Unbreakable Legacies? Redistricting, Political Capital and Political Dynasties

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Abstract

Political dynasties in democracies can rely on shared political capital, such as local connections or the personal vote. This paper provides causal evidence of an institutional reform that destroyed political capital, breaking family legacies. We exploit Britain's 1885 Redistribution of Seats Act, mandating the abolition of constituencies below a population threshold. We find little evidence that redistricting affected incumbents' immediate electoral success. Yet those whose seats were abolished were less likely to have relatives entering the House of Commons afterwards. The differences are not driven by fewer immediate successions in the next election, but by having fewer new relatives over the next few decades. Our paper highlights an understudied long-term consequence of redistricting: shifting the distribution of power across generations.

Keywords: Redistricting, Incumbency, Democratization, Representation, Dynasties

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1 Introduction

Can institutional change break family control over politics? In many emerging and developed democracies today, political dynasties remain prominent. Recent research has uncovered causes of political dynasties (see Geys and Smith 2017). Yet, an equally if not more important question is what can break family legacies in politics. We present causal evidence from 19th century Britain showing that redistricting can effectively break family legacies. We exploit a sudden shock to incumbents' political capital as a result of Britain's 1885 Redistribution of Seats Act. This cross-party brokered reform mandated the abolition of electoral constituencies below 15,000 inhabitants, affecting 15% of incumbents. Using a regression discontinuity design, we can distinguish the effects of redistricting from other contemporaneous political and economic reforms.

We find that redistricting broke political legacies. Politicians whose political capital was broken, as their constituencies were redistricted, were less likely to have relatives entering the House of Commons in the following decades. Further evidence that the causal mechanism of the reform's legacy-breaking effects worked via depreciation of the political capital incumbents could bequeath to relatives derives from an additional instrumental variables design. Here, we employ the population threshold as an instrument of the maximum preserved proportion of a new electoral arena formed of an incumbent's old constituency in population, in an RD-IV design across all pre-reform incumbents. The maximum possible difference between incumbents whose new district's population was 100% made up of their old electorates, versus those who faced almost no familiar voters at all, was to increase the probability of having a relative in politics afterwards by almost 40%. This is a local treatment effect, particularly relevant for those incumbents who narrowly escaped redistricting, but who were close to the threshold below which constituencies were destined to be absorbed.

There is no evidence to suggest that the effect was temporary or only on immediate successions. There is limited evidence that dynastic success was greater among loyalists, who had lend support to the reform, but it was not conditional on their early retirement. We conclude there is no evidence that redistricted incumbents were offered golden parachutes by which relatives were helped into seats. Narrowly redistricted incumbents did serve on average 2 years

less than those who narrowly survived. The legacies of the narrow losers from redistricting were broken, while those who narrowly escaped redistricting more often saw relatives enter parliament afterwards. In sum, we argue the reform altered the value of an incumbent's electoral bequest, by disrupting local political machines and breaking monopolies of personal votes. Redistricting may have been one of the key institutional changes that contributed to the decline of hereditary politics in Britain, thereby paving the way for programmatic competition on policy (Seymour 1915).

2 The Legacies of Political Dynasties

Political dynasties are understudied, even while they dominate emerging democracies (e.g., Chandra 2016; Geys and Smith 2017; Van Coppenolle and Smith 2022). An important explanation for the continued success of political dynasties in democracies is that such dynasties hold electoral advantages, including interpersonal incumbency advantages (Van Coppenolle 2020). Individuals in families may share some fixed or growing political capital, which can include their well-known surname or local political connections. Dynastic candidates are often drawn from families central in social networks, facilitating political exchange (Cruz, Labonne, and Querubin 2017). Once selected, having a familiar surname helps to get elected (Dal Bo, Dal Bo, and Snyder 2009; Feinstein 2010; Rossi 2017), promoted (Smith and Martin 2017), and renominated (Bohlken and Chandra 2014). Evidence shows that this electoral advantage is smaller as surname recognition becomes less important for election, and as national political connections become more relevant, i.e. in more party-centred, elite-dominated or captured political systems (Fiva and Smith 2018; Van Coppenolle 2017; Querubin 2016).

An incumbent's legacy, or political capital, may consist of local political networks and political machines, insulating certain families from political competition, and reinforcing the family monopoly. Another component of a dynasty's local electoral monopoly, or political capital, is the local personal vote cultivated in office, a vote bank that is independent of the partisan composition of electoral districts. The personal vote of politicians is an important component of the incumbency advantage (see for example, Ansolabehere, Snyder, and Stewart

2000; Desposato and Petrocik 2003). The extent to which the electoral system encourages politicians to develop a 'personal vote' with their constituents (e.g., Cain, Ferejohn, and Fiorina 1990; Carey and Shugart 1995; Shugart, Valdini, and Suominen 2005), and the bequest of such local monopolies to relatives, can help explain interpersonal incumbency advantages, or why dynasties are more common in some democracies than others (Smith 2012; Van Coppenolle 2014; Rivera 2015).

Such relatively independent, monopolistic dynastic networks may impede political competition over the long run (Acemoglu et al. 2008), leading to captured democracies (Acemoglu and Robinson 2008). Indeed, dynastic succession promotes growth, but only in the absence of strong executive constraints (Besley and Reynal-Querol 2017). If family monopolies of local political capital can sometimes be a useful check on central power, they may also slow down the development of programmatic party competition. This argument dovetails a long-standing wisdom about the crucial role played by redistricting in Britain's democratization, breaking the personal vote, and dynastic, patronage control (Seymour 1915). Notoriously dynastic 19th century Britain is often heralded as a typical case of gradual democratization (e.g., Acemoglu and Robinson 2000; Aidt and Franck 2015; Aidt and Jensen 2014; Boix 2003) where franchise extension was accompanied by an increase in elite competition, a decline in patronage politics, and an increase in public goods provision (e.g., Lizzeri and Persico 2004). Franchise extension was found to be a necessary, but not a sufficient condition for dynastic decline (Berlinski, Dewan, and Van Coppenolle 2014). In fact, the agreement of existing elites may have been important to explain British non-violent democratisation (Ansell and Samuels 2010; Ansell and Samuels 2014; Mares and Queralt 2015; Ziblatt 2017). By asking why dynastic control declined in currently established democracies, we contribute empirical, historical evidence to understanding democratic development more broadly (Capoccia and Ziblatt 2010), and specifically in Britain (see a.o. Leon 2020).

Can institutional change break dynastic legacies, undoing some of these local political capital monopoly advantages, and improving long run political competition? We suggest that redistricting may have understudied long run consequences. The politics and consequences of gerrymandering have been studied extensively, from electoral responsiveness and partisan

bias (Niemi and Winsky 1992; Gelman and King 1994), to polarization (Carson et al. 2007), the incumbency advantage (Ansolabehere, Snyder, and Stewart 2000; Desposato and Petrocik 2003), political competition (Abramowitz, Alexander, and Gunning 2006), and patterns of candidate entry and exit (Cain 1983; Cox and Katz 2002; Hetherington, Larson, and Globetti 2003; Carson, Engstrom, and Roberts 2006). Yet we do not know the empirical consequences of redistricting for dynastic legacies. We suggest that redistricting breaks up family monopolies, by reducing the value of the political capital that an incumbent can bequeath to relatives in a more competitive local arena.

Only a few previous studies directly considered the dynastic effect of institutional reforms, and fewer still found that they can reduce dynasty formation. Term limits, and the introduction of gender quota, further increased the representation of dynasties (Querubin 2011) or of women from dynasties.[1] Our argument complements recent work arguing that electoral system change, which made personal voting less relevant, reduced the proportion of dynastic candidates running in Japan (Smith 2018). Our paper exploits the relative size of the reform shock at the incumbent level. This design enables us to decouple the effects of redistricting from those of other contemporaneous economic and political reforms, such as improved cooperation among candidates (Kam and Newson 2021), improved party organisation (Ostrogorski 1902; Jusko 2017; Ziblatt 2017) or improved party-orientation among voters (Cox 1987).

3 Theoretical Expectations

Redistricting may affect political legacies, or the value that relatives can derive from the political networks of incumbents. The redrawing of electoral district boundaries brings new voters into a politician's bailiwick, thereby eroding part of the advantage an incumbent enjoys in relation to potential challengers. While in office incumbents have the opportunity to build rapport with voters by conducting constituency service, giving speeches, organizing rallies,

[1] At times when certain minorities are under-represented at the national level, dynastic reputations can address informational inequalities about under-represented groups (Folke, Rickne, and Smith 2020). The introduction of direct elections decreased dynasties (Van Coppenolle 2022). Yet, at the local level, dynasties were not found to improve public goods provision (Bragança, Ferraz, and Rios 2015), but to rather lower political quality (Geys 2017), though the quality of dynastic women was not necessarily lower than that of dynastic men (Labonne, Parsa, and Querubin 2021). The personal returns to relatives can also be important, even at the local level (Folke, Persson, and Rickne 2017).

etcetera (Cain, Ferejohn, and Fiorina 1990). All these activities contribute towards the personal vote of representatives, which accounts for an important component of the incumbency advantage across a wide variety of contexts (Ansolabehere, Snyder, and Stewart 2000; Desposato and Petrocik 2003). The personal vote that representatives cultivate during their time in office therefore forms part of the political capital that may account for the emergence of political dynasties in democracies (Feinstein 2010). This may happen, for instance, if the connection an incumbent builds with voters while in power becomes part of his personal brand, which voters could then associate with any of his relatives if they choose to run for office.

Redistricting, however, involves a negative shock to the personal vote of incumbents. The redrawing of electoral district boundaries brings new voters into a politician's bailiwick, thereby eroding part of the advantage an incumbent enjoys in relation to potential challengers. Therefore, we should expect redistricting to have a negative impact on incumbents' ability to build dynasties (hypothesis 1). The relatives of politicians affected by redistricting have little to go by in places where voters do not recognize them, and therefore have a difficult time getting elected. In this way, redistricting should prove an effective remedy to the unequal distribution of power across generations.

In addition, redistricted incumbents, anticipating a decrease in the probability of re-election, may choose to retire from office (Cox and Katz 2002). We therefore expect that incumbents who were faced with redistricting were more likely to retire from their political careers to never return, or were less likely to run in the next election (hypothesis 2). When an incumbent retires, he may lose his influence over the local nomination process, and see his political network decreased over time. We therefore additionally hypothesize that redistricting may have a heterogeneous effect on dynasty formation for those politicians who chose to retire as a result of the reform.

This account, however, ignores the politics that take place within the legislature in the process of approving a redistricting plan. It has long been recognized that party leaders and incumbents work under objectives that conflict with each other when redefining electoral boundaries (Gelman and King 1994). Parties seek to increase the number of seats in the legislature by changing electoral boundaries, but this goal may come at the expense of incum-

bents who may suffer from a higher level of uncertainty following the adoption of new district boundaries. We argue that by exploring this dimension one may be able to identify potential unintended consequences associated with redistricting. There is a related precedent for this type of situation in the US context. In 1992, the increased uncertainty in the election prospects of incumbents as a result of redistricting, combined with a legislative provision allowing members of Congress elected before 1980 to keep their campaign war chests, led to wave of strategic retirements (Groseclose and Krehbiel 1994). If party leaders are in a position to facilitate the access of individuals to office, they may adopt this strategy to "buy" the support of incumbents affected by a reform. In such a scenario, redistricting may increase the ability of certain politicians to see their relatives in office. Therefore, we also consider the politics that take place within the legislature in the process of approving a redistricting plan. We therefore hypothesize there might be a heterogeneous effect of the reform for those affected politicians who were loyal to the government, potentially conditional on retiring from politics (hypothesis 3).

Finally, we consider the reform's effects on political competition more broadly. Specifically, we expect that in removing local dynastic monopolies, the reform may have facilitated future party competition, for example by the Labour party after its creation more than 15 years later. In the next section we discuss the historical episode we examine to estimate the impact of redistricting on the ability of politicians to build political dynasties.

4 Historical Context

It was during the Liberal prime minister William Gladstone's second ministry that parliament passed the Redistribution of Seats Act of 1885. This legislation ordained the redrawing of boundaries to equalize the number of electors across constituencies. To this aim, the act ordained the abolition of constituencies with a population of less than 15,000 inhabitants, redistributing these small populations to larger constituencies. Some large constituencies were broken up, and constituencies with a population between 15,000 and 50,000 would no longer return more than a single representative. The legislation was part of the wave of reforms that aimed at democratizing the electoral system during the late Victorian period (Seymour 1915; Chadwick 1976).

The final legislation was the result of a drawn-out process of bargaining between the leaders of the Conservative and Liberal parties. Gladstone's original intention was to avoid the issue of redistribution entirely. Indeed, he was forced to consider the issue only after the Conservative members of the House of Lords blocked the passage of the Franchise Bill (Third Reform Act) in July of 1884. The long negotiations culminated in an a week-long inter-party conference between Liberal and Conservative party leaders in the first days of November 1884, where differences over the most contentious issues were ironed out. The differences over the substance of the legislation did not map strictly along party lines (Chadwick 1976).

For instance, Sir Charles Dilke (Liberal) and Lord Salisbury (Conservative) favoured a larger population cut-off for the disenfranchisement of boroughs, but were opposed by influential members of the Liberal party whose constituency would be directly affected with the amended cut-off (e.g., Hugh Childers, chancellor of the Exchequer[2]), or those who thought it would undermine the party's electoral prospects (e.g., Lord Richard Grosvenor, Liberal whip). Similarly, Dilke and Salisbury overcame the opposition of the likes of Joseph Chamberlain (Radical) to adopting single-member districts, who thought that this measure would hurt the ability of the party to win elections in places where the Liberal vote was divided. In the end, party leaders settled for the adoption of single-member districts.

Party leaders saw the compromise as the means to end the seat malapportionment resulting from the population growth the country had experienced in the previous decades, and to further undermine the aristocratic nature of representation in the country (Seymour 1915). The task of demarcating the new boundaries across the country was put in the hands of a Boundary Commission, which fulfilled its duties in an impartial manner (Chadwick 1976; Roberts 2006). Although party leaders had a hand in negotiating the boundaries of specific constituencies, Commission officials were given independence to complete their assigned mission. The impartiality of the Commission was partly attained by the balancing of party preferences within the organization. The Commission consisted of a Chairman (Conservative), and Vice-Chairman

[2]He was not re-elected in in 1885, even though he re-entered the race in the constituency he represented, Pontefract. The constituency kept its boundaries largely unchanged, as it was located right above the eventually chosen threshold, though representation was reduced by one seat. So, even if he tried to influence redistribution outcomes, his attempt was eventually unsuccessful. He also saw no relatives enter politics afterwards. He returned as Home Secretary in 1886 and for that reason sought and won the 1886 by-election in Edinburgh South.

(Liberal), two Local Board of Government Inspectors (one from each party), and two members of the Army (Chadwick 1976, p. 679). As a result, the Commission successfully resisted pressure from MPs to draw constituency boundaries in ways that would bolster the electoral prospects of a given party.[3] The final legislation was introduced to the House of Commons on December 1st, 1884, and received royal assent in June of the next year. A general election followed in November 1885.

For this paper's purposes, it is important that the reform was enacted in such a bipartisan manner, and that the incumbents we study had little to no influence over the details. It is difficult to manipulate such cross-party brokered redistricting to promote party interests, and to do so it can only have been a blunt tool.[4] In contrast, political dynasties were common in both the parties before the reform, with 43% of Conservative incumbents, and 37% of Liberal incumbents, having had a relative in parliament before their own first entry. The 1885 reforms increased the total number of MPs, from 652 to 670 members, so there were no fewer overall opportunities to elect dynastic candidates.

5 Data and Measurement

Our theoretical expectations from redistricting are particularly relevant for incumbents whose districts were completely abolished, and merged into another politician's stronghold. This is precisely what happened after the 1885 reform we are concerned with here. We measure this treatment of redistricting by absorption with a dummy, whose coding we switch for ease of interpretation. Our treatment is therefore (narrow) district survival, versus district absorption, which switches on as population passes the threshold of $15,000$. In a next step, we consider the impact of this redistricting treatment on our proxy of an incumbent's political capital, by

[3]For example, an MP representing East Cornwall unsuccessfully sought to amend the boundaries of his constituency under the final Commission's plan (Chadwick 1976, p. 682). Similarly, Commission officials were able to resist pressures from local Conservative and Liberal party members to split Leeds in ways that satisfied the electoral wishes of either party (Roberts 2006, p. 225).

[4]Redistricting has long been believed to have led to the creation of "tory villas" (i.e. constituencies solidly packed with tory sympathizers belonging to the middle and upper-middle classes) thereby accounting for the electoral success of Conservatives in this period (see, for example, Cornford 1963). Yet recently, scholars have argued that the phenomenon of "villa toryism" is overblown, as there is little evidence that Conservatives were able to secure electoral districts predisposed towards them. Instead Conservatives were able to build broad class coalitions through strong local party associations (Roberts 2006; Ziblatt 2017).

employing narrowly surviving redistricting as an instrument for one's political capital.

Specifically, we use the population of the pre-reform constituency of an incumbent to measure whether his constituency narrowly escaped redistricting after the reform, if that population passed the 15,000 threshold. A forcing variable *Population* measures the pre-reform population of the constituency represented by the 1880 incumbent. As population passes the threshold $Pop15_c$ at 15,000, this dummy takes on a value of one. So, $Pop15_c = 1$ if $population > 15,000$, and vice versa. The variable distinguishes the 1880 incumbents who escaped redistricting from those who saw their constituencies absorbed below the threshold.

To proxy political capital, we compare what percentage of the electorate of the post-reform constituencies is made up by an incumbent's pre-reform constituency electorate. Some absorbed constituencies were merged into larger post-reform constituencies than others. In some cases, a pre-reform constituency was split, and then merged with existing constituencies. We measured the affected political capital as the maximum of the estimated population of an incumbent's pre-reform constituency that remained intact across one or more post-reform constituencies as a percentage of the new electorate, $maxpop$. We estimated $maxpop$ by first connecting the enumerated populations from the 1881 census to thousands of parishes that formed part of the pre-reform constituencies. Then, we calculated the overlap in populations at the parish level, before again aggregating.[5]

We also examined the biographical profiles of members of the House of Commons (Stenton 1976 and Stenton and Lees 1978). For each incumbent politician elected in the 1880 general election, we create a set of binary indicators that take the value of 1 if he had a relative entering the House of Commons for the first time at any point after the 1885 redistribution of seats reform, *FutureRelative*. We then consider the timing of successions, by additionally coding whether the succession occurred *Immediately* after the redistricting,[6] or over some time between 1885 and 1918, i.e. *Before1918*. Each of these outcomes allows us to assess whether

[5]These data on the parish populations, from the IPMUS census records of 1881 and their geographical overlap with pre- and post-reform constituencies, were generously shared by Gidon Cohen and Nick Vyvian, University of Durham. We multiplied the parish population with the proportion of the parish in the pre-reform constituency, and then with the proportion of the parish in the post-reform constituency. Finally, we aggregated these populations by the pre-reform constituency, and evaluated its maximum value.

[6]As two elections followed each other so closely, we define immediate successions to include those in 1885 or 1886.

politicians were able to pass on their political capital to their descendants, and if so, how long it took a political family to reap any benefits. Our choice of 1918 as a cut-off is informed by the fact that in that year the country adopted universal suffrage and the government implemented a further redistribution of seats reform, both of which may have effaced any effect the 1885 reform had on the ability of politicians to build political dynasties.

We also examined the biographical profiles to create a set of indicator variables that record whether an incumbent left office before the 1885 general election and, when applicable, the way (e.g., resignation, death, or accession to a peerage) and timing when he did so (before or after the reform was certain to pass). We discard politicians who were elected in off-cycle elections, for whom it is unclear whether they had been able to build up political capital necessary to build a dynasty. We also drop from the analysis all politicians representing university seats, as these seats did not comprise geographic representation. We further restrict this sample to the 463 politicians who were still alive and in office as late as November 1884, since only these could have been directly affected by the reform.

For our analysis, we code as having *Retired* only those politicians who, regardless of the type of exit (except death), left office after November 1884 - when it was known that redistribution legislation was certain to pass in parliament- and before the general election of 1885, to never run again. We use this outcome to assess the possibility that the prospect of the reform may have encouraged MPs to end their political careers after completing their term in office or shortly before the next general election when an opportunity arose. We also analyse whether the incumbent *Ran* again in 1885, and whether he was *Re-elected.*

As additional pre-treatment covariates we include an incumbent's *Age* and occupational background, so whether he was a *Rentier* (i.e. a landowner or living from private income), a member of the broader *Elite* (i.e. further including army and naval officers, and colonial landowners or merchants), had aristocratic connections *Aristocrat*, or even more broadly formed part of an existing political dynasty, i.e. with previous relatives having served in the House of Commons *Junior*, as has been coded before (Berlinski, Dewan, and Van Coppenolle 2014). Additionally, we include an incumbent's partisan affiliation as has been documented before (Eggers and Spirling 2014), distinguishing *Liberal* and *Conservative*. Finally, we measure a

constituency's *Population in 1881* (which are the population figures considered for the Redistribution of Seats Act of 1885), its rural character *County*, and we create indicator variables for a constituency's *Region* which indicates the country (England, Scotland or Wales) as documented (McCalmont 1971). To assess whether loyalty was rewarded, we use the division for the final Reform bill. We code a variable *Loyaltogov*, which is zero (one) for MPs who voted against (for) the reform, and zero (one) for Liberals (Conservatives) who were absent, given that the reform was introduced by the Liberal government but was supported by the Conservative party. In further analyses, we use this variable to test for heterogeneous treatment effects for those who were loyal, and also for those who were both loyal and retired in 1885, i.e. *Retiredloyal*. Finally, we create a new variable, *Partisan change*, at the individual level. The variable represents the weighted total of conservative party winners, with the weights set to the proportion of the pre-reform area constituency of the MP.[7] This is our measure of immediate partisan changes. To measure party competition effects over the long run, we equally created a variable, *Labour Elected after 1885*, if there was ever a successful labour candidate in any part of the MPs old constituency area after 1885 (before that part further changed boundaries).

Our final sample of incumbents is formed of at most 446 incumbents. We were able to estimate maxpop for 400 of them. Table 1 presents some summary statistics. About 27% of incumbent MPs retired after it was sure the reform would pass, and less than 50% would be re-elected. Yet still about 33% would have a relative entering the House of Commons after 1885. The average preserved maximum proportion of the population of a pre-reform constituency in a constituency after the reform was 74%. Only slightly less than 15% of MPs served in constituencies with a population under 15,000 that were fully absorbed, representing 62 unique pre-reform constituencies.

[7]We use weights simply to obtain an aggregate measure by individual, and will restrict our sample to incumbents with zero change in our robustness check below.

	N	Mean	S.D.	Min	Max
$Pop15$: Population > 15,000	446	0.850	0.358	0	1
$Pop50$: Population > 50,000	446	0.603	0.490	0	1
Maxpop	400	0.736	0.321	0.009	1
Retired	446	0.267	0.443	0	1
Ran in 1885	446	0.682	0.466	0	1
Re-elected	446	0.482	0.500	0	1
Re-elected if ran	304	0.707	0.456	0	1
Future Relative	446	0.327	0.470	0	1
Immediately	446	0.161	0.368	0	1
Before 1918	446	0.309	0.463	0	1
Partisan change*	446	-0.267	0.807	-2	1.999
Labour Elected after 1885*	446	0.635	0.482	0	1

Table 1: **Summary Statistics of Instrument, Main Independent, and Main Dependent Variables**

Note: Summary statistics for instrument, main independent, and main dependent variables, full sample. Summary statistics for additional dependent variables in table A1 in the online appendix. Individuals did not die in office and did not leave before the reform passed. *Variable evaluates the aggregate outcome by incumbent of all post-reform constituent parts, until the next boundary change, of his pre-reform constituency.

6 Empirical Strategy

The goal of our empirical analysis is to identify the impact of redistricting by absorption, as a negative shock to the political capital of an incumbent, on his ability to build a political dynasty. We exploit the provision in the Redistribution of Seats Act of 1885 mandating that borough constituencies with a population below 15,000, i.e. small towns, were to be absorbed by the county constituency(/ies) in which they lay. We use the change in status of an incumbent's constituency (absorbed or not), below or above this 15,000 population threshold, as a shock to our proxy of the capital of this politician. Our main result is the causal effect of escaping redistricting by absorption, which is identified at the jump in the population threshold. In addition, we conduct an additional analysis to gain more traction on the political capital depreciation mechanism through which we propose this causally identified change worked. As the reform aimed to equalize population size in constituencies, some incumbents saw their pre-reform constituency absorbed by one in which total population size was larger, and others saw their constituency of large population size split in several smaller ones.

To assess the importance of political capital in accounting for the existence dynasties, our

analysis exploits the change in the maximum weight of one's electorate of the pre-reform period relative to the electorate for which he had to compete following the reform. We use the population threshold $Pop15_c$ as an instrument for the precise, maximum preserved electorate of a politician's constituency, $maxpop$. Our premise is that the descendants of politicians whose constituency was absorbed saw their ability to enter parliament in the future diminished. Using the threshold as an instrument in the RD-IV design allows us to account for the fact that the treatment (escaping absorption) was not a perfect determinant of political capital depreciation.[8] The RD-IV allows us to deal with the fact that other constituencies further away from the threshold also saw boundary changes, which may have resulted in a similar treatment of dispersion of existing electorates.

In our RD analysis, $Y_{i,c}$ is a binary indicator that takes a value of one if politician i representing the 1880 constituency c had a relative in the House of Commons after the 1885 Act. Our dataset is a simple cross-section of 1880 incumbents. Treatment occurs at the constituency-level, so several politicians could receive a similar shock. However, note that the treatment does not depend on the incumbents' choices of if and where to run for re-election. Because politicians representing a given constituency experienced the same shock, we cluster standard errors at the pre-reform constituency level.

We follow recent advice while implementing our RD analysis (Cattaneo and Titiunik 2019) in favour of a non-parametric local polynomial approximation. We estimate a local linear relation of the forcing variable, within optimal bandwidths that minimise mean squared error at both sides of the cut-off, while using a triangular kernel.[9]

Our research design estimates a local average treatment effect at the population threshold only. Therefore, we can exclude the impact of shocks common to all constituencies, such as the effect of the Corrupt and Illegal Practices Prevention Act of 1883 (Kam 2017), or of other reform-mandated changes that did not directly break the geographical monopolies of

[8]Other constituencies also saw boundary changes. Some 25 constituencies with population levels above 15,000 changed names. Yet their values on $maxpop$, as defined below, range widely between 7% and 94%. In addition, this RD-IV design can deal with a concern that the treatment of absorption may not have been perfectly assigned at the threshold: Specifically, exactly one constituency below this population threshold continued to exist after the reform, with about 95% of its population intact.

[9]Our running variable has only a few mass points where more than one incumbent represented the same constituency. We can therefore apply the continuity-based approach to RD analysis (Cattaneo and Titiunik 2023, 90), and we cluster at constituency level.

incumbents. We also exploit the size of the political capital shock, and implement an RD-IV design on our cross-section of incumbents.

While the design's assumptions can never be tested, we follow standard practice to present available empirical support for them. To assess the no sorting assumption we implemented the no discontinuity of density test (McCrary 2008), depicted in figure A2 in the online appendix, and find no evidence of a significant difference in the number of cases just below or above the threshold.[10] Moreover, the threshold does not distinguish significant differences in pre-treatment characteristics, such as the individual characteristics as defined in section 5 (see table A2 in the online appendix). This supports the idea that incumbents were similar around the threshold, and did not select into treatment. This empirical evidence is consistent with the qualitative account of the case as presented in section 4 of the historical context. Aside from a small committee of main party leaders, incumbent MPs had little influence over the implementation of the reform.

In further robustness checks, we account for other relevant aspects about the reform. We evaluate whether the 50,000 threshold that reduced seats for some constituencies, but did not determine their absorption into other districts, similarly affected dynastic prospects. First, although politicians were not aware of the specific details of the legislation, by the winter of 1884 they were reasonably certain that the redistribution of seats reform would be approved in parliament (Roberts 2006, p. 223). As a result, MPs could have retired from politics before the next general election rather than incurring the cost of waging an unsuccessful electoral campaign under the redrawn constituencies.[11] We consider retiring as one of our main outcomes of interest, but also whether there are heterogeneous treatment effects for those incumbents who stepped down after it was clear the measure would pass. Second, the 1885 Third Reform Act had a significant effect on the size of the franchise of rural counties (Seymour 1915, p.481-82). Therefore, in additional robustness checks we keep only urban boroughs in the sample, or even only single-seat pre-reform boroughs, who could not have been affected by franchise

[10]The original McCrary test has a bias toward detecting sorting at the treatment assignment cut-off in the case of a discrete running variable. This problem is common in the context of RD design exploiting population cut-offs (Eggers et al. 2017). When there is evidence of sorting, they suggest finding the variables that predict sorting and add them as controls in the analysis. Doing so does not change the results.

[11]Indeed, we were able to document a large number of MPs who retired either shortly before or at the end of the 1880-1885 parliament.

extension, or a reduction in representation.

In a further test of the reform's mechanism, our RD-IV design requires that the threshold induces a higher level of *maxpop* for incumbents who represented constituencies that narrowly escaped redistricting. The patterns in Figure A1 in the online appendix show that this is indeed the case. Moreover, figure A1 supports our RD design which treats escaping absorption as a binary treatment determined by passing the population threshold.[12] Along with this evidence, we provide results from the first stage to bolster confidence in instrument relevance: The 15,000 population threshold clearly affected the maximum population preserved for an incumbent after the reform, see table 4 in the next section. The table shows that the population threshold predicts an average increase in an incumbent's maxpop of about 54 percentage points following redistricting. The point estimates of the first stage in table 4 are statistically significant and the P-value on the instrument is small in each regression, rejecting the hypothesis of a weak instrument.[13]

How credible is the exclusion restriction for our argument? Did the reform only affect political capital, or also influence outcomes in other ways? Historians debate the partisan consequences of the redistribution.[14] The reform was enacted in a bipartisan manner by a very small committee of party leaders. Disagreement about the measure crossed party lines. In section 4 of the historical context we provided a qualitative argument. To further support the exclusion restriction assumption, we test whether the reform similarly affected a battery of other outcomes.

[12] An alternative interpretation is that we have a continuous treatment of political capital depreciation, and RD designs for continuous treatments have recently been identified (Dong, Lee, and Gou 2023). However, unlike in their empirical example, we do see a strong difference in average political capital at both sides of the population threshold (see figure A1).

[13] Part of the reason why constituencies with more than 15,000 people were changed is that the Boundary Commission had the goal of equalizing population across constituencies. Thus, boroughs whose population was relatively small in relation to neighboring constituencies could have been absorbed into larger areas in order to accomplish this objective. Aylesbury is an example of this situation. With 28,899 inhabitants, it was absorbed in 1885 by the county division of Buckinghamshire Mid.

[14] Directly testing whether redistricting created districts predisposed to vote Conservative is not possible, as this requires knowing individual party-voting results or intentions.

7 Findings

We first present the simplest RD results in the full sample. This is our main test of hypothesis 1 that redistricting reduced political dynasties. We also present our results for our test of hypothesis 2, that redistricting affected strategic retirement decisions as well as re-election results. Next, we present a number of robustness checks. Here we check whether our exclusion restriction assumption is invalidated by considering a battery of other electoral outcomes. We also restrict the sample to those constituencies that saw no other changes, and consider a placebo threshold. Third, we present the results from a full RD-IV design, whereby the threshold instruments the maximum proportion of the incumbent's electorate that was preserved. This full design helps us to test the proposed mechanism by which the reform affected dynasties, through political capital depreciation. Finally, we also present the results of our tests of hypothesis 3, that the legislative process that preceded the adoption of the reform may have heterogeneously affected incumbents depending on their support for the reform. The absence of support for hypotheses 2 and 3, as we will show, supports the idea that the research design for testing hypothesis 1 is credible.

7.1 Dynastic Legacies: RD Results

We now consider the consequences of the reform for political dynasties, around the population threshold. Table 2 presents the main local-linear result for optimal bandwidths, our test of hypothesis 1. We allow for optimal bandwidths that minimise mean squared error, while estimating a local linear relation of the forcing variable, and using a triangular kernel (Cattaneo and Titiunik 2019).[15] Incumbents who escaped redistricting by absorption right above the threshold were about 26% more likely to see relatives entering the House of Commons after them.

Moreover, there is little evidence that these dynasties were started by immediate successions as a result of the reform, as can be seen from the smaller, insignificant coefficient pre-

[15]Figure A4 in the online appendix presents results graphically. Here, we also show the spread of the data by contrasting equally spaced and quantile spaced bins on either side of the cut-off, see figure A3. We present robust results as suggested by Cattaneo and Titiunik (2019), which means that the presented estimate is not in the middle of the confidence interval.

	MSE-optimal	RD	Robust inference			Clusters		
Variable	bandwidth	Estimator	P-value	95% C.I.		Left	Right	N
Future relative	26759	0.257***	0.008	0.103	0.698	62	74	444
Immediately	28931	0.059	0.196	-0.084	0.408	62	74	444
Before 1918	27386	0.273**	0.010	0.090	0.679	62	78	444
Retired	36006	-0.192	0.413	-0.702	0.289	62	89	444
Ran next election	34288	0.133	0.793	-0.427	0.560	62	89	444
Re-elected	28298	0.027	0.773	-0.491	0.365	62	81	444
Re-elected if ran	34585	-0.008	0.780	-0.615	0.462	32	77	303
Partisan change	32236	0.097	0.525	-0.457	0.896	62	89	444

Table 2: **Full sample results: Incumbents, non-absorbed constituencies**

Estimates of the difference for incumbents escaping redistricting above the 15,000 population threshold on the probability of dynasty formation. Triangular kernel and first order polynomial control for population used. Errors are clustered by constituency. Sample includes 1880 incumbents. Future relative indicates an MP's probability of having a relative entering the House of Commons for the first time after 1885; Immediately if that relative entered in 1885 or 1886; Before 1918 if that relative entered at any point before 1918. Further dependent variables are retiring, running in the 1885 election, and re-election in 1885, as well as partisan change. Partisan change is measured as the weighted total of conservative party winners. *** p<0.01, ** p<0.05, * p<0.1.

sented for the outcome Immediately in table 2. Instead, the local estimate for Before 1918 is of about the same size as the estimate of Future relative, indicating that non-redistricted incumbents had a 27% higher probability of having relatives starting their careers before 1918. We can conclude from these estimates that there was a sizeable decrease in dynastic perpetuation for redistricted incumbents. Hence, we found support for hypothesis 1. Finally, table 2 also shows that there is little evidence that the reform led to politicians strategically retiring, or being less likely to be elected, if their constituency was threatened by absorption.[16] Hence hypothesis 2 receives no support. The estimates are in the expected directions, but not statistically significant. With an estimate close to zero, there is also no evidence for a partisan change.

[16]Several factors could explain the absence of a direct redistricting effect on incumbents. A quarter of incumbents retired, and more than half of incumbents was not re-elected. The reform we considered here affected a comparatively more modest 15% of sitting MPs. Incumbents in Victorian Britain were wealthy individuals, which might explain why some incumbents managed to survive redistricting. Yet these factors were common across the country, and cannot explain our findings for dynasties at the population threshold.

7.2 Robustness of Dynastic Legacy Result

We conducted a number of robustness checks. First, we checked whether our results are unique to the 15,000 threshold. The threshold located at 50,000 reduced seats, but did not significantly decrease political capital for incumbents in constituencies larger than 15,000 but smaller than 50,000. We find no evidence for sorting, but we do not find that this alternative threshold impacted political dynasties in any way.[17] So, the seat reduction below 50,000 did not produce the same dynastic legacy effect as the 15,000 threshold.

As a second robustness check, we exclude partisan changes as an alternative, potential explanation. As before, we consider the weighted average number of Conservatives returned for pre- and post-reform constituencies. We drop any constituency with even the slightest partisan change in 1885 from our sample. Doing so leaves about 88-90% of our sample intact, depending on the specification. Our results are robust, and remain the same when we remove constituencies that see changes in party voting from the sample.[18]

In a third robustness check to exclude alternative explanations, we keep only constituencies that were boroughs, i.e. that saw no changes in enfranchisement. Doing so leaves about 60-98% of our sample intact, depending on the specification. We also further restricted the sample to include only incumbents in borough constituencies with one seat, i.e. that saw no enfranchisement nor district magnitude changes. Doing so leaves only about 23-50% of our sample intact, depending on the specification. The results of these alternative analyses remain the same.[19]

We conduct a final further set of tests, in which we considered alternative outcomes that may have changed at the 15,000 threshold. We code whether the incumbent ran in the same geographic area. We also note the margin of vote by which he won or lost in 1885, whether he ran unopposed, the number of competitors he faced, and the total number of seats available to him across all post-reform constituencies in which at least some part of his pre-reform constituency was located. We find no evidence that redistricted incumbents were less likely to run in the same geographic area, see table 3. Neither are any of the alternative outcomes

[17] See figure A6 and table A3 in the online appendix.
[18] See table A5 and figure A5 in the online appendix.
[19] See table A4 and figure A8 in the online appendix.

	MSE-optimal	RD	Robust inference			Clusters		
Variable	bandwidth	Estimator	P-value	95% C.I.		Left	Right	N
Nr of Competitors	33608	0.052	0.794	-0.601	0.460	32	79	303
Nr of Seats	30599	0.605	0.498	-0.825	1.697	62	73	444
Unopposed	31147	-0.072	0.707	-0.120	0.081	32	88	303
Margin of vote	32610	0.028	0.558	-0.241	0.130	31	74	291
Same geogr. area	34642	0.185	0.255	-0.257	0.969	62	84	444
Total tenure	31847	828*	0.060	-84	4134	62	86	444

Table 3: **Robustness: Other Differences, Incumbents, non-absorbed constituencies**

Estimates of the difference for incumbents escaping redistricting above the 15,000 population threshold on the probability of dynasty formation. *** p<0.01, ** p<0.05, * p<0.1.

affected by the 15,000 population threshold. Finally, redistricting could limit dynasties by cutting short political careers after depreciating family political capital monopolies. We know tenure length tends to increase dynastic prospects in some contexts (Geys and Smith 2017), but not others (Fiva and Smith 2018; Van Coppenolle 2017). Indeed, we find some evidence that the reform cut short political careers. Incumbents above the threshold who were unaffected by redistricting would on average serve a little more than 2 extra years in parliament.

To summarise, we found no evidence that the reform had immediate effects for affected incumbents on their decisions to run, on their odds of being re-elected, or on their decisions to retire never to run again. We also found that the reform did not decisively change whether incumbents ran in the same geographical area right after the reform, though the coefficient is in the expected direction. Yet incumbents with abolished districts spent a little more than 2 years less in service compared to those who escaped redistricting by absorption, indicating that the reform affected individual decisions to run, or odds of being elected, in the future, and also the value of their legacy. This supports our argument that redistricting eroded political capital, and broke dynastic legacies. To further support this argument that the mechanism of political capital destruction was responsible for the reduction in dynasties, we present the full RD-IV results in the next section.

7.3 Political Capital Depreciation: RD-IV Results

The RD results ignored that the magnitude of the redistribution "shock" to one's political capital varied across the country, as different absorbed boroughs were combined with differently

sized counties, changing the relative importance of one's pre-reform area after reform. In addition, the binary treatment (absorption) does not capture the fact that the boundaries of several constituencies above the population threshold were redrawn, which may have affected the long-term prospects of political survival for incumbents representing those boroughs. Here, we use as a treatment the maximum preserved political capital in electorates, $maxpop$. We lose precision by using the population threshold as an instrumental variable for the precise, maximum proportion of the new constituency controlled by an incumbent's old personal votes. This is reflected in the wide confidence intervals around the estimated effects. The results of table 4 present the effect, with a confidence interval, of a one hundred percent increase in $maxpop$. This variable indicates for each incumbent the maximum share of the population of their pre-reform constituency that was preserved in any new districts, as a share of those new districts' populations. This measure is built on the 1881 census figures. These estimates are in exactly the same direction as the RD results presented before. Incumbents whose political capital was less signficantly affected after redistricting above the threshold, which means that they held a larger maximum share of the population of the new constituency or constituencies, were more likely to see relatives entering in the House of Commons afterwards. As a larger proportion of the population in a new district was drawn from an incumbent's old district, say an increase from almost no old voters in the new district to the maximum of 100% of the pre-reform electorate preserved, that incumbent's chances of establishing a dynasty were almost 40% larger. On average, redistricted incumbents below the threshold preserved about 12 percent of the population of the new constituencies from their old districts, while incumbents above the threshold kept about 86 percent after the reform. This 74% difference would have increased the less affected incumbents' chances of perpetuating their political dynasty by about 29%. Finally, even though it just fails to be significant, total tenure seems to have been slightly longer for incumbents with larger population shares preserved as a result of the reform. A 100 percent increase in maximum population and arguably personal votes preserved, is predicted to have extended a political career by about a little less than 4 years.

The validity of the RD-IV results crucially hinges on the exclusion restriction, or whether redistricting by absorption affected dynastic prospects of incumbents only via our proxy of po-

litical capital depreciation, *maxpop*. In further support of this approach, we consider whether the reform affected a range of other outcomes that could potentially have consequences for political competition and dynasties. To test whether the reform had immediate partisan effects, we consider the change in the number of Conservatives returned from districts below and above the threshold, with the number returned weighted by the new constituency's weight in the post-redistricting constituency, *Immediate partisan change*, for which we find no evidence.[20] Above, in table 3, we already considered some measures of electoral competition: Whether the incumbent chose to run in the same geographical area, the number of seats, the number of competitors, whether candidates ran unopposed, the margin of vote, and the total tenure in years of incumbents. We further checked more structural consequences of the reform, such as the minimum and maximum aggregate population in 1885, as well as of the aggregate electorate size and total votes cast in 1885, of old constituency elements making up a post-reform constituency. The goal of the reform was explicitly to harmonise post-reform population sizes in constituencies, which means post-reform constituency size should not differ much. Unsurprisingly, given that redistricted incumbents were from the smallest constituencies, we observed a small difference for the minimum, but not for the maximum.[21] These results bolster our argument that there are no other, easily measurable and significant changes at the cut-off other than the relative importance of one's pre-reform electorate in the post- reform constituency. The exclusion restriction cannot be tested. Yet the evidence from the RD-IV design shows that if this assumption holds, the redistricting reform seems to have worked via our suggested mechanism of political capital depreciation.

7.4 Intra-legislative Bargaining and Long Run Competition

We hypothesized before that party leaders may have offered a deal to secure enough support for the passage of the redistribution bill in parliament. However, the results above for retirement and for immediate dynastic succession do not support these hypotheses 2 and 3 (table 2). Those who narrowly escaped redistricting did serve a little longer over the long run, but

[20] Table A6 in the online appendix shows little evidence of a partisan change towards the Conservatives in constituencies that narrowly escaped absorption.

[21] See table A6 in the online appendix.

Variable	MSE-optimal bandwidth	RD Estimator	Robust inference P-value	95% C.I.		Clusters Left	Right	N
Future relative	34493	0.397**	0.030	0.062	1.207	61	61	400
First stage		0.536***	< 0.001	0.410	0.710			
Immediately	36073	0.148	0.187	-0.150	0.766	61	68	400
First stage		0.533***	< 0.001	0.401	0.706			
Before 1918	35538	0.439**	0.040	0.028	1.149	61	68	400
First stage		0.534***	< 0.001	0.422	0.714			
Retired	42192	-0.285	0.517	-1.310	0.659	61	76	400
First stage		0.521***	< 0.001	0.389	0.707			
Ran next election	42409	0.171	0.851	-1.118	0.923	61	76	400
First stage		0.521***	< 0.001	0.3448	0.699			
Re-elected	37547	0.129	0.675	-1.051	0.681	61	69	400
First stage		0.529***	< 0.001	0.361	0.700			
Re-elected if ran	44113	0.046	0.974	-1.039	1.074	32	65	271
First stage		0.527***	< 0.001	0.4562	0.768			
Partisan change	41363	0.321	0.491	-1.038	1.588	61	80	400
First stage		0.522***	< 0.001	0.436	0.713			
Nr of Competitors	48084	-0.132	0.500	-1.350	0.658	32	67	271
First stage		0.523***	< 0.001	0.442	0.764			
Nr of Seats	43594	0.738	0.206	-0.883	4.085	61	77	400
First stage		0.520***	< 0.001	0.432	0.718			
Unopposed	36318	-0.090	0.854	-0.166	0.200	32	58	271
First stage		0.540***	< 0.001	0.456	0.763			
Margin of vote	41643	0.090	0.794	-0.402	0.307	31	66	262
First stage		0.529***	< 0.001	0.468	0.758			
Same geogr. area	39664	0.331	0.216	-0.453	2.003	61	70	400
First stage		0.527***	< 0.001	0.427	0.751			
Total tenure	38954	1372	0.102	-727	8021	61	70	400
First stage		0.526***	< 0.001	0.372	0.698			

Table 4: **RD-IV full sample results (population overlap): Incumbents**

Estimates of a 100% increase in *maxpop*, a measure of overlap in population based on the 1881 census information by constituent parishes, as instrumented by escaping redistricting above the 15,000 population threshold, on the probability of dynasty formation. Triangular kernel and first order polynomial control for population used. Errors are clustered by constituency. Sample includes 1880 incumbents. Future relative indicates an MP's probability of having a relative entering the House of Commons for the first time after 1885; Immediately if that relative entered in 1885 or 1886; Before 1918 if that relative entered at any point before 1918. Further dependent variables are retiring, running in the 1885 election, and re-election in 1885, as well as partisan change. Partisan change is measured as the weighted total of conservative party winners, with the weights set to the proportion of the pre-reform area constituency of the MP. *** p<0.01, ** p<0.05, * p<0.1.

Variable	MSE-optimal bandwidth	RD Estimator	Robust inference P-value	95% C.I.		Clusters Left	Right	N
Full sample:								
Future relative	26759	0.257***	0.008	0.103	0.698	62	74	444
Sample restricted to:								
Loyal to gov.	29947	0.416**	0.044	0.010	0.778	35	45	251
Retired	36267	0.014*	0.079	-0.051	0.932	28	29	119
Retired Loyal to gov.	35785	0.013	0.382	-0.476	0.182	14	12	66
Ran next election	33999	0.243	0.116	-0.069	0.626	32	68	303

Table 5: **Restricted samples, main results: Incumbents, non-absorbed constituencies**

Estimates of the difference for incumbents escaping redistricting above the 15,000 population threshold on the probability of dynasty formation. Triangular kernel and first order polynomial control for population used. Errors are clustered by constituency. Sample includes 1880 incumbents. Future relative indicates an MP's probability of having a relative entering the House of Commons for the first time after 1885. *** p<0.01, ** p<0.05, * p<0.1.

we argued that resulted from their preserved political capital (tables 3 and 4), not from central party rewards.

To conduct more formal tests of this idea or hypothesis 3, we reran our analysis, restricting the sample to individuals with specific characteristics. The first row of table 5 presents as a reminder the effect of the threshold on Future relative in the full sample, as in table 2. The next row confirms that this result holds in a sample restricted to incumbents who had voted loyally along with the government on the reform legislation, as can be seen from the significant coefficient, similar in size to or even somewhat larger than the main result. If we restrict the sample to incumbents who retired following the reform, the coefficient remains positive and significant but its size is quite small. The effect is similarly small for incumbents who retired after having voted along with the government on the reform, and no longer significant. These results indicate that incumbents who narrowly escaped redistricting successfully established dynasties, but that this effect does not hold conditionally on having retired. Again, we find little evidence for the hypothesis 3 of golden parachutes.

Finally, to investigate the long-term importance of our findings for party competition, we consider the consequences of redistribution for long-term change in support for the Labour party which emerged more than 15 years after the reform. We consider where Labour candidates were more likely to be successful after 1885. In the full samples, we observe little support

for this hypothesis though the effect is in the expected direction.[22]

8 Conclusion

To examine whether redistricting affects the ability of politicians to build dynasties, we analysed Britain's Redistribution of Seats Act of 1885. This policy mandated the abolition of boroughs with a population below 15,000 inhabitants. The reform unambiguously represented a negative shock to the personal vote and political capital of incumbents. Our analysis showed that politicians from abolished boroughs were not particularly more likely to retire from politics following the reform. We also found no evidence for a change in incumbency advantages. However, we did find that these politicians were less successful in securing a place for their relatives in the House of Commons.

Our results improve our understanding of what breaks the perpetuation of political dynasties. Arguably, the loss of personal votes and name recognition, along with the disruption of local political networks, explain why redistricted incumbents were so negatively affected in their dynastic electoral legacies. Even if redistricted incumbents did not immediately retire at higher rates, or were more likely to suffer defeat - which could be related to support promised to them by party leaders-, they did serve slightly shorter tenures. The disruption to their political dynastic succession also does not occur immediately, but over time. The disruption of legacies was no less for those who stayed loyal. So if incumbents were compensated for their support to the reform bill, any such support in electoral competition was short-lived, and not transferable to their relatives. Therefore, the effect of redistricting in fundamentally reshaping personal political capital may be temporarily countered by party leaders, to ensure the measure is passed, and may take some time to trickle down.

The effect of redistricting we identified is a local treatment effect, mostly relevant for those incumbents who narrowly escaped redistricting, but who were otherwise similar to incumbents just below the population threshold whose constituencies were absorbed. This means

[22] See table A7 in the online appendix.
If we exclude constituencies that also observed partisan changes right after the election, the picture that emerges becomes clearer but the effect still fails to reach significance.

that we should be careful with extrapolating this finding that redistricting broke family legacies for any incumbent regardless of their characteristics or those of their districts, such as its population size or the share of their electorates that remained intact. However, for this group of incumbents around the threshold and conditional on the credibility of the exclusion restriction, political capital depreciation formed a plausible mechanism: Those incumbents who kept a larger maximum weight of their district's population in the new, post-reform constituencies were more likely to have a future relative in parliament. Our local treatment effect also crucially hinges on this particular instance of relatively partisan-free redistricting of 19^{th} century Britain, as supported by the historical evidence we presented. We should avoid extrapolating to other instances of redistricting across space and time without further detailed, historical study of these contexts. Previous studies have found that reforms with the specific intent of curbing the prevalence of this form of political organization have failed (Querubin 2011). Our paper identifies redistricting as a reform that was aimed at "democratizing" the electoral system (Seymour 1915; Chadwick 1976), which had the unintended consequence of opening up political competition by affecting past incumbents over the long run, and redistributing political power across generations.

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A Online Appendix

A.1 Figures

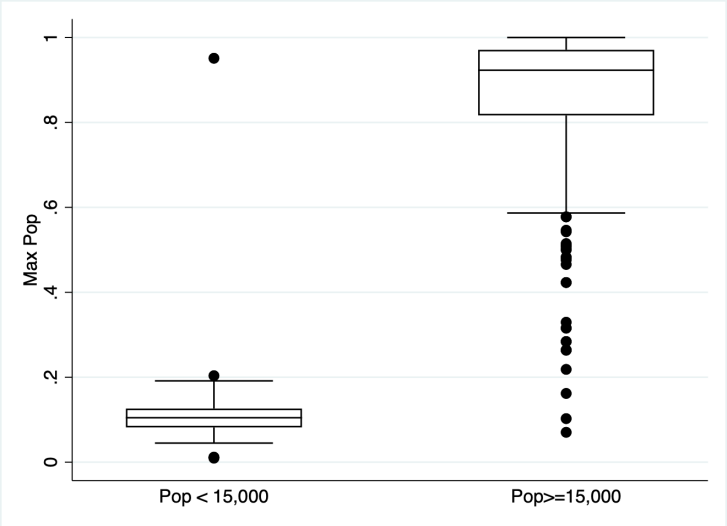

Figure A1: **Distribution of Max Pop by Pre-Reform Population.**

The figure displays boxplots of an incumbent's maximum population of a pre-reform borough relative to the territory of a post-reform constituency by the level of pre-reform population (below and above 15,000 inhabitants). See section 6.

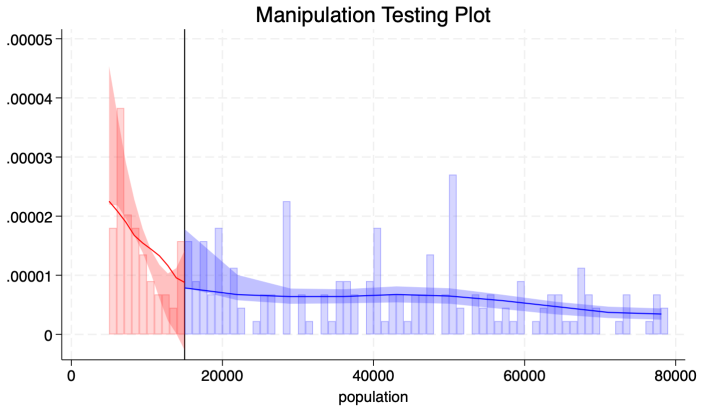

Figure A2: **McCrary Density test**: Manipulation Testing around 15,000 population threshold.

Note: Figure presents the McCrary Density test. Manipulation Testing was conducted using Local-Polynomial Density Estimation with rddensity in Stata, with a first (second) order polynomial used to construct the (bias-corrected) density point estimators (solid black line), a triangular kernel, including 95% confidence interval (shaded grey area), bandwidths optimal at either end. Histogram presents the frequency of observations at either end of the threshold for 1,000 population intervals. Test result equals 1.3730 with associated P-value of 0.1698. See section 6.

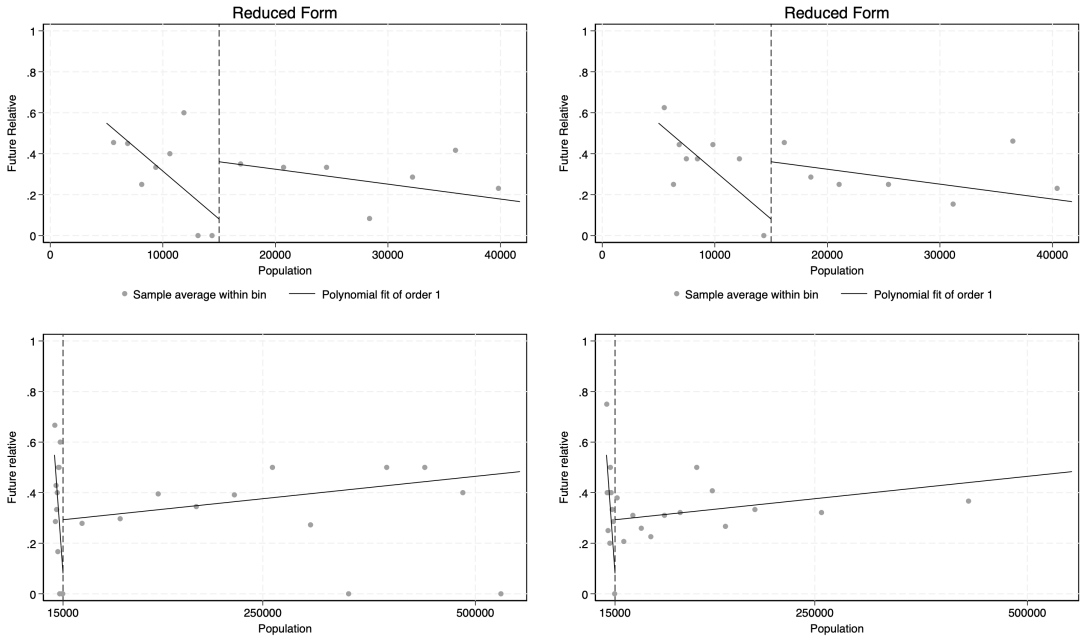

Figure A3: **Effect of Escaping Absorption above Threshold on Future Relative**

Regression discontinuity plots with equally spaced (left) and quantile spaced bins (right), i.e. esmvpr and qsmvpr using polynomial regression, following Cattaneo and Titiunik 2019, at either end of the 15,000 population threshold. The same graph with different scales for the x-axis are presented in two rows, for legibility and completeness respectively. Scattered dots represent binned averages. Lines represent first order polynomial at both sides of the discontinuity, triangular kernel was used.

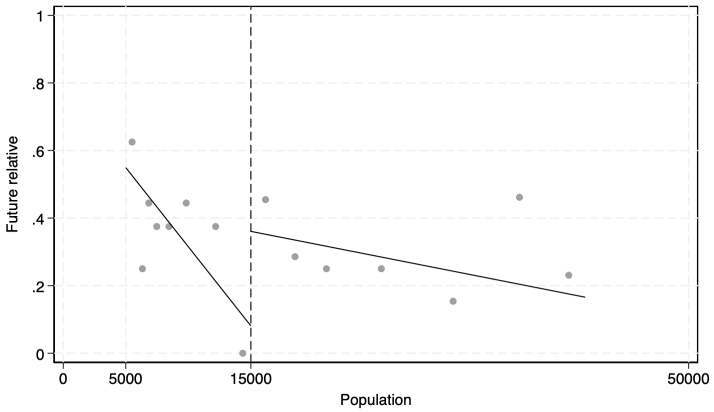

Figure A4: **Effect of Escaping Absorption above Threshold on Future Relative**

Regression discontinuity plot of the RD results in optimal bandwidths Cattaneo and Titiunik (2019) at either end, presenting the probability for incumbents narrowly faced with redistricting (left of threshold) vs. narrowly escaping redistricting (right of threshold) of having a relative entering the House of Commons for the first time after 1885. Scattered dots represent binned averages, for quantile spaced bins using polynomial regression (qsmvpr). Lines represent first order polynomial of population at both sides of the discontinuity, triangular kernel.

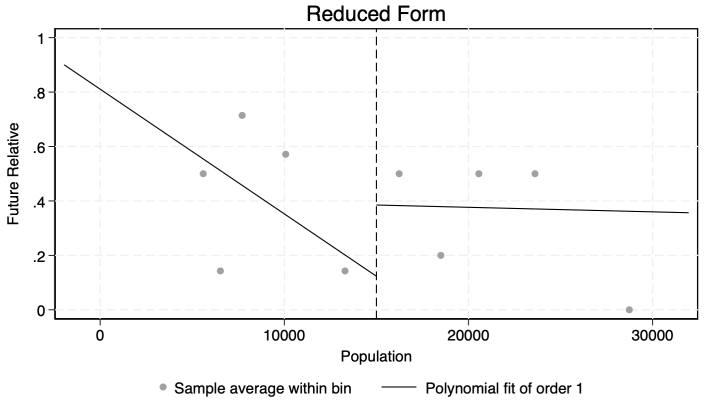

Figure A5: **Robustness Check: Conditional on no Party Swings, Effect of Escaping Absorption above Threshold on Future Relative**

Regression discontinuity plot of the RD results in optimal bandwidths Cattaneo and Titiunik 2019, presenting the probability for incumbents narrowly faced with redistricting (left of threshold) vs. narrowly escaping redistricting (right of threshold) of having a relative entering the House of Commons for the first time after 1885. Scattered dots represent binned averages. Lines represent first order polynomial of population at both sides of the discontinuity, triangular kernel was used. Full results in table A5.

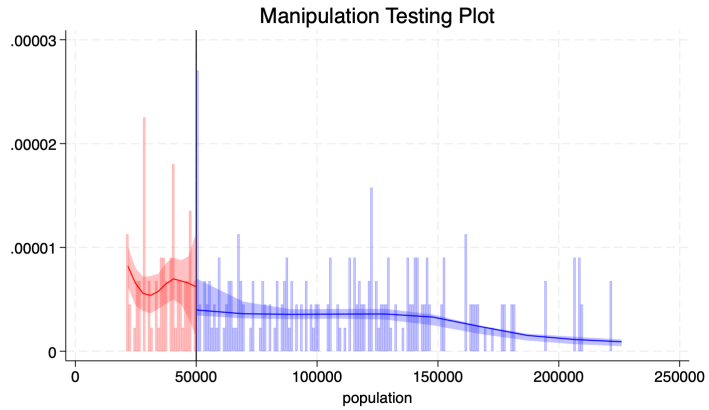

Figure A6: **McCrary Density test alternative threshold**: Manipulation Testing around 50,000 population threshold.

Note: Figure presents the McCrary Density test. Manipulation Testing was conducted using Local-Polynomial Density Estimation with rddensity in Stata, with a first (second) order polynomial used to construct the (bias-corrected) density point estimators (solid black line), a triangular kernel, including 95% confidence interval (shaded grey area), bandwidths optimal at either end. Histogram presents the frequency of observations at either end of the threshold for 1,000 population intervals. Test result equals -0.4287 with associated P-value of 0.668. See section 7.2.

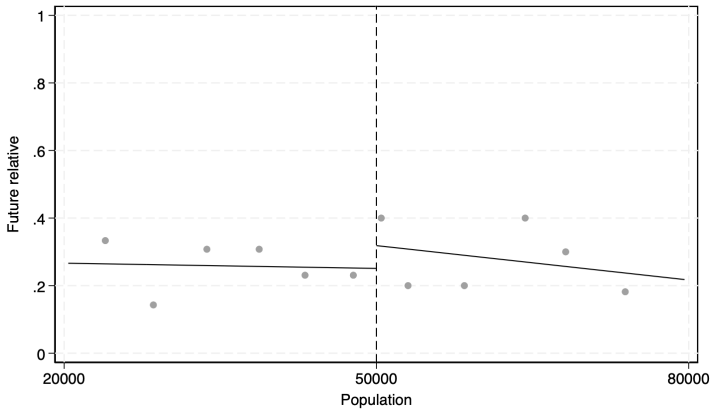

Figure A7: **Placebo Threshold: Future Relative (alternative threshold main RD result)**

Regression discontinuity plot of the RD results in optimal bandwidths Cattaneo and Titiunik 2019, presenting the probability for incumbents narrowly faced with redistricting (supposedly left of placebo threshold) vs. narrowly escaping redistricting (supposedly right of placebo threshold) of having a relative entering the House of Commons for the first time after 1885. Scattered dots represent binned averages. Lines represent first order polynomial of population at both sides of the discontinuity, triangular kernel was used. Full results in table A3.

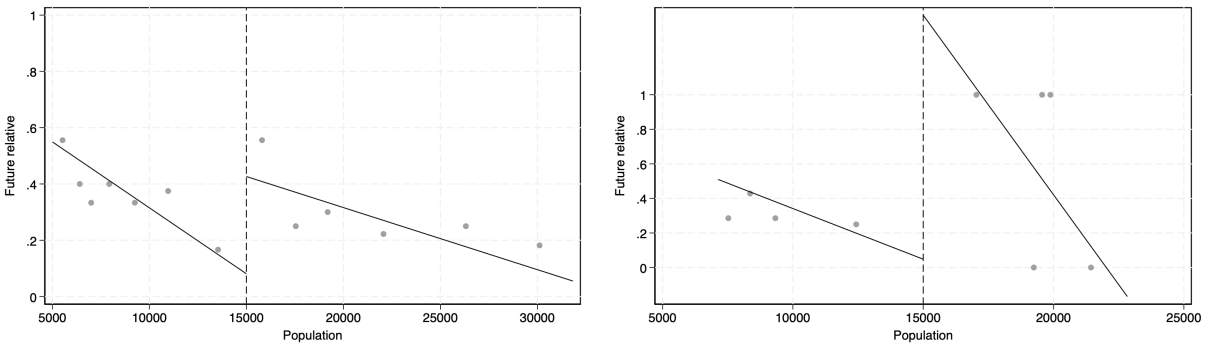

Figure A8: **Robustness: Main findings, only boroughs (left) and only boroughs of 1 seat (right)**

Regression discontinuity plot of the RD results in optimal bandwidths Cattaneo and Titiunik 2019, presenting the probability for incumbents narrowly faced with redistricting (left of threshold) vs. narrowly escaping redistricting (right of threshold) of having a relative entering the House of Commons for the first time after 1885. Scattered dots represent binned averages. Lines represent first order polynomial of population at both sides of the discontinuity, triangular kernel was used. Full results in table A4.

A.2 Tables

	N	Mean	S.D.	Min	Max
Min Population 1885*	425	45954	16773	142	153051
Min Total Votes 1885*	425	5751	4268	0	53752
Min Total Electors 1885*	425	6771	3818	0	34840
Max Population 1885*	425	84464	59288	18941	564981
Max Total Votes 1885*	425	15922	17683	0	95178
Max Total Electors 1885*	425	15124	11143	0	60628
Total Number of Seats in 1885*	446	6.605	6.159	0	38
Margin of Vote in 1885	292	0.084	0.154	-0.361	0.538
Unopposed in 1885	304	0.039	0.195	0	1
Number of Competitors in 1885	304	2.178	0.759	0	5
Ran in Same Geographic Area*	446	0.843	0.364	0	1
Total tenure in days	446	6698.094	4045.674	2063	23988
Loyal to gov.	446	0.563	0.497	0	1
Retired Loyal to gov.	446	0.148	0.355	0	1

Table A1: **Summary Statistics**: Additional Dependent Variables, incl. Pre-Reform Constituency and Individual Incumbent Characteristics

Note: Summary statistics for additional dependent variables, full sample. Individuals did not die in office and did not leave before the reform passed. *Variable evaluates the aggregate outcome by incumbent of all post-reform constituent parts of his pre-reform constituency.

	MSE-optimal	RD	Robust inference			Clusters		
Variable	bandwidth	Estimator	P-value	95% C.I.		Left	Right	N
Liberal	32249	-0.056	0.383	-0.482	0.185	62	89	444
Conservative	32314	0.052	0.594	-0.241	0.421	62	86	444
District magn	27627	0.118	0.433	-0.314	0.733	62	74	444
Age	33525	-3.022	0.944	-13.024	13.990	62	90	444
Aristocrat	29726	0.122	0.264	-0.127	0.462	62	81	444
Elite	33829	0.013	0.711	-0.351	0.515	62	82	444
Rentier	30634	0.065	0.214	-0.108	0.482	62	80	444
Junior	29787	0.200	0.730	-0.355	0.506	62	80	444
England	27742	0.095	0.275	-0.188	0.661	62	84	444
Scotland	25159	-0.017	0.757	-0.135	0.098	62	92	444
Wales	32536	-0.090	0.278	-0.633	0.182	62	80	444

Table A2: **Effect of Narrowly Redistricted Incumbents on Pre-Treatment Characteristics**

Note: Pre-treatment characteristics of incumbents escaping redistricting above the 15,000 population threshold on the probability of dynasty formation. Triangular kernel and first order polynomial control for population used. Errors are clustered by constituency. Sample includes 1880 incumbents. *** p<0.01, ** p<0.05, * p<0.1. See section 6.

	MSE-optimal	RD	Robust inference			Clusters		
Variable	bandwidth	Estimator	P-value	95% C.I.		Left	Right	N
Future relative	29600	0.067	0.700	-0.294	0.438	89	55	444
Immediately	24584	0.037	0.482	-0.094	0.200	70	45	444
Before 1918	27254	0.118	0.484	-0.239	0.504	83	49	444
Retired	26978	-0.190	0.187	-0.536	0.105	85	49	444
Ran next election	26083	0.262*	0.096	-0.048	0.590	78	49	444
Re-elected	18840	0.265	0.262	-0.171	0.629	56	40	444
Re-elected if ran	24777	0.107	0.658	-0.311	0.493	57	37	303
Partisan change	31600	-0.412	0.272	-1.134	0.320	100	59	444

Table A3: **Robustness: Placebo threshold, Incumbents, non-absorbed constituencies**

Note: Estimates of the difference for incumbents above the 50,000 population threshold on the probability of dynasty formation. Triangular kernel and first order polynomial control for population used. Errors are clustered by constituency. Sample includes 1880 incumbents. Future relative indicates an MP's probability of having a relative entering the House of Commons for the first time after 1885; Immediately if that relative entered in 1885 or 1886; Before 1918 if that relative entered at any point before 1918. Further dependent variables are retiring, running in the 1885 election, and re-election in 1885, as well as partisan change. Partisan change is measured as the weighted total of conservative party winners. *** p<0.01, ** p<0.05, * p<0.1.

	MSE-optimal	RD	Robust inference			Clusters		
Variable	bandwidth	Estimator	P-value	95% C.I.		Left	Right	N
Future relative	13251	1.407***	0.004	0.524	2.792	55	12	103
Immediately	13026	-0.435	0.368	-1.624	0.602	55	11	103
Before 1918	13222	1.413***	0.004	0.519	2.814	55	12	103
Retired	14832	-0.278	0.267	-0.985	0.272	55	15	103
Ran next election	14740	0.294	0.355	-0.336	0.937	55	15	103
Re-elected	14150	-0.215	0.684	-1.861	1.221	55	14	103
Re-elected if ran	14228	-0.328	0.537	-2.085	1.086	27	14	66
Partisan change	13602	-0.516	0.223	-1.891	0.442	55	13	103

Table A4: **Robustness: Incumbents, non-absorbed constituencies boroughs of 1 seat only**

Note: Estimates of the difference for incumbents above the 15,000 population threshold on the probability of dynasty formation, boroughs of 1 seat only. Triangular kernel and first order polynomial control for population used. Errors are clustered by constituency. Sample includes 1880 incumbents. Future relative indicates an MP's probability of having a relative entering the House of Commons for the first time after 1885; Immediately if that relative entered in 1885 or 1886; Before 1918 if that relative entered at any point before 1918. Further dependent variables are retiring, running in the 1885 election, and re-election in 1885, as well as partisan change. Partisan change is measured as the weighted total of conservative party winners. *** p<0.01, ** p<0.05, * p<0.1.

Variable	MSE-optimal bandwidth	RD Estimator	Robust inference P-value	95% C.I.		Clusters Left	Right	N
Future relative	16956	0.262**	0.028	0.055	0.940	32	29	136
Immediately	16521	0.157*	0.081	-0.045	0.762	32	29	136
Before 1918	16838	0.319**	0.036	0.032	0.910	32	30	136
Retired	17675	-0.202	0.531	-0.604	0.311	32	31	136
Ran next election	17188	0.172	0.446	-0.284	0.646	32	29	136
Re-elected	18329	0.205	0.740	-0.631	0.889	32	29	136
Re-elected if ran	20872	0.240	0.938	-0.779	0.843	18	27	103

Table A5: **Robustness: Incumbents, non-absorbed constituencies, excluding partisan changes**

Note: Estimates of the difference for incumbents escaping redistricting above the 15,000 population threshold on the probability of dynasty formation. Triangular kernel and first order polynomial control for population used. Errors are clustered by constituency. Future relative indicates an MP's probability of having a relative entering the House of Commons for the first time after 1885; Immediately if that relative entered in 1885 or 1886; Before 1918 if that relative entered at any point before 1918. Further dependent variables are retiring, running in the 1885 election, and re-election in 1885. Partisan change is measured as the weighted total of conservative party winners. Sample includes 1880 incumbents, excluding constituencies with partisan changes. *** p<0.01, ** p<0.05, * p<0.1.

Variable	MSE-optimal bandwidth	RD Estimator	Robust inference P-value	95% C.I.		Clusters Left	Right	N
Partisan change	32236	0.097	0.525	-0.457	0.896	62	89	444
Min population	30686	-20328***	< 0.001	-35692	-11002	55	82	423
Max population	33165	-1191	0.768	-17855	13187	55	75	423
Min electorate	31978	-2988***	< 0.001	-7197	-2069	55	83	423
Max electorate	34101	711	0.767	-1916	2599	55	77	423
Min votes cast	32440	-2413***	< 0.001	-5651	-1658	55	83	423
Max votes cast	36947	-110	0.733	-2462	1731	55	82	423

Table A6: **Effect of Narrowly Redistricted Incumbents on partisan change and Other Pre-Reform Constituency Characteristics**

Note: Difference in partisan change for incumbents escaping redistricting above the 15,000 population threshold. Triangular kernel and first order polynomial control for population used. Errors are clustered by constituency. Sample includes 1880 incumbents. *** p<0.01, ** p<0.05, * p<0.1.

Variable	MSE-optimal bandwidth	RD Estimator	Robust inference P-value	95% C.I.		Clusters Left	Right	N
Full sample:								
Labour elected	31690	-0.103	0.330	-0.878	0.295	62	78	444
Sample restricted to:								
No partisan change								
Labour elected	20780	-0.242	0.118	-1.359	0.153	32	32	136

Table A7: **Long-term results**

Note: Effect for incumbents in narrowly non-absorbed constituencies on the probability of labour party success after 1885, at the 15,000 population threshold, unconditional and conditional on no immediate partisan change Estimates of the difference for incumbents escaping redistricting above the 15,000 population threshold on the probability of dynasty formation. Triangular kernel and first order polynomial control for population used. Errors are clustered by constituency. Sample includes 1880 incumbents. Partisan change is measured as the weighted total of conservative party winners. *** p<0.01, ** p<0.05, * p<0.1.