help us talk about different situations, such as $M > T_1 + 1$, which we will call states of the world, which we denote with S_i. For example, the situation $M > T_1 + 1$ means that the election result will be $(2,3)$, since candidates 2 and 3 both are more than one vote ahead of candidate

1, meaning that wether our voter votes {1} or {1,2}, it won't change the end result. Cox (1984b) defines 12 possible states of the world, for which probabilities $(p_{1,2}, p_{1,3}, p_{2,3})$, representing each election result, can be calculated based on wether our voter casts {1} or {1,2}. For example, our previous state of world $M > T_1 + 1$, has the same probability $(0,0,1)$, regardless of our voter's actions. In the state of world S_2, in Cox (1984b), our situation is $M = T_1 + 1$ and additionally specified $T_2 = T_3$, which is equivalent to $T_3 = T_2 = T_1 + 1$. Here, depending on wether voter casts {1} or {1,2}, our probabilities for election outcomes are either $(0.33, 0.33, 0.33)$ or $(0.5, 0, 0.5)$, respectively. We can also assign a payoff for each outcome based on the voter's preferences. Cox defines, $u(1,2) = 1$, $u(1,3) = b$ and $u(2,3) = 0$, where $1 > b > 0$. Now based on the probabilities and the payoffs, voter can maximize their expected utility.

Now that we have a basic understanding of the voting behavior in two-vote elections, we can start to theoretically examine the plausible explanations for the decrease in split voting. As we briefly mentioned in Section 2.3., non-partisan voting is expected to decline if intra-party differences become smaller. To demonstrate this, let's assume a three candidate race between two liberals, L_1, L_2, and a conservative, C. Our voter has the following preference relation, $(L_1, C) \succ (L_1, L_2) \succ (C, L_2)$. We see that our voter prefers candidate L_1 over the others and then C over L_2. Empirically we observe that between 1857 and 1865, our voter started to prefer L_2 over C. This shift in voting behavior can partially be explained by the decreased political power of an individual MP, which led to the emergence of party discipline.

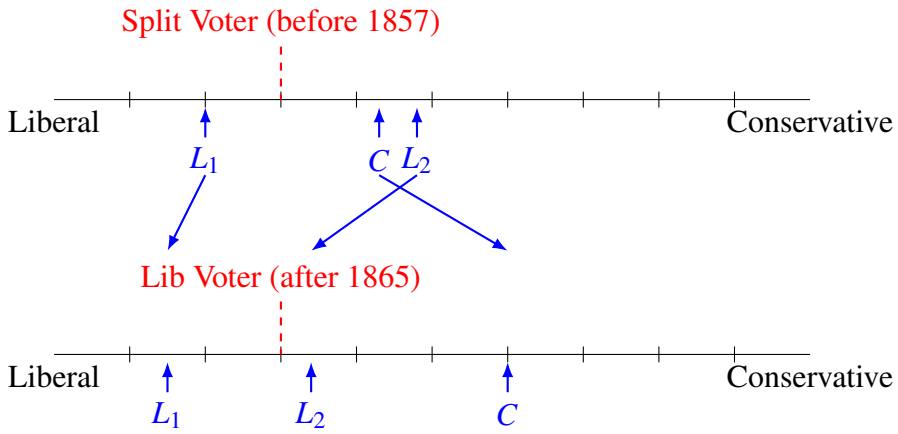


Fig. 4.11: The shift in candidate's positions from 1857 to 1865, which led to the voter switch their vote from $\{L_1, C\}$ to $\{L_1, L_2\}$.

In other words, party discipline brought the candidates closer to their parties position, which meant that policy positions of L_1 and L_2 came closer together. As the difference between L_1 and L_2 became smaller than L_1 and C, our voter switched their vote. This transition is depicted in Figure 4.7., in which shows a one dimensional line depicting some policy issue, where Conservatives and Liberals are at the opposite ends of the line. Our voter is positioned closer to the Liberal end of the line, together with L_1 and C. Voter

votes for the two candidates closest to their own position. As the party discipline starts to form, the Liberal candidates move closer to the Liberal position and the Conservative candidate moves closer to the Conservative position. After this shift, our voter is closer to L_2 than C.

4.5.1 Public voting and alternative explanations for voting behavior

In the previous section, we analyzed the voter's behavior under the assumption of no electoral coercion. However, this assumption is not very realistic, since vote buying was a common phenomenon in 19th century English elections, as is discussed in Kam (2017) and Camp et al. (2014). Due to the prevalence of vote buying and other forms of electoral influence, the decreasing level of split voting that we observe, cannot be entirely attributed to the newly formed party allegiance.

The mechanisms of vote buying, and other forms of electoral influence are discussed in Cox (1987) and Camp et al. (2014). (Cox, 1987, p. 114-117) identifies the classical examples of landlords influencing their tenant's votes and customers influencing business owner's voters. Camp et al. (2014) discuss more explicit forms of influence, mainly vote buying. Cox (1987, 1984b) discuss how vote buying could be related to levels of split voting through "partial" influence, which refers to a situation where the elector only sold one of their votes, but not the other. Thus, one explanation for the decreasing levels of split voting could be a decrease in the levels of vote buying. Camp et al. (2014) argue that even before the introduction of the secret ballot in 1872, the effect of electoral bribery had decreased as median income had increased and the electorate had expanded. The increases in median income and the expansion of the electorate, made vote buying increasingly expensive and thus an unreliable strategy to get elected.

It seems that the decline in electoral influence and vote buying occurred at least partially at the same time. This rises the question, how much of the observed change in electoral behavior can be explained as a result of decline in forms of electoral influence? To our knowledge, (Cox, 1987, p. 116) was the first who attempted to answer this question. Cox largely discounts decline in electoral influence as an explanation for the observed changes in voting behavior. Cox analyzed broad measures of electoral corruption and national trends in voting, and found an insignificant and negative correlation between the two factors. In his own work, Cox has also observed that individual MP's behavior in the House of Commons, affected the amount of split votes they received in the next election. (Cox, 1987, p. 116) gives an example of Liberal MP who supported some of the Conservative measures, who got more split votes in the next election.

Dewan et al. (2020) were the first to give more rigorous empirical proof for the claim that decline in split voting could not be entirely explained by the decline in vote buying. Dewan et al. examine the subsection of voters who were most likely to be affected by vote buying, which were mostly low skilled working class voters. The findings suggest that both the volatile and consistent working class voters, aligned with the Liberals. Dewan et al. conclude that this alignment cannot be purely explained by the decline in vote buying.

To summarize, vote buying and other forms of electoral coercion were a relevant factor in the 19th century English elections, but the importance of these factors lessened as the electorate increased and median income rose. Although, this development occurred in the same time period as our observed changes in voting behavior, changes in voting behavior cannot be solely attributed to the reduction in overall electoral influence.

5. Empirical analysis

In this section we discuss our empirical strategy for the analysis of the poll book data. First we go over the differences-in-differences method and then move on to our specific model and empirical strategy for the poll book data. We also discuss ecological fallacy and show that it is present for the aggregate level poll book data.

Previous empirical analysis of the poll books has mostly been done before the emergence of modern econometric methods. Early analyses of the poll book data were done by Cox (1984a, 1986, 1987). Despite the seminal nature of Cox's work, the aggregate level data used by Cox is ultimately insufficient for making causal inferences. Inferences made using aggregate level data are susceptible to ecological fallacy, which we covered in Section 4.1. To our knowledge, only study that has analyzed changes in voting behavior using individual-level poll book data is Dewan et al. (2020). Our goal is to follow their methodology and replicate and expand their findings using a larger sample of the poll book data.

5.1 Difference-in-Differences method

For estimating the causal effect on split voting rates and liberal vote shares, we use an empirical method called differences-in-differences (DID). In DID estimation, our data is split into two groups, the control and treatment group, which both are observed before and after the treatment. The key identifying assumption in DID is the "parallel trends" assumption, that states, absent the treatment, both groups would have evolved parallel to each other. Formally the parallel trends assumption is defined as,

$$\mathbb{E}[Y_{i,2}(0) - Y_{i,1}(0)|D_1 = 1] = \mathbb{E}[Y_{i,2}(0) - Y_{i,1}(0)|D_1 = 0].$$

In other words, the difference between the potential outcomes ($Y_{i,t}(0)$) stays the same, despite the treatment status ($D_i = 1$ treated, $D_i = 0$ untreated). Another key identifying assumption is the presence of no anticipatory effects. This essentially means, that none of the treated units are allowed to adapt to the treatment before it happens. Presence of anticipation muddies the true effects of the treatment, since the treatment has already affected observations before the treatment. Formally the "no anticipatory effects" is defined as,

$$Y_{i,1}(0) = Y_{i,1}(1), \forall i \in \{D_i = 1\}.$$

This means that the observed effect must remain the same same for all i that are treated, before the treatment happens.

First known, and also one of the most famous, applications of causal inference, that follows the same logic as modern DID estimations, is found in the book by Snow (1855).

Snow studies the 1854 outbreak of cholera in London in an attempt to figure out how cholera spreads and infects people. Snow compares the infection rates between two major water distribution companies'(Southwark & Vauxhall Company and the Lambeth Company) customers before and after the Lambeth Company changed it's water source to a cleaner one. Even though Snow's analysis is rather elementary by today's standards for a DID-estimation, it follows the same underlying logic of establishing a parallel trend and then comparing the *differences in the differences* after some treatment.

The first modern application of the DID-method comes from Card and Krueger (1994). Card and Krueger study the effect of increase in the minimum wage of New Jersey on employment. Card and Krueger compare employment in similar fast food restaurants, near the state border between New Jersey and Pennsylvania, where no minimum wage increase occurred, before and after. Here, fast food restaurants in New Jersey are the treatment group ($Y_{i,t}(1)$), and fast food restaurants in Pennsylvania are the control group ($Y_{i,t}(0)$). In both states, restaurants are observed before ($t = 1$) and after ($t = 2$) the minimum wage increase in New Jersey. The main effect of interest, the average treatment effect on the treated (ATT), is defined as,

$$ATT = \mathbb{E}[Y_{i,2}(1) - Y_{i,2}(0)|D_i = 1].$$

Now the ATT can be interpreted as the causal effect of minimum wage increase on the employment of the treated fast food restaurants. ATT can be estimated from data using the following estimator,

$$\widehat{ATT} = D(\overline{Y}_{1,2} - \overline{Y}_{1,1}) - (1-D)(\overline{Y}_{0,2} - \overline{Y}_{0,1})$$

$$= D_i\left(\frac{1}{N_{D=1,2}}\sum^{N_{D=1,2}} y_{i,2} - \frac{1}{N_{D=1,1}}\sum^{N_{D=1,1}} y_{i,1}\right) - (1-D_i)\left(\frac{1}{N_{D=0,2}}\sum^{N_{D=0,2}} y_{i,2} - \frac{1}{N_{D=0,1}}\sum^{N_{D=0,1}} y_{i,1}\right).$$

Here $\overline{Y}_{1,t}$ and $\overline{Y}_{0,t}$, represent the group averages for the treated and untreated observations at time t. $N_{D=1,1}$, $N_{D=1,2}$, $N_{D=0,1}$ and $N_{D=0,2}$ represent the sample sizes of pre and post treatment and pre and post control groups, respectively.

In the recent years, DID has received a lot of focus in the econometrics literature. Multiple studies have been published focusing on special cases and new variations of DID estimation (Roth et al., 2023) Articles such as, Sun and Abraham (2021) and Callaway et al. (2024), focus on the special case of heterogeneous and continuous treatments, whereas Goodman-Bacon (2021), de Chaisemartin and D'Haultfoeuille (2023) and Callaway and Sant'Anna (2021) focus on variations in treatment timing and multiple time periods. Roth (2022) and Rambachan and Roth (2023) examine limitations in pre-trends testing and solutions for situations where parallel trends assumption may not hold, respectively. Roth et al. (2023) is the most comprehensive review of recent developments in DID estimations.

For our purposes, the relevant developments in DID literature are related to the problems in two-way fixed effects (TWFE) models, when treatment timing is varied and heterogeneous. Goodman-Bacon (2021) and de Chaisemartin and D'Haultfoeuille (2023) discuss in depth how TWFE estimations provide biased results if the treatment timing is

varied. Since we estimate a TWFE model, where we control for year and constituency fixed effects, it is necessary to argue, why we don't need to account for these problems with TWFE.

As we discussed in Section 3.2., our observed alignment of the electorate with the parties was first observed in the aggregate data of all elections from 1857 to 1865. The reason for this alignment cannot thus be any of the major franchise expansions, but rather a combined effect of multiple small but meaningful developments. We are aware that these developments might have had heterogeneous effects and slight timing variations across different constituencies, but since our individual-level data allows us to account for this variation, we are not worried about these possible complications. What we do know, is that these developments affected the working and middle class simultaneously. This is important for us, since we make comparisons between the levels of split voting and liberal voting for working and middle class. In other words, we need not be concerned about problems related to heterogeneous treatments when using TWFE estimation, since we have no reason to believe that our treatment group (working class) and control group (middle class) were treated at different times.

5.2 Analysis of the poll book data

Our analysis of the poll book data and the implementation of DID estimation, follows closely the previous analysis of Dewan et al. (2020). The identification strategy applied relies on the occupational information recorded in the poll books, or the census data. Based on the occupational information, we can divide our observed voters into six different classes, based on the classification of occupations in industrial societies by Eriksson and Goldthorpe (1992). The six classes that we use are; non-skilled workers, skilled workers, farmers, white-collar workers, petite bourgeoisie and landed gentry. Using this classification, we divide our sample into middle (white-collar workers, petite bourgeoisie and landed gentry) and working class voters (non-skilled workers, skilled workers and farmers). Because we are ultimately interested in differences between the middle and working class voters, our classification is robust for minor errors in the more granular classification. In other words, our sample won't be biased, if we misclassify some non-skilled workers as skilled workers, because both are ultimately classified as working class voters.

Previous works of Cox (1987, 1986, 1984a) have identified a significant drop in the split voting rates and non-partisan plumping (NPP) from 1857 to 1865. It is important to note, that this observed drop in split voting rates and NPP happened before the major enfranchisement expansions, the Second Reform Act of 1867 and the Third Reform Act of 1884, which were traditionally though to be the main drivers behind change in voting behavior. After 1865, the levels of split voting and NPP, remained the same till 1910's. We use 1865 as a treatment year in our DID estimation, relying on the observations of Cox.

We estimate the following DID specification,

$$y_{it} = \alpha + \beta_1 1[Workingclass]_{it} + \beta_2 1[Year \geq 1865]_t + \beta_3 1[Workingclass]_{it} \times 1[Year \geq 1865]_t + \varepsilon_{it}.$$

Here, y_{it} is the dependent variable, which in our case is either the rate of split voting or the liberal vote share. $1[Workingclass]_{[}it]$ and $1[Year \geq 1865]_t$ are dummy-variables that select the observations with working class status and observations during or after 1865, respectively. Last, we have the interaction variable $1[Workingclass]_{it} \times 1[Year \geq 1865]_t$, which selects observations that have both, working class status and are observed in 1865 or after. This is our main variable of interest in our DID specification, since it describes the interaction of the treatment year on the (assumed) treatment population, in other words, the average treatment effect on the treated (ATT).

For the constituency-level estimation, we also control for year fixed-effects and parish fixed-effects, if parish information is provided in the poll book. For the pooled data, we control for year, constituency and year-constituency fixed-effects. Controlling for year fixed-effects requires a different specification than the standard DID. For the estimations, that control for these fixed-effects, we use a two-way-fixed-effects regression (TWFE).

5.2.1 Sensitivity analysis

The results of DID estimation can be interpreted as causal effects, if the identification assumptions hold. As previously discussed, the two main identification assumptions for DID is the assumption of parallel trends and no anticipatory effects. These assumptions must be tested in some way, if we want to make causal claims based on our results.

For testing the parallel trends assumption, we estimate a dynamic DID model, following the same specification as Dewan et al. (2020).

$$y_{it} = \gamma + \delta_1 1[Workingclass]_{it} + \sum_{t \neq 1859} \{\delta_{2t} 1[Year = t] + \delta_{3t} 1[Year = t] \times 1[Workingclass]_{it}\} + \zeta_{it}$$

Since in our DID specification, our main variable of interest was the interaction term between working class status and the treatment year, represented by the coefficient β_3, now our focus is on the δ_{3t} coefficient. The dynamic DID estimates the δ_{3t} for all years, setting the year 1859 as the baseline. We select the year 1859 as our baseline, following the strategy of Dewan et al. (2020), since it is the last year before our treatment period. If the assumption of parallel trends holds, δ_{3t} coefficients should not be statistically significant in our pre-treatment period. In Section 6, we provide plots of δ_{3t} coefficients, together with the confidence intervals.

The assumption of no anticipatory effects is not as easy to test for. First, we are not entirely certain, what are the specific mechanisms that caused our treatment. Since there was no specific policy change that we could use as our treatment, we cannot determine, what would have been the change that voters could have adapted to before change happened. As discussed already in Section 2, the emergence of a party oriented electorate was a combination of multiple factors, which makes it rather implausible, that voters could have some how anticipated these changes and altered their voting behavior. Thus, we believe that the assumption of no anticipatory effects holds for our specific case.

6. Results

In this section we go over the results for our empirical analysis. As discussed in the previous section, we follow the methodology established by Dewan et al. (2020). Our attempt is to replicate the results found in Dewan et al. and expand the analysis to new poll books, which have been digitized in the recent years. Our new data comprised 14 new constituencies. Six of these poll books have records of occupational information and eight don't. For these eight, we have matched occupation from census data from years 1851 and 1861. We first discuss the new poll book with occupational information and then the ones with matched occupational information. After that we replicate the results of Dewan et al. (2020) and finally look at the results for the pooled poll book data.

6.1 Replicating the results of Dewan et al. (2020)

In this section, we will reproduce the results of Dewan et al. (2020). We have gathered the three original poll books from UK Data Archive (Study Number 2948, for Ashford; Study Number 977, for Guildford; Study Number 4170, for Sandwich), and reconstructed the data sets to be in accordance with our new poll book data. We have applied our own classification for the occupational information, which is the main driver behind the differences in regression results. For Guildford and Sandwich, occupation and voting information was stored in the same file and we only needed to reformat the data to fit the rest of our data set. In Ashford, we had to combine the data from Ashford Directories file, that contained various information about the characteristics of individuals living in Ashford, such as their occupation, and from the Ashford Poll Books file, that contained the voting data. These files didn't have an ID variable, for identifying individuals across files, so we had to match the persons from directories file and poll book file, by their names. This is the main reason for why our number of observations is different for Ashford, compared to the Ashford data used by Dewan et al. Number of observations slightly differs for Guildford and Sandwich as well. This difference results from our slightly different classification of occupations. Our number of total observations is 10 054, compared to the original 10 445. We believe, that this discrepancy won't inhibit us from making comparable analysis of the data.

Our analysis of the three original poll books, confirms the results found by Dewan et al. (2020). We find no meaningful differences in the size or direction of the effects, thus leading us to believe that the classification of occupations was done properly in the original study.

Results for Split Voting

Panel A: Ashford	(1)	(2)	
Working class	0.004	0.008	
	[0.099]	[0.099]	
Year≥1865	-0.502***		
	[0.073]		
Working class ×Year≥1865	-0.026	-0.029	
	[0.101]	[0.102]	
Constant	0.559***		
	[0.071]		
N	371	371	
R^2	0.34	0.35	
Panel B: Guildford	(3)	(4)	(5)
Working class	0.092***	0.095***	0.094***
	[0.021]	[0.020]	[0.020]
Year≥1865	-0.159***		
	[0.026]		
Working class ×Year≥1865	-0.076**	-0.079**	-0.073**
	[0.034]	[0.034]	[0.034]
Constant	0.282***		
	[0.016]		
N	3185	3185	3184
R^2	0.04	0.14	0.15
Pandel C: Sandwich	(6)	(7)	
Working class	0.039***	0.035***	
	[0.011]	[0.011]	
Year≥1865	-0.044***		
	[0.012]		
Working class ×Year≥1865	-0.043***	-0.039**	
	[0.016]	[0.016]	
Constant	0.106***		
	[0.008]		
N	6498	6498	
R^2	0.01	0.04	
Year FE		✓	✓
Parish FE			✓

Note: Outcome variable is a dummy for a split vote. Standard errors are robust and clustered by voterid and reported in brackets. Statistical significance is signified by asterisks, where * denotes 10%, ** 5% and *** 1% significance level.

Tab. 6.1: Split voting rates of old constituencies.

Table 6.1 shows the regression results of split voting rates, for the three original constituencies. We find that working class status is related to increased levels of split voting in elections before 1865. Exception to this is Ashford, where we find no such relation. We also find that subsequent to 1865, split voting rates significantly decrease among all voters, but this effect is stronger among working class voters. In other words, working class voters split their vote even less than middle class voters, during and after 1865, despite them splitting their vote more than the middle class, in elections before 1865.

For interpretation of the coefficients, we can look at Panel B of Table 6.1, which shows the results for the constituency of Guildford. Here we observe that prior to 1865, the split voting rate was approximately 28.2% ($Constant = 0.282$) for non-working class voters. Rate of split voting for working class voters before 1865 is 28.4% ($Constant + 0.092 = 0.374$). During and after 1865, working class split voting rates fell down to 13.9% ($Constant + 0.092 - 0.159 - 0.076 = 0.139$). For non-working class voters, the rate of split voting subsequent to 1865 is 12.3% ($Constant - 0.159 = 0.123$).

Only meaningful difference between our results and those of Dewan et al. (2020), is found in Guildford, where we find a statistically significant reduction in the levels of split voting among working class voters subsequent to 1865, compared to the insignificant effect found in the original study. However, the direction and magnitude of this effect is the same in the original results, as in ours, which leads us to believe that this result is driven purely by the slight differences in classification of occupations.

Results for Liberal Voting

Panel A: Ashford	(1)	(2)	
Working class	0.083	0.079	
	[0.092]	[0.094]	
Year≥1865	0.471***		
	[0.073]		
Working class ×Year≥1865	-0.066	-0.064	
	[0.106]	[0.107]	
Constant	0.250***		
	[0.064]		
N	371	371	
R^2	0.18	0.18	
Panel B: Guildford	(3)	(4)	(5)
Working class	-0.052*	-0.046*	-0.045
	[0.028]	[0.028]	[0.028]
Year≥1865	0.008		
	[0.036]		
Working class ×Year≥1865	0.142***	0.136***	0.127***
	[0.046]	[0.045]	[0.046]
Constant	0.393***		
	[0.024]		
N	3185	3185	3184
R^2	0.01	0.09	0.10
Panel C: Sandwich	(6)	(7)	
Working class	-0.049**	-0.043**	
	[0.020]	[0.020]	
Year≥1865	0.016		
	[0.025]		
Working class ×Year≥1865	0.087***	0.081**	
	[0.033]	[0.033]	
Constant	0.512***		
	[0.016]		
N	6498	6498	
R^2	0.01	0.04	
Year FE		✓	✓
Parish FE			✓

Note: Outcome variable is a dummy for a liberal vote. Standard errors are robust and clustered by voterid and reported in brackets. Statistical significance is signified by asterisks, where * denotes 10%, ** 5% and *** 1% significance level.

Tab. 6.2: Liberal vote shares of old constituencies.

Table 6.2 shows the regression results for the liberal vote shares. We find that working class voters are less likely to vote for liberal candidates in elections before 1865. We also find that in elections during and after 1865, working class voters are more likely to vote for liberal candidates. Only exception to this, is Ashford, where we find a negative, but statistically insignificant relationship. This result for Ashford is also different compared to the one found in the original study, where this effect is positive, but statistically insignificant. This difference, again, is most likely the result of meaningfully smaller number of observations and slightly different classification of occupations.

For interpretation of the coefficients, we can look at Panel B of Table 6.2, which shows the results for the constituency of Guildford. Here we observe that prior to 1865, the liberal vote share was approximately 39.3% ($Constant = 0.393$) for non-working class voters. Vote share of liberal candidates from working class voters before 1865 is % ($Constant - 0.052 = 0.341$). During and after 1865, liberal vote share from working class rose to 48.38% ($Constant - 0.052 + 0.008 + 0.142 = 0.4838$). For non-working class voters, the rate of liberal voting subsequent to 1865 is 40.1% ($Constant + 0.008 = 0.401$).

Results for the pooled data are found in Table 6.3. We consider these pooled results to be the main findings of this section. Our pooled results strongly support the previous findings and interpretations of Dewan et al. (2020). We find that working class voters were more likely (less likely) to split their vote (vote for liberal candidates) in elections prior to 1865. However, in elections subsequent to 1865, the rate of split voting among working class voters decreased significantly more, than among middle class voters, and the rate of voting for liberal candidates increased.

Due to the similarity in results, we believe that we have successfully implemented the empirical strategy used in the original study. Our independent classification of the occupation data, according to the class framework set by Eriksson and Goldthorpe (1992), and the similarity of our reconstructed data, and the following results, leads us to believe that the classification of occupations was proper in the original study.

Figure 6.1 depicts the coefficients of the dynamic difference-in-difference estimation. The causal interpretation of our results is dependent on the identifying assumptions of DID estimation. As stated previously, the key identifying assumption for DID estimation is the assumption of parallel trends. Parallel trends can be tested by an event study model, shown in Section 5.2.1, where we estimate the main variable of interest, $[Working\ \ Class \times Year = t]$, for each election year, setting 1859, as the baseline. As shown in Figure 6.3, we don't observe statistically significant effects for vote splitting or liberal vote shares, before our treatment period. This confirms that voting patterns of working and middle class voters followed the same *parallel trends*, before our treatment period. Thus, our assumption of parallel trends holds, and our results for Ashford, Guildford and Sandwich can be interpreted as causal.

Pooled Results for Old Constituencies

Panel A: Old Constituencies Split Voting Rates				
	(1)	(2)	(3)	(4)
Working class	0.054***	0.054***	0.053***	0.055***
	[0.011]	[0.010]	[0.010]	[0.010]
Year≥1865	-0.100***			
	[0.011]			
Working class ×Year≥1865	-0.053***	-0.053***	-0.049***	-0.056***
	[0.015]	[0.015]	[0.015]	[0.014]
Constant	0.175***			
	[0.008]			
N	10054	10054	10054	10054
R^2	0.03	0.07	0.12	0.15
Panel B: Old Constituencies Liberal Vote Shares				
	(5)	(6)	(7)	(8)
Working class	-0.047***	-0.045***	-0.042***	-0.042***
	[0.016]	[0.016]	[0.016]	[0.016]
Year≥1865	0.060***			
	[0.020]			
Working class ×Year≥1865	0.084***	0.083***	0.084***	0.090***
	[0.026]	[0.026]	[0.026]	[0.026]
Constant	0.466***			
	[0.013]			
N	10054	10054	10054	10054
R^2	0.01	0.03	0.05	0.07
Year FE		✓	✓	✓
Constituency FE			✓	
Year-Constituency FE				✓

Note: Outcome variable is a dummy for a split vote in Panel A and liberal vote in Panel B. Standard errors are robust and clustered by voterid and reported in brackets. Statistical significance is signified by asterisks, where * denotes 10%, ** 5% and *** 1% significance level.

Tab. 6.3: Results for pooled data of old constituencies.

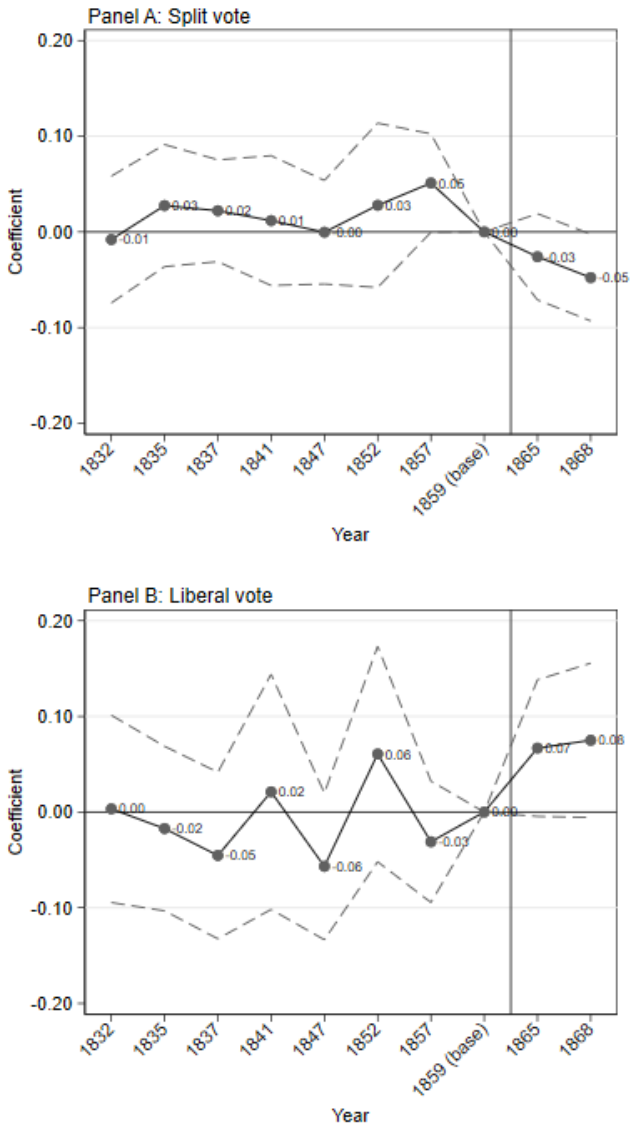


Fig. 6.1: Differences-in-differences estimation test for parallel trends for old constituencies.

6.2 New poll books with occupation

6.2.1 Results on Split Voting Rates

In this section, we discuss the six new constituencies that have the occupational information recorded in the poll books. Tables 6.4 and 6.5, show the results for split voting rates and liberal vote shares, respectfully. We observe rather heterogeneous effects, both in terms of direction and statistical significance, between individual constituencies. For working class status, we observe mostly null results for splitting the vote before the year 1865. In Barnstaple and Lancaster, we observe a that vote splitting was significantly more common for working class individuals before 1865. This result remains significant even when controlling for year fixed-effects. We also observe a significant drop in split voting rates during and after the year 1865 in Aylesbury, Cambridge and Lancaster

(for non-working class voters). For Barnstaple and Bath we observe null results. Our constituency-level results for working class split voting rates during or after 1865, are inconclusive. In most constituencies we observe null results for working class split voting subsequent to 1865, except in Lancaster, where we see a significant drop in split voting rates. This result for Lancaster, remains significant when controlling for year fixed-effects. For more detailed interpretation of coefficients, see Section 6.1.

It is worth noting the stark differences in the overall levels of split voting between constituencies. For example, in Aylesbury we observe that nearly 30% of all non-working class voters, before 1865, were split voters, whereas in Cambridge the equivalent rate was only 1.7%. This level-difference in our opinion, could be the explanation for why we observe a significant drop in split voting rates only in some of our constituencies. However, a counter example to this intuition is also Cambridge, which has the lowest split voting rate, but where we still observe a statistically significant drop after 1865. Still, our results seem to suggest that some constituencies might have been less partisan, for any number of reasons, than others, already before 1865.

In Figure 6.2, we show the change in levels of split voting between each election. Here we also observe the aforementioned heterogeneity between levels and changes in split voting. Our causal interpretation relies on the assumption of parallel trends in levels of split voting, before our treatment period. Despite this not being obvious for each constituency, we provide sensitivity analysis of our DID estimation, in the section discussing the pooled results.

Results for Split Voting

Panel A: Aylesbury	(1)	(2)	(3)
Working class	-0.011	0.002	0.006
	[0.019]	[0.017]	[0.017]
Year≥1865	-0.120***		
	[0.031]		
Working class ×Year≥1865	0.028	0.014	0.010
	[0.036]	[0.035]	[0.035]
Constant	0.299***		
	[0.016]		
N	4159	4159	4159
R^2	0.01	0.12	0.14
Panel B: Barnstaple	(4)	(5)	
Working class	0.048***	0.033**	
	[0.014]	[0.014]	
Year≥1865	0.006		
	[0.020]		
Working class ×Year≥1865	-0.031	-0.016	
	[0.035]	[0.035]	
Constant	0.129***		
	[0.009]		
N	3960	3960	
R^2	0.00	0.06	
Panel C: Bath	(6)	(7)	(8)
Working class	0.000	-0.000	0.008
	[0.006]	[0.006]	[0.007]
Year≥1865	0.002		
	[0.007]		
Working class ×Year≥1865	0.004	0.004	-0.002
	[0.010]	[0.010]	[0.010]
Constant	0.051***		
	[0.004]		
N	10554	10554	7050
R^2	0.00	0.00	0.01
Year FE		✓	✓
Parish FE			✓

(continued)

Panel D: Beverley	(9)	(10)	
Working class	0.045***	0.040***	
	[0.014]	[0.014]	
Year≥1865	-0.038***		
	[0.014]		
Working class ×Year≥1865	0.010	0.015	
	[0.018]	[0.018]	
Constant	0.079***		
	[0.011]		
N	4374	4374	
R^2	0.01	0.01	
Panel E: Cambridge	(11)	(12)	
Working class	0.001	0.001	
	[0.004]	[0.004]	
Year≥1865	-0.014***		
	[0.003]		
Working class ×Year≥1865	0.001	0.001	
	[0.004]	[0.004]	
Constant	0.017***		
	[0.003]		
N	9229	9229	
R^2	0.00	0.01	
Panel F: Lancaster	(13)	(14)	
Working class	0.068***	0.058***	
	[0.017]	[0.016]	
Year≥1865	-0.170***		
	[0.021]		
Working class ×Year≥1865	-0.090***	-0.081***	
	[0.024]	[0.023]	
Constant	0.216***		
	[0.015]		
N	5046	5046	
R^2	0.05	0.13	
Year FE		✓	✓
Parish FE			✓

Note: Outcome variable is a dummy for a split vote. Standard errors are robust and clustered by voterid and reported in brackets. Statistical significance is signified by asterisks, where * denotes 10%, ** 5% and *** 1% significance level.

Tab. 6.4: Results on split voting rates on new constituencies with occupational information.

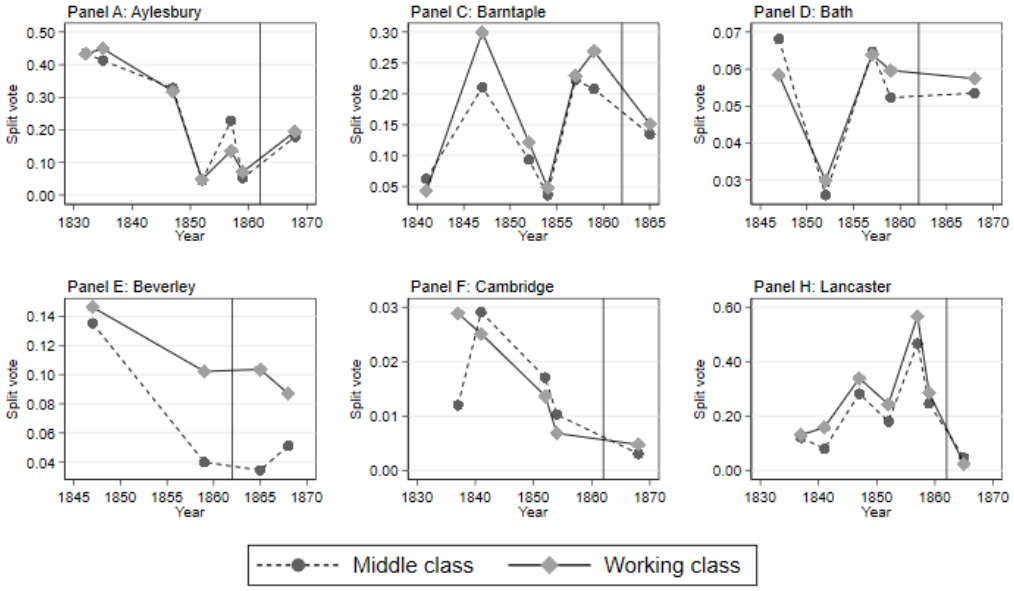


Fig. 6.2: Graphs of split voting rates by constituency.

6.2.2 Results on Liberal Vote Shares

For liberal candidates' vote shares, we observe similar kind of heterogeneity in results, as we saw for rates of vote splitting. Here we see that in Bath and Lancaster, working class voters have always preferred liberal candidates, even before 1865. In Barnstaple and Cambridge, the opposite seems to be true. We also don't observe a significant change in liberal candidates' vote shares anywhere, except in Lancaster, where we observe almost a 20% increase in support (for non-working class voters). We also observe mostly null results for working class voters liberal vote shares during or after 1865. Exceptions to this are Bath and Cambridge, where we observe a statistically significant increase in liberal vote shares. This result for Bath is robust for year and parish fixed-effects and robust for year fixed-effects in Cambridge, where we don't have parish-level voting data. For more detailed interpretation of the coefficients, see Section 6.1.

Figure 6.3, shows the change in levels of liberal candidates vote shares between each election. Heterogeneity in levels of liberal vote shares is once again evident. We provide results for the sensitivity analysis in the section discussing results of the pooled data. For liberal vote shares, the parallel trends assumption does not hold, and thus our results for changes in liberal vote shares, cannot be interpreted as causal.

Results for Liberal Voting

Panel A: Aylesbury	(1)	(2)	(3)
Working class	0.010	0.009	0.006
	[0.024]	[0.024]	[0.024]
Year≥1865	-0.048		
	[0.036]		
Working class ×Year≥1865	-0.007	-0.006	0.003
	[0.042]	[0.042]	[0.042]
Constant	0.447***		
	[0.021]		
N	4159	4159	4159
R^2	0.00	0.03	0.06
Panel B: Barnstaple	(4)	(5)	
Working class	-0.116***	-0.099***	
	[0.024]	[0.024]	
Year≥1865	0.001		
	[0.028]		
Working class ×Year≥1865	-0.009	-0.026	
	[0.044]	[0.044]	
Constant	0.451***		
	[0.017]		
N	3960	3960	
R^2	0.01	0.03	
Panel C: Bath	(6)	(7)	(8)
Working class	0.154***	0.152***	0.143***
	[0.017]	[0.017]	[0.020]
Year≥1865	0.007		
	[0.015]		
Working class ×Year≥1865	0.058***	0.059***	0.049**
	[0.022]	[0.022]	[0.024]
Constant	0.325***		
	[0.011]		
N	10554	10554	7050
R^2	0.03	0.04	0.07
Year FE		✓	✓
Parish FE			✓

*(*continued)

Panel D: Beverley	(11)	(12)	
Working class	0.163***	0.153***	
	[0.026]	[0.026]	
Year≥1865	-0.028		
	[0.027]		
Working class ×Year≥1865	-0.096***	-0.090***	
	[0.031]	[0.031]	
Constant	0.374***		
	[0.023]		
N	4374	4374	
R^2	0.02	0.05	
Panel E: Cambridge	(9)	(10)	
Working class	-0.037**	-0.039**	
	[0.018]	[0.018]	
Year≥1865	0.005		
	[0.019]		
Working class ×Year≥1865	0.131***	0.132***	
	[0.024]	[0.024]	
Constant	0.494***		
	[0.014]		
N	9229	9229	
R^2	0.01	0.02	
Panel F: Lancaster	(11)	(12)	
Working class	0.096***	0.097***	
	[0.017]	[0.017]	
Year≥1865	0.192***		
	[0.036]		
Working class ×Year≥1865	-0.036	-0.037	
	[0.042]	[0.042]	
Constant	0.181***		
	[0.014]		
N	5046	5046	
R^2	0.03	0.08	
Year FE		✓	✓
Parish FE			✓

Note: Outcome variable is a dummy for a liberal vote. Standard errors are robust and clustered by voterid and reported in brackets. Statistical significance is signified by asterisks, where * denotes 10%, ** 5% and *** 1% significance level.

Tab. 6.5: Results on liberal vote shares on new constituencies with occupational information.

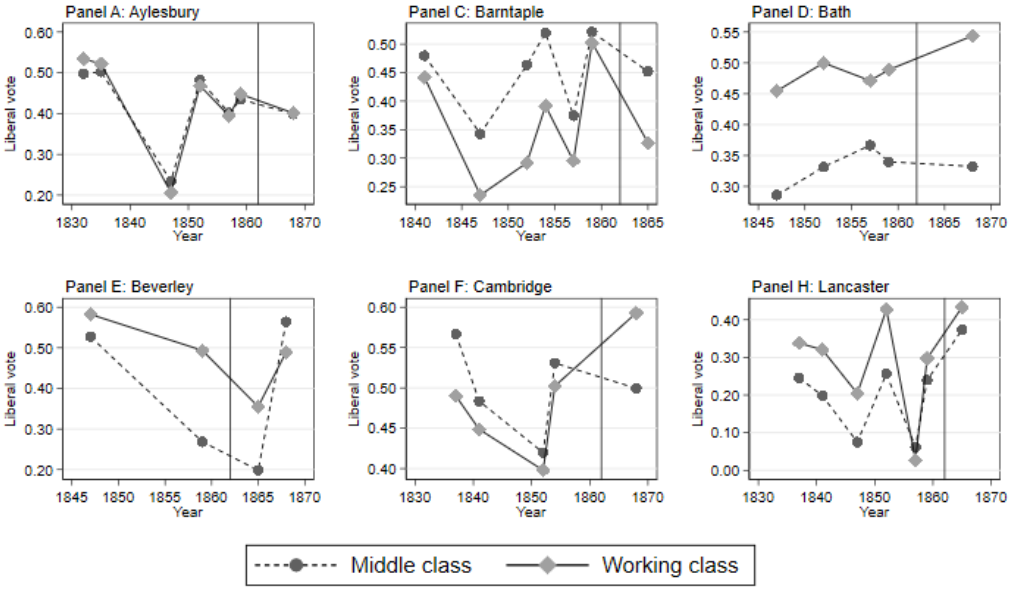


Fig. 6.3: Graphs of liberal voting rates by constituency.

6.3 New poll books without occupation

In this section, we cover the results for the seven constituencies that had no occupational information recorded in the poll book. These constituencies are Boston, Canterbury, Derby, Great Yarmouth, Norwich, Totnes and York. These constituencies are matched to the census data from years 1851 and 1861, to acquire the occupational information required for sorting observations to middle and working class. Due to problems with the matching, which we extensively discussed in Section 4.2., the results from matched data, cannot be interpreted as causal or to be reflective of the underlying changes in the voting behavior.

6.3.1 Results on Split Voting Rates

Tables 6.6 and 6.7 show the results for the split voting rates and liberal vote shares, respectfully. In Table 6.6, we see mostly null results for the relationship between working class status and split voting before 1865, however in Boston, Derby and Great Yarmouth, we do observe a significant effect. We do also observe a statistically significant change in the split voting rates among the non-working class voters during and after 1865, but the direction and the magnitude of this effect changes meaningfully between the constituencies. For our main variable of interest, working class status' effect on split voting during or after 1865, we observe null results across all constituencies, except for specification (2) in Boston and Canterbury. For more detailed interpretation of the coefficients, see Section 6.1.

Results of Table 6.6, go against the results found in the previous sections, which suggest that working class voters reduced their amount of split voting more than non-working class voters subsequent to 1865. This difference in results is meaningful, but should not be viewed as counter evidence, because of the distortions in the data. Figure 6.4, shows

the changes in the levels of split voting between each election by constituency. Here, the null results are quite apparent since middle and working class levels of split voting overlap almost perfectly in most constituencies.

Results for Split Voting

Panel A: Boston	(1)	(2)
Working class	0.037*	0.037*
	[0.022]	[0.022]
Year≥1865	-0.066**	
	[0.028]	
Working class ×Year≥1865	-0.078***	-0.049*
	[0.027]	[0.027]
Constant	0.261***	
	[0.020]	
N	4325	4325
R^2	0.02	0.13
Panel B: Canterbury	(3)	(4)
Working class	-0.007	0.007
	[0.006]	[0.004]
Year≥1865	-0.063***	
	[0.006]	
Working class ×Year≥1865	0.002	-0.019*
	[0.007]	[0.011]
Constant	0.075***	
	[0.005]	
N	15997	14865
R^2	0.01	0.28
Panel C: Derby	(5)	(6)
Working class	-0.011**	-0.012**
	[0.005]	[0.005]
Year≥1865	0.177***	
	[0.018]	
Working class ×Year≥1865	-0.007	-0.006
	[0.014]	[0.014]
Constant	0.066***	
	[0.006]	
N	17932	17932
R^2	0.06	0.08

(continued)

Panel D: Great Yarmouth	(7)	(8)
Working class	-0.064**	0.000
	[0.028]	[0.025]
Year≥1865	-0.291***	
	[0.047]	
Working class ×Year≥1865	0.034	-0.030
	[0.044]	[0.039]
Constant	0.332***	
	[0.030]	
N	2648	2648
R^2	0.07	0.30
Panel E: Norwich	(9)	(10)
Working class	0.001	0.001
	[0.002]	[0.002]
Year≥1865	0.006*	
	[0.004]	
Working class ×Year≥1865	0.001	-0.001
	[0.003]	[0.003]
Constant	0.010***	
	[0.003]	
N	34001	34001
R^2	0.00	0.01
Panel F: Totnes	(11)	(12)
Working class	0.062	0.061
	[0.044]	[0.042]
Year≥1865	-0.103***	
	[0.034]	
Working class ×Year≥1865	-0.064	-0.064
	[0.051]	[0.051]
Constant	0.157***	
	[0.029]	
N	576	576
R^2	0.03	0.04

(continued)

Panel G: York	(13)	(14)
Working class	-0.001	-0.007
	[0.009]	[0.009]
Year≥1865	-0.262***	
	[0.012]	
Working class ×Year≥1865	-0.004	0.002
	[0.009]	[0.009]
Constant	0.303***	
	[0.012]	
N	42727	42727
R^2	0.14	0.17
Year FE		✓

Note: Outcome variable is a dummy for a split vote. Standard errors are robust and clustered by voterid and reported in brackets. Statistical significance is signified by asterisks, where * denotes 10%, ** 5% and *** 1% significance level.

Tab. 6.6: Results on split voting rates on new constituencies with no occupational information.

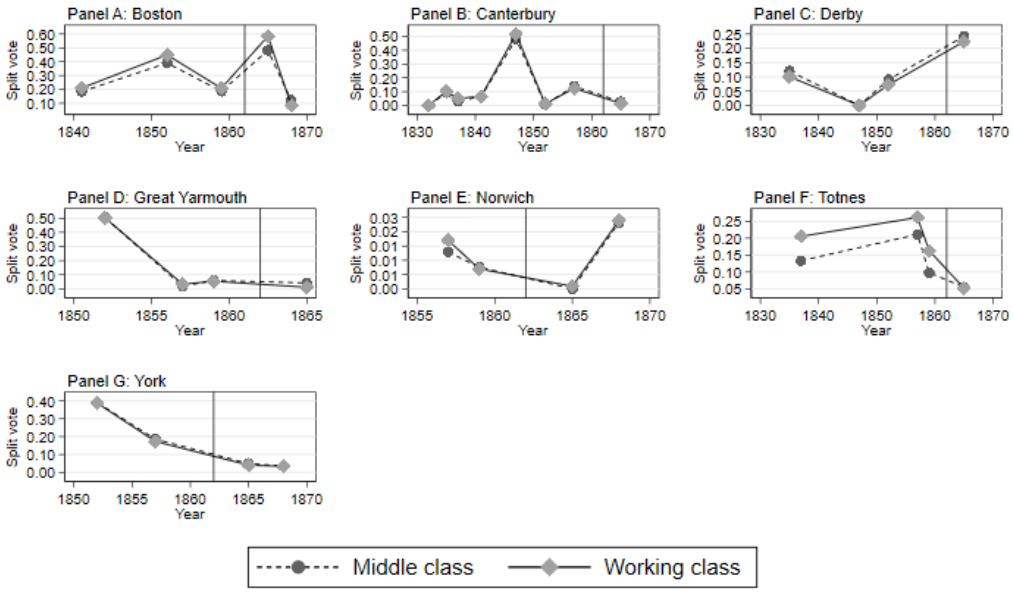


Fig. 6.4: Graphs of matched split voting data.

6.3.2 Results on Liberal Vote Shares

Table 6.7 shows the results for the liberal vote shares. We again mostly observe null results between the working class status and voting for liberal candidates before 1865. Ex-

ceptions to this are Canterbury where we observe a significant negative relationship and Derby where we observe similar sized and significant positive effect. In all constituencies, except Derby and Great Yarmouth, we observe the non-working class vote share for liberals to significantly change, but the magnitude and direction of this effect changes for each constituency. Derby and Norwich are the only constituencies where we observe a significant effect for our main variable of interest, $[Working\ class \times Year \geq 1865]$. For the other constituencies, this effect isn't statistically significant. For more detailed interpretation of the coefficients, see Section 6.1.

Once again, these results differ significantly from those of Dewan et al. (2020) and from constituencies, that didn't require matching. Still, this should not be viewed as counter evidence because of the distortions in the data. The Figure 6.5., shows the changes in level of votes for liberal candidates between elections by constituency. Null results are again apparent since in most constituencies the level of liberal voting is the same across all elections for middle and working class voters.

Results for Liberal Voting

Panel A: Boston	(1)	(2)
Working class	-0.011	0.011
	[0.029]	[0.029]
Year≥1865	-0.104***	
	[0.034]	
Working class ×Year≥1865	0.003	-0.021
	[0.035]	[0.035]
Constant	0.461***	
	[0.026]	
N	4325	4325
R^2	0.01	0.03
Panel B: Canterbury	(3)	(4)
Working class	-0.020*	-0.027***
	[0.011]	[0.010]
Year≥1865	0.052**	
	[0.023]	
Working class ×Year≥1865	-0.002	0.011
	[0.022]	[0.036]
Constant	0.372***	
	[0.015]	
N	15997	14865
R^2	0.00	0.13
Panel C: Derby	(5)	(6)
Working class	0.024**	0.022*
	[0.012]	[0.012]
Year≥1865	0.016	
	[0.020]	
Working class ×Year≥1865	-0.035*	-0.033*
	[0.019]	[0.019]
Constant	0.407***	
	[0.014]	
N	17932	17932
R^2	0.00	0.01

(continued)

Panel D: Great Yarmouth	(7)	(8)
Working class	0.010	0.010
	[0.033]	[0.032]
Year≥1865	-0.088	
	[0.055]	
Working class ×Year≥1865	0.051	0.051
	[0.056]	[0.055]
Constant	0.463***	
	[0.032]	
N	2648	2648
R^2	0.00	0.01
Panel E: Norwich	(9)	(10)
Working class	-0.008	-0.009
	[0.015]	[0.015]
Year≥1865	-0.150***	
	[0.015]	
Working class ×Year≥1865	0.025*	0.026*
	[0.015]	[0.015]
Constant	0.519***	
	[0.015]	
N	34001	34001
R^2	0.02	0.02
Panel F: Totnes	(11)	(12)
Working class	0.011	0.010
	[0.060]	[0.061]
Year≥1865	-0.154**	
	[0.062]	
Working class ×Year≥1865	0.007	0.008
	[0.084]	[0.084]
Constant	0.551***	
	[0.041]	
N	576	576
R^2	0.02	0.04

(continued)

Panel G: York	(13)	(14)
Working class	-0.002	0.008
	[0.009]	[0.008]
Year≥1865	0.139***	
	[0.016]	
Working class ×Year≥1865	0.017	0.006
	[0.011]	[0.010]
Constant	0.290***	
	[0.013]	
N	42727	42727
R^2	0.02	0.08
Year FE		✓

Note: Outcome variable is a dummy for a liberal vote. Standard errors are robust and clustered by voterid and reported in brackets. Statistical significance is signified by asterisks, where * denotes 10%, ** 5% and *** 1% significance level.

Tab. 6.7: Results on liberal vote shares on new constituencies with no occupational information.

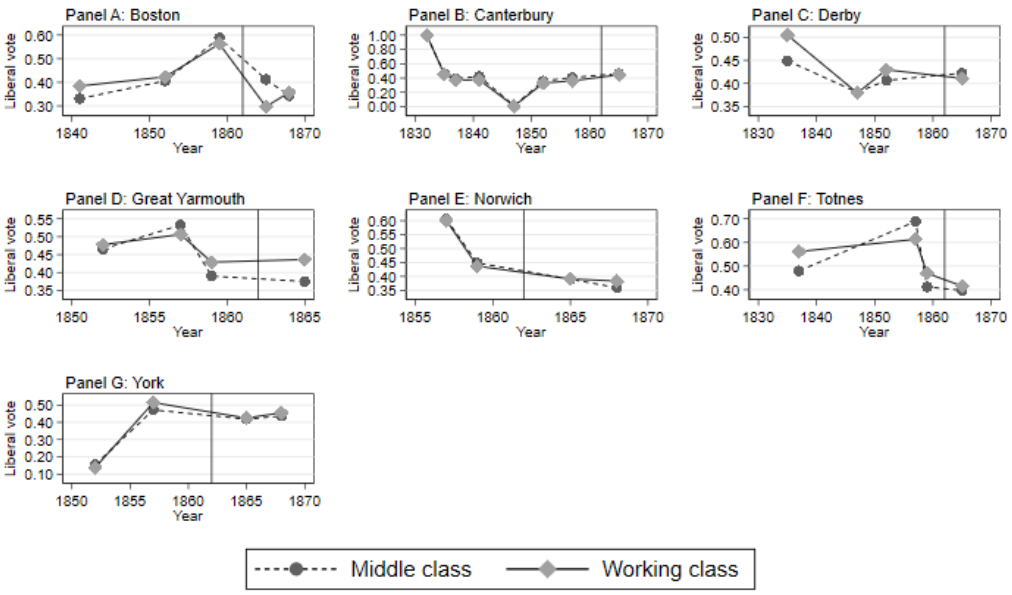


Fig. 6.5: Graphs of matched liberal vote shares.

6.4 Results for the pooled data

In this section I go over the results for the pooled data. The pooled data presented in Table 6.8 is combined from all constituencies, old and new, that have occupational information recorded in the poll books. In total we have observations from 9 constituencies and 11 elections, with a sample size of 47 376. For comparison, the original study of digitized poll book data by Dewan et al. (2020), had data from 3 constituencies and 10 elections, with a sample size of 10 445 observations. Results presented in Table 6.9 are of the pooled data of the 6 new constituencies with occupational information recorded in the poll books.

Our results mostly corroborate the results of Dewan et al. (2020) and our own pooled results for the old 3 constituencies. Panel A of Table 6.8 shows the results for split voting rates, which are highly statistically significant across all specifications. We find that split voting was more common among working-class voters before 1865 and then subsequent to 1865 we observe a significant drop in the rates of split voting among all voters, but especially among working-class voters. This suggests that working class voters became more party aligned, than non-working class voters during the same period.

However, our results start to differ from the pooled results of Dewan et al., as well as from our own pooled results of the three original constituencies, in specifications (2), (3) and (4). The direction of the effect remains the same for all specifications, but the magnitude of the effect decreases substantially. This is especially true for specification (4), where the effect of working class status on vote splitting, before and subsequent to 1865, is more than halved. Specification (4) controls for year and year-constituency fixed effects. This finding suggest that there are some characteristics in our new constituencies, which lead our results to be upwardly biased, unless we control for constituency fixed-effects. These could be for example, the differences in levels of split voting or the characteristics could be unobservable to us. Another possible explanation for this discrepancy between the results could be the fact that the original results were largely driven by the constituency of Sandwich, which represented over 60% of the original 3 constituency sample. Our pooled data is much more evenly distributed between the 9 constituencies. Our largest constituency, Bath, represents only 22.28% of the total sample.

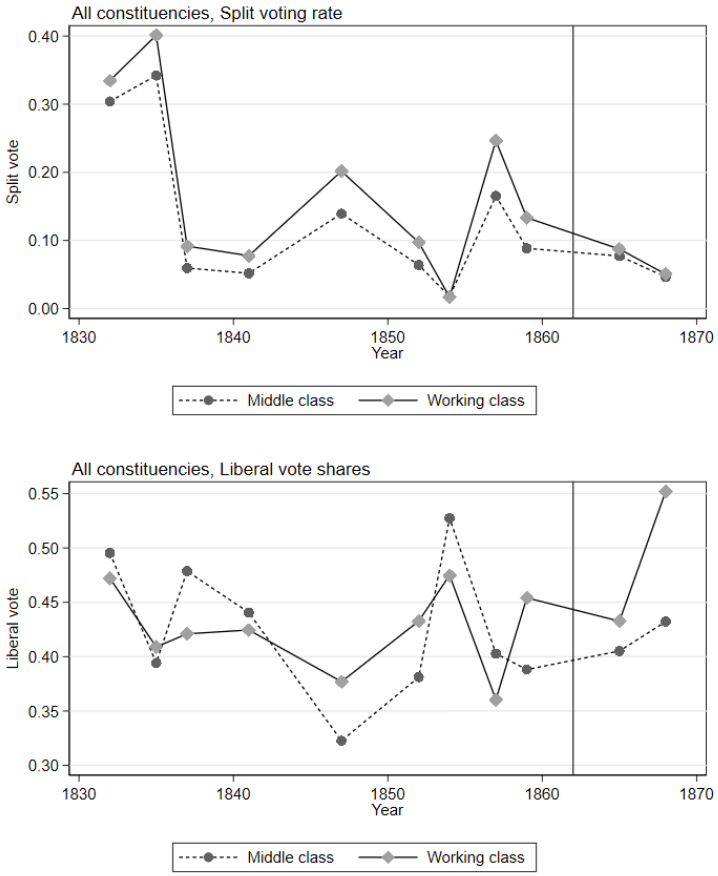


Fig. 6.6: Graphs of pooled data

Panel B shows the pooled results for liberal vote shares. These results are also highly statistically significant across all specifications. Our results are mostly in line with ones Dewan et al., as well as our own pooled data of the three original constituencies. Our findings support the claim that the English working-class did align with the liberal party more than non-working class voters subsequent to 1865. This finding, however, cannot be interpreted as causal. Why this is, we will address later.

The most significant difference in our results and those of Dewan et al. (2020), and our own analysis of the old data, is related to liberal vote shares of working class voters before 1865. We find that working-class voters have always voted more for liberal candidates, even before 1865. This result remains highly statistically significant and robust for all the specifications. This is a meaningful difference from the result's of Dewan et al. (2020), who find that working-class status was negatively associated with voting for liberals before 1865. This means that the observed change in voting behavior is significantly smaller than previously though. This also raises questions of the possible explanations for this observed alignment.

This time we don't observe any meaningful changes in the effect size between specifications. This means that results of specification (5) stay robust even when controlling for year, constituency and year-constituency fixed-effects. This is also interesting in the light of results from Table 6.8, for which this was not true, suggesting that there is some constituency specific characteristic that affect split voting, but not liberal vote shares.

The difference between our results and those found by Dewan et al. (2020), can again

be mostly attributed to the disproportionate effect of Sandwich in the original pooled data. In Sandwich we find a relatively large negative effect between working-class status and liberal vote share before 1865, which explains why the same effect is found in the pooled data. For comparison, among our 6 constituencies, we find a positive relationship for 3 constituencies, negative relationship for 2 constituencies and one null result for one constituency. For our largest constituency , Bath (22.28% of total sample), we find a positive relationship, but for the second largest constituency, Cambridge (19.48% of total sample), we find a negative relationship. Thus, it can be argued that this result isn't only driven by the 2 largest constituencies, who represent 41.76% of the total sample.

Pooled Results for All Constituencies with Occupational information

Panel A: All Constituencies Split Voting Rates				
	(1)	(2)	(3)	(4)
Working class	0.052***	0.045***	0.027***	0.025***
	[0.004]	[0.004]	[0.004]	[0.004]
Year≥1865	-0.058***			
	[0.005]			
Working class ×Year≥1865	-0.048***	-0.041***	-0.027***	-0.015***
	[0.006]	[0.006]	[0.006]	[0.006]
Constant	0.115***			
	[0.003]			
N	47376	47376	47376	47376
R^2	0.02	0.08	0.13	0.16
Panel B: All Constituencies Liberal Vote Shares				
	(5)	(6)	(7)	(8)
Working class	0.021***	0.020***	0.027***	0.026***
	[0.008]	[0.008]	[0.008]	[0.008]
Year≥1865	0.022***			
	[0.008]			
Working class ×Year≥1865	0.073***	0.074***	0.069***	0.073***
	[0.011]	[0.011]	[0.011]	[0.011]
Constant	0.399***			
	[0.006]			
N	47376	47376	47376	47376
R^2	0.01	0.01	0.03	0.05
Year FE		✓	✓	✓
Constituency FE			✓	
Year-Constituency FE				✓

Note: Outcome variable is a dummy for a split vote in Panel A and liberal vote in Panel B. Standard errors are robust and clustered by voterid and reported in brackets. Statistical significance is signified by asterisks, where * denotes 10%, ** 5% and *** 1% significance level.

Tab. 6.8: Results for pooled data of old and new constituencies with occupational information.

Pooled results for the six new constituencies are found in Table 6.9. These results don't meaningfully differ from the results of pooled data of all constituencies, except for specification (4). This is mainly because these six new constituencies represent 78.8% of the total observations. However, these results differ significantly from the results of the old three constituencies.

In Panel A of Table 6.9, we highlight specification (4), where we observe the only statistically insignificant effect of working-class status on split voting subsequent to 1865. In the same specification we see that the effect of working-class status on split voting before 1865 is greatly diminished. This is in stark contrast with results found by Dewan et al. and with our own analysis of the original three constituencies.

To us, our results from the pooled data highlight the differences between our poll book sample and the sample used by Dewan et al. (2020). It seems that our new constituencies have significant constituency specific characteristics that affect the results, which are only evident after controlling for constituency and constituency-year fixed effects. This is not the case with the original three constituencies of Ashford, Guildford and Sandwich, for which the results stay robust even after controlling for these constituency specific fixed-effects. It is also interesting that these constituency specific characteristics only seem to affect the split voting results, since our results for liberal vote shares remain rather robust even after accounting for these fixed-effects. With liberal vote shares, our only significant departure from the original results is the effect of working-class status on voting for liberal candidates before 1865. Unlike the results of Dewan et al., we find that working-class voters have voted more for liberal candidates than non-working class voters, already before 1865.

Pooled Results for New Constituencies with Occupational Information

Panel A: New Constituencies Split Voting Rates				
	(1)	(2)	(3)	(4)
Working class	0.049***	0.041***	0.020***	0.016***
	[0.005]	[0.004]	[0.004]	[0.004]
Year≥1865	-0.047***			
	[0.005]			
Working class ×Year≥1865	-0.043***	-0.036***	-0.021***	-0.004
	[0.006]	[0.006]	[0.006]	[0.006]
Constant	0.099***			
	[0.003]			
N	37322	37322	37322	37322
R^2	0.02	0.10	0.15	0.17
Panel B: New Constituencies Liberal Vote Shares				
	(5)	(6)	(7)	(8)
Working class	0.039***	0.035***	0.047***	0.047***
	[0.008]	[0.008]	[0.009]	[0.009]
Year≥1865	0.011			
	[0.009]			
Working class ×Year≥1865	0.074***	0.080***	0.068***	0.066***
	[0.012]	[0.012]	[0.012]	[0.012]
Constant	0.381***			
	[0.007]			
N	37322	37322	37322	37322
R^2	0.01	0.03	0.04	0.05
Year FE		✓	✓	✓
Constituency FE			✓	
Year-Constituency FE				✓

Note: Outcome variable is a dummy for a split vote in Panel A and liberal vote in Panel B. Standard errors are robust and clustered by voterid and reported in brackets. Statistical significance is signified by asterisks, where * denotes 10%, ** 5% and *** 1% significance level.

Tab. 6.9: Results for pooled data of new constituencies, with occupational information.

Figures 6.7 and 6.8 show the estimates of the dynamic DID estimation, discussed in 5.2.1. The reason we do this estimation, is to confirm that our assumption of the parallel trends holds, before our treatment period. If the parallel trends assumption holds, we should see null results for our coefficients before the base year of 1859. Based on the

Figures provided below, our assumption of parallel trends holds for the estimation of split voting rates, but not for liberal vote shares. This is already somewhat evident in Figure 6.4, which shows the changes in split voting rates and liberal vote shares between elections.

Because of the failure of parallel trends assumption, our results regarding liberal vote shares, cannot be interpreted as causal. Our finding suggests that working-class and middle-class voters' voting of liberal candidates has not evolved along the same trend, and thus the observed difference in voting for liberal candidates after 1865, cannot be reliably attributed to our treatment. However, the results for split voting rates seem to have evolved along the same trend between working-class and middle-class voters, and thus the findings for split voting rates can be interpreted as causal.

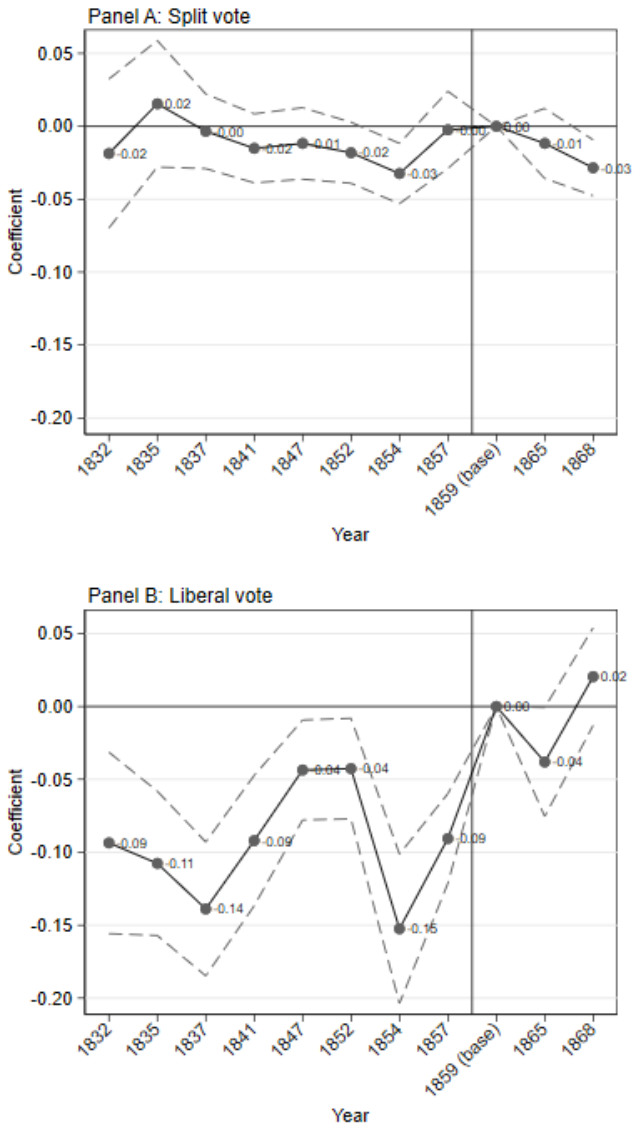


Fig. 6.7: Differences-in-differences estimation test for parallel trends for all constituencies.

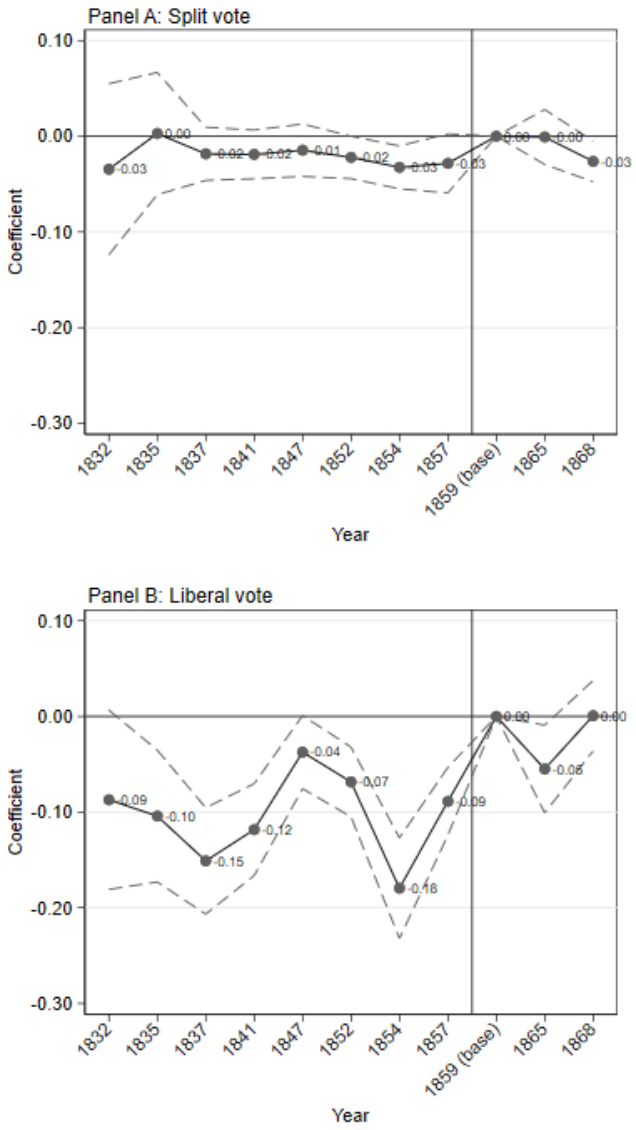


Fig. 6.8: Differences-in-differences estimation test for parallel trends for new constituencies.

6.4.1 Pooled results for the matched data

Table 6.10 shows the pooled results for the constituencies with no occupational information. In Panel A, we find null results across all specifications, except for specification (1), where we see a statistically significant reduction among non-working class voters before year 1865. In Panel B, we also find null results, except for our main variable of interest in specification (8), where we find a positive statistically significant relationship between voting for liberals and working class status, subsequent to 1865.

In Table 6.11, we have the pooled results for the constituencies that had no occupational information, and we have removed the top 150 most common names observed in the data, to reduce the distortion of *John Smith*-observations. Here in Panel A, we start to see some significant effects relating to working class status and split voting before 1865, but the size of these effects is very small. In Panel B, we observe no statistically significant ef-

fects. As already discussed in 4.2.2., the removal of most of the *John Smith*-observations, doesn't fix the underlying class distribution, which is most likely significantly different after matching.

It is notable, that constituencies of Norwich and York represent 64.8% of the total matched sample and 59.0 % of the total matched sample with top 150 names removed. This means that results of Norwich and York, largely drive the pooled results as well. Another notable observation is the difference in the sample sizes before and after removing the top 150 most common names. The top 150 most common names account for 48 635 observations, which is 41.1% of the total observations. In total we have 12 775 unique names in our pooled data. In other words, the top most common percent (150 / 12 775 = 0.011) of the names represents over 40% of the total sample.

Finally in Figure 6.9., we have the graphs for levels of split voting and liberal vote shares for the matched data. The null results are once again highlighted, as we observe almost no difference in the voting behavior of middle and working class.

Pooled Results for the New Constituencies with no Occupational Information

Panel A: New Constituencies Split Voting				
	(1)	(2)	(3)	(4)
Working class	-0.006	-0.001	-0.005	0.001
	[0.004]	[0.004]	[0.003]	[0.003]
Year≥1865	-0.089***			
	[0.006]			
Working class ×Year≥1865	-0.003	-0.010**	0.003	-0.008**
	[0.005]	[0.004]	[0.004]	[0.003]
Constant	0.145***			
	[0.005]			
N	118206	118206	118206	118206
Panel B: New Constituencies Liberal Vote Shares				
	(5)	(6)	(7)	(8)
Working class	-0.002	-0.003	-0.001	0.001
	[0.006]	[0.006]	[0.005]	[0.005]
Year≥1865	0.014			
	[0.009]			
Working class ×Year≥1865	0.010	0.010	0.008	0.009
	[0.008]	[0.007]	[0.007]	[0.006]
Constant	0.390***			
	[0.007]			
N	118206	118206	118206	118206
R^2	0.00	0.03	0.04	0.06
Year FE		✓	✓	✓
Constituency FE			✓	
Year-Constituency FE				✓

Note: Outcome variable is a dummy for a split vote in Panel A and liberal vote in Panel B. Standard errors are robust and clustered by voterid and reported in brackets. Statistical significance is signified by asterisks, where * denotes 10%, ** 5% and *** 1% significance level.

Tab. 6.10: Pooled results for the new constituencies with no occupational information.

Pooled Results for the New Constituencies with no Occupational Information and 150 Most Common Names Removed

Panel A: New Constituencies Split Voting				
	(1)	(2)	(3)	(4)
Working class	-0.008*	-0.005	-0.006	0.002
	[0.004]	[0.004]	[0.004]	[0.004]
Year≥1865	-0.080***			
	[0.005]			
Working class ×Year≥1865	0.000	-0.006	0.003	-0.006
	[0.005]	[0.005]	[0.005]	[0.004]
Constant	0.138***			
	[0.004]			
N	69571	69571	69571	69571
R^2	0.02	0.04	0.09	0.21
Panel B: New Constituencies Liberal Vote Shares				
	(5)	(6)	(7)	(8)
Working class	0.000	-0.000	0.003	0.005
	[0.007]	[0.007]	[0.007]	[0.006]
Year≥1865	0.002			
	[0.009]			
Working class ×Year≥1865	0.011	0.012	0.009	0.007
	[0.009]	[0.008]	[0.008]	[0.008]
Constant	0.404***			
	[0.008]			
N	69571	69571	69571	69571
R^2	0.00	0.03	0.04	0.05
Year FE		✓	✓	✓
Constituency FE			✓	
Year-Constituency FE				✓

Note: Outcome variable is a dummy for a split vote in Panel A and liberal vote in Panel B. Standard errors are robust and clustered by voterid and reported in brackets. Statistical significance is signified by asterisks, where * denotes 10%, ** 5% and *** 1% significance level.

Tab. 6.11: Pooled results for the new constituencies with no occupational information and top 150 most common names removed.

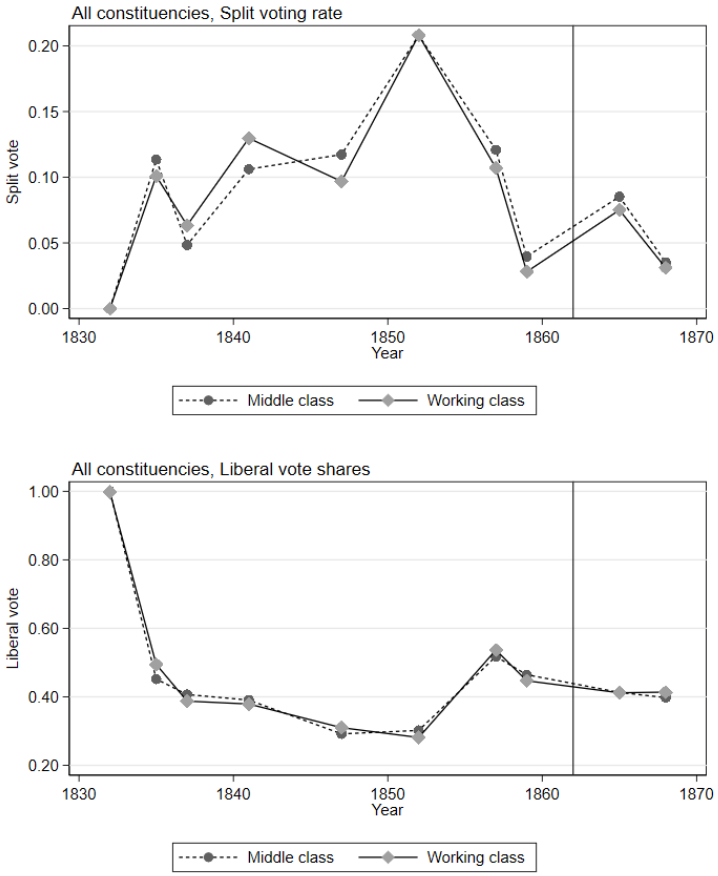


Fig. 6.9: Graphs of pooled matched data.

7. Conclusions

In this study we have empirically examined the change voting behavior in 19th century England, which served as the beginning of a new political era in which parties are the main wielders or political power, not individual candidates. We provide new evidence on the nature of this development by using previously unanalyzed poll book data from 13 new constituencies. This poll book data allowed us to analyze individual-level changes in voting behavior without fear of fallacious ecological inference, which researchers normally need to be concerned when working with aggregated data. Digitized poll books are a very unique source of data in voting research from a time before the introduction of secret voting. Our work is a continuation of the study by Dewan et al. (2020), in which the authors gave evidence on the emergence of a party oriented electorate before the Second Reform Act of 1867 and Secret Ballot Act of 1872.

In our analysis we have reproduced the findings of Dewan et al., by reconstructing their sample of three constituencies and adding our own classification of the occupations. We also have expanded this analysis by using data from 13 previously unanalyzed constituencies. The findings from our new data in large part corroborate those of Dewan et al. Our results only differ in the magnitude of the effects we observe. This is most likely explained by the fact that the three original constituencies were not a representative sample of the English electorate. Our new sample might also suffer from problems of representation, but the number of observations we have is almost four times larger than the one used by Dewan et al. Our sample is also much more balanced in the sense that our results aren't mainly driven by a single constituency, unlike those of Dewan et al. However, our findings still support and strengthen the claim that party oriented electorate was born in England, prior to the Second Reform Act of 1867. This is evidenced by the fact that we do observe a statistically significant drop in rates of split voting, especially among the working class voters, already in the general election of 1865 and subsequent elections. To us, this is evidence of class alignment with the two major political parties, which had become more organized from late 1850's onward.

Unlike Dewan et al., we cannot comment if this alignment was more focused towards the Liberals or the Conservatives, since we don't observe parallel trends in the voting behavior between the working-class and middle-class when we study the vote shares of liberal candidates. This lack of parallel trends makes our difference-in-difference analysis unreliable for causal inference, since we cannot say if our treatment period was the cause for our observed change. Still, we agree with the main thesis of Dewan et al. (2020), which states that this observed alignment was the result of programmatic appeals. This explanation seems entirely plausible in the light of previous literature, which has shown that industrialization and urbanization increased the demand for public goods among all classes of society (Camp et al., 2014; Lizzeri and Persico, 2004), which only parties could provide after the political power of an individual MP was diminished.

Our sample of 13 previously unanalyzed constituencies included 7 constituencies, which did not have occupational information recorded in the poll books. In our attempt to account for this lack of data, we matched the voters from these 7 constituencies, and their poll books, to census records from 1851 and 1861. Due to lack of compatible data, we were only able to use the names of the voters in the matching procedure which intro-

duced a novel problem, which we call the *John Smith*-problem, in which the data from these 7 constituencies was polluted by additional observations. We attempted to account for this problem by applying the matching procedure to 3 constituencies, for which the true class distribution was available, which showed to us that the distribution of votes and class shares after matching, did not reflect the true distribution from the poll book data. Thus, the results for the 7 constituencies, which required matching, cannot be used to make reliable inferences about the underlying changes in voting behavior. The *John Smith*-problem is an interesting mathematical challenge, which we believe is solvable, but unfortunately beyond the scope of this study.

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