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## *Political Dynasties in the UK House of Commons: The Null Effect of Narrow Electoral Selection*

Does power persist within families? This article considers whether members of the UK House of Commons with longer legislative careers after 1832 were more likely to establish a political dynasty. Tenure can create opportunities to promote relatives. A regression discontinuity design for re-election races helps to rule out the confounding influence of inheritable traits. There is no evidence for a causal effect of tenure length on establishing or continuing a dynasty. Established families may have constrained further dynasty development, explaining the null result of tenure.

In the 19th century, UK parliamentary politics was 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). During that century power would gradually shift away from these aristocratic elites along with widening democratic participation. Political dynasties seemingly became part of the past: The number of MPs from the same families declined over time. However, they never disappeared entirely: Certain political dynasties survived, and some new entrants would see relatives entering in their footsteps. Does power then still persist within families in democracies?

Dynastic succession occurs in all professions. However, for the profession of politics in democracies succession is not organized along dynastic lines but depends on running successful electoral campaigns. Understanding whether political power can still be bequeathed through elections is then particularly important. The tension between dynastic persistence and democratic representation has long been a central concern for political science (Michels [1911] 1968; Mosca [1896] 1939). Yet identifying what constitutes such power bequests in democracies is

difficult. Elections (or peerages in the United Kingdom) are not won at random. The same talents or drive that voters appreciate in the founders of the dynasty may explain the election of their successors. In spite of these challenges to identifying an effect of holding power on transmitting power to relatives, recent research provides evidence for such a causal connection by exploiting exogenous sources of power (Dal Bó, Dal Bó, and Snyder 2009; Querubin 2015; Rossi 2015). One such identified cause of dynasties is the tenure length of a politician. Holding power for longer may create advantages for relatives such as name recognition or an extensive political network that are independent from fixed characteristics shared within families, such as peerages and landholdings of prominent families in the UK context.

This article considers what evidence there is in the United Kingdom for a tenure effect on dynasty formation as has been found for the United States (Dal Bó, Dal Bó, and Snyder 2009). Surprisingly, in light of many historical anecdotes of its powerful families, I find no evidence in favor of power bequest through elections in 19th-century United Kingdom, nor afterwards. Employing a regression discontinuity design to close re-elections, I find that MPs who served longer were no more or less likely to establish or continue a political dynasty. A breakup of the analysis over different time periods provides qualitatively similar results: throughout the 19th century, or even beyond until as recently as 2001, there is little evidence for power bequest through elections.

This null result of narrow electoral selection means that tenure does not cause dynastic power to persist in the United Kingdom, at least not for marginally re-elected individuals who served longer due to good fortune at the polls. Having several senior relatives as well as the total number of years of political experience in the family, while not distributed at random among MPs, are much stronger predictors of dynasty formation than tenure, and the probability of continuing the dynasty for such junior relatives is not significantly increased or decreased by narrowly winning a first re-election. Perhaps power bequest did not occur through parliamentary service. Throughout the 19th century, power became increasingly concentrated in the cabinet, shifting away from individual MPs and parliament (Cox 1987). Therefore, the success of the relatives of cabinet ministers is one obvious way to study the importance of having relevant political connections. 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 the cabinet. While selection to the cabinet is not random, cabinet service forms a good predictor of starting a dynasty, and there is no heterogeneous effect of tenure length for these future cabinet ministers. While

there was no purely electoral advantage to bequeath, new dynasties still emerged while old ones managed to survive. One way in which they may have done so was by selecting their scions into the cabinet.

Political dynasties appear to be present in all democracies. Yet in contrast to results in the existing literature (Dal Bó, Dal Bó, and Snyder 2009; Querubin 2015; Rossi 2015), I do not find an effect of tenure. This null result suggests that electoral success of newcomers does not always lead to advantages for their relatives. To understand the causes of political dynasties, we therefore must pay attention to existing institutions and the role of established elites.

### **Political Dynasties in Democracies**

Early work in political science noted the tendency for dynasties to form even in democratic systems (Michels [1911] 1968; Mosca [1896] 1939). How and why democratic systems provide opportunities for dynastic domination by political elites is still relevant to researchers because of adverse selection concerns (e.g., Besley 2005). The identity of policy makers tends to influence the types of policies they implement (Besley, Montalvo, and Reynal-Querol 2013; Pande 2003), and legislative careers may be particularly rewarding (Eggers and Hainmueller 2009, 2013, 2014; Querubin and Snyder 2009, 2013).<sup>1</sup> Voters may reward politicians from established families for perceived shared talent or drive, and this may make them more successful than unconnected newcomers. Evidence from India, the United States, the Philippines, Japan, and Belgium 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 (Bohlken and Chandra 2015; Cruz, Labonne, and Querubin 2015; Feinstein 2010; Querubin 2015; Smith 2012; Van Coppenolle 2014). Children of narrowly elected Swedish mayors were also found to have higher yearly earnings on average (Folke, Persson, and Rickne 2015). The presence of political dynasties in democracies can also indicate elite entrenchment. Elites may search for ways to survive in captured democracies (e.g., Acemoglu and Robinson 2008). In spite of the challenges to identify such power perpetuation among elites, recent work employing exogenous shocks to tenure length has found evidence of dynastic power perpetuation in the contexts of the United States, Argentina, and the Philippines (Dal Bó, Dal Bó, and Snyder 2009; Rossi 2015; Querubin 2015). Moreover, the extent to which dynastic elites can thrive in democracies, or political concentration, has been shown to be related to the initial distribution of wealth

among elites (Rossi 2011) and has been found to affect current economic outcomes (Acemoglu et al. 2008; Ferraz and Finan 2009).

By employing a similar causal identification strategy to identify whether a United States-like power effect (Dal Bó, Dal Bó, and Snyder 2009) exists in the United Kingdom, the results of this article are closely comparable to those of other settings. The unexpected null result indicates that context matters for dynastic persistence and is therefore relevant for understanding political dynasties beyond the United Kingdom. Political dynasties have been shown to thrive in different institutional contexts and, across time, their presence may appear to be unrelated to the existing institutional setup of a country. However, the results of this article suggest that the mechanisms underlying dynastic persistence could vary. In recent work, Fiva and Smith (2015) also found no evidence of power perpetuation in Norway, and they relate this to the party-centered system. Dynastic persistence could be more common in electoral systems that encourage a personal vote (Smith 2012). Other work has found that particular institutional changes can have no, or even counterproductive, effects on the persistence of political dynasties. For example, there is no evidence for an effect of franchise extension on the electoral success of dynastic candidates in the United Kingdom (Berlinski, Dewan, and Van Coppenolle 2014), while the introduction of term limits in the Philippines further encouraged dynastic persistence (Querubin 2011). A move away from elections that insulated appointments of aldermen from the democratic process has been found to lead to an increase in the number of local dynastic politicians in Italy (Geys 2015). Beyond the causes of dynasties, there is also some recent work on the differences in the behavior of these politicians: Dynastic politicians do not necessarily increase the quality of public services (Bragança, Ferraz, and Rios 2015), though they could channel more funds to their districts (Asako et al. 2015). There is little evidence so far for differences among dynastic legislators: for example, female dynastic mayors in the Philippines do not behave differently from male dynastic mayors once in office (Labonne, Parsa, and Querubin 2015). While dynasties are common in different democratic countries, the results of my article suggest that what causes the extent of this phenomenon may be dependent on the underlying institutional context and the role of already established elites.

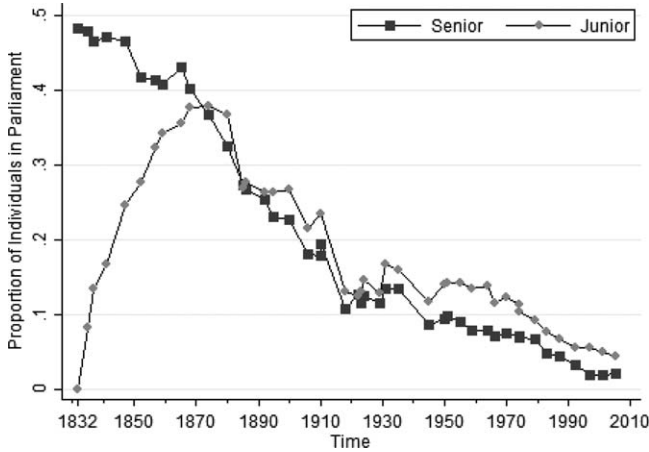
### **Data**

The biographical data for this study report individual characteristics for all members of the UK House of Commons elected in general

elections 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 and their party label. There is also a record of individual characteristics for each MP, including their date of birth, date of death, education, professional occupation before entering parliament, whether they had aristocratic connections, and information about their dynastic links to other MPs. The information originated from biographies, in particular Stenton and Lees's (1976, 1978, 1979, 1981) *Who's Who of British Members of Parliament*.<sup>2</sup> All available information about dynastic links between MPs was used, ensuring that if an MP was reported to have a dynastic link to another MP, that person would also be inversely linked to the first MP.<sup>3</sup> The data set does not report the links between MPs and peers explicitly, but we do know whether an MP was connected to the aristocracy.<sup>4</sup> A randomly drawn subsample of 823 individuals from across all time periods<sup>5</sup> was checked against publicly available information about family links.<sup>6</sup> The checked sample confirmed 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). To ascertain that the full universe of MPs after 1832 was covered, the original data were matched to a list of elected MPs from Craig (1971, 1983, 1984, 1989a, 1989b). I checked whether the correct number of individuals was returned for each constituency after 1918.<sup>7</sup> For each missing individual, additional information about parliamentary service, aristocratic connections, and dynastic links was collected.<sup>8</sup> Next, the cabinet ministers in the sample were identified by matching the names to a list of cabinet ministers from Cook and Keith (1975), Butler and Butler (2000), and Berlinski, Dewan, and Dowding (2012). The final data cover all MPs elected in general elections.<sup>9</sup> Finally, I matched 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 after winning his first general election and serving in parliament. The main close re-election sample used in this article is limited to elections up to and including the December 1910 general election to allow for sufficient time for the MPs' offspring to appear in parliament. The choice of this date as end point in the analysis also makes the results closely comparable to the United States (Dal Bó, Dal Bó, and Snyder 2009), an interesting point of reference for that time period. An extension of this sample for first re-elections up to 2001 is made to test whether the effect remains robustly null throughout the 20th century.

Political dynasties have primarily been a 19th-century phenomenon in the United Kingdom (Figure 1). At the start of the period under

FIGURE 1  
Evolution of Political Dynasties



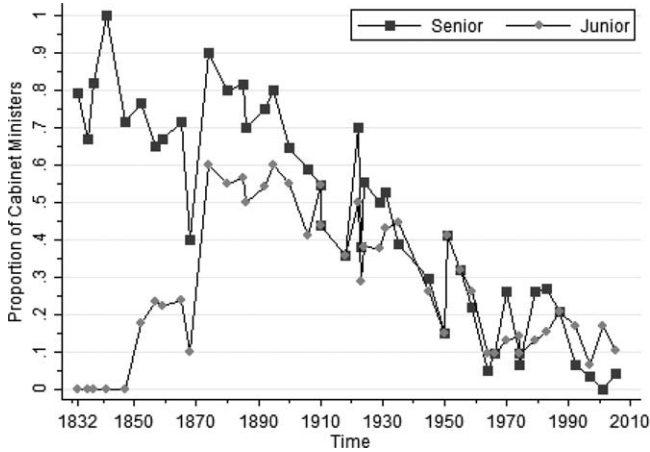
*Note:* Proportions of juniors (seniors) cannot be perfectly observed at the start and end of the data.

study, the proportion of MPs establishing or continuing a dynasty was almost 50%. However, even in the 19th 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. A similar figure for the proportion of cabinet ministers with dynastic links confirms the picture that the 19th century was more the age of dynasties than the 20th (see Figure 2). However, compared to Figure 1, 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. Note that this figure does not include cabinet ministers drawn from the House of Lords (who may have entered the cabinet after inheriting their peerages). This figure seems to suggest that there were additional advantages for power bequest beyond elections and length of service in the cabinet. However, election to the Commons and selection to a cabinet position is not random.<sup>10</sup> In the next section, I discuss the identification strategy.

### Empirical Strategy

As name recognition advantages and more established political networks increase with longer tenure, we can expect an MP's probability

FIGURE 2  
Evolution of Political Dynasties in Cabinet



*Note:* Proportions of juniors (seniors) cannot be perfectly observed at the start and end of the data.

of establishing a dynasty, that is, of becoming a senior, to increase as the MP serves longer. However, members with longer tenures may be different from members with fewer terms in various 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 that 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.<sup>11</sup> To consider whether serving longer increases the probability of establishing a dynasty, I employ a regression discontinuity design to these close first re-elections ( $f$ ).<sup>12</sup> I estimate the following reduced-form model for observations near the zero vote margin:

$$P(\text{senior})_i = \beta_1 + \beta_2 \text{win}_{i,c,p}^f + \beta_3 X_i + \beta_4 X_r + \beta_5 X_p + \varepsilon_i \quad (1)$$

with  $\text{win}$  a dummy for winners,  $i$  an indicator for the MP,  $c$  an indicator for the constituency,  $p$  an indicator for the parliamentary term,  $X_i$  a vector of individual characteristics for the MP, and fixed effects for regions ( $X_r$ ) and parliamentary terms ( $X_p$ ), respectively. Errors are clustered at broad constituency level.<sup>13</sup>

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 19th 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 *vote margin* is defined as follows:

$$\text{votemargin}_{i,c,p} = \frac{\text{votes}_{i,c,p} - \text{votes}_{j,c,p}}{\text{totalvotes}_{c,p}} \quad (2)$$

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 were 2,486 contested and nonpetitioned first re-election attempts in general elections between 1837 and 1910, and about 69% of these MPs also won their first re-election attempt. This sample disregards first re-elections in 1835, the second parliament of the data set, 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 data set).

Table 1 shows how winners across all first re-elections are more likely to become seniors. Winning a first re-election attempt is associated with a 2.5% increase (4.3% controlling for individual covariates) in the probability of starting (or continuing) a political dynasty. Serving at least two terms is associated with an increased probability of establishing or continuing a dynasty of about 7.8% (or 9.6% after adding individual controls of Table 1). Longer tenure is clearly associated with dynasty formation. However, these models could overestimate the true effect of tenure if omitted variables explain why certain individuals are more likely to win and serve longer as well as establish a dynasty. The RD approach estimates a tenure effect only among comparable individuals within small vote margins around the zero threshold who only differ in whether they won or lost the election. When considering all first re-election attempts, there are important differences in terms of the characteristics of the individuals returned in these races (see Table A2 in the online supporting information). For example, junior dynastic MPs are

TABLE 1  
OLS Results for All First Re-Elections 1837–1910

Senior						
Win	0.025*	0.026	0.043***			
	[0.015]	[0.017]	[0.015]			
Longterm				0.078***	0.082***	0.096***
				[0.016]	[0.018]	[0.017]
Controls	No	Yes	Yes	No	Yes	Yes
Juniors incl	No	No	Yes	No	No	Yes
Observations	2,414	2,063	2,873	2,414	2,063	2,873

*Note:* Results of regressing senior on winning and longterm. Sample includes winners and losers of all (unpetitioned) first re-elections. Controls as in Table A2. All models include parliament and region fixed effects. Errors clustered at (broad) constituency level. \* $p < 0.1$ , \*\* $p < 0.05$ , \*\*\* $p < 0.01$ .

more likely to have run in an unopposed first re-election race. Therefore, dynasties tend to run for their first re-election in less competitive environments, and in general MPs are not evenly distributed across race type. Among those who run in contested elections, there is no evidence that junior dynastic candidates performed better (the estimate for junior regressed on vote margin is not significant in Table A2).

For the RD approach to be reliable for estimating causal effects, there should be no significant differences between winners and losers within small margins of winning or losing the marginal seat in constituencies, other than winning re-election. In support of the approach, we find almost no differences between winners and losers that are systematically significant across these two vote margins (see Table A3 in the online supporting information). To address any remaining imbalances, the proceeding analyses will include these characteristics as controls. Moreover, Figure A2 shows that family characteristics are not significantly associated with the type of race at both sides of the discontinuity and along the forcing variable. Another assumption is that there is no systematic sorting of individuals around the zero vote margin, which Figure A3 in the online supporting information supports.

I also present the results from an RD-IV estimation strategy, which employs a two-stage instrumental variable approach using winning the first re-election as an instrument for serving longer.<sup>14</sup> The dummy variable *longterm* identifies those MPs who have served two parliamentary terms or longer. By definition, those MPs who won a first re-election attempt (win equals “1”) served longer. By using winning a first

TABLE 2  
Reduced-Form RD Results 1837–1910

Senior	5%			2.5%		
Win	−0.001 [0.031]	−0.001 [0.032]	0.004 [0.030]	−0.013 [0.044]	−0.014 [0.045]	−0.003 [0.045]
Controls	No	Yes	Yes	No	Yes	Yes
Juniors incl	No	No	Yes	No	No	Yes
Observations	563	504	682	306	274	376

*Note:* Reduced-form regression results of senior regressed on winning. Includes winners and losers of marginal seats only. Controls as in Table A3. All models include parliament and region fixed effects. Errors clustered at (broad) constituency level. \* $p < 0.1$ , \*\* $p < 0.05$ , \*\*\* $p < 0.01$ .

re-election attempt as an instrument for tenure length, we can take account of the fact that some of those who lost their first re-election attempt still served more than two terms, after winning a subsequent election.

Finally, as an alternative to comparing means at both sides of the discontinuity within these narrow bandwidths, local linear regressions can be estimated using different bandwidths, including an optimal bandwidth (Imbens and Kalyanaraman 2012).

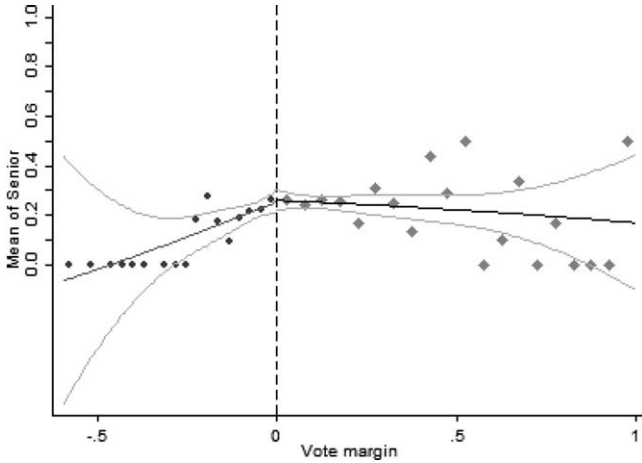
## Results

To help interpretation, the following results are shown separately after excluding or including those who were already part of existing dynasties. The tables present the results at a 5% and a 2.5% bandwidth, and the sample covers the years 1837 to 1910. In this way, the results are closely comparable to the positive findings for the United States.

### *Main Results*

Table 2 presents the differences between winners and losers of first close re-election attempts within a 5% and a 2.5% vote margin. The estimates in all models, with or without controls, are both small and not statistically significant. These null results are estimated to be close to zero, and the 95% confidence intervals around these estimates allow for an effect of up to 6% in either direction (see model 1 in Table 2). Figure 3 further confirms that there is no clear discontinuity around the zero vote margin. Table 3 presents similar results, but this time winning a first re-election is employed as an instrument for the length of the

FIGURE 3  
Senior Along Vote Margin



Note: Quadratic fit and 95% confidence interval of senior along vote margin. Sample covers 1837–1910.

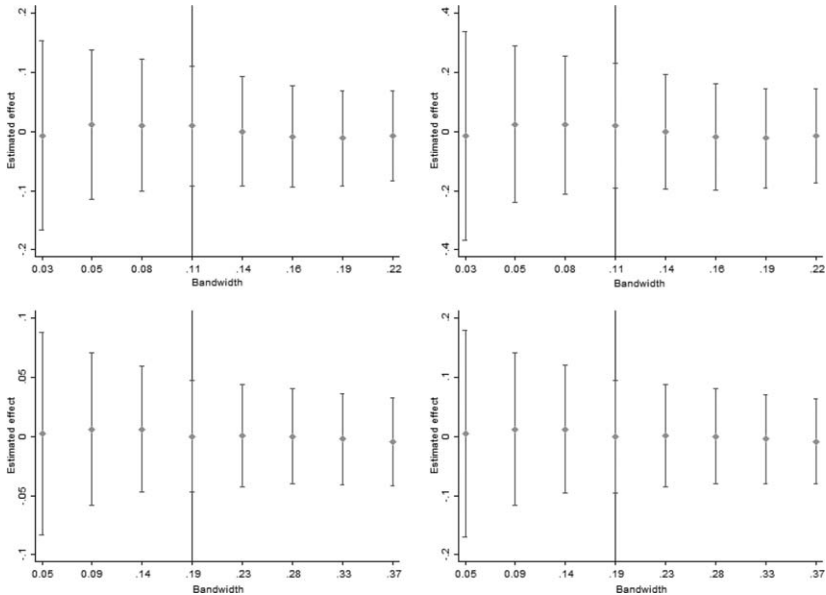
TABLE 3  
RD-IV Results 1837–1910

Senior	5%			2.5%		
Longterm	-0.002	-0.002	0.007	-0.022	-0.027	-0.006
	[0.052]	[0.053]	[0.054]	[0.074]	[0.076]	[0.083]
First stage:						
Longterm	5%			2.5%		
Win	0.580***	0.562***	0.536***	0.569***	0.530***	0.505***
	[0.030]	[0.030]	[0.026]	[0.040]	[0.046]	[0.037]
Controls	No	Yes	Yes	No	Yes	Yes
Juniors incl	No	No	Yes	No	No	Yes
Observations	563	504	682	306	274	376

Note: Results from the 2sls RDIV estimation of senior regressed on longterm instrumented by winning a first re-election attempt. Sample includes winners and losers of marginal seats only. Controls as in Table A3. All models include parliament and region fixed effects. Errors clustered at (broad) constituency level. \* $p < 0.1$ , \*\* $p < 0.05$ , \*\*\* $p < 0.01$ .

career.<sup>15</sup> The first-stage results of a two-stage least-squares estimation in the bottom half of Table 3 confirm that the relevance condition holds: Winning a marginal first re-election attempt significantly increases

FIGURE 4  
Estimates of Local Linear Regression at Different Bandwidths



*Note:* Bandwidth with corresponding estimate and 95% confidence interval for local linear regressions. Graphs depict model estimations of RD for the main sample (upper left) and RD-IV for the main sample (upper right), RD for the full sample (lower left) and RD-IV for the full sample (lower right). The samples include MPs who were juniors themselves, the main sample covers the period 1837–1910, the full sample 1837–2001. The vertical line indicates the optimal bandwidth (Imbens and Kalyanaraman 2012; Nichols 2011).

career length. However, Table 3 still shows second-stage null results similar to those of the reduced-form approach. The effect of a long career on the probability of establishing a political dynasty is small across all models and cannot be distinguished from zero.

These results are also stable across bandwidths. The choice of the bandwidth involves a trade-off between bias and variance of the estimates, so Figure 4 presents the results of several local linear regressions at different bandwidths. These estimations employ a triangular kernel function and are shown for different multiples of the optimal bandwidth of about 11%. For both the strategies of Tables 2 and 3, the estimates of such alternative local linear regressions remain close to zero at different bandwidths (the upper graphs of Figure 4).

*Differences Over Time*

Perhaps name recognition in elections became more important as democratic participation widened. If this is the case, we may expect the effect to show up only in certain time periods. Therefore, I define a first period that starts with the election of the third parliament after the First Reform Act of 1832, which coincides with the start of the data set. This first period ends right before the adoption of the Second Reform Act by parliament in 1867, which led to the second franchise extension, and the second period starts with the first election run under the new franchise. A third period begins with the first election in 1885 after the third franchise extension. I define a fourth period for the final three parliaments before the onset of World War I. Table 4 presents the reduced-form results for the period of 1837 to 1865 (upper-left panel), 1868 to 1880 (middle-left panel), 1885 to 1900 (upper-right panel), and 1906 to 1910 (middle-right panel).

By limiting the sample to specific time periods, the number of observations and the precision of the estimates decreases. Conclusions from these results should therefore be drawn carefully, especially for differences between time periods. Still, the results from Table 4 do not confirm that tenure became increasingly important over time. Before the Third Reform Act (results in the upper-left part of Table 4) when the franchise was most restricted, winning is estimated to have had both positive and negative effects, and these are generally not statistically significant. After the Third Reform Act extended the franchise further, the estimates become negative (upper half of right side of Table 4) and sometimes even significant for a period of time, but they change sign and become statistically indistinguishable from zero later on (middle-right panel). Finally, Table 4 presents results for an extended sample, including the years 1918 to 2001. A natural breakpoint was chosen after the end of World War II. The estimates are slightly larger for this first period of the 20th century, but only one is distinguishable from zero. The positive effect of the 5% bandwidth is driven by MPs who were already part of dynasties, and while the estimates increase somewhat in the smaller bandwidth, they are not significantly different from zero. Local linear regressions using a triangular kernel are presented for the full sample of 1837 to 2001 in the lower part of Figure 4. These plots further confirm that the null result holds on average in the full sample.

*Interpretation of the Causally Identified Null Results*

The results from Tables 2 to 4 do not allow us to conclude definitively that longer tenure did not matter at all for dynasty formation. The

TABLE 4  
Reduced-Form Results Across Time

Senior	5%	1837–1865	2.5%	Senior	5%	2.5%				
Time:		1837–1865		Time:	1885–1900					
Win	-0.037 [0.065]	0.002 [0.083]	-0.011 [0.070]	0.136 [0.120]	-0.001 [0.110]	Win -0.047 [0.048]	-0.122** [0.056]	-0.159** [0.077]	-0.114 [0.087]	
Observations	165	132	174	79	90	Observations 176	221	98	89 122	
Time:		1868–1880		Time:	1906–1910					
Win	0.075 [0.071]	0.058 [0.099]	0.020 [0.080]	0.083 [0.086]	0.054 [0.118]	0.009 [0.097]	Win 0.057 [0.060]	0.062 [0.085]	0.085 [0.087]	0.106 [0.132]
Observations	97	93	136	64	61	85	Observations 125	117	65	60 79
Time:		1918–1935		Time:	1945–2001					
Win	0.024 [0.031]	0.047 [0.041]	0.083** [0.039]	0.066 [0.055]	0.117 [0.078]	0.118 [0.075]	Win 0.0004 [0.016]	0.032 [0.022]	0.030 [0.023]	0.036 [0.022]
Observations	217	189	210	108	98	107	Observations 370	349	381	200 189 209
Controls	No	Yes	Yes	No	Yes	Yes	Controls No	Yes	Yes	No Yes
Juniors incl	No	No	Yes	No	No	Yes	Juniors incl No	No	Yes	No Yes

Note: Reduced-form regression results of the dependent variable senior on winning. Sample includes winners and losers of marginal seats only. All models include parliament and region fixed effects. Errors clustered at (broad) constituency level. \* $p < 0.1$ , \*\* $p < 0.05$ , \*\*\* $p < 0.01$ .

estimated effect is a local average treatment effect for marginal elections of the marginal seats only. 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 baseline estimation of all these elections (see Table 1) suffers from omitted variable bias. These causally identified null results are very different from findings for the United States and for Argentina, which suggest an increase of about 8% on the probability of establishing a dynasty when serving at least two terms or five additional years, respectively (Dal Bó, Dal Bó, and Snyder 2009; Rossi 2015).<sup>16</sup> Similar random variation in tenure length seems to produce different results in different contexts. There is no a priori reason to believe that new entrants would not have had the same interest in building dynasties as established elites or elites in different countries. One explanation could be that there was less additional name recognition to be gained from an additional parliamentary term in the United Kingdom. If the average length of parliaments in the United Kingdom was greater, MPs serving a first term would have already been better known by their constituents for their actions in parliament. However, the average length of parliaments before 1918 in the United Kingdom was only 4.4 years compared to an average of 3 years for each term in the United States (i.e., an average of 2 years for congressmen but 6 years for senators; see Dal Bó, Dal Bó, and Snyder 2009, 118).

There is another possible explanation for these contrasting results, for which I will now present some tentative evidence. Whether new legislators can use electoral success to build political networks (e.g., political machines and electoral campaign strategies, a central position within the party, etc.) depends on the extent to which existing elites are embedded in the democratic system. The tenure effect could be dependent on their informal influence. The fact that these data might 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, can be thought to support this interpretation. However, such influence is not necessarily proxied well by their numbers only, as these were tending downwards over time. Dynasties may, for example, have been able to influence the electoral context, as suggested by the finding that dynastic candidates were more likely to run unopposed. In further support of this interpretation, the next section describes the types of dynasties in more detail and evaluates the evidence for a tenure effect conditional on family type. Finally, I consider cabinet selection as an alternative measure of networks and consider the evidence for a conditional tenure effect on cabinet service.

### New and Established Political Dynasties

In this section, I describe how juniors from dynasties differed from other MPs. Junior dynastic status cannot cause other personal characteristics determined at birth (such as gender), nor can we causally identify an effect on certain career factors that 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. To differentiate between different types of families, two additional variables are considered. *More relatives* is a dummy for MPs who had at least two previous relatives in parliament. A continuous variable *family experience* measures the previous experience in an MP's family at first entry, in parliamentary terms.

In Table 5, we can investigate some personal characteristics. The sample used in these models 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 19th century. In fact, even when still including in the analysis these years in which women were not yet allowed to run, we are able to identify a positive, significant association between junior dynastic status and a dummy variable female. Among juniors, there were about 2% more women than among other MPs, and this association is statistically significant. This result is substantive, given that only 3% of MPs since 1832 were 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. *More relatives* is not a significant predictor, while more parliamentary experience in the family is negatively associated with an indicator for women. 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,<sup>17</sup> particularly if they had several previous relatives and more experience in the family. This underlines how political dynasties are symptomatic of aristocratic establishment in the United Kingdom. However, juniors are also more likely to have held university degrees, and degrees 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  
Personal Characteristics and Family Types

	Female	Female	Aristocrat	Aristocrat	University degree	University degree	Oxbridge	Oxbridge
1832-2005								
Junior	-0.010** [0.004]	0.020*** [0.007]	0.414*** [0.013]	0.155*** [0.018]	0.133*** [0.014]	0.013 [0.015]	0.229*** [0.014]	0.067*** [0.015]
More relatives		-0.007 [0.008]		0.183*** [0.026]		-0.014 [0.020]		0.010 [0.019]
Family exp		-0.0001** [0.0001]		0.002*** [0.0002]		0.0005** [0.0002]		0.001*** [0.0002]
Female				-0.022* [0.013]		0.179*** [0.032]		-0.152*** [0.026]
Aristocrat		-0.006* [0.003]				-0.079*** [0.012]		0.083*** [0.012]
University degree		0.032*** [0.006]		-0.056*** [0.009]				0.524*** [0.009]
Oxbridge		-0.033*** [0.006]		0.070*** [0.010]		0.622*** [0.008]		
Age		0.0002 [0.0002]		-0.005*** [0.0004]		-0.004*** [0.0004]		
Constant	0.032*** [0.002]	0.062*** [0.011]	0.095*** [0.003]	0.280*** [0.020]	0.457*** [0.006]	0.464*** [0.024]	0.218*** [0.005]	-0.002*** [0.0004]
Occupations	No	Yes	No	Yes	No	Yes	No	Yes
Observations	9,070	7,913	9,070	7,913	9,070	7,913	9,070	7,913
R-squared	<0.001	0.073	0.175	0.346	0.010	0.467	0.039	0.441

Note: Results from regressing individual characteristics on junior dynamic status, more relatives (juniors with at least two previous relatives) and family experience (in number of parliamentary terms before first entry) and individual controls at entry. Robust standard errors. \* $p < 0.1$ , \*\* $p < 0.05$ , \*\*\* $p < 0.01$ .

TABLE 6  
Heterogenous Effects for Family Types 1837–1910

Senior	5%			2.5%		
Win	0.001 [0.031]	-0.0002 [0.030]	-0.005 [0.032]	-0.009 [0.047]	-0.010 [0.046]	-0.029 [0.047]
Win × Junior	0.047 [0.083]	0.046 [0.108]	0.052 [0.089]	0.101 [0.122]	0.021 [0.143]	0.104 [0.130]
Junior	0.361*** [0.064]	0.156** [0.078]	0.272*** [0.068]	0.354*** [0.088]	0.186* [0.100]	0.253*** [0.093]
Win × More relatives		-0.054 [0.138]			0.066 [0.189]	
More relatives		0.443*** [0.098]			0.382*** [0.137]	
Win × Family exp			-0.001 [0.001]			-0.003* [0.002]
Family exp			0.005*** [0.001]			0.007*** [0.002]
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Juniors incl	Yes	Yes	Yes	Yes	Yes	Yes
Observations	682	682	682	376	376	376

*Note:* Reduced-form estimation results of regressing senior on winning interacted with junior, more relatives (juniors with at least two previous relatives) and family experience (in number of parliamentary terms before first entry). Sample includes winners and losers of marginal seats only. Controls as in Table A3. All models include parliament and region fixed effects. Errors clustered at (broad) constituency level. \* $p < 0.1$ , \*\* $p < 0.05$ , \*\*\* $p < 0.01$ .

While these associations should be interpreted cautiously as these characteristics are strongly correlated, they are in line with the expectation that more relatives and family experience form good proxies of the more established families. Therefore, I now revisit the main results of the article to test whether tenure has a different effect for these different family types. The results are presented in Table 6. Again, the coefficients on win are not statistically significant at the 5% or 2.5% margin. There is also little evidence for a differential effect for narrow winners from political dynasties with additional previous relatives or with more extensive family experience (the interaction effects). However, the estimated coefficients of the number of seniors and family experience are large and statistically significant at the 1% level in all specifications. In other words, being part of an existing and established political dynasty made an MP much more likely to continue the dynasty, regardless of whether his or her narrow re-election resulted in a win or a loss.

TABLE 7  
Cabinet Selection and Family Types

Ever Cabinet	1837–1910			1918–2005		
Junior	−0.002 [0.007]	−0.018* [0.010]	0.031 [0.018]	0.023 [0.024]	0.065*** [0.018]	0.027 [0.024]
Junior × with Senior Cabinet Minister	0.078*** [0.020]	0.074 [0.068]	0.216*** [0.045]	0.358*** [0.108]		
Junior × with Senior Cabinet Minister after 1918					0.210*** [0.067]	0.314*** [0.124]
More relatives		−0.001 [0.015]		−0.016 [0.040]		−0.010 [0.039]
More relatives ×with Senior Cabinet Minister		−0.069 [0.074]		−0.213* [0.127]		
More relatives ×with Senior Cabinet Minister after 1918						−0.158 [0.162]
Family exp		0.001 [0.001]		−0.001 [0.001]		0.001* [0.001]
Family exp ×with Senior Cabinet Minister		0.0003 [0.001]		0.002 [0.001]		
Family exp ×with Senior Cabinet Minister after 1918						−0.001 [0.001]
Female				0.025 [0.022]		0.023 [0.022]
Aristocrat		0.037*** [0.011]		0.024 [0.021]		0.027 [0.021]
University degree		0.014 [0.010]		0.028*** [0.010]		0.028*** [0.010]
Oxbridge		0.031*** [0.012]		0.046*** [0.013]		0.046*** [0.013]
Age		−0.0003 [0.0003]		0.0002 [0.0004]		0.0001 [0.0004]
Constant	0.041*** [0.003]	0.085*** [0.026]	0.070*** [0.004]	0.034 [0.023]	0.070*** [0.004]	0.044 [0.023]
Occupations	No	Yes	No	Yes	No	Yes
Observations	4,578	4,096	4,481	3,911	4,481	3,911
R-squared	0.008	0.045	0.022	0.065	0.017	0.060

*Note:* Results from regressing ever cabinet (future cabinet status) on junior dynastic status. Robust standard errors. With Senior Cabinet Minister after 1918 indicates MPs who had at least one relative who was a cabinet minister after 1918. The first two columns consider 1837–1910. The final four columns restrict the sample to MPs who served after 1918 only. \* $p < 0.1$ , \*\* $p < 0.05$ , \*\*\* $p < 0.01$ .

## Political Networks and Cabinet Selection

This section considers whether cabinet selection could have formed an alternative mechanism to parliamentary service in explaining dynastic

TABLE 8  
Heterogenous Effects for Cabinet ministers 1837–1910

	Senior		of a Cabinet Junior	
	5%	2.5%	5%	2.5%
Win	0.007 [0.032]	-0.007 [0.047]	0.018 [0.013]	0.016 [0.019]
Win × Ever Cabinet	-0.187 [0.185]	0.173 [0.161]	-0.104 [0.154]	0.073 [0.106]
Ever Cabinet	0.281* [0.165]	-0.165 [0.110]	0.150 [0.146]	-0.094 [0.100]
Controls	Yes	Yes	Yes	Yes
Observations	682	376	682	376

*Note:* Reduced-form estimation results of regressing senior and senior of a future cabinet junior on winning interacted with future cabinet status. Sample includes winners and losers of marginal seats only. Individuals were not juniors themselves. Controls as in Table A3. All models include parliament and region fixed effects. Errors clustered at (broad) constituency level. \* $p < 0.1$ , \*\* $p < 0.05$ , \*\*\* $p < 0.01$ .

persistence. There is little evidence that junior dynastic members were more likely to enter the cabinet at some point in their careers (Table 7), except if they had a senior who had served as cabinet minister before (columns 1 and 2). This effect increases still 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. However, among those with a previous relative in the cabinet, there is no evidence that juniors from more established families were very different from other juniors (see the interaction effects with more relatives and family experience).

These associations should not be interpreted as causal effects. Similar to the baseline estimate of serving longer, the estimated effects may still suffer from omitted variable bias and overestimate the true effect. Still, compared to the baseline estimates of the effect of serving longer (in Table 1), 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 7% to as much as 36% more likely to become a cabinet minister themselves, which is much larger than the average probability for all MPs to ever become a cabinet minister (about 6%, or 8% considering MPs after 1918

only). This result suggests that political networks may have mattered for career progression.

What evidence is there that such networks were more important than narrow electoral selection? Cabinet selection itself is not random, which makes it hard to answer this question. However, we can revisit the main analysis to test whether tenure at least has a different effect for individuals who would serve as a cabinet minister at some point in their careers. Table 8 again confirms that winning re-election was inconsequential for the dynasty, while there is also no evidence for a differential tenure effect conditional on serving as a cabinet minister. Yet at least in the 5% vote margin, cabinet service does continue to form a strong predictor of starting a dynasty (column 1) regardless of the outcome of the first re-election.

### Conclusion

This article considered the origins of the success of political dynasties in the United Kingdom. Longer tenure length alone was not found to matter as much in the United Kingdom as in the United States or Argentina (Dal Bó, Dal Bó, and Snyder 2009; Rossi 2015). I propose that this is consistent with the different starting position of existing British elites. The political-power bequest that occurred in the United Kingdom did not include individuals who only narrowly won a re-election. In fact, compared to other MPs who won or lost a narrow re-election, MPs with more than one previous relative, regardless of whether they won or lost, were more than 50% more likely to continue a political dynasty than other MPs. A comparison of all MPs since 1832 further showed that juniors of political dynasties were more likely to be selected to a cabinet position if they had a relative in cabinet before them. This cabinet selection effect was not solely a 19th-century phenomenon.

Why did serving longer not increase dynastic prospects for the narrowly re-elected in the United Kingdom? While the United Kingdom and the United States were similar in terms of electoral system and average length of service in the 19th century, there were important differences. Political dynasties of multiple generations and aristocratic families in general were more prolific in the United Kingdom than in the United States around 1830. These established elites may have formed a barrier to entry to ambitious and talented narrow winners. The tenure effect identified in the literature is therefore likely to be conditional on the informal influence that newcomers can wield in a given context. As in the United States, juniors from existing political dynasties were more likely to run in less competitive environments in the United Kingdom.

Yet among closely comparable narrow re-election winners and losers, family characteristics remained much more important than tenure in explaining dynasty formation and even career advancement. Aristocrats who lost re-election could move from the Commons to inheritable seats in the House of Lords. Unlike in the United States, the franchise for men was restricted and property based until 1918, though this article showed that a widening of the franchise did not immediately improve dynastic prospects for nonelite narrow winners. Therefore, the informal influence that newcomers could hold seems to have been more independent from tenure in the United Kingdom than in the United States. The absence of this local average treatment effect of tenure could be related to differences in political development of the competing parties, the nature of electoral campaigns, local interests, and political machines, which future research should study in more detail.

The broader conclusion from the surprising null result identified in this article is that there is variation in the extent to which power is transmitted within families in democracies. Researching what explains this variation is crucial to better understand the democratic legitimacy of dynasties.

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## NOTES

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1. Eggers and Hainmueller (2009) and Querubin and Snyder (2009, 2013) estimate positive returns to service in the United Kingdom and the United States, though Eggers and Hainmueller (2013, 2014) find little advantage for US legislators holding US portfolios.

2. The original data were collected and are described in more detail by Rush (2001).

3. The biographical entries used to create the data set would not commonly report reciprocal links.

4. Aristocratic connections are defined as the son, grandson, or nephew of the holder of a hereditary peerage or baronetcy—that is, hereditary knighthood—with a concomitant extension to women MPs from 1918 (Rush 2001, 31).

5. Corresponding to about 25% of individuals of whom the surname occurs more than once, but who are not listed as related.

6. Information was publicly available if the individual was found to have a Wikipedia entry. The results are robust to recoding the checked individuals according to this information.

7. Digitized information about MPs before 1918 as well as 19th-century electoral results was drawn from Craig (1989a, 1989b) as compiled and analyzed by Eggers and Spirling (2014) and kindly made available by the authors for this project. Electoral information for 1918 to 2001 was mainly digitized from Craig (1971, 1983, 1984) and Rallings and Thrasher (1998).

8. These additional data were drawn from Stenton and Lees (1976, 1978, 1979, 1981).

9. The sample of MPs has been carefully checked. However, as the information is drawn from biographies, some errors are likely to remain in the coding of individual control variables. For that reason, all analyses present baseline results without individual controls.

10. In recent years, for example, selection to the frontbenches has been found to be more likely if policy preferences are closer to those of backbenchers (Kam et al. 2010), while the duration of ministers at least partly depends on characteristics fixed at entry (Berlinski, Dewan, and Dowding 2012).

11. See Lee (2008) and Lee, Moretti, and Butler (2004) for well-known examples of the regression discontinuity design.

12. 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 losing candidates who never served using Wikipedia, *Who's Who*, and *The Times Guide to the House of Commons*. For the large majority of losing candidates, no biographical and/or family information at all could be found.

13. Many constituencies in the 19th century elected more than one MP. These constituencies were often abolished, merged, or split throughout the 19th century and sometimes even revived in the 20th century. As many of these constituencies share a common history of elections, the most conservative approach identifies the greatest common boundaries of these related constituencies. Groups of constituencies were identified employing information about mergers and splits (*Source*: [www.leighrayment.com/commons.htm](http://www.leighrayment.com/commons.htm)). Whenever appropriate, the analyses cluster standard errors at these larger, merged constituency units.

14. Estimations from both strategies can be used to compare to existing findings in the literature. The reduced-form approach was adopted by Querubin (2015) to identify a power-treatment effect of first re-elections in the Philippines. The RD-IV approach is similar to the one employed by Dal Bó, Dal Bó, and Snyder (2009) in their analysis of political dynasties in the United States.

15. A difference from the approach of Dal Bó, Dal Bó, and Snyder (2009) 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, 259–67). However, the downside of this approach is that the instrumented effect is more difficult to interpret. Estimating the alternative specification yields qualitatively similar results.

16. In contrast to clear evidence of electoral advantages for junior family members, Querubin (2015) finds no additional advantages of winning a first re-election attempt and serving longer.

17. This effect remains if we restrict the sample to years after 1918; results not presented here.

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### **Supporting Information**

Additional Supporting Information may be found in the online version of this article:

Figure A1: Size of Political Families

Figure A2: Family Types Along Vote Margin

Figure A3: McCrary Density Test First Re-Elections 1837–1910

Table A1: Types of Family Relations

Table A2: First Re-Elections

Table A3: Characteristics of Winners and Losers of Marginal Seats