

Political Dynasties and Elections

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Declaration

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I confirm that Chapter 4 was jointly co-authored with Dr Samuel Berlinski and Professor Torun Dewan and I contributed 60% of this work.

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Abstract

This dissertation studies political dynasties in democratic countries. Dynasties are common in all professions. However, for the profession of politics, in which succession depends no longer on dynastic succession but on running successful electoral campaigns, understanding how and why political power can be bequeathed is particularly important. Factors such as name recognition (the voter demand side) and political networks (the elite supply side) are potential explanations of the continued presence of dynasties in parliaments. This dissertation studies both the voter demand side and the elite supply side of the phenomenon. I first discuss the related literature on political dynasties, political selection, political quality, and the personal vote. Voting for dynasties can be rational, and the presence of dynastic legislators perfectly legitimate. Political dynasties may thrive in electoral systems that encourage personal voting, such as is used in Belgium. In a first paper, I show that in the Belgian 2010 General Election voters preferred dynastic candidates. Institutional changes may change such (dynastic) elite equilibria. In a second paper, we exploit the constituency-level variation in the franchise extension associated with the Second and Third Reform Acts in Britain. However, we find no effect of these reforms on the position of dynasties or the aristocracy in politics. Changes to the political career of legislators may also affect their chances of establishing or continuing a dynasty. The third paper studies dynasties in the UK House of Commons. I employ random variation in tenure length introduced by winning vs. losing a first re-election by a narrow margin. Surprisingly, I find no effect of tenure length on an MP's chances of establishing a dynasty in the nineteenth century. However, selection into cabinet is more likely if the MP had a relative in the cabinet before.

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Chapter 1

Introduction

Dynastic political succession is not usually considered to be democratic. Political dynasties are mostly thought to thrive in non-democratic societies. However, there are also many examples of political dynasties in democracies. Are there structural factors that encourage dynasty formation? Can institutional changes break existing dynastic equilibria? This PhD dissertation is the result of my study of the phenomenon of political dynasties in democracies. The three papers that the dissertation consists of are each concerned with the study of one aspect of the mechanism that explains their continued electoral success.

Previous work has documented and studied the presence of political dynasties in a handful of countries. Only recently have studies placed political dynasties at the centre of their investigations and moved beyond describing particular families. Yet researching broad empirical patterns in dynastic representation can help us to look beyond formal institutions and understand how democratic selection functions in practice. To research the patterns we need to have better descriptions of individual countries. At the same time the difficulty with explaining the presence of political dynasties, or why some countries seem to have more of them than others, lies in attributing their survival to factors that we can measure and test in isolation. A number of recent papers have exploited natural experiments and exogenous variation in tenure length and wealth to study the causes of political dynasties in democracies. On par with the current state of the literature on political dynasties, my dissertation provides a description of the presence of political dynasties in two countries, Belgium and the UK, while I also aim to identify part of the causal mechanism that underlies their continued success.

In the first paper of the dissertation, I investigate whether voters prefer dynastic candidates. Previous work has studied countries with single-member districts. Yet if voters have no choice between different candidates from their preferred party it is impossible to distinguish a dynastic preference from their party preference when citizens vote for their party's dynastic candidate. Dynastic persistence can then be driven by strategic party nominations rather than voter preferences. Therefore, I decided to study Belgium, a country that employs an electoral system of preferential list proportional representation. I collected a novel dataset about the links of all candidates to previous legislators, for the most recent Belgian General Election of 2010. In the first paper, I study the electoral advantages that junior dynastic candidates enjoy by considering the number of preference votes they received in this election. Concurring with the previous literature, I conclude that voters hold a dynastic preference.

Next, I considered what factors may change dynastic persistence over time. To study these questions I cleaned and further extended a biographical dataset of the Members of the UK House of Commons since 1832. This data was used in the next two papers that form part of this dissertation. Surprisingly, I found no evidence that either large changes in democratic institutions or small changes in an individual legislator's career had an effect in changing the destiny of the UK's political dynasties.

The second paper of this dissertation considers the effect of franchise extension on the electoral success of the British aristocracy in the nineteenth century. This paper is collaborative work with Dr Samuel Berlinski and Professor Torun Dewan. Exploiting the sharp change in the electorate caused by franchise extension, we separate the effect of the Second and Third Reform Acts from that of underlying constituency level traits correlated with the voting population. We find that the franchise extension following the reforms had little effect upon the political position of the aristocracy, the elites and the political dynasties.

Finally, the third paper considers the political dynasties of the UK House of Commons over time. Results from a regression discontinuity design show that serving longer did not affect an MP's probability of establishing a political dynasty. However, a comparison of MPs over time shows that junior members of political families are more likely to enter the cabinet if they are relatives of a previous cabinet minister.

In the next chapter, I will present a broad overview of the core literature on political dynasties. The context-specific literature on political dynasties is small and discussed

in each of the separate papers. I will also discuss a selection of two broader and related strands of literature on the personal vote and on political quality. The next chapters will in turn present the three empirical papers of this dissertation.

Chapter 2

Literature

2.1 Political Dynasties

Dynastic politics in democracies strikes many people as a puzzling phenomenon. Public and scholarly interest in the topic has always been large. Concerns that the political class may become hereditary if not in law then in practice were already raised at the start of the twentieth century by such authors as [Mosca \(1939\)](#) and [Michels \(1968\)](#). A political class can be self-perpetuating if it can use democratic institutions to control entry into politics. However, it is also possible that a vocation for public service is instilled in children of politically active parents. These individuals may be more motivated to enter into politics. Voters may understand better what policy choices they will make. They may even consider them more talented and up to the task. While certainly more benign and probably very real explanations for the continued hereditary politics in democracies, these arguments fail to be entirely convincing. Why do we then observe such diversity in the presence of hereditary politics across democracies? The difficulty in studying hereditary politics lies in teasing out which part of the dynastic equilibrium can be attributed to a preference for this type of politicians from voters, and which part can be attributed to political strategies that will necessarily depend on the electoral environment.

The first paper to exploit exogenous variation in tenure length to identify dynastic entrenchment is the seminal paper by [Dal Bó, Dal Bó and Snyder \(2009\)](#) about the US Congress. They consider all US legislators over the period between 1789 and 1996. In

this period about 9% of Congress had a relative following or preceding them. Using regression discontinuity and an instrumental variable approach the authors identify a dynastic incumbency effect, or what they call a *power-treatment effect*: having served in Congress for more than one term almost doubles a politician's chances of having a relative in office after him. The fact that exogenous shocks to dynastic power (winning or losing by small margins) affect the political chances of junior family members indicates that power is self-perpetuating: shared political capital (e.g. fixed traits such as talent or drive) cannot be the only explanation of the presence of political dynasties. These results are consistent with a theory that inheritable time-varying advantages from power, such as local political connections or name recognition, explain the presence of political dynasties. Comparing dynastic and non-dynastic legislators, they find that the former are less likely to have gained previous public service experience, are less likely to represent another state than the one they were born in and are less likely to start their careers in the House rather than in the Senate. They also find that more political competition is associated with fewer political dynasties. Finally, they show that the dynastic prevalence in politics is extraordinary: using the General Social Surveys, and controlling for each profession's share in the population, they compile a dynastic bias index which reflects the odds that father and son share the same profession compared to a baseline in which a son's profession is independent from that of the father. They calculate that the dynastic bias is largest for the group of legislators, almost ten times stronger than the dynastic bias for the second most dynastic occupation in the group, economists, and almost fifteen times stronger than the bias for the third most dynastic group, doctors. The authors point out that the external validity of their results should be tested in different cultural and institutional contexts. The third paper of this dissertation is closely related to this study, but my results are surprisingly in contrast to theirs. I find no effect of tenure length on the probability of establishing a dynasty. I suggest that this difference could be explained by the different starting positions of the US and the UK. Paradoxically the fact that serving longer facilitated dynasties in the US but not in the UK, may underline how, at least initially, there was less competition from existing political elites in the US to block new entry.

Another contribution exploits exogenous variation in tenure length following a natural experiment that occurred in Argentina at the time of return to democracy (Rossi, 2009). In 1983, three hundred new members of parliament and senators were elected

for the bicameral Argentinean parliament. However, the constitution specified that one half of the House was to be renewed every two years and a third of the Senate every three years. So, to decide which half (respectively third) would be renewed first, the newly elected members of parliament and senators were randomly assigned different tenure lengths. Rossi uses this random assignment in tenure length to instrument total tenure length. In the second stage of a two-stage least squares regression he uses the instrumented total tenure length to predict the possibility of having a relative entering Congress afterwards. In this way he concludes that five additional years in office (the average tenure length for legislators in Argentina) are associated with an 8% increase in the possibility of having posterior relatives entering Congress. He also finds evidence in favour of the name recognition channel. One additional year of tenure has a positive and statistically significant effect on having a relative with the same surname entering Congress afterwards, but little or no effect on having a relative with a different surname. Also, when interacting tenure length with a dummy for common surnames (one when a surname belongs to the ten most common surnames in Argentina), the positive effect of one additional year of tenure disappears. In my first paper I also show how name recognition matters, but to members of the political dynasty only. Other candidates lucky enough to share the same surname with a political dynasty, but for whom I could not find evidence that they were related, do not receive electoral advantages. Finally, it is not so straightforward to compare the estimates from [Rossi \(2009\)](#) to [Dal Bó, Dal Bó and Snyder \(2009\)](#), because the latter estimate the effect of serving for more than one term (estimated to increase the probability of establishing a dynasty by between 2.5 to 8.3% depending on the specification), while the former estimates the effect of an additional year in tenure length. At first sight these results may seem very similar, but the different operationalisation of total tenure length in the models may obscure an interaction with the institutional setting.

A similar research design established a causal effect of winning a first election on dynastic perpetuation by considering all winners and losers of congressional and gubernatorial races in the Philippines since 1946 ([Querubin, 2010](#)). Interestingly, and similar to the results of the third paper of this dissertation, he finds no additional effect of winning a first re-election. In that sense, his paper establishes a true effect of power-treatment, while studies of incumbents measure rather an effect of holding that power for a considerably longer period of time. These different results may well point at

different mechanisms of dynastic perpetuation. More precisely, when holding power shortly affects an individual's probability of establishing a dynasty we are probably measuring more the power of obtaining the right political connections as a result of election, while the effect of serving longer would rather seem to emphasise the importance of slowly accumulated "political capital", such as name recognition or influence within the party.

All these papers have in common that they identify a dynastic perpetuation effect. However, they cannot say much about whether name recognition (the voter demand side) or political networks (the elite supply side) are responsible. Other papers have considered some part of the mechanism in more detail, though often with no clear identification strategy. One exception is a study by [Rossi \(2011\)](#) about the role of wealth. Besides name recognition and networks, wealth would seem to be an important supply side factor in explaining dynastic persistence. He uses the distance of plots of land distributed to families in the sixteenth century to the then formally established city of Buenos Aires as exogenous variation in wealth, and finds that families that received plots closer to the city are still more likely to have relatives in politics.

Recently interest in the topic of political dynasties has grown. [Bohlken and Chandra \(2013\)](#) investigate dynasties in India and ask to what extent dynastic success can be explained by the party's promotion of these candidates. They find that dynastic candidates with relatives in politics before them are more likely to be re-nominated by their parties, and when they are re-nominated by their parties they also receive a higher vote share in the election. They argue that party organisation matters. Sequential dynasties (dynasties whose members follow one another but do not serve together) are more prominent in the parties with weak leadership, while concurrent dynasties are more common in parties with strong leadership. They argue that family ties delineate a party faction. Weak leadership would be hesitant to give these family factions a lot of influence, while a strong leadership would be less hesitant to distinguish between the two types of dynasties (and also have less dynasties overall). Their argument is consistent with the positive association they find between an interaction of the number of concurrent family members and party membership of a national party and re-nomination. They argue that they go further in determining the mechanisms leading to dynastic persistence than the existing literature, by pointing out the importance of party organisation. While these factors are likely to matter, their argument suffers from reverse

causality concerns. In a paper similarly prone to reverse causality concerns, [Chhibber \(2011\)](#) for example argues that what matters in India is not so much the weakness of the party organisation as the fact that the national parties have centrally managed funds with which they support local candidates. The Congress party for example has a weak organisational structure, but strong leaders (the Gandhi family), and its funding is highly centralised. In the first paper of this dissertation, I also consider the role of parties and how they promote candidates by considering whether they give dynastic candidates better ranks on the party-district list. In contrast to the evidence for India, I do not find clear evidence that parties promote dynasties. For Japan, [Asako et al. \(2012\)](#) develop a model in which they assume that dynastic candidates have more bargaining power, and can channel more funds to their districts in this way. They argue that their empirical results are consistent with their model: districts represented by dynastic legislators receive more discretionary grants. To study the role of party organisation, party campaign funding and party factions in more detail is certainly a fruitful area for further research. Other recent work has considered potential results of dynastic politics. In his dissertation, [Rahman \(2013\)](#) documents the presence of political dynasties in Bangladesh and investigates the association between dynastic status and criminal records and corruption. He finds that dynastic legislators in Bangladesh are associated with worse attendance records, more criminal records and more corruption. He also looks at the effect of political assassinations and finds both in a dataset for Bangladesh and a cross-country dataset that political leaders are likely to be succeeded by relatives (right) after (successful) assassination attempts. [Ferraz and Finan \(2009\)](#) find that a measure of political concentration before the dictatorship in Brazil is associated both with more families in power and long-term developmental outcomes after the regime transition. Similarly, political inequality is associated with worse developmental outcomes in Colombia today ([Acemoglu et al., 2008](#)). Finally, [Feinstein \(2010\)](#) offers an overview of the past work on dynastic succession in the US Congress. In the empirical part of the paper, he considers the difference in electoral success between the democratic and the republican candidate in open-seat House races between 1994 and 2006. Controlling for the difference in experience, the difference in expenditures and district partisanship, he finds that the dynastic electoral advantage (of the democratic candidate over the republican candidate) is between 0.72 and 7.90% of the vote. Arguably, a three-point scale (zero if the republican candidate is dynastic, one

if neither candidate is dynastic and two if the democratic candidate is dynastic) is not the ideal way to test the dynasty effect and it may obscure a differential dynastic effect according to the party the candidate belongs to. It is also not so easy to interpret. Employing dummy variables instead seems a better test of the theory, although this would not offer one single dynastic advantage estimate.

My dissertation clearly contributes to this emerging literature on political dynasties. More related and context-specific research will be discussed in the individual papers. In the next sections, I will discuss the literature from two fields that I feel are strongly related to this topic, the personal vote literature and work on political selection.

2.2 The Personal Vote

Different electoral systems have often been thought to create different incentives for parties, candidates and voters. The extent to which candidates emphasise their party label, or their personal characteristics, may therefore vary. Dynastic status could be an important part of a candidate's electoral appeal. Pure candidate-voting is rare (almost all candidates belong to a party), so voters can rarely vote for candidates without voting for a party. However, some systems allow voters to vote only for some candidates of a particular party or to elect candidates from different parties (Karvonen, 2004). In these cases we can be more certain that if a voter prefers a dynastic candidate over another candidate from the same party that is because the voter has a dynastic preference (controlling for other individual factors of the candidates). Candidate selection in electoral systems with no intra-party choice is de facto determined by party organisations, because voters cannot disagree with the party's selection of candidates. In systems with intra-party candidate choice, voters have some influence over which candidates are selected. This essential difference between electoral systems has often been theorised to lead to different incentives for candidates and politicians (for a summary of these theories see Karvonen (2004)).

The definition of a personal vote is “that part of a candidate's vote that results from his or her own individual characteristics or actions, rather than from his or her party label” (Shugart, 2005, 46). Distinguishing a party vote from a personal vote is not always easy nor possible as some electoral systems do not offer voters any choice over candidates within or across parties. A theoretical contribution by Carey and Shugart

(1995) classifies existing and conceptually possible electoral systems according to their theorised effects on the incentive to cultivate a personal vote. Three electoral system variables are hypothesised to affect these incentives: (1) the degree of control party leaders have over ballot access, (2) the type and number of votes electors have, and (3) whether and how these votes are pooled. A fourth variable, district magnitude, is theorised to have an opposite effect on personal vote cultivation incentives according to whether voters can disturb the ranks of candidates on the list or not. In closed lists the incentives to develop a personal reputation decline with district magnitude and vice versa in open lists. Several other authors have complemented Carey and Shugart's theoretical classification with empirical tests. In previous work, I used data from the Comparative Study of Electoral Systems to test whether voters also respond to the personal vote cultivation strategies politicians are modelled to have. In line with their theory, I found that list type and district magnitude affect how people process information, i.e. whether or not they remember any names of candidates that ran in their constituency (Van Coppenolle, 2013c). I found that the effects on name recognition were stronger for non-voters than for voters. These results support an interpretation suggesting that the type of system shapes incentives for candidates to present certain types of campaign messages, and that voters as a consequence process information about these candidates differently. Other research has focused on studying this theoretical relationship between district magnitude and list type by focusing on the behaviour of politicians once elected. One example is the investigation of Stratmann and Baur (2002) into the behaviour of German legislators elected through either first-past-the-post or proportional representation rules. The former are much more likely to develop a personal reputation or link to their home constituency by choosing to sit on committees in which they can provide geographical services to that constituency. In contrast, legislators elected through PR rules are more likely to choose committees that treat issues that concern the entire nation. Hence, they are more concerned with the overall party reputation. Focusing on the links legislators maintain with their home constituencies is only one way of investigating whether personal reputations are developed. Another strand of research measures the extent of personal reputations by looking at such variables as legislative party cohesion, or campaign spending and corruption. For example, an investigation of the legislative votes of parliamentarians in nineteen countries shows that party voting cohesion is lower when intra-party competition is larger (Carey, 2007). Chang and

Golden (2006) show cross-nationally that countries with open (closed) lists and larger (smaller) average district magnitudes are related to a higher (smaller) index of corruption perceptions. They complement that argument with a sub-national investigation of Italy which had an open list PR system until 1994. They show that larger districts are associated with more corruption. In sum, in various contributions the electoral system has been found to influence politicians' incentives to develop a personal reputation and hence their behaviour. Besides behaviour, the attributes or identities of politicians may also prove valuable in distinguishing themselves from fellow candidates. According to the type of electoral system, it is possible that more or less candidates of a certain type decide to run in the first place or are elected more or less often. These issues are brought up by Shugart, Valdini and Suominen (2005) whose primary aim is to investigate whether being born in a constituency improves a candidate's chances of being elected. They can only look at elected legislators, but show nevertheless that in closed lists with rising district magnitude fewer legislators are native to the district they represent (and vice versa for open lists). Another strand of research that (indirectly) investigates particular characteristics is the literature on the representation of certain social groups such as ethnic minorities or women in legislatures (see for example Norris (2004)). The personal vote literature has been primarily concerned with studying the behaviour of politicians elected in various electoral systems (issues of moral hazard), but attention is now shifting to the type of candidates that are elected (issues of adverse selection). The personal vote literature is relevant to a study of political dynasties, because it can inform us where we would expect candidates to place particular emphasis on their dynastic status, where this characteristic could be most informative to voters, and why it may affect politicians' behaviour once in office. The effect of electoral rules on dynastic status could be investigated, and it can guide case selection in particular. The question of who presents themselves for election in the first place, which determines the pool of available candidates, is obviously also relevant for a study on political dynasties. The next section considers political selection which has been central to the broader literature on political quality.

2.3 Political Quality

In studying institutional effects, political scientists have been primarily concerned with issues of moral hazard, in other words how to incentivise politicians to behave once in office. Voters (the principals) have incomplete information about whether the politician they vote for will act out his or her promises, and refrain from "bad behaviour" like being involved in corrupt activities. However, voters can also be thought to be incompletely informed about the type of the politicians they elect, i.e. whether they are honest, trustworthy, or capable. The political selection problem can be thought of as an adverse selection issue. Certain institutional features may improve or exacerbate political selection. The concept of political quality can be used to describe the type of politicians. In a theoretical overview of these issues, [Besley \(2005\)](#) argues that the political quality or competence of legislators more broadly is relevant, because these characteristics may influence political outcomes. He argues for example that aside from equality issues the representation of women is important because it has been shown that these legislators give more attention to women's issues. He argues that electoral institutions are likely to play an important role in selection, but that the large variation in institutional rules may make it difficult to gain general insights, while there remains a huge amount to be done in terms of data collection about the qualities of political candidates. Indeed, most formal contributions have remained institution-free and empirical work is limited, probably because measuring talent, competence or honesty is not straightforward.

In the formal theory strand of the adverse selection literature, [Caselli and Morelli \(2000\)](#) derive a theory that departs from a pool of candidates with given quality. As more low-quality politicians run the social status of the profession declines, which discourages good politicians from running. As a result of the lag in political rewards path-dependency can also arise. Legislatures inherit much from previous (bad) cabinets (e.g. budget deficits) and often wages cannot be raised enough to make political office attractive to better candidates. These equilibria can persist even if voters have full information about the quality of the candidates. Other related formal models¹ derive different conclusions about how factors such as historical accident, wage increases or the type of political career can affect the quality of a legislator. However, they do

¹For other examples see [Mattozzi and Merlo \(2007, 2008\)](#); [Messner and Polborn \(2004\)](#).

not incorporate specific institutions and quality is perceived as a given characteristic of politicians. Which framework is more helpful to describe actual political selection and political quality equilibria should be derived from empirics. However, testing these models directly is difficult, because they do not incorporate much institutional detail. Moreover, their results are highly dependent on the initial parameters, which are often empirically unobservable.

The concept of political quality is difficult to define and measure. [Besley \(2005\)](#) argues that political quality can be thought of to consist of items of valence (everybody prefers more of these). Talent and honesty could be considered part of political quality, for example. Of course talent and honesty are also difficult to perceive, and politicians would have incentives to keep their true types hidden. There is a vast empirical literature that considers the representativeness of assemblies and that connects the identities of politicians to the policies they pursue in office, which is beyond the scope of this short literature review. Political quality has been measured by proxies such as tenure length and education. Using ordinal rankings of the effectiveness of North Carolina state legislators made by the North Carolina Center for Public Policy Research of about five hundred legislators, lobbyists and journalists, [Padró I Miquel and Snyder \(2006\)](#) find that the effectiveness of legislators rises sharply with tenure. They argue that at least part of the effect is also due to on the job learning. Other empirical contributions have looked at other characteristics that are perhaps more directly related to initial quality. [Besley, Montalvo and Reynal-querol \(2013\)](#) provide some evidence for the idea that the education of leaders matters for growth, by looking at leadership transitions after an unexpected death. Elsewhere, [Besley and Reynal-Querol \(2011\)](#) already argued that democracies select more educated leaders than autocracies. Hence, these authors give some support to the idea that characteristics (such as education) can serve as a proxy for the political quality concept and that these attributes also matter for outcomes (such as economic growth).

Political quality remains an elusive concept. One may wonder if education or tenure length are good proxies of the concept of political quality. Still, political quality can be a useful concept in thinking of why political dynasties persist. Voters may feel they have more information about the political quality of dynastic politicians than of other politicians, especially when voters have not yet had the opportunity to observe their behaviour in office. A related argument is that voters may more easily understand

the type of policies that the dynastic politician will pursue, which they might assume to be a continuation of those of the senior dynastic politician. In the next section, I will summarise how the research from this dissertation combines the insights from these different literatures.

2.4 Summary

In this dissertation I approach the phenomenon of political dynasties from a voter demand and an elite supply side perspective. I argue that institutions are likely to matter a great deal in the extent to which dynastic politics occurs in democracies. The three papers that form part of this dissertation contribute to and build on insights from the emerging literature on dynastic politics. I argue that intra-party competition between candidates is likely to determine how valuable being part of a political dynasty is in electoral competition.

From the personal vote literature, we may think that voters are likely to take specific characteristics of candidates into consideration when the electoral system encourages more competition between candidates of the same party. Dynastic status is a piece of information which is easy to confer to people (i.e. a shared family name). Surprisingly, all of the existing literature considers single-member district elections, whereas intra-party competition is likely to be important. In fact, by studying preference votes (as I have done in the first paper of this dissertation), we can be more confident that the vote we study measures the dynastic preference of voters, and not merely their party choice, where the dynastic preference could be fully explained by the nomination strategies of parties. In the next two papers I focus more closely on identifying part of the elite supply side of the explanatory mechanism. From the literature on political quality, we can take away that the identities of politicians matter for their policy choices. Equilibria of "bad politicians" are also likely to persist. These relatively institution-free theoretical models offer important insights, but we still need to measure their relevance by assessing how empirics corroborate their arguments. What happens to the political equilibrium between elites when institutional changes are introduced, such as the large franchise extension in nineteenth century Britain? Are parliaments the microcosms of societies we often tend to think they are? In the second paper of the dissertation, we considered these questions. In contrast to what we might expect, even a large increase

in the franchise does not seem to have changed the political equilibrium much. Finally, in the third paper of the dissertation I return to the question of whether the length of tenure can be identified to affect an MP's prospects of establishing a dynasty. Even when previous research has identified such an effect it has not been able to say much about what explains this estimate, whether the mechanism is name recognition (voters prefer dynasties) or political networks (the elite supply side). Disentangling these factors is challenging. In contrast to the current common belief that these effects are always likely to play a role, I find no evidence for such a causal effect of electoral success in the UK (see the third paper of my dissertation). However, I do find that having had a relative in cabinet before substantially increases one's chances to become a cabinet minister. Therefore, I conclude that not so much name recognition as political networks seem to have been important in the UK. Taken together, these three papers are related to many of the papers I have discussed in this literature review. The next sections will present each of the papers in turn.

Chapter 3

Political Dynasties and the 2010 Belgian General Election

3.1 Introduction

This paper studies the electoral success of junior members of political dynasties in the 2010 general election in Belgium. Relatives of previous legislators may have several electoral advantages. Some of these advantages could result from nepotism, but their success may also be explained by voter preferences. The electoral success of political dynasties necessarily depends on the electoral arena in which they compete. The continued success of dynastic candidates in Belgium may be encouraged by its electoral system, which allows for extensive intra-party choice over candidates. Rational voters could use heuristic shortcuts to choose among the many candidates presented by a party. The dynastic attribute in particular could form a convenient cue of the political quality of a candidate.

In Belgium's preferential list proportional representation system voters can choose to vote for the full party list or to express a preference for particular candidates from the list. Candidates are pre-ranked on the party lists. A party list vote implies that the voter agrees with this ranking. However, voters can also disturb the party ranking and express as many preference votes for individual candidates as they like. After counting the ballots and distributing the seats among parties, candidates are elected if they have received the required number of preference votes. If a candidate's preference votes

are not enough to reach this quorum, the pool of list votes is used to top-up their total. The remaining list votes are then offered to the person on the next rank. Therefore, the party ranking is influential because the higher a candidate is placed on the list, the more likely it is that he or she will be elected. The preference vote is used extensively and candidates are regularly elected before their higher-ranked colleagues. Therefore, these preference votes can offer the key to attributing dynastic advantages to voter preferences. Compared to other settings and to party list votes, preference votes offer more information. They express a voter's preference for a particular candidate beyond the voter's party preference. This paper then considers the electoral advantage in preference votes for dynastic candidates.

In the 2010 general election candidates were elected from thirteen districts with different district magnitudes and different party list compositions. To determine whether candidates were dynastic, I compared their surnames to a list of previous members of government and of the parliaments of the national as well as of five regional assemblies. About 3% of candidates, and 10% of election winners, have at least one relative in politics. In addition, I compiled a range of individual characteristics about the candidates, such as their rank on the party-district list, their gender, the number of years of experience in national or regional assemblies they had accumulated in 2010, whether they held a ministerial portfolio in a national or regional government before at that time and whether they were running for the Chamber or the Senate. Finally, I also created a measure of media attention for a subsample of Dutch-speaking candidates.

I find that dynastic status is associated with receiving more preference votes as well as with an increased probability of being elected, with or without controlling for all these factors. I find no separate effect of dynastic status on list rank or media attention. This goes some way to addressing concerns about post-treatment bias for the regressions that control for these factors that may be influenced by dynastic status. While my approach cannot eliminate all potential omitted variables concerns, I argue that the most important confounding factors in the Belgian context have been controlled for: list rank, experience and media coverage.² Allowing for these two caveats, which this paper shares with most of the existing research on political dynasties, my results

²Unfortunately, I cannot control for campaign spending because the relevant information is not available. However, campaign spending is limited by Belgian law, so that potential different access to campaign funding is unlikely to challenge these results and conclusions much.

strongly suggest that dynastic candidates have an electoral advantage that stems from voters' expressed preferences for such candidates. There is no direct evidence that the dynastic advantage results from nepotism in the Belgian context.

3.2 Literature and Theory

This paper contributes to the literature on political dynasties. Considering US legislators, [Dal Bó, Dal Bó and Snyder \(2009\)](#) find that narrowly winning a first re-election increases the probability of having a relative in office afterwards by between 2.5% to 8%. Other recent work about dynasties has discovered positive electoral advantages for dynasties, but the sizes of the estimated advantages differ. For example, [Rossi \(2009\)](#) identifies a positive effect of the tenure length of senior family members on the electoral success of their juniors in Argentina. One additional term is estimated to increase the possibility of having a relative entering Congress afterwards by about 8%. [Querubin \(2010\)](#) studies all congressional and gubernatorial candidates between 1946 and 2007 in the Philippines. He finds that narrowly winning an election increases the probability of having a relative entering office by about 15%-22%. However, in [Van Coppenolle \(2013b\)](#) I found no effect of narrowly winning a first re-election for UK legislators. I propose that these different results may arise due to the fact that there were already deeply entrenched political elites in the UK, the supply side of dynasties. More generally, studying such variation in the presence of political dynasties across countries can give insight into why dynasties survive in democracies. In this paper, I study whether voters prefer dynastic candidates in Belgium, or the demand side. Voters may prefer dynastic candidates for a number of reasons. The most intuitive one is name recognition. Voters may prefer candidates who share a surname with an established politician. For dynastic candidates the brand of their surname can help to signal their political quality. Dynastic candidates can also have other advantages to recommend them to voters. For example, dynastic candidates could use their party connections to become more experienced, obtain positions in the government, or to be better ranked on the party-district list. Dynastic candidates may also attract more media attention. For the US, [Feinstein \(2010\)](#) estimates a brand name advantage for dynastic candidates of about 0.7 to 8 percentage points of the total vote in elections for the US Congress and Senate. Moreover, he finds no evidence in support of the hypothesis that

dynastic candidates can amass more campaign funds or are more experienced. This paper studies the intra-party variation in candidate characteristics. Similarly, I find an electoral advantage for dynastic candidates. Unfortunately, I could not consider advantages in obtaining campaign funding, as information about campaign spending per candidate is only publicly available for elected candidates (Maddens et al., 2006).³ Similar to Bohlken and Chandra (2013) I investigate to what extent dynastic success can be explained by the party's promotion of these candidates. In contrast to what they find for India, I find little evidence that Belgian parties directly promote their dynastic candidates in the nomination process, i.e. the party list ranking.

This paper also contributes to the country-specific research about political families and about the determinants of preference voting in Belgium. Candidates from more politicised backgrounds (including political dynasties) were found to be involved in politics and to be elected at an earlier age (van Liefferinge, Devos and Steyvers, 2011). Belgian mayors from politically active families would also have been younger on average when they first took up that position (van Liefferinge and Steyvers, 2009). In the context of the 2003 Belgian general election (for Flemish constituencies), celebrity status (a variable which included dynastic status) was found to affect the extent of media attention received by candidates (Van Aelst et al., 2008). Belgium is an interesting case to study political dynasties. With its many regional parliaments as well as its federal level, Belgium elects a large number of politicians. The approach in this paper of comparing candidate surnames to the surnames of previous legislators across these levels, can therefore be expected to identify many of the politically active families. Moreover, the Belgian electoral system employs preferential list proportional representation. Each election presents voters with large party-district lists with many candidates. Not all dynastic candidates are members of nationally famous political dynasties, there are also relatives of less well-known (or locally well-known) families. Previous work has argued that rational voters, when overwhelmed with choice between candidates from their preferred party, will use candidate attributes to guide their choice. Shugart, Valdini and Suominen (2005) argue that local candidates have just such an advantage. Similarly, dynastic status could provide voters with additional

³Campaign spending is limited to 5000 euros per candidate (2500 for substitute candidates) with a limited number of candidates allowed to spend a higher maximum amount (Maddens et al., 2006, 162), so it is unlikely that there was a lot of variance in campaign spending.

information. The perceived political quality of the senior dynastic politician can be used to infer the political quality of the junior candidate. In the next section, I will discuss the Belgian case and the data in more detail. I will also introduce the hypotheses that this paper sets out to test.

3.3 The Belgian Case and the 2010 General Election

The Belgian federal elections were held in June 2010 to elect 150 members for the parliament's Chamber and 40 members for the Senate. For the Chamber there are eleven constituencies, and there are two different regional constituencies for the Senate. A schematic illustration of the constituencies and district magnitudes can be found in Figure A.1 in the appendix. To study whether voters prefer dynasties, Belgium is a particularly interesting example as voters have extensive intra-party choice over candidates. Voters first choose a party ballot in their district. These party-district ballots contain the names of a number of candidates, ranked from 1 (the top-ranked candidate) to n . Voters can then choose whether to agree with the order in which the candidates were ranked by extending a list vote, or whether to give one or more preference votes to particular candidates from one party-district list. The number of seats a party has won in a particular district is determined first, based on the total number of ballots received. Then, these seats are distributed among the candidates from the party-district list. List votes are distributed to candidates in order of their rank. These votes are used to top-up the preference votes the candidate has received until the electoral quorum, the number of votes needed to be elected, is reached.⁴ The remaining list votes are then transferred to the second candidate until he or she has reached the quorum, and so on. In general therefore, the chances of being elected become smaller the further down the candidate is ranked on the list. The preference vote was used extensively in the 2010 election. On average about 56% of ballots included one or more preference votes for particular candidates by party-district list (with a standard deviation of about 14%). Extending preference votes is also useful: it is possible to disturb the party-district ranking. For example, about 24% of the elected candidates were elected before a higher-ranked col-

⁴The electoral quorum or the number of votes a candidate needs to have obtained to be elected depends on the number of seats assigned to the party-district list and the total number of ballots for the list.

league, as a result of the preference votes they received.⁵ The possibility of extending a preference vote allows us to distinguish more clearly the voter preference for dynastic candidates. If dynastic candidates are primarily successful because the party promotes them above other candidates, we should expect these candidates to obtain a better rank on the party-district list, i.e. closer to rank number 1. If on the other hand voters also prefer these candidates, we should expect them to receive more preference votes.

3.4 Data and Hypotheses

The dataset contains information for all candidates running in the 2010 general election in Belgium.⁶ Information was collected about each candidate's name, party, electoral district, rank on the district list, gender, number of preference votes received, whether he or she ran for the Chamber or for the Senate and whether or not he or she was elected. In order to determine whether a candidate is dynastic, I used a surname-matching approach. First, I collected information about all members of parliament for the national parliament (Chamber and Senate) and for regional parliaments (Brussels parliament, Flemish parliament and Walloon parliament)⁷ over a period of fifteen or more years. For the national parliament, information was readily available on its website for all its members from 1991 (Chamber) and 1995 (Senate) onwards. For the

⁵When disregarding the candidates ranked last on the list, the traditional "list pushers" who are often well-known politicians, the percentage of candidates elected before their higher-ranked colleagues is still 16%. Arguably, this shows that preference votes are important in determining who will be elected in the Belgian context.

⁶To manage the data collection process, I excluded parties who did not win any seats, as well as the Parti Populaire, a new party that won only one seat in the Chamber. Party lists in Belgium contain two sub-lists, one with "real" candidates and one with "replacement" candidates. If an elected candidate decides not to take up his or her seat, or quits parliament at any point during that legislative session, the next in line of the "replacement" list takes up that seat (rather than the next candidate on the list of "real" candidates). This paper reports only the results for the subgroup "real" candidates. While the effect of variables such as rank differs for both groups of candidates, the main dynasty effect is robust to alternative specifications which consider both groups separately or together.

⁷There are two more regional parliaments that were not included in the analysis: the French-speaking and German-speaking community parliament respectively. No elections are held to configure the make-up of the former, but members are appointed to this assembly from the Walloon parliament. Hence, its members are included in the collection of members from the Walloon parliament. The members of the German-speaking community are elected, in contrast, but the assembly is so small that for practical purposes it was not considered in this analysis. These potentially additional dynastic politicians from the German-speaking region are highly unlikely to challenge the results of this paper.

regional parliaments, similar information was used from 1979 (Flemish and Walloon parliament) and 1989 (Brussels parliament) onwards.⁸ Then, a candidate's surname was matched to the surnames of these former members of parliament. If a match occurred, I searched for confirmation of this potential dynastic link, using information obtained from personal websites of the candidates or parliamentarians or from other sources (newspaper articles, Wikipedia, blogs, etc.) resulting from a Google search. If in this way no evidence at all could be found about the relation between surname-matched persons, candidates were not coded to be dynastic. Some systematic measurement error may have been introduced at this stage of the project, as one might imagine that there is a difference between candidates who gladly share information about their family background and those who go to great lengths of keeping it secret. However, in defence of my approach I argue that on average voters are unlikely to be better informed than based on what can easily be derived from campaign materials. Next, a comparison with the list of former parliamentarians also allowed identifying whether a candidate was a new candidate in the 2010 election, or whether he or she had already gained some previous experience in a regional or the national assembly. In the same way, a comparison with a list of former members of government⁹ allowed identifying those candidates that held a cabinet portfolio before. Finally, in order to control for media attention, a variable measuring the number of articles in which a candidate was mentioned in the online version of one of the most widely-read Flemish newspapers ("Het Laatste Nieuws"), between the day when parliament was dissolved until the day before polling day, was also compiled. Compiling this information is very labour intensive so that the investigation was limited to the online version of the most widely-read Flemish newspaper, for the subsample of Dutch-speaking candidates. Some summary statistics are presented in tables 3.1 and 3.2.

About 3% of candidates for the 2010 election, and about 10% of those that were elected, are junior members of political dynasties, i.e. they had a relative in politics

⁸In the early years of these regional parliaments members were not elected to these bodies, but appointed from the Chamber. Therefore, this information from the regional parliaments somewhat complements the information from the national parliament, which is only available from 1995 onwards, because many members of the national parliament can be expected to have gained experience in a regional assembly first.

⁹The executives from the national as well as regional parliaments were considered (with the exception of the German-speaking community executive) since 1999 (or since 2003 for the French-speaking community executive).

Table 3.1: Summary Statistics for the 2010 General Election Candidates (I)

	N	%
Dynastic	40	3.2
Female	616	48.5
New candidate	964	75.9
Candidate for the Chamber	1,036	81.5
Candidate for the Senate	235	18.5
Candidate with government experience	63	5.0
All candidates	1,271	100

Table 3.2: Summary Statistics for the 2010 General Election Candidates (II)

	N	Mean	Standard Deviation	Min	Max
Preference votes	1,271	11,247	31,108	3	785,776
Log (Preference votes)	1,271	8.43	1.35	1.10	13.57
Experience	1,271	2.16	5.22	0	31
Experience squared	1,271	31.38	107.07	0	961
Rank	1,271	9.95	6.33	1	25
Number of articles	991	1.15	4.46	0	64

log(PV) is the natural logarithm of the number of preference votes, rank shows the order in which candidates are ranked (from 1 to n), so higher numbers are lower on the list and have a lower probability of being elected. Number of articles measures the number of articles of the online version of the (Flemish) newspaper ‘Het Laatste Nieuws’ in which a candidate was mentioned. For the last variable N is smaller, because only a subsample of Dutch-speaking candidates was considered.

before them. Despite the low number of dynastic candidates (forty individuals), there is much variation between them in terms of experience, party, district, rank, gender. Some summary statistics for these variables are shown in tables 3.1 and 3.2.

The dependent variable of interest (number of preference votes) ranges from as little as three to more than seven hundred thousand preference votes, with a mean of about eleven thousand votes. Clearly then, the distribution of this variable is skewed, which warrants considering its natural log-transformation in the analysis instead.¹⁰

¹⁰The results remain when the dependent variable used is instead the percentage of preference votes of the total preference votes for the party in the district. Results are presented in the appendix.

In order to distinguish the dynastic name recognition effect on electoral success from other, potentially unfair, advantages I will present the results of the fitted model:

$$\ln(\text{preferencevotes})_{i,p,d} = \beta_1 + \beta_2 \text{dynastic}_i + \beta_3 Z_{p,d} + \epsilon_{i,p,d} \quad (3.1)$$

where i indicates an individual candidate from a party p in a district d . All models include fixed effects for party-districts, $Z_{p,d}$, and errors $\epsilon_{i,p,d}$ are clustered at party-district level. The main estimate of interest is β_2 , the effect of dynastic status on the log of preference votes received. If voters prefer dynastic candidates, this estimate should be positive. I will also present results from the estimation of 3.1 that includes a dummy *samename* as control. If there is a name recognition advantage that extends beyond the actual members of the political dynasty to candidates who share the same surname this estimate should be positive. Finally, to estimate whether any electoral advantage in preference votes also translates into a higher probability of being elected, I also estimate the following logit model, where the dependent variable *elected* is a dummy equal to one if the candidate was elected:

$$\text{logit}(\text{elected})_{i,p,d} = \beta_1 + \beta_2 \text{dynastic}_i + \beta_3 Z_{p,d} + \epsilon_{i,p,d} \quad (3.2)$$

Unfortunately, these models are not free from omitted variable concerns. Various omitted variables correlated with both dynastic status and preference votes might fully explain the dynastic electoral advantage. Therefore, I next control for the most obvious individual-level potential confounding factors: rank on the party-district list, previous participation in government, previous experience in parliament, gender, the type of election for which the candidate ran (Chamber versus Senate) and media attention.

However, a concern with the specifications that include these controls is that some of these variables (such as rank, experience or media attention) could be influenced by dynastic status. If this is the case, the estimate of the dynastic status β_2 will be biased. In spite of these concerns, it seems important to control for rank when analysing the dynastic advantage in preference votes. Candidates ranked higher on the party-district list tend to receive more preference votes, perhaps because these are the strongest

candidates, or because voters read the candidate names on the party-district list from the top down. In the same way, it seems important to control for the fact that dynastic candidates may have been advantaged by having participated in government before or having gained more experience as former parliamentarians. If a dynastic bonus in preference votes remains even after controlling for these factors, this is certainly suggestive of an effect, though its size may be biased. To address these concerns, I also consider the effect of dynastic status on each of the controls separately.¹¹ If part of the effect of being a dynastic candidate works through any of the controls we should find positive effects. For example, if rank is found to be negatively influenced by dynastic status, this would provide evidence that part of the dynastic advantage is due to a party preference for such candidates.

3.5 Results

Dynastic status matters. Dynastic candidates receive more preference votes than otherwise similar candidates (Table 3.3). In the baseline model (column 1 of table 3.3) the effect of dynastic status is to increase the number of preference votes by more than 60%. Once we control for rank, gender, experience, government experience and whether the candidate ran for the senate or not, the effect of being dynastic decreases much, but it is still estimated to increase preference votes by about 25%. This effect is statistically significant at any conventional level of statistical significance. In column 3 of table 3.3 a dummy indicating whether a candidate has the same surname as at least one previous politician is added. The size of the effect of dynastic status, which can be interpreted as the effect over and above holding the same surname, remains about as large as before - increasing preference votes by about 23%. The effect is still significant at the 5% level. The dummy for holding the same name is not statistically significant. The results are similar for the subsample of Flemish constituencies. After adding the additional control of media attention, the effect of dynastic status is estimated to be slightly smaller (17%-18%) but remains significant.¹² Again, a dummy

¹¹In the appendix, I present results of a matching approach. Matching will not address post-treatment bias concerns, but it can address concerns about model dependence.

¹²Media attention could be directed by voter preferences, so that controlling for media attention establishes a lower bound of the effect of voter preferences.

for candidates with the same name is not statistically significant. Therefore, we can conclude that the electoral advantage of a sharing the surname with one of the political dynasties does not accrue to candidates who are not part of the dynasty. These results suggest that voters have an outspoken preference for dynasties, rather than that they are passively swayed by a familiar name.

The estimated effect of dynastic status is very robust and its size remains remarkably stable, especially if we consider that only 3% of all observations in the dataset was coded to be dynastic. There is thus sizeable evidence of an electoral advantage for dynastic candidates in Belgian politics. Whether or not this difference in preference votes really matters in determining election outcomes depends on whether it also makes these candidates more likely to be elected. Table 3.4 shows the results of logit estimations of the odds of being elected.

According to the estimates of column 1 of table 3.4, we can say that the odds of being elected for dynastic candidates in Belgium are 3.25 times the odds for non-dynastic candidates, holding all other variables constant. The effect is statistically significant at the 5% level. The effect stays qualitatively similar if we include a dummy for same name candidates (column 2), while this dummy itself still falls short of reaching statistical significance. For the subsample of Flemish constituencies and after controlling for media attention (columns 3 and 4) the results are no longer statistically significant at any conventional level. Nevertheless, the size of the estimated coefficient remains remarkably similar to the estimate for the whole sample. The odds of being elected for Flemish dynastic candidates are about 3.4 times the odds for Flemish non-dynastic candidates, holding all other variables constant.

From the first two tables of results, we can conclude that the dynastic electoral bonus is substantial. Dynasties receive this bonus over and above other potential advantages, such as a better list rank or more media attention. To test whether these factors can partly explain why dynasties are successful, I next consider the evidence for whether dynasties benefit from three types of indirect advantages: list rank, experience and media attention. The results from an estimation of the effect of dynastic status on list rank are reported in table 3.5.

There is little evidence that dynastic status affects the rank on the party list. The estimated effect is small (less than two ranks) and not statistically significant at any conventional level of significance. Perhaps surprisingly, we can conclude that there is

Table 3.3: Results Dynastic Vote Bonus

log(Preference votes)	Belgium	Belgium	Belgium	Flanders	Flanders	Flanders
Dynastic	0.613*** (0.112)	0.249*** (0.081)	0.226** (0.089)	0.510*** (0.127)	0.178** (0.085)	0.171* (0.093)
Rank		-0.048*** (0.003)	-0.048*** (0.003)		-0.034*** (0.004)	-0.034*** (0.004)
Female		0.083** (0.041)	0.083** (0.041)		0.197*** (0.041)	0.197*** (0.041)
Senate		1.528*** (0.011)	1.526*** (0.011)		1.483*** (0.014)	1.482*** (0.014)
Experience		0.074*** (0.013)	0.074*** (0.013)		0.031** (0.011)	0.030** (0.012)
Experience squared		-0.002*** (0.001)	-0.002*** (0.001)		0.000 (0.001)	0.000 (0.001)
Government experience		0.817*** (0.086)	0.818*** (0.086)		0.213* (0.109)	0.215* (0.107)
Number of articles					0.044*** (0.006)	0.044*** (0.006)
At least one article					0.477*** (0.045)	0.477*** (0.045)
Same name			0.030 (0.044)		0.010 (0.044)	0.010 (0.044)
Observations	1,271	1,271	1,271	881	881	881

Column 1 shows the regression results for the log of preference votes for the whole sample without controls, column 2 includes control variables. Column 4 and 5 show results for restricted sample of Flemish constituencies, with column 5 including a control for media attention. Columns 3 and 6 include a dummy for sharing the same name with a previous politician. All models include party-district dummies. Errors are clustered at the party-district level. *** p<0.01, ** p<0.05, * p<0.1

Table 3.4: Results Dynastic Election Bonus

Elected	Belgium	Belgium	Flanders	Flanders
Dynastic	1.177** (0.525)	1.108* (0.596)	1.219 (1.113)	1.378 (1.241)
Rank	-0.471*** (0.086)	-0.472*** (0.087)	-0.441*** (0.137)	-0.442*** (0.136)
Female	0.182 (0.195)	0.187 (0.197)	1.198** (0.466)	1.204** (0.475)
Senate	0.765*** (0.269)	0.762*** (0.272)	0.222 (0.468)	0.262 (0.449)
Experience	0.295*** (0.086)	0.294*** (0.087)	0.141 (0.103)	0.146 (0.099)
Experience squared	-0.004 (0.004)	-0.004 (0.004)	0.003 (0.005)	0.003 (0.005)
Government experience	3.569** (1.528)	3.572** (1.520)	0.280 (1.016)	0.200 (1.036)
Number of articles			0.560*** (0.155)	0.566*** (0.160)
At least one article			2.905*** (1.005)	2.914*** (0.987)
Same name		0.093 (0.413)		-0.216 (0.596)
Observations	1,072	1,072	737	737

Columns 1 and 2 show the regression results for the log of the odds of being elected for the whole sample, columns 3 and 4 include a media attention control for restricted sample of Flemish constituencies. Columns 2 and 4 include a dummy for sharing the same name with a previous politician. All models include party-district dummies. Errors are clustered at the party-district level. *** p<0.01, ** p<0.05, * p<0.1

Table 3.5: Dynastic Rank on Party List

Rank	Belgium	Flanders
Dynastic	-1.654 (1.121)	-1.567 (1.147)
Female	0.090 (0.212)	-0.251 (0.296)
Senate	1.066*** (0.145)	1.441*** (0.190)
Experience	-0.673*** (0.119)	-0.279* (0.159)
Experience squared	0.026*** (0.005)	0.017** (0.007)
Government experience	0.613 (1.306)	3.185* (1.692)
At least one article		-0.225*** (0.053)
Number of articles		-4.318*** (0.781)
Observations	1,271	881

Results from OLS estimation. Column 1 shows the regression results. Column 2 presents the results for the restricted sample with an additional control. All models include party-district dummies. Errors are clustered at the party-district level. *** p<0.01, ** p<0.05, * p<0.1

Table 3.6: Dynasties and Experience

	Belgium Experience	Belgium Gov Exp	Flanders Experience	Flanders Gov Exp
Dynastic	3.157** (1.273)	1.228*** (0.473)	2.141* (1.108)	1.168** (0.534)
Female	-1.672*** (0.334)	-0.891*** (0.262)	-1.262*** (0.360)	-0.824** (0.364)
Senate	0.989*** (0.054)	1.303*** (0.036)	1.063*** (0.055)	1.311*** (0.042)
Observations	1,271	751	881	457

Column 1 shows the regression results for experience in years, columns 2 presents the logit estimation results for government experience. Columns 3 and 4 show results for the restricted sample. All models include party-district dummies. Errors are clustered at the party-district level.

*** p<0.01, ** p<0.05, * p<0.1

little evidence that dynastic candidates receive better list ranks, controlling for other factors.

A second potential indirect advantage could be the fact that the dynastic candidates in this sample are different from non-dynastic candidates in terms of the amount of experience they have been able to accumulate, in parliament or as a member of the government. The results of this estimation are presented in table 3.6. There is some evidence that dynastic candidates are on average more experienced than non-dynastic candidates, by about three years for all Belgian candidates and two years for Flemish candidates (significant at 5% and 10% levels respectively). At the same time, the odds that dynastic candidates have government experience are about 3.4 times the odds of non-dynastic candidates having government experience (3.2 times for the subsample of Flemish candidates). These associations indicate that there could be additional benefits to dynasties (more access to experience, faster promotions) which may work through their being better connected. However, this is not the only possible explanation: it could also be the case that dynastic candidates survive longer as a result of their dynastic electoral advantage.

A third potential indirect advantage could be increased media attention. Results of this final estimation for the subsample of Flemish candidates are presented in table

Table 3.7: Dynasties and the Media

	Nr Articles	Attention (Yes/No)
Dynastic	-0.075 (1.493)	0.315 (0.468)
Rank	-0.153*** (0.032)	-0.178*** (0.039)
Female	-0.592** (0.245)	-0.907*** (0.196)
Senate	-0.139 (0.226)	1.207*** (0.095)
Experience	0.289** (0.122)	0.477*** (0.070)
Experience squared	-0.012* (0.007)	-0.013*** (0.003)
Government experience	9.024*** (2.209)	2.608** (1.078)
Observations	881	862

Column 1 shows the regression results of number of articles, column 2 from an logit estimation of whether or not a candidate's name was mentioned in at least one article. Both models include party-district dummies. Errors are clustered at the party-district level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

3.7. Table 3.7 shows perhaps surprisingly that there is no statistically significant effect of dynastic status on either the number of articles in which a candidate is mentioned (column 1), or the odds that a candidate's name is mentioned in at least one article (column 2). As is to be expected, experience and whether or not a candidate was a member of a national or regional government before, do have a positive effect on media attention. Indeed, many of the previous members of government were mentioned in articles which considered topics related to their particular portfolios.

From tables 3.3 to 3.7 we can conclude that dynastic candidates are shown to have a substantial electoral advantage versus non-dynastic candidates, controlling for factors such as rank, gender, chamber and previous experience. This effect could work through the channel of the dynastic surname functioning as a brand for dynastic candidates, and not simply through name recognition because a similar advantage does not exist for those candidates who hold the same surname but are not part of the political

dynasty. The evidence for party promotion advantages is weak. While dynasties on average were estimated to hold better list ranks, these effects could not be statistically significantly distinguished from zero. Also no evidence was found that the average dynastic candidate receives more media attention than the average non-dynastic candidate. However, there is evidence that dynastic candidates have accumulated more experience in parliaments and governments.

When interpreting these effects we should bear in mind that these are descriptive associations. However, this paper has controlled for the most obvious potential omitted variables. Therefore, I conclude that dynastic candidates held electoral advantages in the 2010 Belgian election. Surprisingly, media attention and a better list rank, which could be indicative of additional advantages working through connections, were not found to be likely candidates of the mechanism.

3.6 Conclusion

There is evidence that voters prefer dynastic candidates over non-dynastic candidates. This indicates that dynastic candidates enjoy electoral advantages above and beyond additional advantages from their connections, such as easier access to experience. The electoral advantage of dynastic candidates is large: on average they receive 17 to 25% extra preference votes. Having a political background also really matters: their odds of being elected are more than three times those of similar non-dynastic candidates. The effect remains if I control for some measure of media attention.

When considering whether dynastic status also offers more indirect advantages, the evidence was less clear-cut. Dynastic candidates were on average not found to be associated with a better list rank or with more media attention. They are on average two to three years more experienced, and have a higher probability of having gained government experience. On balance, these factors may, but are not necessarily indicative of more nepotistic advantages. In light of this evidence, I therefore argue that Belgian dynastic candidates benefit primarily in a direct way from their dynastic status, in terms of a preference vote bonus. Dynastic candidates seem to benefit from their surname brand, in ways that candidates with the same name who are not part of a dynasty do not. This could indicate that voters perceive dynastic status as an indicator of candidate quality.

Chapter 4

Franchise Extension and the British Aristocracy

4.1 Introduction

Does the expansion of voting rights lead to elected assemblies that are a microcosm of the societies that they represent? Or are the background characteristics of the men and women elected to office unaffected by differences in the rules governing the franchise? The question is pertinent if, as recent evidence suggests, the identity of politicians affects their subsequent performance: studies of changes in mandated forms of representation in the developing world show that identity is causally related to different outcomes (Pande, 2003); and recent contributions in political science show that the background characteristics of elected MPs affects their performance (Galasso and Nannicini, 2011); whilst those of cabinet members affect how long they maintain their positions (Berlinski, Dewan and Dowding, 2007).

Establishing a relationship between franchise extension and the identity of elected politicians can, moreover, shed light on an intriguing puzzle in the study of political development. As noted by Aidt and Jensen (2009) there is a “growing consensus that the extension of the franchise contributed positively to the growth in government”. But it is unclear which, if any, political mechanisms links these factors. In principle there can be several. An obvious candidate is that newly enfranchised voters are more inclined toward leftist or reform parties. Yet neither Berlinski and Dewan (2011) or

Larcinese (2011) find evidence for such an effect with respect to massive increases in the English and Italian electorates in the nineteenth and early twentieth centuries, respectively.

Here we return to Victorian Britain and ask whether franchise extension lead to political institutions that were more open and inclusive. In particular we explore the impact of franchise extension on a politically powerful group: the British aristocracy. Britain is often seen as the exemplar case of gradual political development. Unlike France, where the fate of the aristocracy depended on the form of the constitution, Britain had an established constitutional monarchy following the passage of the Bill of Rights Act of 1689. Whereas in France the Constituent National Assembly of 1789 swept away the privileges of the first estate, (although, of course, not permanently), in Britain successive reform acts in the nineteenth century established a broadly representative democracy through gradual extension of voting rights to ordinary men and women. This provides an opportunity to separate the impact on aristocratic power that was due to change in the franchise from other aspects of constitutional reform.

The impact of franchise extension on the British aristocracy is disputed. On the one hand, Canandine argues, in *The Decline and Fall of the British Aristocracy*, that the first three reform acts, passed by Parliament in 1832, 1867, and 1884 respectively, coincided with a decline in the influence of this group in British political and economic life.¹³ This was a period of dramatic change in British society with rapid industrial and urban expansion. Such changes may have affected greater social mobility so that all professions became more open. Thus to support Canandine's claims we need to find evidence that change in the British political class, such as it was, was causally related to changes in the rules governing the franchise and not incidental to them.

On the other hand, Harold Laski published an article in the *American Political Science Review* in 1928 where he argued that

“The three reform acts of the nineteenth century had little effect upon the position of the aristocracy in politics. Policy may have changed, but the men who made policy came, in much the same degree, from the same origins as their predecessors.”¹⁴

¹³Canandine (1999)

¹⁴Laski (1928)

In making his case Laski looked at the composition of the English Cabinet between 1801 and 1924, focussing in particular on the personnel changes before and after the reforms. His statistics suggested that little changed between these years: between 1832 and 1866, the period that encompasses the First Great Reform Act, the percentage of aristocrats in the Cabinet decreases from 73 to 64%; whilst between 1867 and 1884, the period that covers the Second and Third Great Reform Acts, the percentage of aristocrats in the Cabinet decreases further from 64 to 60%. Whilst Laski's qualitative assessment is that these figures are evidence of no significant change such aggregate statistics may be misleading for two main reasons.

Firstly, Laski's claim rests on analysis of aggregate data. He does not consider that this picture might conceal considerable constituency level effects of franchise reform. Suppose that in areas most affected by reforms aristocrats were less likely to run for office and/or where they did run were less likely to be elected. Correspondingly, suppose that aristocrats were more likely to run in other areas (less affected by the reforms) and/or to be elected when they did so. Then an impression formed from looking at aggregate patterns in the data would be quite misleading, and the claim that the reform had no effect false. Secondly, as already noted, it is quite plausible that even the small aggregate changes noted by Laski were incidental to the three Reform Acts; this was, after all, a period of rapid social and economic changes that could have had an independent impact on the composition of the political class.

To deal with these issues we follow the empirical strategy deployed by [Berlinski and Dewan \(2011\)](#). They note that the effect of the Second Reform Act of 1867, in particular, was varied at the constituency level: urban boroughs were, by design, more affected than the rural districts; and, more importantly, whilst some urban boroughs saw a huge increase in the franchise others were largely unaffected. They exploit this cross-sectional variation, matching it to political outcomes of the 1868 and 1874 elections. They thereby show that areas most affected by reform were not more likely to elect Liberal candidates— hence the Liberal victory was incidental to the reform. Instead “the major impact of franchise extension was in increasing political competition at the constituency level.” Specifically incumbents were less likely to run and previously uncontested seats more likely to be contested in those areas most affected by reform. Here we take a step further in asking whether the constituency level variation in the impact of reforms had a causal impact on the political power of the British

aristocracy. We measure variation in power as the likelihood that a member of the aristocracy is returned from a given constituency.

Our data matches the constituency level difference in the number of registered voters per district with data that records the background characteristics of elected MPs. We consider changes from the Second Reform Act as well as changes from the Third Reform Act which was similarly varied at constituency level. Deploying relevant econometric techniques that allow us to separate the effect of the reform from underlying constituency level traits correlated with the voting population, as well as broader trends, we can then evaluate Laski's null hypothesis that franchise extension had no effect on the position of the aristocracy in British politics.

We first ask whether there is a direct causal relationship between the impact of franchise extension at the constituency level and the likelihood that a member of the aristocracy was elected. Specifically we ask whether constituencies most affected (treated) by reform were subsequently more or less likely to elect a member of the aristocracy as their representative. As noted by Cox (1987), the accountability of the Cabinet to the majority in Parliament was already well established by the end of the nineteenth century. Even if, as claimed by Laski, selection to the Cabinet did not change, outcomes could be different if the principal body changed its composition as a direct consequence of the reforms.

An additional channel is the ability of politicians to build political dynasties: even if franchise extension had no impact on aristocrats' immediate political fortunes it may have weakened their ability to pass on electoral seats to their offspring. This was an important element of aristocratic power during this period when Britain still had a bicameral system in which the Lords played a decisive role. As Canandine notes, the sons of aristocrats served in the Commons until such time as they could enter the upper house as a hereditary peer. Dynastic succession in the Commons was thus an important element of aristocratic power. We ask whether seats that were more affected by reform were subsequently less likely to elect the sons and daughters of those elected to office.

Finally, we explore directly the impact of franchise reform on the composition of the cabinet. It is possible that the same people were returned to power but that outcomes were different as they were now responsible to a changed electorate. One possibility that we do not look at is whether the type of bills proposed on the floor and voting on those bills changes as a direct consequence of the reforms. However,

[Berlinski and Dewan \(2011\)](#) show that voting on bills considered either side of the passing of the Second Reform Act in parliament was unaffected by its passage. A related hypothesis is that the areas where franchise extension had the largest effects were more likely to see their representatives, whether aristocratic or not, enter the cabinet and so increase their influence. Were this true then it could be seen as evidence of an increase in the influence of the unskilled working classes even though, as Laski claims, “the men who made policy came, in much the same degree, from the same origins as their predecessors.”

A major focus in this paper is the Second Reform Act of 1867 that, unlike previous and later reforms, did not compound franchise extension with other constitutional measures that would confound attempts to isolate its causal impact. In our analysis we look at a sample of boroughs where franchise reform was not associated with any change in the constituency boundaries or the number of parliamentary seats between the general elections of 1865 and 1868. Exploiting the constituency level variation in the impact of franchise reform in this sample we are able to isolate its subsequent effect on the 1868 parliament.

Our results from the years 1865-1868 for this sample show that, once we condition on fixed aspects of a constituency, there is no direct causal relationship between the growth of the franchise and the likelihood that an MP becomes a member of the Cabinet. Moreover, the constituencies that are most affected by the reforms were not more likely to elect an aristocrat to Parliament than those less affected by them. Nor does the data suggest that dynastic succession was related to the impact of the reforms. These results hold when we adopt a very narrow definition of aristocracy as in Laski’s paper, namely the sons of men possessing hereditary titles, or a broader definition of such elites that includes landowners and military and naval officers or rentiers.

As a further check we explore whether franchise extension was related to the wealth of politicians elected to parliament. Lacking good data on the individual wealth of British parliamentarians we instead exploit a well understood positive relationship between longevity and socio-economic status that is confirmed by numerous studies including recently by ([Piketty, Postel-Vinay and Rosenthal, 2007](#)). Specifically we ask whether the age at death of a parliamentarian elected in 1868 is causally related to the increase in his constituency level franchise. As shown in [Berlinski and Dewan \(2011\)](#) the newly enfranchised voters were the unskilled urban working classes. If the

inclusion of a large tranche of such voters in these constituency lead to differences on average in the socioeconomic status of those elected, then we should also expect this to be reflected in the age at death of those MPs. However, we find no evidence suggesting that constituencies more affected by reform were more likely to elect MPs of lower socioeconomic status.

Our analysis thus suggests that Harold Laski was correct, at least with respect to the Second Reform Act: in terms of the personnel of the British parliament and its executive body the reform act had no effect. Policies may have changed- this was an era of substantial Liberal reform - but this was not because parliament was less representative of established elites. In fact the newly enfranchised electorate were no less inclined to elect such elites to office.

Next we allow a longer time frame for the reforms to have an effect. We ask whether there was an impact of the Second Reform Act on those elected to office in 1874. We find little evidence for a delayed effect of the reform. We cannot make any strong causal claims, however, due to the introduction of the secret ballot in 1872 that confounds our analysis.

Finally, we consider the impact of the Third Reform Act which placed the counties on an equal footing to the boroughs in 1884. The effect of this reform was also varied at constituency level, but further electoral reform in 1885 changed many counties into single-member districts. However, in a restricted sample of unchanged constituencies we find again little evidence that the franchise extension changed the composition of parliament.

4.2 Electoral Reform in Victorian Britain

Elections in Britain in the Victorian period under investigation took place under the first-past-the-post voting system that is still in place. Whilst some constituencies were single-member districts, most elected two candidates and a few elected three and four. The constituency elections were contested by candidates who aligned with one of two major parties, the Conservatives and the Liberals. The Liberals brought together a loose coalition of Whigs, Radicals, and Peelites (a faction that had broken from the Conservatives) and by 1860 formed a cohesive parliamentary block. Following Lord Palmerston's death in 1865 the Liberals were led by William Gladstone. For the im-

mediate period preceding the elections of our investigation, Liberals had held the key ministries of government. However, between 1865 and 1868 the Conservatives formed a minority government, first under Lord Derby and then under Benjamin Disraeli.

The Representation of the Peoples Act, otherwise known as the Second Reform Act, was passed by Parliament on August 15th, 1867. As its name suggests it was the second major voting reform bill that transformed the political landscape in the Great Britain. The first major extension of the franchise in the UK took place in 1832. The Great Reform Act of that year introduced several measures that mitigated malapportionment: increasing representation in the industrialized cities, and taking away seats from the so-called “rotten boroughs” with small voting populations. The act also increased the male franchise to around 650,000.

The Second Reform Act, that became law in England and Wales in 1867, extended the franchise in the boroughs to all males over the age of 21 who were inhabitant occupiers, whether house-owners or tenants, and to male lodgers whose rent was at least £10 per year. A residence of at least one year in the borough was required and women were still unable to vote. In counties, the franchise was extended to holders of life interests, copyholds and leases of sixty years and more worth £5 per annum (from a previous threshold of £10) and to tenants occupying land worth £12 (from a previous threshold of £50 per annum). The Reform Act for Scotland was delivered in 1868.¹⁵ The Second Reform Act increased the number of eligible voters more in some constituencies than in others. The reform was also swiftly followed by the election of a new parliament in 1868. This provides us with a unique window of opportunity for identifying the impact of franchise extension. Later Reform Acts introduced the secret ballot (1872), placed the counties on an equal footing with the urban boroughs (1884), reduced the number of multiple member districts (1885), extended the franchise to all males (1918), and provided women with electoral equality (1928). Of these Acts only the Third Reform Act of 1884 had a similarly varied effect at constituency level. A general election took place in 1885, though only after the number of multi member districts was reduced. To identify the effect of the Third Reform Act’s franchise extension we will therefore need to exclude constituencies with changes in district magnitudes.

Table 4.1 presents the number of registered electors in 1859, 1865, 1868, 1874,

¹⁵Ireland had a reform act in 1868 but unlike in England, Scotland and Wales the impact of the reform on registered voters was marginal.

Table 4.1: Number of registered electors by year county and location

Year	England	Scotland	Wales	Boroughs	Counties
1859	900,059	105,608	56,033	491,446	570,254
1865	970,076	105,069	61,656	544,661	592,140
1868	1,880,368	231,376	127,385	1,369,848	869,281
1874	2,097,206	271,240	137,143	1,594,732	910,857
1880	2,338,809	293,581	149,838	1,783,876	998,352
1885	4,064,515	545,487	277,037	2,642,859	2,244,180

1880 and 1885 for England, Wales, and Scotland in both boroughs and counties.¹⁶ The Second Reform Act led to an increase in the franchise not witnessed either before or after the 1868 election. From 1859 to 1865 the registered electorate in England, Scotland and Wales increased by 7% and from 1868 to 1874 by 12%; this compares with a 97% increase between 1865 to 1868. Between 1880 and 1885 the introduction of the Third Reform Act further increased the electorate by about 76%.

After 1867 the new franchise rules allowed the registration of more than 1,000,000 new electors. The increase was more pronounced in the more densely populated urban boroughs where the number of registered electors increased (on average) by 152% with respect to a 47% increase in the counties. Indeed historians have noted that the most striking feature of the Second Reform Act was the unexpectedly wide extension of the franchise in the boroughs, when compared to the counties. Nevertheless we observe effects of similar magnitude with respect to the impact of the Third Reform Act in the counties between 1880 and 1885. The number of registered electors increased (on average) by 125% in the counties compared to a 48% increase in the boroughs.

¹⁶These figures exclude the electors registered in the university constituencies which were allowed to vote both in the university constituency and in their town of residence.

Figure 4.1: Changes and the Level of Enfranchised Population in Boroughs (1865-1868)

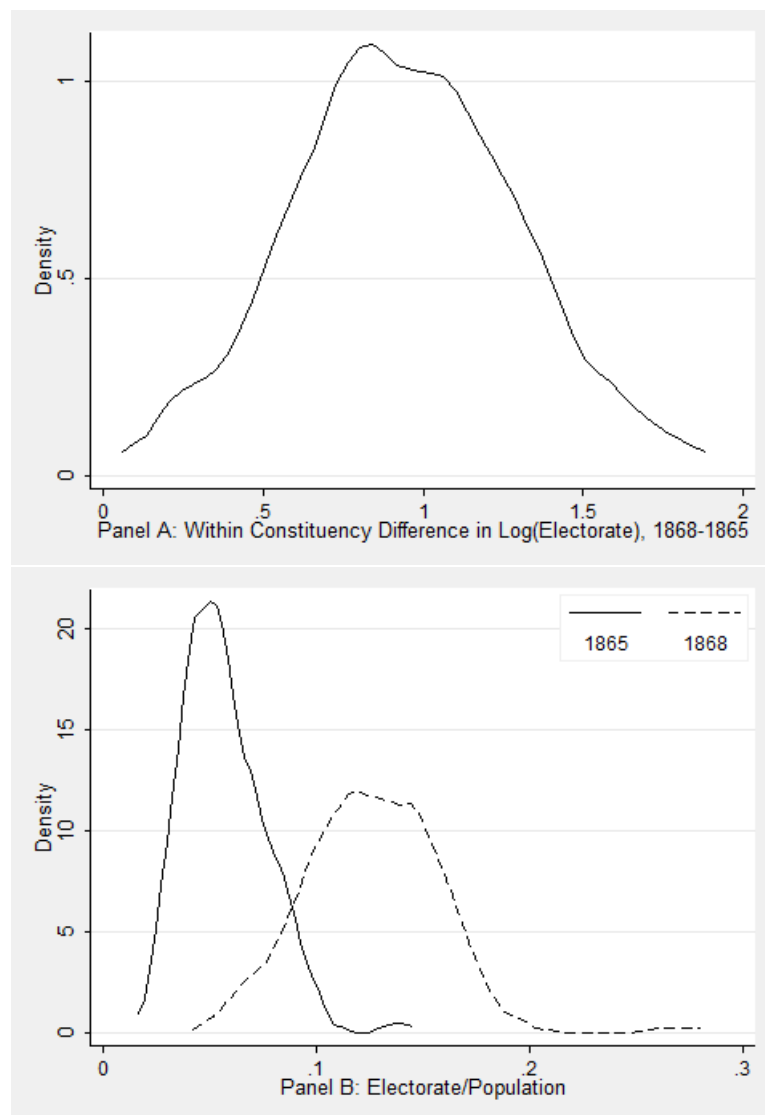
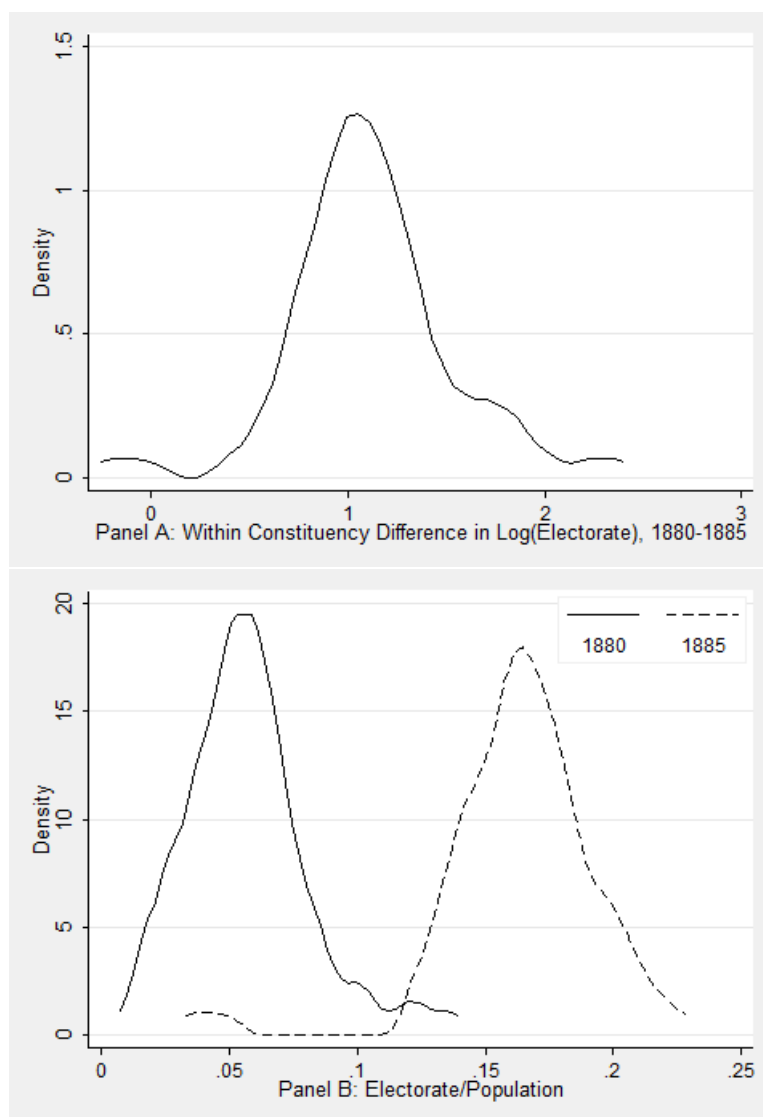


Figure 4.2: Changes and the Level of Enfranchised Population in Counties (1880-1885)



A key advantage in studying the British reforms is that their effects were varied at the constituency level. Specifically, the kernel densities for the logged difference in the registered voters between 1865 and 1868, as illustrated in figure 4.1, show a wide degree of variance in the effect of franchise extension in these constituencies: in some boroughs the changes in the voting rules had little discernible impact on the number of eligible voters, whereas in others the size of the (registered) electorate increased considerably. Figure 4.2 draws a similar picture with respect to the effect of the Third Reform Act. Adopting the language of the experimental literature, we can view the extension of the franchise as a ‘treatment’ varying in intensity from (just below) 0 to (just over) 2 with an average around 1.

The 1867 reform was unaccompanied by other constitutional changes that would confound any attempt to isolate the causal impact of reform. The reduction in multi member districts that accompanied the 1884 reform restricts our sample of unchanged constituencies much. However, both the Second and the Third Reform Act were accompanied in certain constituencies with changes in the number of representatives and/or in constituency boundaries. To distinguish the effect of the franchise extension from these confounding factors, we consider a sample of constituencies where the only change was an increase in the electorate. First, we exclude Irish as well as university constituencies from our sample. Franchise extension in Ireland was timed differently and electors registered in the university constituencies were allowed to vote both in the university constituency and in their constituency of residence. Table 4.2 specifies the number of boroughs and counties, as well as the total number of seats by year in England, Scotland and Wales. In the period before and after the Second Reform Act (1865 to 1874), there were 113 borough and 60 county constituencies without changes in seats and without boundary changes. The boroughs returned 171 MPs, while 95 MPs were elected in counties. Table 4.2 also presents our sample for the period of the Third Reform Act (1880 to 1885). Along with a further extension of the franchise, most multi-member constituencies were transformed into single-member constituencies. As a result, many constituencies were transformed, i.e. split into a number of single-member constituencies and/or merged with other constituencies. There are 166 boroughs and 46 counties that appear in both 1880 and 1885. Our sample of constituencies without seat changes contains 37 counties and 116 boroughs that elected 37

and 138 MPs respectively.¹⁷

Table 4.2: Total Number of Constituencies and Seats in England, Scotland, and Wales (1865-1885)

	Boroughs		Counties	
	Constituencies	Seats	Constituencies	Seats
Total in 1865 election	221	357	112	192
Total in 1868 election	222	327	127	219
Total in 1874 election	220	323	127	219
Total in 1880 election	220	323	127	219
Total in 1885 election	305	328	228	228
Total in constituencies that appear in 1865, 1868 & 1874	207	308*	97	166
Total in constituencies without changes in seats, 1865-1874	164	252	97	166
Total in constituencies without changes in seats and no boundary changes, 1865-1874	113	171	60	95
Total in constituencies that appear in 1880 & 1885	166	189**	46	46***
Total in constituencies without changes in seats and not transformed, 1880-1885	116	138	37	37

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

Notes: *The number of seats corresponds to the 1868-1874 period. In the 1859-1865 period there were 335 seats.

** The number of seats in 1885. In 1880 there were 240 seats. *** The number of seats in 1885. In 1880 there were 55 seats.

4.3 The New Voters and their Representatives

As we have seen, the Reform Acts brought into the franchise voters from previously unenfranchised income brackets. [Berlinski and Dewan \(2011\)](#) provide a detailed account of who were the new voters after 1867 based on [Mackenzie \(1921\)](#) and [Bowley \(1937\)](#) who estimate the income of the head of the household at median, quartile and lowest decile of the income distribution in 1860; and [Mackenzie \(1921\)](#) also provides estimates of household budgets for a typical family (man, wife, and 3 schoolchildren) which include the amount paid for rent.

Here we provide information on the backgrounds of those elected to parliament by the new voters. In gathering this information we relied on personal information about MPs from biographical sources, most importantly [Stenton and Lees \(1976, 1978, 1979, 1981\)](#) *Who's Who of British Members of Parliament*.¹⁸ The data includes information about each MP's parliamentary service. It records whether he is of aristocratic descent and his primary occupation before entering parliament. It also records dates of birth

¹⁷The number of MPs is larger than the number of boroughs as some boroughs remain multi member constituencies.

¹⁸The dataset was collected and provided by Professor Michael Rush.

and death. Finally our data records whether the individuals had family connections to other Members of Parliament who served prior to them or who subsequently entered the House of Commons.¹⁹

We consider all members of parliament elected in general elections since 1832. To check how close the sample of MPs covered resembles the full universe of MPs after 1832, we compared this sample against a full list of elected MPs derived from [Craig \(1989a,b\)](#). Our sample includes MPs elected in the general elections of 1865, 1868, 1874, 1880 and 1885.²⁰ To identify cabinet ministers in our sample we used a list of ministers building on [Cook and Keith \(1975\)](#). We exclude cabinet ministers who were not elected (at least once) in a general election. And we exclude non-elected peers about whom we have no individual information.

Dynastic links refer only to other elected MPs, but can be to any MP, elected in general elections, by-elections or petitions since 1832. The dataset does not report the links between MPs and peers explicitly. However, we have implicit information about such links as the data reports aristocratic connections that indicate whether the individual was the son, grandson or nephew of the holder of a hereditary peerage or baronetcy.

We consider seven individual characteristics, coded as indicator variables, that record: (1) whether the MP had aristocratic connections; (2) whether his listed occupation was landowner, private income, army officer, naval officer or colonial landowner-merchant that indicates he was a member of an elite; (3) whether he was a rentier, a subdivision of elite, so that his listed occupation was landowner or private income. We also record: (4) the MP's age at death; (5) whether he was a cabinet minister; (6) an indicator variable recording whether he is a junior member of a dynasty, that is has a family link to another MP who entered parliament before him; and finally (7) an

¹⁹We exploited all information about dynastic links available in the dataset and ensured that if person A is reported to have a dynastic link to person B, person B is also reported to have the inverse dynastic link to person A. The biographical entries used to create the dataset for persons A and B would not commonly report these reciprocal links. Consider an example of person B, who is the son of person A. While the biography of person B would indicate he is the son of person A, the biographical entry for person A would not necessarily indicate that he is the father of person B, for example because person B had not yet served in parliament at the time of writing. Therefore, we ensured that all available one-way dynastic indications between two individuals were transformed into reciprocal links in the dataset.

²⁰For each individual that we found to be missing in our dataset, we collected additional information about parliamentary service, aristocratic connections and dynastic links from [Stenton and Lees \(1976, 1978\)](#).

indicator for whether he has a family link to another MP who entered the House of Commons subsequent to his first term in parliament. These variable definitions are summarised in table 4.3.²¹

Tables 4.4 and 4.5 below present the differences in the key characteristics we study before and after the franchise extensions. Table 4.4 looks at the impact of the Second Reform Act on the aggregate composition of Parliament between 1865 and 1868. There is a decrease in the percentage of constituencies that had representatives in Cabinet which reflects the increase in the overall number of constituencies.²² Otherwise with respect to our key variables of interest nothing much changes between these years: the share of aristocrats, elite members, and rentiers, as defined, remains remarkably consistent. As does the percentage of MPs with dynastic relations. For example, the percentage of aristocrats increased only very slightly across all constituencies from 35 to 37% after the franchise extension. However, when using our measure of elite background, we find instead a small decrease in the number of such individuals returned across all constituencies, from 58 to 53%. The number of MPs who had a relative in parliament before them, i.e. juniors, slightly increased from 35 to 37%. However, the number of MPs that would establish or continue a political dynasty, i.e. seniors, decreased from 44 to 40%. As with the increase in the percentages of aristocrats, the increase in percentages of juniors was driven by an increase in the counties, from 42 to 49%. In the boroughs, precisely where we would expect to see the largest effect from the franchise extension, all our measures show a slight decrease. However as noted, we should not place too much stock on these figures that may mask considerable constituency level variation.

Table 4.5 looks at the impact of the Third Reform Act on the aggregate composition of Parliament between 1880 and 1885. Here we observe a larger aggregate effect: the size of the aristocracy decreases by roughly one half depending on which measure we use. We observe a smaller change with respect to dynastic relations, but the percentages of juniors and seniors also decline.

²¹Further detail about the distribution over time of the characteristics that we consider is provided by tables B.1 and B.2 in the appendix.

²²Numbers for cabinet ministers suggest that percentages of cabinet ministers drawn from the Commons (as compared to from the Lords) did not change much with the reforms. About 30% of cabinet ministers were peers in 1865 and about 29% in 1868. In 1880 about 30% of cabinet ministers were peers, while their numbers remained constant (9 peers) in 1885 but their proportion increased to 36% in 1885.

To measure the effect of subsequent treatments of franchise extension, we merged our data to the size of the electorates and population in each constituency building on [Berlinski and Dewan \(2011\)](#).²³ Population figures are derived from [Vincent and Stenton \(1971\)](#). Population in 1865, in 1868 and in 1874, in 1880 and in 1885 is proxied by the population figures from the 1861, 1871, 1881 and 1891 census respectively.

We discuss our empirical strategy for obtaining consistent and unbiased estimates of the causal effect of reforms in the following section.

²³Sources: [Craig \(1989a,b\)](#) and [Vincent and Stenton \(1971\)](#)

Table 4.3: Variable definitions

Variable	Definition
Aristocracy	Dummy equal to one if the MP was the son, grandson or nephew of the holder of a hereditary peerage or baronetcy
Elite	Dummy equal to one if the MP's occupation is listed as landowner, private income, army officer, naval officer or colonial landowner/merchant
Rentier	Dummy equal to one if the MP's occupation is listed as landowner or private income
Age at death	Age of the MP in years at time of death
Cabinet	Dummy equal to one if the MP was a cabinet minister
Junior	Dummy equal to one if the MP has a family link to an MP who entered parliament before the MP's first parliamentary term
Senior	Dummy equal to one if the MP has a family link to an MP who entered after the MP's first parliamentary term
Missing	Dummy equal to one if the MP has missing information for any of the variables defined above

Table 4.4: Distribution of Characteristics, 1865 and 1868

	Boroughs		Counties		All	
	1865	1868	1865	1868	1865	1868
Aristocracy (%)	28	27	46	51	35	37
Elite (%)	44	38	78	73	58	53
Rentier (%)	28	24	45	47	35	34
Age at death (Mean)	72	72	72	72	72	72
Cabinet (%)	4	2	3	0	3	2
Junior (%)	31	28	42	49	35	37
Senior (%)	38	35	52	47	44	40
Missing (%)	7	4	3	3	5	3
Nr of MPs	396	369	262	289	658	658

Table 4.5: Distribution of Characteristics, 1880 and 1885

	Boroughs		Counties		All	
	1880	1885	1880	1885	1880	1885
Aristocracy (%)	23	22	41	23	31	22
Elite (%)	29	23	55	28	41	25
Rentier (%)	16	11	33	15	24	13
Age at death (Mean)	73	74	72	72	72	73
Cabinet (%)	4	3	3	2	3	2
Junior (%)	32	27	42	26	36	27
Senior (%)	31	30	37	26	33	28
Missing (%)	4	4	2	3	3	3
Nr of MPs	363	348	289	322	652	670

4.4 Empirical Strategy: Identifying the Effect of Franchise Extension on Electoral Outcomes

To understand how electoral outcomes are causally related to the change in rules governing the eligibility to vote, we need to isolate the effect of a change in the franchise from other possibly confounding factors. Although franchise extension was applied nationally and simultaneously in all constituencies, the magnitude of the change at the constituency level reflects local conditions. In particular the local impact of a change in the electoral law is related to the constituency level distribution of income and housing at the time of the reform. In estimating the causal effect of franchise expansion on electoral outcomes we face the problem that the change in franchise is systematically related to a set of constituency level characteristics that are likely to have an independent effect on electoral outcomes. Without controlling for these confounding factors, our estimates are likely to be biased and inconsistent.

In order to isolate the effect of franchise extension from other changes in electoral rules, we focus on a sample of constituencies that do not experience changes in boundaries or in the number of seats between elections. Moreover, because of the different nature and populations affected by the Second and Third Reform Acts we estimate their impact separately. We use the following benchmark model for MP i in constituency j at time t :

$$Y_{ijt} = \alpha + \beta \text{Log}(R_{jt}) + \lambda_t + \delta_j + \epsilon_{ijt} \quad (4.1)$$

where Y_{ijt} is an electoral outcome of interest (e.g., whether the elected MP has aristocratic background); $\text{Log}(R_{jt})$ is the log of constituency level registered voters; λ_t is a time dummy that captures (among other things) any direct effect of the reform that does not run through changes in constituency franchise and affects equally all constituencies; δ_j is a constituency fixed effect and, finally, ϵ_{ijt} is a random error term. To study the Second (Third) Reform Act we use the years 1865 (1880) and 1868 (1885) for our main analysis.

The parameter of interest in equation 4.1 is β , the effect of the franchise level on electoral outcomes. This model recovers a causal effect if the constituency fixed effects capture the community characteristics that determine the level of the franchise at each

point in time and are also directly associated with the electoral outcomes. This is likely to be the case for the very short span of time between elections that we consider as we can imagine that these community characteristics (e.g., income, rent, housing) are fixed (or vary slowly). Of course, this is essentially a difference-in-differences design where the crucial assumption is that, conditional on time and constituency fixed effects, there would not have been any systematic differences in electoral outcomes between constituencies with different franchise levels in the absence of the reform.

In our empirical implementation, we use registered electors as a measure of enfranchised voters. This variable does not capture all citizens who are eligible to vote. In particular, $r_{jt}E_{jt} = R_{jt}$ where E_{jt} is eligible voters and r_{jt} is the registration rate. When the registration rate is constant over time across constituencies, $r_{jt} = r_j$, using $\text{Log}(R_{jt})$ or $\text{Log}(E_{jt})$ as the causing variables in equation 4.1 result in identical estimates provided we condition on constituency fixed effects.

Finally, if the franchise level is the ratio of registered voters to the relevant constituency population then, provided that the population remains fixed or its change is uncorrelated with changes in R_{jt} , equation 4.1 is similar to regressing Y_{ijt} on the log franchise level. Because this may not be the case we present estimates while controlling for the local population by including the (logged) population, $\text{Log}(P_{jt})$. Including this term we then estimate

$$Y_{ijt} = \alpha + \beta \text{Log}(R_{jt}) + \gamma \text{Log}(P_{jt}) + \lambda_t + \delta_j + \epsilon_{ijt} \quad (4.2)$$

For brevity, we will focus on the results from the estimation of equation 4.2.²⁴ All models take into account the potential correlation between outcomes at the constituency level over time by clustering the standard errors at the constituency level (i.e. the unit of variation in the franchise).

Our empirical strategy goes a long way towards controlling for potential confounders in the relationship between franchise extension and electoral outcomes. However, our identification strategy may still suffer from concerns about the exogeneity of our measure of franchise expansion if, for example, the registration rate varies randomly over time (i.e., a problem of classical measurement error)²⁵ or is correlated with

²⁴Results for equation 4.1 are available from the authors upon request.

²⁵The problem is analogous to measuring $\log(E_{jt})$ with error. In the classical measurement error framework, $\log(R_{jt}) = \log(r_j \omega_{jt} E_{jt})$ where ω_{jt} is a random variable independent of E_{jt} .

the growth or decline of the aristocratic power at the local level. It may also be the case that in places where the power of the aristocracy is eroding faster (e.g., for economic reasons) the increase in the franchise is larger (i.e., there is reverse causality).

Several papers have suggested that suffrage reform was not exogenous to national economic conditions. For example [Acemoglu and Robinson \(2000\)](#) and [Boix \(2003\)](#) argue that suffrage reform is introduced to avoid a revolutionary threat while [Lizzeri and Persico \(2004\)](#) argue that industrialisation provides the conditions for new political alliances, who benefited from franchise extension, to form. As argued in [Berlinski and Dewan \(2011\)](#) there are good reasons to believe that although the implementation of suffrage reform was political it is unlikely that, given the bluntness of this instrument, its implementation could have been manipulated at the constituency level. In particular it is unlikely that the elites could have configured a rule that varied across constituencies in a way that maintained aristocratic power, even if that were their intention.

Nevertheless, and in order to evaluate the issue of reverse causality, we can use the lagged outcome in equation 4.2 as a dependent variable rather than its current value.²⁶ More generally, we can instrument the level of the franchise using the level of the electorate in 1832 and the current population size interacted with time dummies. These variables can be used as instruments under the assumption that they are correlated with changes in the electorate but not directly correlated with subsequent changes in electoral outcomes.²⁷

²⁶ We can also study the problem of classical measurement error in registration by instrumenting $\text{Log}(R_{jt})$ with $\text{Log}(R_{jt-1})$. Unfortunately, the instrument is weak. Results available from the authors upon request.

²⁷ See tables B.3 and B.4 to consider the evidence for reverse causality and tables B.5, B.6 and B.7 for the instrumental variable estimates. We find no evidence for reverse causality. The instrumental variable estimates tend to be similar to the ordinary squares results. However, we find that the Second Reform Act in 1867 had a positive effect on the election of aristocrats in the 1874 Parliament when restricting to the sample with no boundary or seat changes. Whereas the Third Reform Act had a negative impact when restricting to the same sample. The former result should be treated with caution: we are unable to isolate the impact of the 1867 reform due from that of the Secret Ballot Act of 1872. We should also be cautious with respect to the Third Reform Act. The sample where we find an effect is small and the magnitude of the county level changes are small also when compared with those of the Second Reform Act. Moreover part of the estimated effect may be due to the changing fortunes of Conservatives incumbents.

4.5 Results

4.5.1 The Franchise Extension of 1867

Table 4.6 considers the changes in characteristics of the elected candidates under the new franchise introduced by the Second Reform Act. The results for boroughs without any boundary or seat changes are shown in panel A. The boroughs are the constituencies most affected by the franchise extension where we most expect to see a difference. For comparison, panel B presents estimates for the counties without boundary or seat changes. All models include a control for the log population of the constituency, as well as time and constituency fixed effects. All outcomes are measured at the level of the elected MP. Some constituencies returned more than one MP, therefore errors are clustered at constituency level. Panel A of table 4.6 shows that the effect of the franchise extension on the probability that the elected MP(s) can be characterised by any of our measures of an elite background cannot be distinguished from zero. For example, increasing the eligible voting population by 100% is even predicted to slightly increase the share of elected candidates with aristocratic connections by about 1.7%.²⁸ The other estimates (column 2 to 7) are all in the expected direction, i.e. negative except for age at death. The estimated effect is slightly larger for our alternative measures, elite and rentier, with an increase of 100% in log electorate resulting in an expected decline of about 8% and 6% respectively. A similar change decreases the probability of an MP who starts or continues a political dynasty (senior) by about 13%.

Although the estimated effects are in the expected direction – a reduction in the role of the aristocracy as the size of the electorate increases – they are not large enough, when considering the standard errors, to allow us to refute the null hypothesis that franchise extension had no direct effect on the 1868 parliament. Our estimates, moreover, suggest that the doubling of the electorate did not lead a significant decrease in the share of aristocrats in the commons. This is perhaps surprising given the findings of [Berlinski and Dewan \(2011\)](#) who, based on calculations from [Mackenzie \(1921\)](#) and [Bowley \(1937\)](#), show that the newly enfranchised voters were the unskilled urban workers.

Panel B of table 4.6 present similar estimates for a sample of counties without

²⁸A doubling in the electorate is equivalent to a 0.69 increase in $\ln(\text{electors})$.

Table 4.6: Changes in Characteristics of Elected Candidates Under the New Franchise (1868-1865).

	Aristocrat (1)	Elite (2)	Rentier (3)	Age at Death (4)	Cabinet (5)	Junior (6)	Senior (7)
<u>Panel A: Boroughs without boundary or seat changes</u>							
ln(electors)	0.024 [0.089]	-0.12 [0.083]	-0.09 [0.080]	3.323 [3.164]	-0.001 [0.038]	-0.018 [0.111]	-0.195 [0.151]
Observations	342	330	330	340	342	342	342
R-Squared	0.521	0.627	0.59	0.618	0.274	0.531	0.574
<u>Panel B: Counties without boundary or seat changes</u>							
ln(electors)	0.475 [0.308]	0.164 [0.342]	0.334 [0.295]	1.062 [7.042]	0.036 [0.045]	0.017 [0.440]	0.082 [0.429]
Observations	190	183	183	190	190	190	190
R-Squared	0.548	0.442	0.469	0.542	0.256	0.49	0.435
Control Population Change?	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Source: Craig (1989) and Vincent and Stenton (1971).

Notes: OLS regressions. All regressions include a constant term. Sample includes one observation per elected MP by constituency and year.

Standard errors clustered by constituency are in parentheses.

boundary or seat changes. The estimates here suggest an increase in the probability of returning an MP from elite backgrounds, but again the standard errors are so large that we cannot statistically significantly distinguish these estimates from zero.

The evidence for the idea that the effect of franchise extension was delayed is also weak. Table 4.7 presents the estimation results for equation 4.2 comparing 1865 to 1874, the second general election under the new franchise.²⁹ Again, for the sample of boroughs where nothing but the number of voters changed (panel A), the estimates are small and none attains statistical significance. In contrast to right after the reform, the probability of returning an aristocrat, as well as the estimated age at death, is lower. The estimated decreases are still small, about 8% and 0.14 years respectively for an increase in log electorate by 100%, and they cannot be statistically significantly distinguished from zero. The estimates for the delayed effect of increasing the electorate on the probability of returning elites and rentiers are also smaller, and not statistically sig-

²⁹Of course, looking further ahead in time for consequences of the Second Reform Act we run the risk that not all else is equal. The second general election took place seven years after the introduction of the reforms and nine years after the base year. While not too far in time from the reform, this provides a reasonably wide time window to investigate any long term effects. Nevertheless, we should be cautious in attaching significance to the findings as the intervening years saw the introduction of the secret ballot in 1872.

nificant. Panel B does show some effect in counties where only the franchise changed, at least for age at death, our proxy of wealth. Doubling the electorate is significantly related to an increase in the average age at death of MPs by about 12 years. This is a large change, especially compared to the equivalent direct effect of the Second Reform Act (panel B of table 4.6) which was to increase the expected average age at death by less than a year and was not found to be statistically significant. Still, this change occurred further along in time and for most other characteristics there is little evidence for an effect of the franchise extension.

Table 4.7: Changes in Characteristics of Elected Candidates Under the New Franchise (1874-1865).

	Aristocrat (1)	Elite (2)	Rentier (3)	Age at Death (4)	Cabinet (5)	Junior (6)	Senior (7)
<u>Panel A: Boroughs without boundary changes or seat changes</u>							
ln(electors)	-0.11 [0.120]	-0.084 [0.117]	-0.027 [0.101]	-0.198 [3.294]	-0.084 [0.063]	0.018 [0.119]	-0.25 [0.157]
Observations	342	328	328	340	342	342	342
R-Squared	0.45	0.53	0.516	0.48	0.288	0.419	0.51
<u>Panel B: Counties without boundary changes or seat changes</u>							
ln(electors)	0.306 [0.410]	-0.076 [0.386]	0.257 [0.313]	17.533** [7.277]	0.001 [0.031]	0.411 [0.347]	0.004 [0.319]
Observations	190	185	185	190	190	190	190
R-Squared	0.478	0.428	0.43	0.438	0.418	0.46	0.448
Control Population Change?	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Source: Craig (1989) and Vincent and Stenton (1971).

Notes: OLS regressions. All regressions include a constant term. Sample includes one observation per elected MP by constituency and year.

Standard errors clustered by constituency are in parentheses.

In sum we find no strong evidence with respect to the Second Reform Act that allows us to refute Laski's null hypothesis that franchise reform did little to alter the power of the British aristocracy.

4.5.2 The Franchise Extension of 1884

In 1884 the franchise was extended further to place the counties on an equal footing to the boroughs. The descriptive statistics of table 4.5 suggested that the Third Reform Act affected the type of elected candidates more than the Second Reform Act (table 4.4). In contrast to the Second Reform Act, the Third Reform Act was accompanied by

a large reduction in the number of multi-member districts in 1885 (the Redistribution Act). As a result, the sample of constituencies that did not change the number of MPs to be elected or that were not transformed, i.e. merged with (parts of) other constituencies or split into new constituencies, is very small. The results in this section are therefore presented for comparison and should be interpreted more cautiously.

Table 4.8 shows estimates for equation 4.2 for untransformed counties (panel A) and boroughs (panel B) between 1880 and 1885. No statistically significant effect of the Third Reform Act can be identified. The estimates for counties in panel A, where we most expect to see changes, are negative as expected. While generally slightly larger in magnitude than the effects of the Second Reform Act (table 4.6), they are still not statistically distinguishable from zero. For this most restrictive sample, the constituency fixed effects fully explain all variation in MPs who become cabinet ministers; there are too few cabinet ministers in the sample to estimate a separate effect of the franchise extension.

Table 4.8: Changes in Characteristics of Elected Candidates Under the New Franchise (1880-1885).

	Aristocrat (1)	Elite (2)	Rentier (3)	Age at Death (4)	Cabinet (5)	Junior (6)	Senior (7)
<u>Panel A: Counties that were not transformed and without seat changes</u>							
ln(electors)	-0.299 [0.277]	-0.192 [0.235]	-0.12 [0.174]	-6.633 [6.727]		0.06 [0.319]	-0.031 [0.214]
Observations	73	70	70	72		73	73
R-Squared	0.746	0.864	0.952	0.69		0.74	0.836
<u>Panel B: Boroughs that were not transformed and without seat changes</u>							
ln(electors)	0.012 [0.067]	-0.122 [0.081]	-0.103 [0.080]	0.706 [1.989]	-0.057* [0.032]	-0.045 [0.079]	-0.032 [0.074]
Observations	276	264	264	276	276	276	276
R-Squared	0.597	0.583	0.524	0.517	0.538	0.599	0.635
Control Population Change?	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Source: Craig (1989) and Vincent and Stenton (1971).

Notes: OLS regressions. All regressions include a constant term. Sample includes one observation per elected MP by constituency and year. Standard errors clustered by constituency are in parentheses.

The estimates of panel B (untransformed boroughs with no seat changes) are not very different from the estimates for the counties (panel A of table 4.8). Most of the effects cannot be statistically distinguished from zero, except for a slight decrease in the probability of returning an MP who would be selected into the cabinet. Doubling the electorate after the Third Reform Act is expected to decrease that probability by

about 4% (significant at the 10% level). Aside from this effect in boroughs only, the franchise extension of the Third Reform alone, as that of the Second Reform before, cannot be causally related to the change in the composition of parliament.

4.6 Conclusion

Was franchise extension causally related to a decline in the political power of the British aristocracy? During our period of investigation Britain's bicameral system ensured the aristocracy some political power but may have weakened their control in the House or their ability to form dynasties. Historians and political scientists have reached different conclusions from the evidence. In part this reflects the difficulty in isolating the impact of the reforms from other factors that changed the composition of elites in this period. Exploiting the varied constituency level impact of the Second and Third Reform Acts provides a rigorous and reliable test for Laski's claim that "the three Reform Acts of the nineteenth century had little effect upon the position of the aristocracy in politics".

Whilst it is reasonable to expect that, at the margins, those constituencies most affected by the reforms would be less likely to elect aristocrats to parliament than those less affected, the evidence does not allow us to reject Laski's null hypothesis. Indeed in the sample of constituencies where the only thing that changed between 1865 and 1868 was the impact of the Second Reform Act on the number of registered voters there is no effect on the election of aristocrats whether narrowly or broadly defined. The same is true of the Third Reform Act. We also find no strong evidence that franchise extension alone altered the composition of the elected part of the Cabinet, thus weakening the aristocracy's control. Nor does the evidence suggest that franchise extension affected the ability to form political dynasties.

We conclude that the franchise extension, whilst likely a necessary condition for a change in the composition of the British parliament over the longer term, was not on its own sufficient to induce an immediate decrease in aristocratic power.

Chapter 5

Political Dynasties in the UK House of Commons: the Null Effect of narrow Electoral Selection

5.1 Introduction

In the nineteenth century, UK parliamentary politics was highly dominated by a few established families. The peerages of the House of Lords were bequeathed from generation to generation. The House of Commons too was dominated by a few landowning families (Canandine, 1999). In the course of that century power would gradually shift away from these aristocratic elites along with the widening democratic participation. However, political dynasties survived. Their numbers declined over time, but there are still MPs who followed in the footsteps of their relatives.

Dynastic succession occurs in all professions. However, for the profession of politics, in which succession in democracies depends no longer on dynastic succession but on running successful electoral campaigns, understanding how and why political power can be bequeathed is particularly important. However, identifying what constitutes power bequest in democracies is difficult. Elections are not random. The same talents or drive that elect the founders of the dynasty into parliament may cause their

successors to be successful too. In spite of these challenges to identification, recent research provides evidence for a causal connection between the length of tenure of a legislator and the future electoral success of their relatives (Dal Bó, Dal Bó and Snyder (2009); Querubin (2010); Rossi (2009)). To identify the mere effect of holding power, these papers exploit exogenous sources of an increase in power. One such factor is the tenure length of the senior dynastic politician. The longer an MP serves, the more name recognition and political networks he or she will be able to build. These factors form additional electoral advantages, therefore explaining why success can still be inheritable in democracies, independently from family characteristics that are fixed over time. This paper considers what evidence there is for these mechanisms in the UK. Surprisingly, in spite of many historical anecdotes of its powerful families, I find no evidence in favour of power bequest through elections in nineteenth century UK. However, I do find an association between dynastic descent and cabinet selection. The relevance of this counterintuitive null result extends beyond the UK context. These findings suggest that the variation across democracies in the extent to which the individuals who obtain power at the margin are able to use their newly gained positions to bequeath their power, is still insufficiently understood.

First, I will provide evidence that those MPs of the UK House of Commons who won their first re-election attempt by a small margin before 1918, and subsequently served longer in parliament, were in no particular way different from those who narrowly lost. Therefore, the first re-elections of these MPs introduced random variation in tenure lengths among otherwise similar individuals. A reduced form approach, as well as one employing winning a first re-election as an instrument for tenure length, finds no significant effect of tenure length on the probability of establishing a dynasty. A break up of the analysis over different time periods provides qualitatively similar results: throughout the nineteenth century there is little evidence for power bequest through elections, suggesting that name recognition did not drive dynastic perpetuation in the UK.

I then consider the evidence for the self-preservation of elites in another powerful institution: the cabinet. Cabinet gradually came to dominate parliament throughout the nineteenth century (Cox, 1987). Therefore, the success of the relatives of cabinet ministers is one obvious way to study the importance of having relevant political connections. Crucially, I find that junior dynastic MPs were more likely to obtain a cabinet

position, but only conditional on being the political heir of an MP who had previously served in cabinet. These diverging results for electoral and cabinet selection are important for the interpretation of power bequest in democracies. We know that throughout the nineteenth century power became increasingly concentrated in the cabinet, shifting away from individual MPs and parliament. One way in which elites may have managed to bequeath their power even in face of widening democratic participation then, was by selecting its scions into cabinet.

This paper is concerned with identifying the effect of holding power. Of course, talents or the political socialisation that occur within families may still explain much of the observed persistence of dynasties in parliament. However, these results point out that the political power bequest that did occur in the UK did not include the individuals who only narrowly won a re-election. Increased name recognition or networks do not seem to have provided additional electoral opportunities to new legislators, as they may have done in other contexts such as the US. Moreover, the dynastic advantage may not have worked directly through elections, but holding power for longer may still have had an impact in more indirect ways, for example through cabinet selection. A possible explanation for why my results are in contrast to those of the current literature is that elites were already deeply entrenched in UK politics and increasingly competitive elections in the nineteenth century did not help promote new dynastic politicians. This paper therefore suggests that the context, or the existing political opportunities for establishing dynasties, matters for dynastic persistence in democracies.

5.2 Literature

In line with recent research ([Dal Bó, Dal Bó and Snyder \(2009\)](#); [Querubin \(2010\)](#); [Rossi \(2009\)](#)), this paper aims to identify a causal effect of serving longer in parliament on the probability of establishing or continuing a political dynasty. In contrast to these papers, there is no evidence of an effect of tenure length as such on dynastic succession in parliament. Therefore this paper talks to the literature on how elites can survive in captured democracies (see for example [Acemoglu and Robinson \(2008\)](#)). This study of how marginal political success affects political outcomes is also related to [Ferraz and Finan \(2009\)](#) and [Acemoglu et al. \(2008\)](#) who find that political concentration af-

fects current economic outcomes. This paper studies how political concentration (or narrow re-elections) affects dynastic persistence, a political outcome. However, narrow electoral success mattered little for an MP's chances to bequest his political power in the UK, even while the electoral system became increasingly democratic as a result of different franchise extensions. Indeed, institutional changes can have no or even counterproductive effects on the persistence of political dynasties: [Berlinski, Dewan and Van Coppenolle \(2013\)](#) find no effect of franchise extension on the electoral success of dynastic candidates in the UK. [Querubin \(2011\)](#) finds even counterproductive effects of the introduction of term limits in the Philippines. The results here suggest that the existing status quo between elites in democracies matters. This idea concurs with the finding that among the factors influencing the status quo between elites and promoting dynastic perpetuation is the initial distribution of wealth [Rossi \(2011\)](#). The contribution of this paper to these existing studies is that it investigates dynastic power transmittal in one of the oldest parliaments, and obtains counterintuitive results. By employing a similar strategy to identify a power effect, the results are closely comparable to those of other settings. The findings show that context matters, and are therefore relevant beyond the UK setting.

The absence of what is essentially an inter-personal incumbency advantage could also be interpreted as another piece of evidence for (increased) party- rather than person-based voting in the UK. Consistent with my result and this particular interpretation is that there is little evidence for an incumbency advantage in nineteenth century UK, as derived by [Eggers and Spirling \(2013\)](#) in a recent working paper.

Voters may have been primarily concerned with choosing the party that controlled the cabinet, rather than the characteristics of their representative. Throughout the nineteenth century party-based voting increased and the cabinet became increasingly powerful [Cox \(1987\)](#). In this context, it is all the more striking that dynasties survived, and in powerful positions too, in an increasingly democratic United Kingdom.

Understanding dynastic persistence is important. How and why democratic systems provide opportunities for dynastic domination by political elites is relevant to researchers from adverse selection concerns (see [Besley \(2005\)](#)). Legislative careers may be particularly rewarding³⁰ and the identity of policy makers tends to influence

³⁰[Eggers and Hainmueller \(2009\)](#) and [Querubin and Snyder \(2009\)](#) estimate positive returns to service in the UK and the US, though [Eggers and Hainmueller \(2013\)](#) find little advantage to US legislators

the types of policies they implement.³¹ Therefore, investigating the interplay between legitimate voter demand and potentially nepotistic family supply of dynastic candidates is crucial. In theory, it could be that name recognition is all that matters. It can serve as an indicator of political quality to voters who have at least some say over who represents them in democracies. Citizens may then reward politicians from established families for perceived shared talent or drive and this is what makes them more successful than unconnected newcomers. Evidence from several countries indicates that junior members of political families indeed tend to amass more votes behind their names even after controlling for other factors, making them more likely to win electoral contests (Feinstein (2010); Querubin (2010); Van Coppenolle (2013a)). However, this paper finds no direct evidence of such name recognition advantages in the UK. One explanation could be that the political quality of dynasties in the perception of the voters depends on pre-existing levels of elite capture.

5.3 Data

The biographical data for this study reports individual characteristics for all UK members of parliament since 1832 up to and including 2005. Every MP has a parliamentary service record, indicating the date of entry and exit into the House of Commons for each constituency the MP has served for and the reason for exit. There is also a record of individual characteristics for each MP. This includes their date of birth, date of death, education, professional occupation before entering parliament, details about their elevation to the peerage or knighthood, aristocratic connections, parentage, dynastic links to other MPs and party membership. The dataset was collected using biographical sources, most importantly Stenton and Lees (1976, 1978, 1979, 1981) *Who's Who of British Members of Parliament*.³²

To ascertain that the full universe of MPs after 1832 was covered, the original data were matched to a list of elected MPs.³³ For each individual of the list of MPs that

holding US portfolios.

³¹See Besley, Montalvo and Reynal-querol (2013); Pande (2003).

³²The original data is described in more detail in Rush (2001).

³³The list of MPs were derived from Craig (1971, 1983, 1984, 1989a,b). After 1918 I checked whether the correct number of individuals was returned for each constituency. Digitised information about MPs before 1918 as well as nineteenth century electoral results drawn from Craig (1989a,b) were

could not be matched to an individual in the dataset, I collected additional information about parliamentary service, aristocratic connections and dynastic links from [Stenton and Lees \(1976, 1978, 1979, 1981\)](#). The final data covers all MPs elected in general elections.

Many constituencies in the nineteenth century elected more than one MP. These constituencies were often abolished, merged or split throughout the nineteenth century and sometimes even revived in the twentieth century. As many of these constituencies share a common history of elections, the most conservative approach identifies the common greatest boundaries of these related constituencies. Groups of constituencies were identified employing information about mergers and splits.³⁴ When appropriate, the analyses cluster standard errors at these larger, merged constituency units.

Next, the cabinet ministers in the sample were identified by matching the names to a list drawn from [Cook and Keith \(1975\)](#), [Butler and Butler \(2000\)](#) and [Berlinski, Dewan and Dowding \(2012\)](#).

The data reports whether the MP was related to another MP who served in parliament before him or her. All available information about dynastic links was used, ensuring that if person A is reported to have a dynastic link to person B, person B is also reported to have the inverse dynastic link to person A.³⁵ The dataset does not report the links between MPs and peers explicitly,³⁶ but we do know whether an MP was connected to the aristocracy,³⁷ and whether he or she succeeded to a peerage. A small subsample of 823 individuals drawn across all time periods³⁸ was checked against pub-

kindly provided for this project by Andrew Eggers and Arthur Spirling.

³⁴Source: www.leighrayment.com/commons.htm

³⁵The biographical entries used to create the dataset for persons A and B would not commonly report these reciprocal links. Consider an example of person B, who is the son of person A. While the biography of person B would indicate he is the son of person A, the biographical entry for person A would not necessarily indicate that he is the father of person B, for example because person B had not yet served in parliament at the time of writing. Therefore, we ensured that all available one-way dynastic indications between two individuals were transformed into reciprocal links in the dataset.

³⁶While the dynastic links refer to all individuals in the dataset, the sample considered in the analyses that follow considers only individuals elected in general elections and excludes individuals from constituencies with disputed elections (by petition) as well as the MPs who replaced them.

³⁷Aristocratic connections are defined as the son, grandson, or nephew of the holder of a hereditary peerage or baronetcy—i.e. hereditary knighthood—with a concomitant extension to women MPs from 1918 ([Rush, 2001, 31](#)).

³⁸This corresponds to about 25% of individuals of whom the surname occurs more than once, but who are not listed as family.

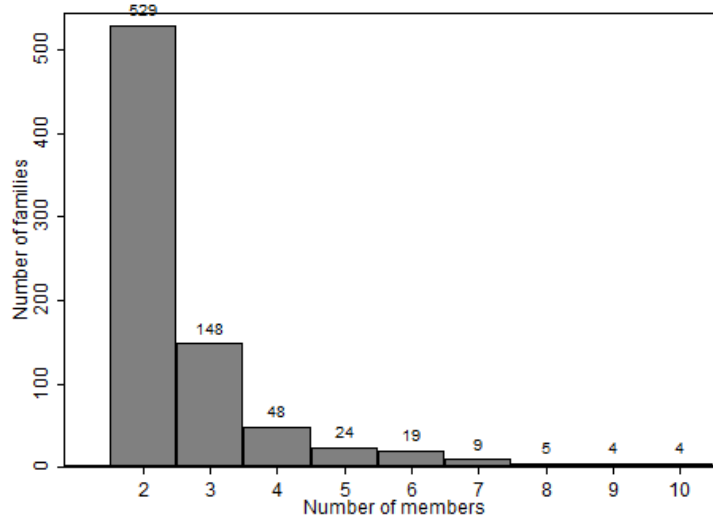


Figure 5.1: Size of Political Families

licly available information about family links.³⁹ The checked sample underscores that the information about family links from the biographies is of good quality and quite complete, especially for close dynastic links (fathers, sons and brothers).

Finally, I matched the biographical information of MPs to the results of their first re-election attempts. The first re-election attempt of an MP is defined as the first general election in which the MP stands as a candidate at any point in time after entering parliament as a result of winning his first general election. Data availability limits the investigation to elections before and including the December 1910 General Election. However, as many of the family links studied concern grandparents and grandchildren, we also need to allow for sufficient time for the MP's offspring to appear in parliament. The choice of this date as end point in the analysis also makes the results closely comparable to the study about the US (Dal Bó, Dal Bó and Snyder, 2009), an interesting point of reference for that time period.

Table 5.1 offers an overview of the family links in the House of Commons. Of all

³⁹Information was publicly available if the individual was found to have a Wikipedia entry. Only for about 49 individuals a family link was found that was not reported in our biographical information. In many cases, the family link found was not in fact to other individuals holding the same surname, but to other MPs who were related through marriage. Only 24 of the 49 links identified can be considered close links (fathers, sons or brothers). Even in these close cases, the coding of the senior and junior variable is not always affected, depending on in which order the relatives entered parliament. The results presented below are robust to recoding the checked individuals according to the Wikipedia information.

	Count	Percent	Percent
Senior	1,651	67%	18%
Junior	1,505	61%	17%
Parent	697	28%	8%
Child	818	33%	9%
Grandchild	212	9%	2%
Grandparent	188	8%	2%
Uncle or Aunt	285	12%	3%
Nephew	314	13%	3%
Cousin	138	6%	2%
Sibling	831	34%	9%
Married	63	3%	1%
Through marriage	379	15%	4%
Other family link	838	34%	9%
Total family links	2,459	100%	27%
Total individuals	9,044		100%

Table 5.1: Types of Family Relations

# members	Most occurring surname	Count	2nd surname	Count	3rd surname	Count
72	Cavendish	9	Egerton	9	Ormsby-Gore	6
52	Stanley	10	Beaumont	7	Fellowes	5
44	Buxton	9	Grey	5	Wyndham	5
30	Duncombe	7	Cust	3	Stanhope	3
25	Peel	9	Paget	5		
23	Milton	5	Dundas	4	Wentworth-Fitzwilliam	4
22	Ridley	6	Guest	5	Smith	4
20	Smith	5	Sandon	3		
20	Percy	5	Dalkeith	4	Stuart-Wortley	4
20	Churchill	3	Curzon	3	Somerset	3
19	Howard	5	Agar-Robartes	3		
18	Baring	5	Grosvenor	5	Thynne	3

Table 5.2: Examples of Large Families

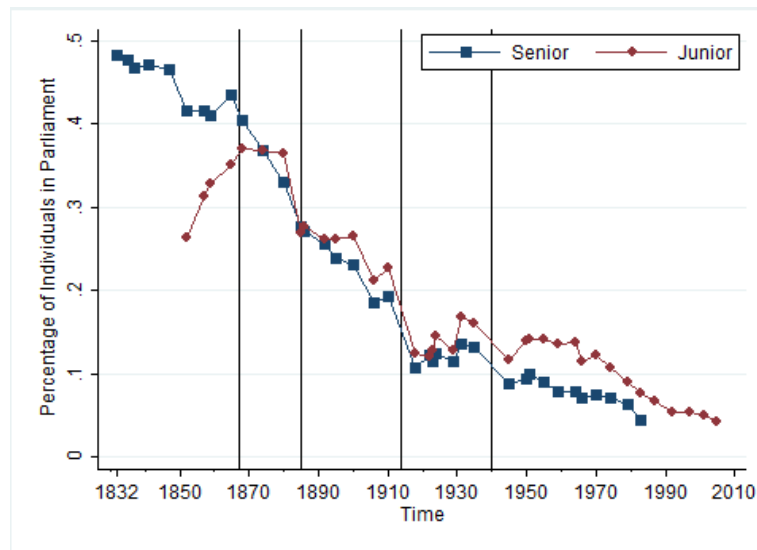


Figure 5.2: Evolution of Political Dynasties over Time

MPs elected in general elections between 1832 and 2005, 27% had a family connection to at least one other MP in that period. 18% of MPs can be identified as a *senior*, i.e. an MP who has relatives entering parliament after his or her first parliament, and 17% can be identified as a *junior*, i.e. an MP who entered parliament after a relative of theirs.

Figure 5.1 shows that the size of the families is highly positively skewed.⁴⁰ In fact, most family connections between MPs were limited to two or three generations of members in different parliaments. Table 5.2 gives examples of a number of very large families. The first column displays the number of family members in parliament, the second column the most widely occurring surname within this group, followed by the number of members carrying this surname, the next column shows the second most occurring surname within that family and so on. As a result of the construction of the dataset so that it identifies all links for which some evidence exists, some renowned names can be found in the same family group. Arguably, this is because they became interlinked through marriage.

Finally, figure 5.2 shows that political dynasties have primarily been a nineteenth century phenomenon in the UK. At the start of the period under study, the proportion of MPs establishing or continuing a dynasty was almost 50%. However, even in

⁴⁰Some large outliers were omitted from this graph to improve legibility. Some examples of these large outliers can be found in table 5.2.

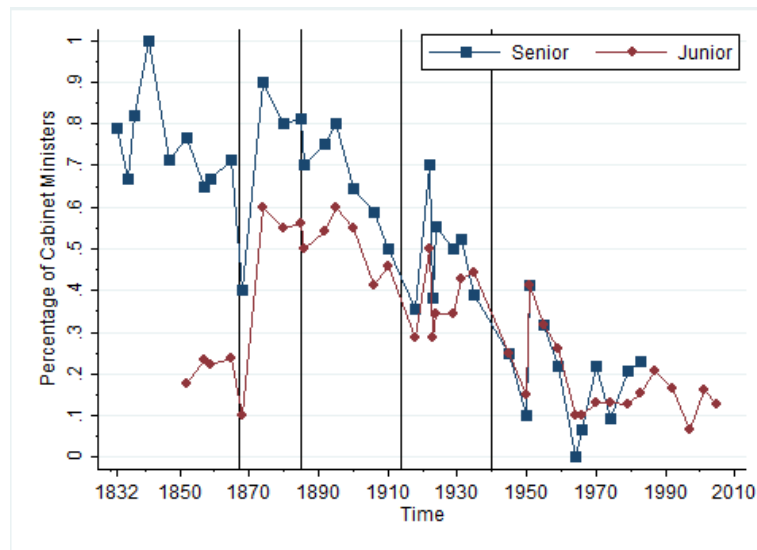


Figure 5.3: Evolution of Political Dynasties in Cabinet over Time

the nineteenth century the trend for this proportion of senior MPs was already clearly downward. Their presence in parliament has declined over time to such a degree that this particular type of MP seems to be almost extinct now.⁴¹ The vertical lines indicate the timing of the second and third Reform Acts which increased the eligible voting population,⁴² and the onset of the two World Wars. A similar figure for the proportion of cabinet ministers with dynastic links confirms the picture that the nineteenth century was more the age of dynasties than the twentieth (see figure 5.3). However, compared to figure 5.2, we can observe that the proportions of cabinet ministers who were dynastic are even larger than the proportions of MPs who were dynastic over time. This figure does not include cabinet ministers drawn from the House of Lords (who may have inherited their peerages from previous cabinet ministers). So, this figure seems to suggest that there are additional advantages for power bequest beyond elections. However, election and selection into the cabinet is of course not random. In the next section, I will discuss the identification strategy and present the results.

⁴¹Figure 5.2 presents the time series up to 1974, because MPs entering after this point may not all have had the same probability of seeing relatives enter politics up until 2005.

⁴²In contrast to what this figure suggest, [Berlinski, Dewan and Van Coppenolle \(2013\)](#) find no evidence that the introduction of new tranches of voters in otherwise unchanged constituencies can be causally linked to the decline of dynasties observed in the time-series.

5.4 Results

If the existing status quo between elites does not matter, we should find similar electoral effects in the UK compared to other countries. In this section, I aim to identify whether serving longer increases an MP's chances of establishing a political dynasty.

5.4.1 Identifying the Effect of Tenure Length

As name recognition advantages and more established political networks increase with longer tenure, we can expect a legislator's probability of establishing a dynasty, i.e. of becoming a senior, to increase as the MPs serves longer. However, members with longer tenures may be different from members with fewer terms in a number of ways that may confound the effect of a longer parliamentary service alone. To identify the effect of tenure length alone, I employ a regression discontinuity design which considers only those first re-elections where the outcome was very close. The identifying assumption is that those MPs who narrowly won their first re-election attempt are in no particular way different from those MPs who narrowly lost.⁴³ First, I present results for a reduced form approach, which considers the effect of winning a first re-election attempt on the probability of seeing a relative entering parliament afterwards (i.e. on the probability of being a senior). Next, I present the results from a RD-IV, which employs a two-stage instrumental variable approach using winning the first re-election as an instrument for serving longer. In this case, we take into account that while everyone who won their first re-election attempt served at least two terms, some of those who lost their first re-election attempt nevertheless served more than two terms, after winning a subsequent election. Therefore, in the RD-IV approach the probability of serving more than two terms is instrumented by narrowly winning or losing a first re-election attempt. In this way, we can assess the causal effect on the probability of establishing a dynasty that derives only from being in power longer.

⁴³See [Lee, Moretti and Butler \(2004\)](#) for a topical example of a regression discontinuity design in political research.

5.4.1.1 Identification Strategy

To consider whether serving longer increases the probability of establishing a dynasty, I now employ a regression discontinuity design to close first re-elections (f).⁴⁴ The dependent variable is a dummy variable *longterm* which identifies those MPs who have served for two parliamentary terms or longer. By definition, those MPs who won a first re-election attempt (*win* equals one) served a long term. Therefore, I estimate the following reduced form model:

$$P(\textit{senior})_i = \beta_1 + \beta_2 \textit{win}_{i,c,p}^f + \beta_3 X_i + \beta_4 X_r + \beta_5 X_p + \epsilon_i \quad (5.1)$$

with i an indicator for the MP, c an indicator for the constituency, p an indicator for the parliamentary term, and X_i a vector of individual characteristics for the MP, and fixed effects for regions (X_r) and parliamentary terms (X_p) respectively.

The identifying assumption for the causal effect of winning a first re-election attempt in this model relies on the idea that there are no differences between close winners and losers. However, as many constituencies elected more than one MP in the nineteenth century, the loser of the first seat in such a constituency might still have won the second seat and served in parliament. To ensure that the difference between winners and losers is only tenure length, I consider only winners and losers of the marginal seat in a constituency. Close elections are then defined as those elections for marginal seats in which the margin of vote between the winner and the loser of that marginal seat is very small. A variable *marginvote* is defined as follows:

⁴⁴To assess the electoral advantages of junior dynastic candidates, one might argue that we should ideally compare winners and losers. On the other hand, only those candidates who have served at least once would ever be called part of a "dynasty" if their juniors enter politics afterwards. In any case, biographical information is much more difficult to find for losing candidates, especially for those who lost and never served. For a subsample of close first elections I attempted to collect information for 262 losing candidates who never served using Wikipedia, Who's Who, and The Times Guide to the House of Commons. However, for the large majority of losing candidates no biographical information at all could be found for these candidates using the first two sources. While the Times Guide to the House of Commons does describe losing candidates, there was no information on family links to MPs which could mean this information was not collected, or that no candidate was a junior of a political family. In total, only 2 individuals could be distinguished as seniors and 3 as juniors. Therefore, this paper can only consider first re-elections.

$$\text{marginvote}_{i,c,p} = \frac{\text{votes}_{i,c,p} - \text{votes}_{j,c,p}}{\text{electors}_{c,p}} \quad (5.2)$$

with i an indicator for the MP, j an indicator for the closest candidate the MP won or lost from, c an indicator for the constituency and p an indicator for the parliamentary term.

There are 2,773 narrow first re-election attempts in general elections between 1837 and 1918 and about 72% of MPs also won their first re-election attempt. This sample disregards first re-elections in 1835, the second parliament of the dataset, and starts with elections from the third parliament in 1837 onwards. In this way, the effect cannot be driven by false positives (i.e. by coding many of the MPs in the second parliament as running for re-election in the first general election after the start of the dataset). All further re-election analyses will use the same start point for the re-election sample. When considering all first re-election attempts (for marginal seats), there are important differences in terms of the characteristics of the individuals returned in these races (see table 5.3). For example, senior and junior dynastic MPs are more likely to have run in an unopposed first re-election race. Perhaps surprisingly, an increasing vote share in a first re-election for a marginal seat is negatively associated with a senior running, but not significantly associated with more junior dynastic MPs. Broadly speaking, MPs are not equally distributed across race type, and dynasties tend to run for their first re-election in less competitive environments. As a result, OLS results comparing all winners and losers of a first re-election attempt across all races are likely to be biased. The bias may arise if we cannot control for omitted variables that make certain individuals both more likely to serve longer and to establish a dynasty. Table 5.4 presents the baseline OLS results. Table 5.4 already suggests that there is unlikely to be any effect of tenure length on dynastic persistence. The estimate on winning the first re-election is close to zero, and is generally not statistically significant. However, serving a long term is associated with an increased probability of establishing (or continuing) a dynasty by about 8%. These results seemingly contradict each other. The OLS estimates are also likely to suffer from omitted variable bias. Therefore, a closer investigation of close re-elections only is warranted.

For the RD approach to be reliable for estimating causal effects, the differences

Table 5.3: Characteristics of MPs and First Re-elections

	Unopposed	S.E.	N	Vote Share	S.E.	N
Senior	0.069***	[0.018]	3,110	-0.102*	[0.060]	2,317
Junior	0.032*	[0.017]	3,110	0.069	[0.058]	2,317
Aristocrat	0.091***	[0.017]	3,110	-0.020	[0.057]	2,317
University degree	-0.060***	[0.020]	3,110	0.258***	[0.069]	2,317
Oxbridge	-0.035*	[0.019]	3,110	0.018	[0.063]	2,317
Age	-2.290***	[0.451]	3,057	-0.346	[1.495]	2,282
Army	0.039**	[0.016]	2,814	-0.123**	[0.051]	2,104
Banker	0.002	[0.005]	2,814	-0.085***	[0.017]	2,104
Barrister	-0.050***	[0.017]	2,814	0.008	[0.059]	2,104
Business	-0.001	[0.001]	2,814	0.001	[0.004]	2,104
Civil Service	-0.002	[0.004]	2,814	-0.001	[0.013]	2,104
Company Director	-0.056***	[0.016]	2,814	0.226***	[0.056]	2,104
University	-0.014***	[0.005]	2,814	0.011	[0.019]	2,104
Engineer	-0.005	[0.004]	2,814	-0.003	[0.015]	2,104
Diplomat	0.006	[0.004]	2,814	-0.005	[0.013]	2,104
Press	0.003	[0.004]	2,814	0.018	[0.013]	2,104
Insurance Finance	-0.002	[0.004]	2,814	0.021*	[0.013]	2,104
Landowner	0.071***	[0.015]	2,814	-0.206***	[0.047]	2,104
Miner	-0.005	[0.004]	2,814	0.064***	[0.014]	2,104
Navy	0.004	[0.006]	2,814	-0.029	[0.019]	2,104
Solicitor	-0.009	[0.007]	2,814	0.080***	[0.026]	2,104
School	0.006**	[0.003]	2,814	0.005	[0.008]	2,104
Private income	0.016*	[0.009]	2,814	-0.080***	[0.029]	2,104
Health	-0.001	[0.004]	2,814	0.026*	[0.015]	2,104
Religion	-0.001	[0.002]	2,814	-0.001	[0.008]	2,104
Trade Union	-0.001	[0.001]	2,814	0.002	[0.004]	2,104
Merchant	0.010	[0.009]	2,814	-0.085***	[0.030]	2,104
Manufacturer	0.0004	[0.002]	2,814	-0.005	[0.007]	2,104
Administrator	-0.009**	[0.004]	2,814	0.014	[0.015]	2,104
Farmer	0.020***	[0.005]	2,814	0.027**	[0.013]	2,104
Writer Journalist	0.004	[0.007]	2,814	0.047**	[0.023]	2,104
Conservative	0.055***	[0.020]	3,110	-0.150**	[0.067]	2,317
Liberal	-0.139***	[0.020]	3,110	-0.289***	[0.069]	2,317
Labour	-0.019***	[0.005]	3,110	0.033*	[0.019]	2,317
Year	-13.069***	[1.026]	3,110	75.988***	[3.080]	2,317

Characteristics regressed on running unopposed and vote share respectively.

Sample includes winners and losers of marginal seats only. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table 5.4: OLS results

Senior						
Win	0.028*	0.019	0.025			
	[0.017]	[0.017]	[0.017]			
Win*Junior			0.056			
			[0.046]			
Longterm				0.085***	0.082***	0.079***
				[0.016]	[0.017]	[0.018]
Longterm*Junior						0.105*
						[0.059]
Junior			0.340***			0.291***
			[0.039]			[0.054]
Aristocrat		0.037	0.126***		0.034	0.121***
		[0.029]	[0.023]		[0.029]	[0.023]
University degree		-0.041*	-0.035		-0.038	-0.035
		[0.024]	[0.023]		[0.023]	[0.023]
Oxbridge		0.0003	0.019		-0.005	0.016
		[0.024]	[0.024]		[0.024]	[0.024]
Age		-0.0005	-0.001		-0.0003	-0.001
		[0.001]	[0.001]		[0.001]	[0.001]
Conservative		0.031	0.001		0.031	0.005
		[0.027]	[0.027]		[0.027]	[0.026]
Liberal		0.048*	0.013		0.051**	0.018
		[0.026]	[0.025]		[0.026]	[0.025]
Labour		0.073	0.078		0.063	0.072
		[0.062]	[0.064]		[0.062]	[0.064]
Occupations	No	Yes	Yes	No	Yes	Yes
Observations	1,972	1,770	2,421	1,972	1,770	2,421
R-squared	0.053	0.066	0.283	0.061	0.074	0.289

OLS regression results of the dependent variable senior on winning and longterm. Sample includes winners and losers of all narrow first re-elections. Individuals did not die in office. All models include parliament and region fixed effects. Errors clustered at (broad) constituency level. Models 3 and 6 include juniors. *** p<0.01, ** p<0.05, * p<0.1.

between winners and losers should be small within small margins of winning or losing the marginal seat in constituencies. To give support to this assumption table 5.5 presents the results of t-tests for differences in means between winners and losers of close first re-election attempts, for narrow re-elections with a margin of vote of 5% and 2.5% respectively. The results from this table confirm that there are few differences between winners and losers, and any remaining differences are not systematically significant across the two vote margins (except for *year*, the year in which the election took place). This lends credibility to the internal validity of the regression discontinuity design in this context.

5.4.1.2 RD

The results of the estimation of the reduced form model of equation 5.1 are presented in table 5.6. In this table, the first three columns present results for those close re-election attempts with a window for marginvote of 5%, the last three columns for a margin of vote window of 2.5%. A first set of models for these windows excludes individual controls (columns 1 and 4). A second set of models includes individual controls (columns 2 and 5). These first two sets of models restrict the sample to those MPs who were not a junior family member themselves. In this way the effect of serving longer for families new to the House of Commons can be distinguished. Finally, the last set of results includes those MPs that were already juniors from political families as comparison. These models include an additional control for junior dynastic status as well as an interaction term to determine whether there is a differential effect of serving longer for different types of MPs (columns 3 and 6). All models exclude MPs who died in office.⁴⁵

The estimates in all columns are both small and not statistically significant. Hence, these results do not indicate that there is a causal effect of winning a first re-election attempt alone on establishing or continuing a political dynasty which is statistically significantly different from zero. Moreover, there is no statistically significant different effect of serving at least two terms for those MPs who were continuing rather than

⁴⁵MPs who died in office may have been immediately succeeded by a relative elected in a by-election, so we want to exclude them from consideration. This also makes the results as closely comparable as possible to those of the existing literature. Similar results were found when including MPs who died in office.

Table 5.5: Characteristics of Close Winners and Losers

	5%			2.50%		
	Win	S.E.	N	Win	S.E.	N
Senior	-0.003	[0.030]	601	0.009	[0.040]	329
Aristocrat	0.008	[0.027]	601	0.034	[0.037]	329
University degree	0.041	[0.041]	601	0.046	[0.055]	329
Oxbridge	0.067*	[0.036]	601	0.063	[0.049]	329
Age	-0.844	[0.840]	589	-0.399	[1.183]	324
Army	-0.024	[0.027]	544	-0.032	[0.039]	298
Banker	-0.007	[0.012]	544	-0.009	[0.016]	298
Barrister	0.011	[0.037]	544	0.023	[0.050]	298
Business	-0.004	[0.004]	544			
Civil Service	0.004	[0.006]	544	0.002	[0.010]	298
Company Director	0.045	[0.034]	544	0.021	[0.045]	298
University	0.005	[0.013]	544	0.030**	[0.013]	298
Engineer	0.011	[0.008]	544	0.023*	[0.012]	298
Diplomat	0.008	[0.009]	544	0.002	[0.010]	298
Press	0.008	[0.007]	544	0.017	[0.013]	298
Insurance Finance	-0.011	[0.008]	544	-0.004	[0.012]	298
Landowner	-0.003	[0.025]	544	-0.019	[0.034]	298
Miner	-0.007	[0.005]	544			
Navy	-0.007	[0.010]	544	-0.009	[0.016]	298
Solicitor	0.016	[0.016]	544	-0.015	[0.018]	298
School	0.004	[0.004]	544			
Private income	-0.028*	[0.014]	544	-0.033	[0.021]	298
Health	0.004	[0.010]	544	0.011	[0.015]	298
Religion	0.0002	[0.005]	544	0.008	[0.007]	298
Merchant	0.002	[0.018]	544	0.018	[0.026]	298
Manufacturer	0.004	[0.004]	544			
Administrator	0.0005	[0.009]	544	0.003	[0.013]	298
Farmer	-0.003	[0.011]	544	0.003	[0.013]	298
Writer Journalist	-0.021	[0.015]	544	-0.031	[0.023]	298
Conservative	0.070*	[0.039]	601	0.016	[0.054]	329
Liberal	-0.06	[0.041]	601	-0.006	[0.056]	329
Labour	0.004	[0.009]	601	-0.004	[0.011]	329
Year	6.003***	[1.834]	601	5.807**	[2.559]	329

Characteristics regressed on winning at a 5% and 2.5% margin of vote respectively.

Sample includes winners and losers of marginal seats only. Individuals did not die in office and were not juniors themselves. *** p<0.01, ** p<0.05, * p<0.1.

Table 5.6: Reduced Form RD Results

Senior	5%			2.5%		
Win	0.011 [0.030]	0.008 [0.030]	0.005 [0.031]	0.011 [0.042]	0.033 [0.045]	0.029 [0.046]
Junior			0.350*** [0.059]			0.339*** [0.077]
Win*			0.039 [0.081]			0.122 [0.120]
Junior						
Aristocrat		-0.02 [0.051]	0.058 [0.044]		-0.083 [0.069]	0.043 [0.057]
University degree		-0.05 [0.051]	-0.041 [0.050]		-0.046 [0.076]	-0.036 [0.070]
Oxbridge		-0.055 [0.047]	-0.035 [0.049]		-0.036 [0.074]	-0.033 [0.071]
Age		-0.0002 [0.002]	-0.003* [0.002]		0.002 [0.002]	-0.002 [0.002]
Conservative		0.057 [0.057]	0.027 [0.053]		0.108 [0.088]	0.027 [0.082]
Liberal		0.056 [0.048]	0.013 [0.050]		0.061 [0.075]	0.007 [0.073]
Labour		0.181 [0.168]	0.182 [0.153]		0.014 [0.077]	0.066 [0.082]
Occupations	No	Yes	Yes	No	Yes	Yes
Observations	601	534	718	329	294	407
R-squared	0.039	0.102	0.235	0.05	0.138	0.268

Reduced form regression results of the dependent variable senior on winning. Sample includes winners and losers of marginal seats only. Individuals did not die in office. All models include parliament and region fixed effects. Errors clustered at (broad) constituency level. Models 3 and 6 include juniors. *** p<0.01, ** p<0.05, * p<0.1.

establishing a political dynasty (column 3 and 6 of table 5.6). These null results are not only estimated to be close to zero, but the 95% confidence intervals around these estimates are also quite narrow. For example, the results from the first model suggest that winning rather than losing a first re-election attempt affects the expected probability of establishing a dynasty by anything between decreasing the probability by about 5% to increasing it by about 7%. As the winning margin is narrowed to 2.5%, the estimates increase only very slightly. From model 4 we can conclude that winning rather than losing the first re-election attempt may have affected the probability of establishing a dynasty by either decreasing it by about 7% or increasing it by about 9%, or anything in between. If this range is still somewhat large, we can at least conclude that if there was any effect, positive or negative, it was still very small in size.

5.4.1.3 RD-IV: Instrumenting Longterm with Winning a Close First Re-election Attempt

By definition, all MPs who won their first re-election attempt served a longterm. However, the MPs who lost their first re-election attempts may have successfully competed in subsequent elections, hence re-entering parliament later and serving a longterm. For this reason I now instrument longterm by winning a first re-election attempt and then estimate the effect of serving a long term on the probability of establishing a dynasty.⁴⁶

The first stage results of a 2SLS estimation are presented in table 5.7 and table 5.8 shows the results of the RD-IV. Table 5.7 confirms that the relevance condition (winning a first re-election attempt significantly affects longterm) is fulfilled. However, table 5.8 shows that the effect of serving a longterm (as instrumented by winning a first re-election attempt with a narrow margin) on the probability of establishing a political dynasty cannot be statistically significantly distinguished from zero. The estimated effects using the RD-IV design are small across all models, whether or not we add

⁴⁶This conforms to the empirical strategy used by Dal Bó, Dal Bó and Snyder (2009). A difference with their approach is that I employ only one instrument, winning a first re-election attempt. Adding additional instruments (e.g. interaction terms of winning with personal characteristics) can increase the precision of the estimate (see Angrist and Pischke, 2009, 259-267). However, the downside of this approach is that the instrumented effect is more difficult to interpret. As the surprising result in this paper is a null result, I prefer to present the results that can be more straightforwardly interpreted. Therefore, I restrict the sample to points around the discontinuity rather than employing a fully parameterized model with polynomial controls which could better attenuate bias in the estimate. Estimating the alternative specification yields qualitatively similar results.

individual controls or include MPs who were juniors themselves in the model. In all cases, the null hypothesis of no effect of serving at least two terms cannot be rejected. Hence, these models also offer little evidence that serving longer alone increased the probability of UK MPs to establish or continue a political dynasty.

Table 5.7: First Stage of 2sls IV

	5%				2.5%			
	Longterm	Longterm	Longterm	Longterm*Jr	Longterm	Longterm	Longterm	Longterm*Jr
Win	0.622*** [0.028]	0.600*** [0.031]	0.591*** [0.030]	-0.005 [0.006]	0.602*** [0.036]	0.580*** [0.039]	0.570*** [0.039]	-0.008 [0.010]
Junior			-0.126** [0.064]	0.482*** [0.055]			-0.057 [0.076]	0.518*** [0.060]
Win*			0.076	0.514***			-0.004	0.470***
Junior			[0.058]	[0.052]			[0.072]	[0.058]
Aristocrat		0.057 [0.044]	0.037 [0.034]	0.008 [0.022]		-0.003 [0.068]	0.017 [0.050]	0.016 [0.034]
University degree		-0.104** [0.045]	-0.073* [0.041]	0.005 [0.022]		-0.093 [0.069]	-0.059 [0.061]	-0.001 [0.030]
Oxbridge		0.105** [0.047]	0.058 [0.041]	-0.017 [0.021]		0.142* [0.077]	0.082 [0.065]	-0.007 [0.032]
Age		-0.005*** [0.002]	-0.005*** [0.001]	-0.002** [0.001]		-0.006** [0.003]	-0.007*** [0.002]	-0.002*** [0.001]
Conservative		-0.033 [0.060]	-0.081 [0.052]	-0.063** [0.025]		-0.043 [0.089]	-0.111 [0.073]	-0.090** [0.035]
Liberal		-0.001 [0.056]	-0.042 [0.048]	-0.040* [0.023]		0.008 [0.077]	-0.064 [0.061]	-0.075** [0.030]
Labour		0.079 [0.150]	0.029 [0.147]	-0.049 [0.034]		0.361 [0.226]	0.308 [0.220]	-0.053 [0.043]
Occupations	No	Yes	Yes	Yes	No	Yes	Yes	Yes
Observations	601	534	718	718	329	294	407	407
R-squared	0.459	0.478	0.453	0.817	0.423	0.453	0.43	0.796

First stage of the 2sls estimation, showing longterm regressed on winning. Sample includes winners and losers of marginal seats only. Individuals did not die in office. All models include parliament and region fixed effects. Errors clustered at (broad) constituency level. Models 3 and 6 include juniors. *** p<0.01, ** p<0.05, * p<0.1.

Perhaps name recognition in elections became more important as the country increased democratic participation. If this is the case, we may expect the effect to show up only in certain time periods. For this reason, we can run the same analysis by looking at different periods. Table 5.9 presents the reduced form results for the period of 1837 up to the parliament of 1865, 1868 up to the parliament of 1880, 1885 up to the parliament of 1900 and 1906 up to the parliament of 1910. The first period starts with the elections for the third parliament after the First Reform Act, or the first franchise extension, and the start of the dataset. In this way, we can avoid that the effects may be mechanically driven by the fact that many MPs ran for a first re-election in the second parliament. The first period ends right before the introduction of the Second Reform

Table 5.8: RD-IV Results

Senior	5%			2.5%		
Longterm	0.018 [0.048]	0.014 [0.047]	0.009 [0.050]	0.018 [0.068]	0.058 [0.070]	0.054 [0.074]
Junior			0.084 [0.157]			0.242 [0.214]
Longterm*			0.306** [0.125]			0.225 [0.157]
Junior						
Aristocrat		-0.021 [0.048]	0.057 [0.042]		-0.083 [0.062]	0.038 [0.052]
University degree		-0.048 [0.048]	-0.041 [0.047]		-0.041 [0.070]	-0.032 [0.066]
Oxbridge		-0.056 [0.045]	-0.034 [0.047]		-0.044 [0.069]	-0.036 [0.069]
Age		-0.0001 [0.002]	-0.003* [0.002]		0.002 [0.002]	-0.001 [0.002]
Conservative		0.058 [0.054]	0.033 [0.053]		0.111 [0.080]	0.054 [0.083]
Liberal		0.056 [0.046]	0.017 [0.049]		0.061 [0.068]	0.028 [0.074]
Labour		0.180 [0.160]	0.186 [0.148]		-0.006 [0.079]	0.062 [0.085]
Occupations	No	Yes	Yes	No	Yes	Yes
Observations	601	534	718	329	294	407
R-squared	0.044	0.105	0.241	0.054	0.149	0.284

Results from the 2sls RDIV estimation of senior regressed on longterm as instrumented by winning a first re-election attempt. Sample includes winners and losers of marginal seats only. Individuals did not die in office. All models include parliament and region fixed effects. Errors clustered at (broad) constituency level. Models 3 and 6 include juniors.

*** p<0.01, ** p<0.05, * p<0.1.

Act, which was adapted by parliament in 1867 and produced the second franchise extension. The second period starts with the first election run under the new franchise up to and including the election of 1880 and the third period begins after the third franchise extension, in 1885, up to and including the parliament of 1900. Finally, I define a fourth period for the final three parliaments between 1906 and 1910 before the onset of the First World war.

In fact, the results from table 5.9 are not in the direction we would expect if name recognition became increasingly important over time. Before the Third Reform Act (results in the left hand part of table 5.9) when the franchise was most restricted, the estimated effect of narrowly winning a first re-election attempt is estimated to be both positive and negative, and the effects are generally not statistically significant. If anything, there is some indication of a positive effect of increased name recognition and/or political networks. After the Third Reform Act extended the franchise further, the estimates become negative (upper half of right side of table 5.9), or positive (lower half of right side of table 5.9), but generally remain statistically indistinguishable from zero. The results for a similar time break-up of the RD-IV are similar in nature. Table 5.10 shows that the effects are positive for the first two and last time period, and negative for the third (upper right half of table 5.10). The main difference is that some effects achieve statistical significance in some specifications, but not consistently across specifications and time. As the results from the reduced form approach, these results seem to confirm that if anything, the effects of name recognition and political networks were positive before 1885 and not very much increased after the Second Reform Act. After the Third Reform Act in 1885, if anything serving longer as a result of narrowly winning a first re-election attempt is found to negatively affect one's prospects of establishing a dynasty. However, the effect becomes positive again after about twenty years. To summarise, there is little evidence that name recognition or political networks became more important as the UK increased democratic participation in the way we expected it to have evolved.

Table 5.9: Reduced Form Results across Time

Senior	5%		2.5%		Senior		5%		2.5%	
Time:	1837-1865					1885-1900				
Win	-0.013 [0.074]	0.049 [0.101]	-0.001 [0.097]	0.137 [0.186]	0.045 [0.167]	-0.037 [0.043]	-0.051 [0.046]	-0.045 [0.051]	-0.078 [0.055]	-0.091 [0.070]
Junior			0.265** [0.144]		0.237 [0.190]	Junior		0.377*** [0.122]		0.455*** [0.120]
Win *			0.002 [0.196]		0.016 [0.274]	Win*		0.135 [0.149]		0.249 [0.193]
Junior			154 [0.196]		85 [0.274]	Junior		260 [0.149]		145 [0.193]
Observations	146	112	154	59	85	Observations	196	260	115	106
R-squared	0.0002	0.227	0.263	<0.0001	0.38	R-squared	0.217	0.336	0.016	0.264
Time:	1868-1880					1906-1910				
Win	0.057 [0.078]	0.065 [0.093]	0.107 [0.090]	0.154 [0.115]	0.223* [0.119]	Win	0.049 [0.078]	0.057 [0.077]	0.109 [0.079]	0.137 [0.103]
Junior			0.565*** [0.111]		0.543*** [0.137]	Junior		0.188 [0.139]		0.057 [0.190]
Win *			-0.213 [0.170]		-0.464 [0.298]	Win*		0.064 [0.184]		0.184 [0.285]
Junior			137 [0.170]		89 [0.298]	Junior		167 [0.184]		88 [0.285]
Observations	100	96	137	60	89	Observations	130	167	76	69
R-squared	0.005	0.203	0.429	0.392	0.498	R-squared	0.191	0.258	0.024	0.412
Individual controls	No	Yes	Yes	Yes	Yes	Individual controls	No	Yes	No	Yes
Occupations	No	Yes	Yes	Yes	Yes	Occupations	No	Yes	No	Yes

Reduced form regression results of the dependent variable senior on winning. Sample includes winners and losers of marginal seats only. Individuals did not die in office. All models include parliament and region fixed effects. Errors clustered at (broad) constituency level. Models 3 and 6 include juniors. *** p<0.01, ** p<0.05, * p<0.1.

Table 5.10: RD-IV Results across Time

Senior		5%		2.5%		Senior		5%		2.5%	
Time:		1837-1865				Time:		1885-1900			
Longterm	0.039	0.086	-0.001	0.01	0.223	0.071	Longterm	-0.079	-0.149	-0.147	-0.183*
	[0.122]	[0.148]	[0.148]	[0.146]	[0.215]	[0.212]		[0.066]	[0.101]	[0.101]	[0.108]
Junior		0.264	[0.252]		0.221	[0.297]	Junior	0.226	0.156		0.156
Longterm*		0.003	[0.320]		0.035	[0.394]	Longterm*	[0.262]	[0.266]		[0.266]
Junior	146	112	154	77	59	85	Junior	0.285	0.536		0.536
Observations	0.06	0.241	0.263	0.035	0.268	0.398	Observations	[0.310]	[0.360]		[0.360]
R-squared							R-squared	0.001	0.181	0.115	106
								0.181	0.334	0.187	0.467
Time:		1868-1880				Time:		1906-1910			
Longterm	0.092	0.116	0.196	0.078	0.325*	0.464**	Longterm	0.105	0.072	0.209	0.250*
	[0.134]	[0.137]	[0.138]	[0.175]	[0.197]	[0.217]		[0.080]	[0.101]	[0.130]	[0.150]
Junior		0.759***	[0.201]		1.045***	[0.335]	Junior	0.05	0.085	0.05	-0.316
Longterm*		-0.367	[0.258]		-0.952**	[0.480]	Longterm*	[0.306]	[0.306]	[0.579]	[0.579]
Junior	100	137	137	61	60	89	Junior	0.206	0.206	0.206	0.566
Observations	0.049	0.221	0.429	0.099	0.376	0.385	Observations	[0.367]	[0.367]	76	69
R-squared	No	Yes	Yes	No	Yes	Yes	R-squared	0.038	0.246	0.042	0.394
Individual controls	No	Yes	Yes	No	Yes	Yes	Individual controls	No	Yes	No	Yes
Occupations	No	Yes	Yes	No	Yes	Yes	Occupations	No	Yes	No	Yes

Results from the 2sls RDIV estimation of senior regressed on longterm as instrumented by winning a first re-election attempt. Sample includes winners and losers of marginal seats only. Individuals did not die in office. All models include parliament and region fixed effects. Errors clustered at (broad) constituency level. Models 3 and 6 include juniors. *** p<0.01, ** p<0.05, * p<0.1.

5.4.1.4 Interpretation of the Causally Identified Null Results

The tentative conclusion we can draw from the identification strategy employed and the results presented in tables 5.6 to 5.8 is that serving longer in parliament alone did not increase an MP's probability of establishing or continuing a political dynasty. This result is surprising, as we expected to identify a positive effect of serving longer that works through increased name recognition and more extensive political networks. The results do not allow us to conclude definitively that name recognition did not matter at all. The estimated effect is a local average treatment effect for marginal elections of the marginal seats only. As other first re-elections were shown to be different (see table 5.3), the external validity of this result to all re-elections is not straightforward. However, it is likely that the positive effect of tenure length identified in a normal OLS estimation of all these elections (see table 5.4) suffers from omitted variable bias. In light of recent results in the literature, the causally identified null result of electoral selection may not be as surprising as it seems.

Querubin in a similar analysis for the Philippines identified a null result when considering re-elections. In spite of finding much evidence of electoral advantages for junior family members, he finds no additional advantages of winning a first re-election attempt and serving longer (Querubin, 2010, 38). In contrast, results for the US and for Argentina suggest an increase of about 8% on the probability of establishing a dynasty when serving a longterm or an additional five years respectively (Dal Bó, Dal Bó and Snyder (2009); Rossi (2009)). Taken together with the findings presented here for the UK, this suggests that random variation in tenure length has different effects in different contexts. I suggest one possible explanation for these contrasting results, related to the two theoretical mechanisms suggested in the literature, political networks and name recognition. First, the extent to which new legislators can use electoral wins to build political networks (e.g. political machines, dominant electoral campaign strategies, capital, centrality within the party, etc.) depends on how much the existing elites have already captured the democratic system. The results are consistent with the idea that in the UK elites were already so well-entrenched that there was little room for additional, new elites to enter. The fact that these data underestimate the real presence of political dynasties because they can only discern links to other MPs, and not to peers in the House of Lords, also argues for this interpretation of the null result. Second, once

citizens have elected (dynastic) legislators, they are able to update their beliefs about the politicians' political quality. The extent to which citizens do this may be crucially related to the pre-existing level of elite capture. In contexts as the Philippines and nineteenth century Britain, where dynasties are so common that they cannot serve as a clear signal of exceptional quality this updating about the quality of the dynasty may occur before the first re-election attempt, so that there is no additional intra-personal bonus of winning a first re-election attempt in these countries.

An alternative explanation is that a first re-election may have meant very different things in the US and the UK at the time. In the US for example, the additional name recognition gained from an additional parliamentary term may have been greater than in the UK. This might be the case if the average length of parliaments in the UK was greater than in the US so that MPs serving a first term would have already been better known by their constituents for their actions in parliament. However, while some parliaments lasted as much as five, six or even eight years, the average length of parliaments before 1918 in the UK was only 4.4 years (with a standard deviation of 1.95 years). Compared to the average length of a term in the US, 2 years for Congressmen but 6 years for senators⁴⁷ this difference is not too large to make the results incomparable. On average, a legislator in the US served 3 years for each term, compared to the 4.4 years in the analyses conducted here. One could also argue that not so much serving a second term as the sort of career experience the MP gained mattered in the UK (e.g. front bench vs. back bench). However, selection to the front bench should in any case relate more to political networks and less to name recognition than winning elections and serving longer in parliament.

While I could not identify a positive causal effect of tenure length, the next section assesses the importance of political networks to dynasties more closely. To start this analysis, I now consider a sample of individual MPs and investigate to what extent the junior MPs in parliament differed from others in terms of factors that are more closely correlated with political networks than with name recognition in elections. In particular, I investigate how their prospects of being selected into the cabinet depend on their relatives' careers.

⁴⁷Dal Bó, Dal Bó and Snyder, 2009, 118.

5.4.2 Political Networks and Cabinet Selection

In this section, I again consider all MPs since 1832 and investigate how dynastic MPs are different from other MPs in terms of personal and career characteristics. Serving in the cabinet is a major career event for an MP. Therefore, the association between junior dynastic MPs and cabinet selection will be of particular interest.

Junior dynastic status can in no sense be causal to other personal characteristics determined at birth (such as gender), nor can we causally identify an effect on certain career factors which were determined later. Nevertheless, the associations presented below can offer a better insight into who these political families were and what mattered for their political careers to succeed.

Table 5.11: Personal Characteristics and Juniors

	Female	Female	Aristocrat	Aristocrat	University degree	University degree	Oxbridge	Oxbridge
Junior	-0.010** [0.004]	0.014*** [0.005]	0.414*** [0.013]	0.315*** [0.014]	0.159*** [0.014]	0.029** [0.011]	0.240*** [0.014]	0.091*** [0.011]
Female				-0.033** [0.013]		0.172*** [0.033]		-0.152*** [0.026]
Aristocrat		-0.008** [0.003]				-0.053*** [0.011]		0.097*** [0.011]
University degree		0.030*** [0.006]		-0.040*** [0.009]				0.534*** [0.009]
Oxbridge		-0.032*** [0.006]		0.089*** [0.011]		0.643*** [0.007]		
Constant	0.032*** [0.002]	0.071*** [0.008]	0.097*** [0.003]	0.041*** [0.007]	0.434*** [0.006]	0.273*** [0.013]	0.212*** [0.005]	-0.045*** [0.009]
Occupations	No	Yes	No	Yes	No	Yes	No	Yes
Observations	9,044	8,022	9,044	8,022	9,044	8,022	9,044	8,022
R-squared	0.0004	0.074	0.172	0.296	0.014	0.464	0.042	0.442

Results from regressing individual characteristics on junior dynastic status. Robust standard errors. *** p<0.01, ** p<0.05, * p<0.1.

In table 5.11 we can investigate some personal characteristics first. The sample used in these models is intended to give a broad overview across time, and so is no longer restricted to marginal electoral competitions before 1918. In other words, we can now investigate the association with gender for example, as women were not allowed to run for parliament in the nineteenth century. In fact, even when I still include the years in which women were not yet allowed to run, I find a positive, significant association between junior dynastic status and a dummy variable female. Among juniors there were about 1% more females than among other MPs and this association is

Table 5.12: Career Characteristics and Juniors

	Longterm	Longterm	Age at first entry	Age at first entry	Ever Peerage	Ever Peerage	Ever Knighthed	Ever Knighthed
Junior	0.033*** [0.013]	0.005 [0.015]	-6.746*** [0.289]	-2.981*** [0.323]	0.008 [0.007]	-0.004 [0.008]	0.045*** [0.016]	-0.001 [0.017]
Female		0.047 [0.029]		0.651 [0.608]		-0.024*** [0.008]		-0.081*** [0.019]
Aristocrat		0.054*** [0.015]		-5.284*** [0.358]	0.134*** [0.014]	0.121*** [0.015]	0.136*** [0.018]	0.101*** [0.020]
Aristocrat x Junior					0.059*** [0.022]	0.068*** [0.022]	-0.190*** [0.028]	-0.159*** [0.029]
University degree		0.056*** [0.014]		-2.858*** [0.288]		0.008 [0.006]		0.009 [0.012]
Oxbridge		0.040*** [0.014]		-1.695*** [0.315]		0.031*** [0.008]		0.038*** [0.014]
Constant	0.700*** [0.005]	0.713*** [0.015]	44.289*** [0.119]	46.460*** [0.297]	0.027*** [0.002]	0.015*** [0.006]	0.193*** [0.005]	0.099*** [0.010]
Occupations	No	Yes	No	Yes	No	Yes	No	Yes
Observations	9,044	8,022	8,884	7,904	8,487	7,665	9,037	8,022
R-squared	0.001	0.024	0.058	0.181	0.079	0.092	0.009	0.046

Results from regressing career characteristics on junior dynastic status. Robust standard errors. *** p<0.01, ** p<0.05, * p<0.1.

statistically significant. This result is substantive, given that only 3% of individuals are women. This result is broadly suggestive of the idea that it has been easier for women to enter parliament as a member of a political dynasty. While political dynasties may have been useful to women to break into traditional patterns of political power, they seem to have been valuable to aristocrats as well in defending their traditional claim to power. Junior dynastic members are found to be significantly more likely to have held links to the aristocracy.⁴⁸ This underlines how political dynasties may serve as symptoms of aristocratic entrenchment in the UK system. However, juniors are also more likely to have held university degrees upon election and they were more likely to have obtained a degree from Oxford or Cambridge in particular. These results could be interpreted as signals of their more traditional, elite backgrounds, or of how political dynasties may be preferred by the public because of perceived political talents shared in the family which could be associated with a higher likelihood to attend university.

Table 5.12 presents some associations between juniors and certain career characteristics. After controlling for other personal factors, there is only very weak evidence that juniors were more likely to serve at least two terms. The estimate is small and no longer statistically significant when we control for other background characteristics.

⁴⁸This effects remains if we restrict the sample to years after 1918, results not presented here.

In contrast, junior dynastic members are found to have been younger on average at the start of their first parliamentary term, by between about 3 and 7 years. This suggests they may also have been less likely to have held previous political experience, though I cannot directly test this idea. Junior dynastic members were not more likely to have been rewarded by (or succeeded to) a peerage at some point in their careers, unless they were aristocrats, which can of course indicate that these junior aristocrats were more likely to inherit a peerage. Likewise, there is also weak evidence for an association with knighthoods once other factors are controlled for, unless the junior was also an aristocrat in which case being knighted was less likely.

Arguably, the most interesting career factor can be found in table 5.13. There is only weak evidence that junior dynastic members were more likely to enter the cabinet at some point in their careers, except for the juniors of seniors who had served as cabinet ministers before, who were significantly more likely to obtain a cabinet position (column 1 and 2). This effect remains and becomes even larger when restricting the sample to the period after 1918 (columns 3 and 4) and if we consider juniors of cabinet ministers who served after 1918 (columns 5 and 6). Juniors of senior cabinet members are more likely to enter the cabinet compared to all members, but also compared to other juniors alone. Of course, these associations cannot be interpreted as causal effects. It could be that political dynasties share important talents which voters value and this is why they are able to progress more quickly to a cabinet position, or why they are elected at a younger age. Therefore this estimate may, similarly to the OLS estimate of serving longer, still be subject to omitted variable bias. These numbers may be overestimates of the true effect. Still, compared to the (overestimated) OLS estimates of the effect of serving longer, the association between being a junior of a cabinet minister and ever serving in cabinet is estimated to be much larger. Depending on the specification, relatives of cabinet ministers are on average 11% to as much as 25% more likely to become a cabinet minister themselves. Compared to the average probability for all MPs to ever become a cabinet minister (6%, or 8% considering MPs after 1918 only), that is a large effect. The result that junior political dynastic members are more likely to enter the cabinet when they are the junior of someone who has served in cabinet before after controlling for observables, supports the idea that political networks matter, and more so than name recognition, at least for career progression.

Table 5.13: Cabinet Selection and Juniors

	Ever Cab Min	Ever Cab Min	Ever Cab Min	Ever Cab Min	Ever Cab Min	Ever Cab Min
Junior	-0.005 [0.007]	-0.015* [0.008]	0.017 [0.018]	-0.002 [0.020]	0.058*** [0.019]	0.029 [0.021]
Junior* of a senior who was Cab Min	0.121*** [0.020]	0.113*** [0.021]	0.243*** [0.047]	0.246*** [0.048]		
Junior* of a senior who was Cab Min after 1918					0.221*** [0.068]	0.239*** [0.069]
Female		0.035* [0.021]		0.024 [0.022]		0.021 [0.022]
Aristocrat		0.030*** [0.009]		0.028 [0.022]		0.045** [0.023]
University degree		0.023*** [0.007]		0.019* [0.011]		0.019* [0.011]
Oxbridge		0.038*** [0.009]		0.045*** [0.014]		0.045*** [0.014]
Constant	0.052*** [0.003]	0.050*** [0.008]	0.073*** [0.004]	0.057*** [0.010]	0.073*** [0.004]	0.056*** [0.010]
Occupations	No	Yes	No	Yes	No	Yes
Observations	9,044	8,022	4,092	3,602	4,092	3,602
R-squared	0.01	0.043	0.024	0.06	0.016	0.054

Results from regressing career characteristics on junior dynastic status. Robust standard errors. *** p<0.01, ** p<0.05, * p<0.1. The first two columns consider all time periods. The final four columns restrict the sample to MPs who served after 1918 only.

5.5 Conclusion

This paper considered the origins of the success of political dynasties in the UK. Longer tenure length alone was not found to matter as much in the UK as in other contexts. While those legislators who narrowly won a first re-election in the US, for example, were able to use the opportunities thus created to bequest their political capital, this appears not to have been the case for UK MPs. I propose that this is due to the original starting positions of elites across countries. A comparison of all MPs since 1832 further showed that juniors of political dynasties were more likely to be selected to a cabinet position when their relatives were cabinet ministers before them. This cabinet selection effect was not solely a nineteenth century phenomenon.

Whether dynastic occupational interest and family networks in politics are beneficial to the democratic process is ultimately a question which voters decide on. However, this paper exemplifies the importance of understanding why mechanisms play out differently in different contexts, which is crucial to a better understanding of the democratic legitimacy of dynasties. The broader conclusion from the surprising null

result identified in this paper, relevant for further research on dynasties, is that there is cross-national and inter-temporal variation in elite persistence in democracies.

Chapter 6

Conclusion

This dissertation concerns political dynasties and elections. To what extent are political dynasties successful in elections? What explains the continued persistence of dynasties in democracies? Questions about the causes of political dynasties can be categorised in questions concerning the voter demand side of the phenomenon, or to what extent the electoral system may encourage voters to take cues from dynastic status, and questions concerning the elite supply side, or to what extent politicians can establish dynasties. Research questions concerning both sides of the explanation of the phenomenon were addressed in the three papers presented in this dissertation.

The first paper estimated the dynastic vote bonus in preference votes and in the probability of being elected in the Belgian 2010 election. In the context of Belgium, preference votes can measure a preference of voters for junior members of political dynasties over and beyond a preference for the party they are from. Controlling for individual characteristics and factors that could potentially be influenced by nepotistic dynastic control (i.e. list rank and media attention), I found that political dynasties receive on average 17 to 25% more preference votes and are three times more likely to be elected. There is no separate effect of political dynastic status on list rank or media attention. While these results may still be prone to omitted variable bias, the main omitted variable, campaign spending, is limited by Belgian law and is therefore unlikely to bias the coefficients much. When distinguishing between candidates that were found to be members of political dynasties and candidates that were merely found to have the same surname as a previous politician, the dynastic effect remained only for the confirmed political family members. Hence, the name recognition advantage does

not spill over to outsiders who happen to share the same surname. These results suggest that voters have a genuine preference for dynastic candidates. This paper contributes to the existing literature in several ways. The choice of the Belgian case aimed to shed more light on the relevance of the voter demand side in explaining dynastic success. More precisely, current studies of voting in electoral systems with single-members constituencies cannot clearly separate voters' preferences for candidate characteristics from a preference for the party's candidate, so that they cannot distinguish whether the party selects these dynastic candidates, or whether voters prefer them. The particular list type system with preference voting in Belgium allows to estimate a preference for dynastic candidates even when controlling for list rank, which could be influenced by nepotistic networks of dynasties within parties. Finally, the paper concurs with current findings in the literature that dynastic candidates are more experienced and are more likely to have had governing experience.

The second paper considered whether the number of politicians with certain characteristics, for example dynastic status, changes when a country democratises. In the UK, the 1867 the Second Reform Act broadened the franchise to include poorer individuals in boroughs. Key aspects of this reform were that (1) it was unaccompanied by other constitutional changes (2) it widened the franchise to varying extents across constituencies, affecting the boroughs more heavily than the counties and to varying extents, and (3) its exact effects were largely unexpected. This allows us to identify the effect of the 1867 franchise extension on the individual characteristics of the member(s) returned. We find no statistically significant effect of the 1867 reform on the type of MPs returned (e.g. dynastic or aristocratic). When studying the Third Reform Act of 1884, when the franchise in the counties was brought on equal footing to the franchise in the boroughs, we also find no effect of franchise extension on representation. Unfortunately, the changes of the 1884 reform were concurrent with changes in the number of members to be returned and the transformation of many constituencies, making the sample of unchanged constituencies that we can study small. Nevertheless, the main result remains: we cannot attribute the changes in representation to the franchise extension alone. Therefore, if there were any changes in the composition of Parliament, as argued by [Canandine \(1999\)](#), they cannot be causally attributed to the changes in the franchise extension. The results support the claim that little changed for the position of the aristocracy in politics, made by [Laski \(1928\)](#) about the cabi-

net. More broadly, this paper contributes to the knowledge of political dynasties by offering empirical evidence of how institutional changes, even when these encompass democratisation efforts, can have little or no effect on the position of elites in politics.⁴⁹

The third paper reconsidered the case of the UK and plotted the evolution of political dynasties in particular over time. The descriptive statistics indicate that the two reform acts of the nineteenth century, as well as the onset of the first and second World War, caused breaks in the trendline of senior and junior dynastic presence.⁵⁰ While the presence of political dynasties over time in parliament declined steadily, there is no such clear pattern in the cabinet. Using close re-election results to estimate the power-treatment effect of serving a longer term in parliament, I could not identify an effect that is statistically significantly different from zero. This result runs counter to what was found in the similar seminal analysis for the US (Dal Bó, Dal Bó and Snyder, 2009). I argue that this surprising result indicates that the existing status quo between elites is relevant when considering whether elections matter for dynastic entrenchment. In the UK, there is more evidence for the importance of political networks (relative to electoral advantages), as exemplified by the fact that junior dynastic members are more likely to become cabinet ministers if their previous relatives were members of the cabinet too, compared to other (dynastic or non-dynastic) MPs.

The three papers of this dissertation have studied the voter demand and the elite supply side of the phenomenon of political dynasties in elections. The evidence collected here suggests that both are important causal mechanisms for the continued electoral success of political dynasties, but the relevance of either mechanism depends on the electoral context. The expanding literature suggests that the extent of power concentration in the hands of a few families has important economic and political consequences. Therefore, the topic of political dynasties should clearly remain on the research agenda of political scientists. In understanding democratisation processes, and in designing (electoral) institutions, it matters to know in what ways existing elites can shape and/or use formal institutions to their success.

⁴⁹Similarly, Querubin (2011) finds that the introduction of term limits in the Philippines perversely affected the dynastic success and promoted their presence further.

⁵⁰However, as studied in the second paper presented in this dissertation (Berlinski, Dewan and Van Coppenolle, 2013) we know that the changes resulting from the franchise reforms were incidental to the changes in the number of enfranchised individuals on the characteristics of the individuals returned.

Appendix A

Appendix to Chapter 3

A.1 Levels of Representation

Figure A.1 gives a schematic picture of which part of the parliament voters elect and at which level their votes are aggregated. For the House, there are three subdivisions of the assembly, Brussels, Flanders and Wallonia. For each subdivision there are separate candidate lists and voters can only elect candidates from the lists in their subdivision. Voters elect candidates from eleven smaller constituency lists which contain different candidates. Votes are aggregated at constituency level. For the senate, the electoral district borders are different. Flemish voters can only elect candidates from the Dutch-speaking lists and Walloon voters can only elect candidates from the French-speaking lists. However, voters in Brussels can choose whether to vote for the Dutch- or the French-speaking lists (not both). The lists are the same across all lower-level constituencies and votes are aggregated at the subdivision level.

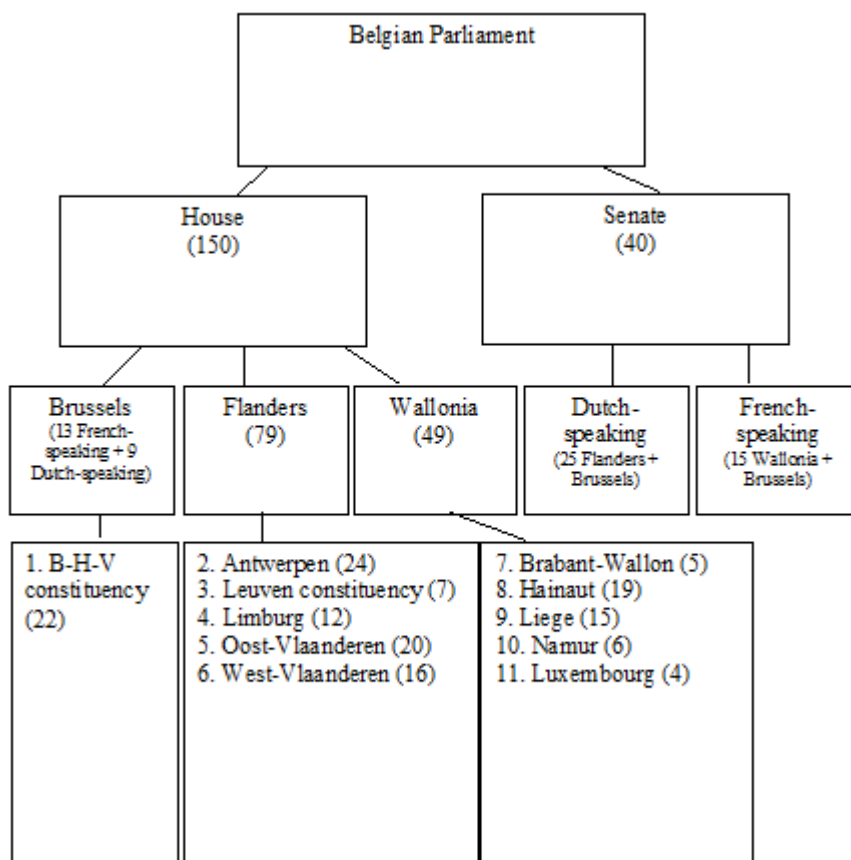


Figure A.1: Schematic Picture of Levels of Representation based on info from <http://www.ibz.rrn.fgov.be>

A. 2 Robustness for Alternative Measurement of Preference Votes

The table below presents the main results for an alternative measure of preference votes, the percentage of all preference votes for a party that the candidate received. Dynastic candidates are predicted to obtain on average between about 1.5% to 6% of all preference votes expressed in a particular party-district list. The estimates are significant at least at the 10% level except when controlling also for candidates who share a surname with a dynasty and for the subsample of Flemish candidates when we control for media attention. Broadly, these estimates are supportive of the main results presented in this paper.

A. 3 Matching as Robustness Check

Finally, I will present results from a matching approach. Matching can address concerns about model dependence, i.e. how the misspecification of the functional form of covariates can bias the estimated causal effect of the variable of interest (Ho et al., 2006). While matching cannot directly address concerns about omitted variable bias or post-treatment bias, it can go some way to address the issue that dynastic candidates may be very different from non-dynastic candidates. For example, one might worry that dynastic candidates are never "hopeless" as certain other candidates in the full dataset may be. Matching on covariates can reduce bias introduced by for example potentially mis-specifying the functional form of rank. Following the advice in Ho et al. (2006) I present results from combining a propensity score pre-processing matching approach with parametric modelling. I pre-match here on all variables, regardless of their timing. The results from these matching approaches cannot address the post-treatment bias concerns, but they go some way in addressing the concern of model dependence. The estimates after matching as we can see in table A.2 below are qualitatively similar to the main results. The matching results therefore lend additional credibility to the main argument of this paper, keeping the appropriate caveats in mind.

Table A.1: Results Dynastic Vote Bonus

	Belgium	Belgium	Belgium	Flanders	Flanders	Flanders
Percentage Party Preference Votes						
Dynastic	0.060*** (0.019)	0.027* (0.015)	0.025 (0.015)	0.043** (0.019)	0.017 (0.013)	0.016 (0.014)
Rank		-0.004*** (0.000)	-0.004*** (0.000)		-0.002*** (0.000)	-0.002*** (0.000)
Female		0.000 (0.003)	0.000 (0.003)		0.007** (0.003)	0.007** (0.003)
Senate		-0.016*** (0.002)	-0.016*** (0.002)		-0.015*** (0.002)	-0.016*** (0.002)
Experience		0.006*** (0.001)	0.006*** (0.001)		0.002 (0.001)	0.002 (0.001)
Experience squared		-0.000* (0.000)	-0.000* (0.000)		0.000 (0.000)	0.000 (0.000)
Government experience		0.095*** (0.014)	0.095*** (0.014)		0.007 (0.020)	0.008 (0.020)
Samename			0.003 (0.004)			0.002 (0.003)
Number of articles					0.006*** (0.001)	0.006*** (0.001)
At least one article					0.025*** (0.006)	0.025*** (0.006)
Observations	1,271	1,271	1,271	881	881	881

Column 1 shows the regression results for the log of preference votes for the whole sample without controls, column 2 includes control variables. Column 4 and 5 show results for restricted sample of Flemish constituencies, with column 5 including a control for media attention. Columns 3 and 6 include a dummy for sharing the same name with a previous politician. All models include party-district dummies. Errors are clustered at the party-district level. *** p<0.01, ** p<0.05, * p<0.1

Table A.2: Results for the (Propensity-Score) Matched Sample

Approach 3:	log(PV)	PercPartyPV	Elected
Dynastic	0.546*	0.051*	2.115
	0.279	0.029	1.428
Rank	-0.018	-0.004***	-0.549***
	0.038	0.001	0.142
Female	-0.531	-0.012	0.995
	0.787	0.024	1.651
Senate	0.406	-0.042	0.934
	1.079	0.061	5.202
Experience	0.055	0.016	-0.060
	0.065	0.010	0.416
Experience squared	-0.004	-0.001	0.008
	0.003	0.001	0.023
Government experience	1.294*	0.125*	5.039
	0.657	0.071	5.873
	n=80	n=80	n=48

Column 3 show the regression results for the log of the odds of being elected. All models include party-district dummies. Errors are clustered at the party-district level. *** p<0.01, ** p<0.05, * p<0.1

Appendix B

Appendix to Chapter 4

Table B.1: Characteristics of Elected MPs in England, Scotland, and Wales (1865-1874)

Year	1865			1868			1874		
	N	Mean	SD	N	Mean	SD	N	Mean	SD
Number of MPs	549			546			542		
Constituencies	333			349			347		
Aristocracy	549	0.348	0.477	546	0.366	0.482	542	0.345	0.476
Elite	523	0.564	0.496	529	0.512	0.500	528	0.483	0.500
Rentier	523	0.350	0.477	529	0.329	0.470	528	0.275	0.447
Age at death	545	72.516	11.724	545	72.158	11.752	541	72.468	11.843
Cabinet	549	0.031	0.173	546	0.015	0.120	542	0.017	0.128
Junior	549	0.361	0.481	546	0.375	0.485	542	0.376	0.485
Senior	549	0.444	0.497	546	0.421	0.494	542	0.393	0.489

Table B.2: Characteristics of Elected MPs in England, Scotland, and Wales (1880-1885)

Year	1880			1885		
	N	Mean	SD	N	Mean	SD
Number of MPs	540			559		
Constituencies	347			537		
Aristocracy	542	0.338	0.473	560	0.246	0.431
Elite	525	0.425	0.495	543	0.276	0.448
Rentier	525	0.248	0.432	543	0.142	0.349
Age at death	537	72.814	12.207	559	73.755	11.201
Cabinet	542	0.037	0.189	560	0.029	0.167
Junior	542	0.395	0.489	560	0.284	0.451
Senior	542	0.365	0.482	560	0.298	0.458

Table B.3: Is the New Franchise correlated with Characteristics of Candidates elected in the previous Election? Second Reform Act.

	Aristocrat (1)	Elite (2)	Rentier (3)	Age at Death (4)	Cabinet (5)	Junior (6)	Senior (7)
<u>Panel A: Boroughs without boundary or seat changes</u>							
ln(electors)	0.059 [0.101]	0.137 [0.105]	0.113 [0.096]	-2.866 [3.691]	-0.006 [0.022]	-0.123 [0.084]	-0.064 [0.163]
Observations	342	324	324	338	342	342	342
R-squared	0.488	0.578	0.555	0.519	0.615	0.552	0.507
<u>Panel B: Counties without boundary or seat changes</u>							
ln(electors)	-0.167 [0.237]	-0.002 [0.308]	-0.404 [0.388]	1.873 [6.148]	0.032 [0.049]	-0.322 [0.257]	-0.017 [0.279]
Observations	190	180	180	189	190	190	190
R-squared	0.514	0.539	0.456	0.482	0.35	0.629	0.631
Control Population Change?	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Source: Craig (1989) and Vincent and Stenton (1971).

Notes: OLS regressions. All regressions include a constant term. Sample includes one observation per elected MP by constituency and year. We estimate the effect of ln(electors) in 1865 and 1868 on outcomes in 1859 and 1865 respectively. Standard errors clustered by constituency are in parentheses.

Table B.4: Is the New Franchise correlated with Characteristics of Candidates elected in the previous Election? Third Reform Act.

	Aristocrat (1)	Elite (2)	Rentier (3)	Age at Death (4)	Cabinet (5)	Junior (6)	Senior (7)
<u>Panel A: Counties that were not transformed and without seat changes</u>							
ln(electors)	0.241 [0.227]	0.254 [0.257]	0.28 [0.185]	-0.032 [2.847]	0.045 [0.072]	0.146 [0.253]	0.063 [0.237]
Observations	73	71	71	72	73	73	73
R-squared	0.75	0.767	0.862	0.829	0.514	0.699	0.774
<u>Panel B: Boroughs that were not transformed and without seat changes</u>							
ln(electors)	-0.049 [0.051]	0.031 [0.066]	0.044 [0.045]	3.356** [1.602]	0.019 [0.029]	-0.038 [0.056]	-0.015 [0.055]
Observations	276	265	265	276	276	276	276
R-squared	0.742	0.671	0.719	0.611	0.514	0.709	0.707
Control Population Change?	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Source: Craig (1989) and Vincent and Stenton (1971).

Notes: OLS regressions. All regressions include a constant term. Sample includes one observation per elected MP by constituency and year. We estimate the effect of ln(electors) in 1880 and 1885 on outcomes in 1874 and 1880 respectively. Standard errors clustered by constituency are in parentheses.

Table B.5: Characteristics of Elected Candidates Under the New Franchise (1868): Instrumental Variable Estimates

	Aristocrat (1)	Elite (2)	Rentier (3)	Age at Death (4)	Cabinet (5)	Junior (6)	Senior (7)
<u>Panel A: Boroughs without boundary or seat changes</u>							
ln(electors)	0.079 [0.173]	-0.046 [0.144]	0.105 [0.130]	-3.369 [4.073]	-0.051 [0.060]	-0.064 [0.165]	-0.137 [0.181]
Observations	342	330	330	340	342	342	342
Overidentification test (p-value)	0.357	0.284	0.614	0.045	0.964	0.130	0.288
<u>Panel B: Counties without boundary or seat changes</u>							
ln(electors)	0.537 [0.430]	0.604 [0.517]	0.478 [0.436]	-14.785* [7.888]	0.079 [0.079]	-0.322 [0.568]	0.251 [0.566]
Observations	190	183	183	190	190	190	190
Overidentification test (p-value)	0.721	0.829	0.251	0.813	0.845	0.240	0.330
Control Population Change?	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Source: Craig (1989) and Vincent and Stenton (1971).

Notes: Instrumental Variables regressions. All regressions include a constant term. Sample includes one observation per elected MP by constituency and year. Standard errors clustered by constituency are in parentheses. Instruments are: Log(electorate, 1832) and Log(population, 1861) interacted with a dummy for 1868.

Table B.6: Characteristics of Elected Candidates Under the New Franchise (1874): Instrumental Variable Estimates

	Aristocrat (1)	Elite (2)	Rentier (3)	Age at Death (4)	Cabinet (5)	Junior (6)	Senior (7)
<u>Panel A: Boroughs without boundary changes or seat changes</u>							
ln(electors)	0.409** [0.199]	0.033 [0.198]	0.015 [0.140]	-3.173 [6.013]	-0.049 [0.082]	0.049 [0.210]	-0.040 [0.205]
Observations	342	328	328	340	342	342	342
Overidentification test (p-value)	0.465	0.992	0.345	0.324	0.883	0.149	0.228
<u>Panel B: Counties without boundary changes or seat changes</u>							
ln(electors)	0.505 [0.526]	0.236 [0.499]	0.185 [0.266]	8.47 [9.657]	-0.050 [0.050]	0.408 [0.473]	0.409 [0.449]
Observations	190	185	185	190	190	190	190
Overidentification test (p-value)	0.465	0.719	0.389	0.827	0.659	0.247	0.083
Control Population Change?	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Source: Craig (1989) and Vincent and Stenton (1971).

Notes: Instrumental Variables regressions. All regressions include a constant term. Sample includes one observation per elected MP by constituency and year.

Standard errors clustered by constituency are in parentheses. Instruments are: Log(electorate, 1832) and Log(population, 1861) interacted with a dummy for 1874.

Table B.7: Characteristics of Elected Candidates Under the New Franchise (1885): Instrumental Variable Estimates

	Aristocrat (1)	Elite (2)	Rentier (3)	Age at Death (4)	Cabinet (5)	Junior (6)	Senior (7)
<u>Panel A: Counties that were not transformed and without seat changes</u>							
ln(electors)	-0.641* [0.363]	-0.173 [0.229]	-0.025 [0.038]	-10.688 [6.792]		-0.433 [0.280]	0.162 [0.249]
Observations	62	59	59	61	62	62	62
Overidentification test (p-value)	0.251	0.671	0.322	0.979		0.783	0.757
<u>Panel B: Boroughs that were not transformed and without seat changes</u>							
ln(electors)	0.082 [0.102]	-0.236* [0.122]	-0.190** [0.092]	-1.015 [2.445]	-0.082** [0.038]	-0.065 [0.086]	-0.039 [0.100]
Observations	254	243	243	254	254	254	254
Overidentification test (p-value)	0.8486	0.6259	0.2443	0.5385	0.4686	0.1525	0.4886
Control Population Change?	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Source: Craig (1989) and Vincent and Stenton (1971).

Notes: Instrumental Variables regressions. All regressions include a constant term. Sample includes one observation per elected MP by constituency and year.

Standard errors clustered by constituency are in parentheses. Instruments are: Log(electorate, 1832) and Log(population, 1881) interacted with a dummy for 1885.

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