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**Reining the Party In: The Price of
Taking Back Control**

Aboa Centre for Economics

Discussion paper No. 180

Turku

October 2026

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economics departments of the University of Turku and
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ISSN 1796-3133

Printed in Uniprint
Turku
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ABSTRACT

Parties may trade local choice for leadership control when recruiting candidates, either to promote loyalists or to diversify candidate pools. We quantify the resource cost of centralization using the British Conservative Party's 2006 A-list reform, which increased national influence over nominations. A-list parliamentary candidates were less locally embedded than other Conservative candidates. Difference-in-differences estimates show that A-list candidacies raised party (non-personal) campaign spending by roughly 11–21%, with no detectable change in candidates' personal spending or short-run vote share. The increase occurred for both female and male A-list candidates. By 2015, the party further increased its spending. A simple vote-production model maps these estimates into the cost of offsetting weaker local appeal. For vote–spending elasticities between 0.10 and 0.25, offsetting a one-percentage-point appeal deficit requires roughly 11–30% more party funds. The results quantify a previously implicit price of centralized party control.

JEL Classification: D72, P16, C23, C26

Keywords: centralized nominations, party control, campaign finance, political selection, vote elasticity, difference-in-differences, United Kingdom

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Acknowledgements

Robert Pylkkänen, Valtteri Kimmo, and Saara Vainio provided excellent research assistance. We thank Toke Aidt, Sascha O. Becker, Gideon Rahat and conference participants at FEEAM 2026, EPCS 2026 and EPSS 2026 for helpful comments. This research is funded by the European Union (ERC, INTRAPOL, 101045239). Views and opinions expressed are only those of the authors and do not necessarily reflect those of the European Union or the European Research Council. Neither the European Union nor the granting authority can be held responsible for them.

1 Introduction

Political parties play a pivotal gate-keeping role in representative democracies by choosing which candidates appear on the ballot (Gallagher and Marsh 1988; Schattschneider 1942; Besley 2005; Dal Bó and Finan 2018). Who controls this process, national leaders, local activists, or party members, is a defining question of party organization (Hazan and Rahat 2010; Cohen et al. 2008). Leaders face a recurring dilemma: whether to delegate the candidate nomination to local organizations which likely select candidates that appeal more to local constituents, or whether to centrally choose candidates who better advance the leadership’s internal and policy objectives (Bawn et al. 2012). This choice can be complicated. Allowing locally chosen candidates can increase electoral support (Campbell et al. 2019), while affecting candidate quality and ideological positioning, which shape electoral selection and outcomes (Galasso and Nannicini 2011; Hirano and Snyder 2014; Serra 2011). In turn, candidate-level electoral accountability is itself conditioned by party competition: in the UK expenses scandal, for instance, electoral penalties for implicated MPs were smaller in constituencies where partisan stakes were higher (Eggers 2014). At the same time, principal-agent tensions shape the implementation of ground campaigns (Enos and Hersh 2015), and related models explain why principals may prefer loyal but less competent agents (Egorov and Sonin 2011).¹ Moreover, political selection models show how organizational and career incentives can sustain mediocre or leadership-aligned politicians (Mattozzi and Merlo 2015; Besley et al. 2017; Çakır, Matakos and Tukiainen 2022). Institutional incentives also affect who enters politics and how selected politicians perform (Gagliarducci and Nannicini 2013; De Paola and Scoppa 2011).

Research on political recruitment has concentrated overwhelmingly on the selected individual candidates. We know much less about how the organizations respond after those candidates are chosen. When national leaders press local organizations to nominate preferred candidates, do they merely change the name on the ballot, or do they also redirect scarce campaign resources?

¹Historical evidence from Westminster likewise finds little support for replacement as the source of rising party cohesion, while showing that more loyal MPs were more likely to obtain ministerial posts (Eggers and Spirling 2016).

If leadership-preferred candidates are less appealing in local communities, can additional resources compensate for the resulting electoral disadvantage, and at what rate? These questions bear on leaders' control, parties' internal organization, and the quality of political competition. We make no normative claim about the welfare implications of *centralized* political control, which depend on how the leadership's agenda aligns with citizens' preferences. Our aim is positive: to measure the organizational resources associated with greater central control.

The tension between electoral incentives and internal control was visible in the British Conservative Party's candidate-selection reforms after David Cameron became leader in 2005. Cameron's modernization project sought to broaden the party's appeal and to increase the representation of women and ethnic minorities among its parliamentary candidates (Childs, Webb and Marthaler 2009; Hill 2013; Dommett 2015; Bale 2016; Krook 2009). The initial April 2006 plan called for a committee to reduce roughly 500 approved aspirants to a Priority List (A-list) of approximately 100–150 candidates. Contemporary reporting stated that 140 associations in winnable seats would initially be required to choose from this list and that the list would contain equal numbers of men and women, alongside a substantial minority-ethnic presence (Woodward and Branigan 2006). Implementation of this idea subsequently evolved. Nine of the first twenty associations reported in the press had selected local candidates rather than A-listers (Branigan 2006). The reform hence changed the available candidates and the leadership signal within a formally local selection process, but it did not operate as a single immutable rule throughout the entire selection cycle.

We compile constituency-level data linking candidate assignments to detailed campaign spending and election outcomes. While the list of individuals on the A-list was never formally published by the party, we recovered a roster of 103 named A-list individuals using the *Way-back Machine*, which was published in 2006 on the well-trusted *ConservativeHome* website, a site run by party insiders, 53 of whom became official 2010 Conservative prospective parliamentary candidates. 43 corresponding constituencies can be assigned to our campaign spending

and election results panel.² The representational aim of the reform is visible in the electoral outcome: the number of Conservative women MPs rose from 17 in 2005 to 49 following the completed 2010 election (UK Parliament 2010), while the number of Conservative MPs from minority-ethnic backgrounds rose from two to eleven (House of Commons Library 2025).

Because A-list candidates were strategically placed, we report difference-in-differences estimates using both a full constituency panel and our preferred probability-matched panel built on pre-reform electoral history, electorate, and average campaign intensity. Three findings emerge. First, party (non-personal) spending rises substantially in A-list constituencies: descriptively, by approximately 21% in 2010 in the raw data specification and 11% in the matching specification. Second, candidates' personal spending does not change in a statistically discernible way. Third, the matched short-run vote-share estimate is close to zero but imprecise; the 95% interval permits changes of roughly minus 1.0 to plus 1.2 percentage points.

Dynamic estimates reveal an interesting medium-run pattern. By 2015, the overall matched event study shows a party-spending effect equivalent to about 49%, while the intent-to-treat vote-share estimate is positive but imprecise. Because these medium-run estimates also reflect incumbency, candidate careers, and subsequent party decisions, we interpret them descriptively.

We organize these facts with a formal vote-production framework that distinguishes voter-valued candidate appeal from leadership-valued candidate attributes. Campaign resources and voter appeal are imperfect substitutes, while the leadership may accept lower initial electoral fit in return for greater control or representational value. The framework yields comparative statics for candidate assignment and resource allocation and supports a conditional calibration of the spending required to offset an assumed candidate disadvantage. However, it does not point-identify the vote–spending elasticity.

We make three contributions. First, we provide evidence that greater central influence over nominations can raise the monetary cost of constituency competition without producing a detectable average short-run vote gain. Second, we distinguish party-level from candidate-level

²The remaining 10 cannot be assigned because they were newly created via the 5th boundary review of Westminster Parliamentary constituencies.

spending and show that the estimated response is concentrated in party (non-personal) resources. Third, we connect candidate selection to campaign finance through a transparent calibration of the combinations of candidate disadvantage and vote–spending elasticity that are consistent with the reduced-form estimates. This supplies a fiscal counterpart to debates about party centralization (Katz and Mair 1995; Hazan and Rahat 2010), activist resistance to leadership-preferred centrists (May 1973; Broockman et al. 2021), and improved descriptive representation implemented from the top down (Phillips 1995; Mansbridge 1999; Krook 2009).

Our work also relates to research on candidate-selection institutions. Primary elections have been linked to elected-official quality and candidate strength (Hirano and Snyder 2014; Carey and Polga-Hecimovich 2006), while preference-vote victories affect subsequent promotion within parties (Folke, Persson and Rickne 2016). More inclusive or decentralized procedures can also carry costs, including ideological extremity (Serra 2011) and activist resistance to centrists (Broockman et al. 2021). In the campaign-finance literature, U.S. studies document strong raw associations for challengers but more modest average effects under designs that address endogeneity (Jacobson 1978; Levitt 1994; Stratmann 2005; Spenkuch and Toniatti 2018). British studies document positive associations between constituency campaign spending and electoral performance and strategic targeting toward competitive seats (Pattie, Johnston and Fieldhouse 1995; Denver et al. 2003). Changes in British campaign spending limits also affect campaign expenditure and electoral competition (Fouirnaies 2021). We add evidence on how a centralized selection intervention changes the availability of constituency campaign resources.

The remainder of the paper proceeds as follows. Section 2 describes the institutional setting and model. Section 3 presents the data, localness evidence, and empirical strategy. Section 4 reports the main results and robustness checks. Section 5 examines gender, strategic targeting, candidate continuation, downstream UKIP and Leave-vote outcomes, and the conditional calibration. Section 6 concludes.

2 Centralized Selection and Campaign Resources

The Conservative Party entered the mid-2000s after three consecutive general-election defeats. The Priority List was one of the most visible instruments of Cameron’s modernization project. Candidates were screened centrally and promoted to associations in electorally important seats (Childs, Webb and Marthaler 2009; Hill 2013; Dommett 2015; Bale 2016). The initial rules were relatively restrictive, but the list expanded and some associations selected local candidates outside it (Woodward and Branigan 2006; Branigan 2006). We therefore define treatment as central sponsorship through the A-list, not as unilateral appointment by headquarters.

2.1 A vote-production framework with leadership value

Consider electoral districts $i = 1, \dots, N$. Candidate c has voter-valued attributes $h_c \in \mathbb{R}$ and leadership value $z_c \in \mathbb{R}$. Let $L_{ic} \in \{0, 1\}$ indicate whether the candidate is a local insider in district i . Effective voter appeal is

$$Q_{ic} = \exp(\gamma h_c + \psi L_{ic}), \quad \gamma > 0, \psi \geq 0. \quad (1)$$

The party’s vote share in district i is produced according to

$$s_i(K_i, c) = A_i K_i^\alpha Q_{ic}, \quad \alpha > 0, \quad (2)$$

where $A_i > 0$ captures baseline partisan advantage and $K_i > 0$ denotes party campaign resources.

The leadership values both winning seats and electing candidates who advance its organizational, ideological, or representational objectives. One reduced-form way to represent this choice is

$$\begin{aligned} & \max_{\{c(i), K_i\}_{i=1}^N} \sum_{i=1}^N [B_i + \lambda z_{c(i)}] \mathbf{1}\{s_i(K_i, c(i)) \geq v_i\} \\ & \text{subject to } \sum_{i=1}^N K_i \leq \kappa \quad \text{and} \quad c(i) \neq c(j) \text{ for } i \neq j, \end{aligned} \quad (3)$$

where v_i is the vote-share target, B_i is the electoral value of winning district i , and $\lambda \geq 0$ measures the weight placed on leadership-valued candidate attributes. The term z_c is distinct from voter-valued appeal h_c : a candidate may be especially valuable to the leadership while initially being less well matched to a local electorate.

For a fixed candidate and target, define district difficulty $\tau_i \equiv v_i/A_i$. The minimum resources required to reach the target are

$$K_i^*(v_i; A_i, c) = \left(\frac{v_i}{A_i Q_{ic}} \right)^{1/\alpha} = \left(\frac{\tau_i}{Q_{ic}} \right)^{1/\alpha}. \quad (4)$$

Conditional on the set of seats the party seeks to win, the allocation problem reduces to assigning candidates and minimizing the resources in (4). The leadership may nevertheless choose a candidate with lower Q_{ic} when the associated gain in z_c is sufficiently large.

Proposition 1 (Comparative statics and assignment) *Let $\alpha > 0$ and $\psi \geq 0$.*

(1) K_i^* is increasing in district difficulty and decreasing in effective voter appeal:

$$\frac{\partial \ln K_i^*}{\partial \ln \tau_i} = \frac{1}{\alpha} > 0, \quad \frac{\partial \ln K_i^*}{\partial \ln Q_{ic}} = -\frac{1}{\alpha} < 0.$$

(2) For $\tau_i \geq \tau_j$ and $Q^{hi} \geq Q^{lo}$, total required resources are weakly lower when the higher-appeal candidate is assigned to the harder district:

$$\left(\frac{\tau_i}{Q^{hi}} \right)^{1/\alpha} + \left(\frac{\tau_j}{Q^{lo}} \right)^{1/\alpha} \leq \left(\frac{\tau_i}{Q^{lo}} \right)^{1/\alpha} + \left(\frac{\tau_j}{Q^{hi}} \right)^{1/\alpha}.$$

(3) Replacing a local candidate ($h_L, L = 1$) with a leadership-preferred outsider ($h_A, L = 0$) changes required resources by

$$\frac{K_A^*}{K_L^*} = \exp\left(\frac{\psi - \gamma(h_A - h_L)}{\alpha}\right).$$

Spending therefore rises when the loss of local embeddedness, ψ , exceeds the gain in

voter-valued attributes, $\gamma(h_A - h_L)$. The leadership may still choose the outsider because $z_A > z_L$ in (3).

The model separates two ideas that were previously bundled together. Q_{ic} captures traits that affect voters, while z_c captures traits valued by the leadership, including loyalty, modernization, or descriptive representation. Greater central control can therefore carry a resource cost even when the leadership views its preferred candidates as intrinsically valuable. The empirical implications are asymmetric: central sponsorship may raise party resources, personal spending need not move, and a small observed vote response can coexist with a negative direct candidate channel that is offset by additional campaign inputs.

3 Data and Empirical Strategy

3.1 Institutional setting and data

The United Kingdom elects Members of Parliament from single-member constituencies under first-past-the-post. The Conservative Party nominates one candidate per constituency, traditionally through local associations operating within national approval rules. The unit of observation is a candidate–constituency–election return.

For 2001 onward, campaign spending comes from Electoral Commission candidate returns. We add 1997 spending from the long-run candidate-finance data assembled by Cagé and Dewitte (2025). Election outcomes and electorates come from the House of Commons Library constituency-results series (House of Commons Library 2020). The panel covers the 1997, 2001, 2005, 2010, and 2015 general elections, three before the A-list and two after. Appendix OG provides further source and measurement details.

Because the raw constituency accounts report total and personal outlays, we construct party spending as

$$\text{Party}_{it} = \text{Total}_{it} - \text{Personal}_{it}. \quad (5)$$

In the returns, personal spending is authorized and paid for by the candidate or agent, while other campaign expenditure includes items paid for by the local association or national party on the candidate’s behalf. We therefore use the label “party (non-personal) spending” for the organizational component of the reported budget.

We study four outcomes: total spending, personal spending, party (non-personal) spending, and Conservative vote share in percentage points. Spending outcomes enter as $\log(1 + y)$, which retains zero-spending observations. Because coefficients from $\log(1 + y)$ depend on the outcome’s scale and are not exact elasticities when zeros are present (Chen and Roth 2024), we report them as effects on the transformed outcome and use $\exp(\hat{\beta}) - 1$ only as a descriptive scale translation. Appendix Table OC3 reports pure-log and inverse-hyperbolic-sine specifications.

3.2 Treatment, panel construction, and baseline model

A constituency is treated if its official Conservative candidate in 2010 was promoted through the A-list. The coding flow is 103 rostered individuals, 53 official prospective parliamentary candidates, and 43 treated constituencies that can be assigned to the harmonized panel. Forty-two of these constituencies have the pre-reform observations and complete covariates required for the preferred match. Thirsk and Malton has no pre-reform Conservative observation and therefore does not enter the preferred matched analysis. Newly created or substantially reconfigured 2010 constituencies that cannot be linked consistently to a pre-reform series are excluded rather than assigned synthetic predecessor observations. Constituency-panel identifiers harmonize capitalization, punctuation, and other non-substantive name variants.³

For the short-run analysis we estimate

$$Y_{it} = \beta (\text{A-list}_i \times \mathbf{1}\{t = 2010\}) + \mu_i + \tau_t + \varepsilon_{it}, \quad (6)$$

using the 1997, 2001, 2005, and 2010 elections. Here μ_i denotes constituency-panel fixed ef-

³Chesterfield is retained, but because no 2001 spending return was submitted there, the spending outcomes remain missing for that election and the matching measure uses the constituency’s observed 2005 campaign intensity.

fects and τ_i election fixed effects. The coefficient β measures the differential change between the pre-reform elections and 2010 in constituencies receiving an A-list candidate, relative to other Conservative constituencies. Under conditional parallel trends, the weighted estimate identifies the intent-to-treat effect of A-list placement for treated constituencies represented by the matched comparison group. Standard errors are clustered by constituency panel.

3.3 Strategic targeting and the preferred match

A-list placements were strategic. Figure 1 plots the 2005 Conservative margin for constituencies that did and did not receive an A-list candidate. Treated constituencies cluster around competitive margins and modest Conservative advantages, whereas other Conservative constituencies include many safe losses and safe victories. This rules out a causal interpretation of post-reform cross-sectional differences and motivates both the within-constituency design and pre-reform matching.

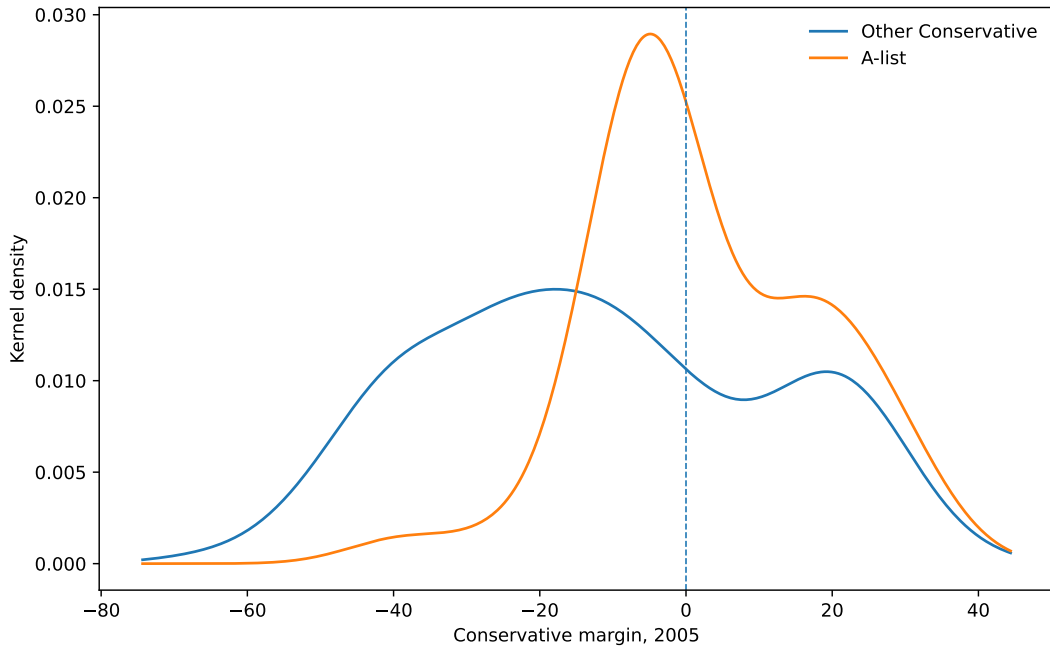


Figure 1: A-list placement and the 2005 Conservative margin

Notes: Kernel densities of the 2005 Conservative margin, defined as Conservative vote share minus the largest non-Conservative vote share, for constituencies that did and did not select an A-list candidate in 2010. The dashed vertical line marks a zero margin.

The preferred match uses only variables fixed before the reform: Conservative vote share in 1997, 2001, and 2005; log electorate in the same elections; and average campaign intensity in 2001 and 2005, measured as total candidate spending as a percentage of the legal limit. We estimate a logit propensity score and form Epanechnikov average-treatment-on-the-treated kernel weights with bandwidth 0.06 and common support. All 42 treated panels with complete covariates are retained; 476 controls receive positive weight, producing 518 matched constituency panels. The effective control sample size is 309.6, and the largest absolute standardized mean difference is 10.2%.

The propensity model excludes 1997 spending so that the corresponding 1997 event-study coefficients provide a held-out outcome diagnostic, although other 1997 covariates such as vote share and electorate do enter the propensity score. The matched pre-reform coefficients are 0.012 in 1997 and -0.002 in 2001 for total spending, with a joint pre-trend p -value of 0.867. For party spending, the corresponding coefficients are 0.020 and 0.012, with a joint p -value of 0.926. This diagnostic is informative but is not a wholly out-of-sample validation exercise.

To examine dynamics we estimate

$$Y_{it} = \sum_{s \in \{1997, 2001, 2010, 2015\}} \beta_s (\text{A-list}_i \times \mathbf{1}\{t = s\}) + \mu_i + \tau_t + \varepsilon_{it}, \quad (7)$$

with 2005 omitted. We jointly test whether the 1997 and 2001 interactions equal zero. These tests assess whether the weighted control group reproduces the pre-reform trajectory; they are not used to select the reported treatment effect.

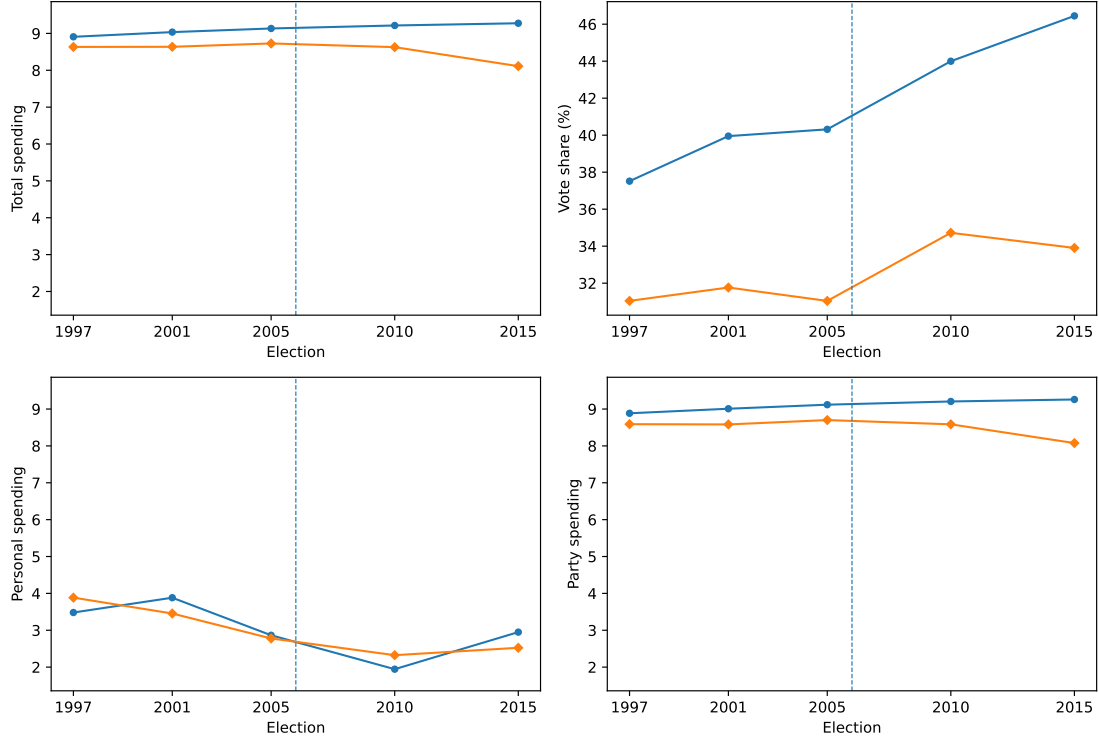


Figure 2: Mean outcomes before and after the A-list reform

Notes: Circles denote constituencies that selected an A-list Conservative candidate in 2010; diamonds denote other Conservative constituencies. The dashed vertical line marks the reform between the 2005 and 2010 elections. Spending outcomes are plotted in their $\log(1 + y)$ transformation.

3.4 Candidate characteristics and local ties

One mechanism suggested by our framework is that centrally promoted nominees may be less rooted in the constituencies in which they stand. We therefore reconstruct each 2010 Conservative candidate's connection to the seat before selection. A candidate has a direct tie when the evidence documents a meaningful constituency-level connection, such as residence, upbringing, employment, council service, local political activity, or earlier candidacy in the seat or an immediately adjacent area. A strong regional tie is a sustained connection to the surrounding county or city-region. We classify a candidate as a broad parachute only when the evidence is sufficient to show neither type of connection; otherwise the case remains unresolved.

Pre-selection local ties were coded through two independent AI-assisted searches covering

all 631 Conservative candidates, as described in Appendix OE. Claude Opus 5 and OpenAI GPT-5.6 Sol Pro, accessed through ChatGPT, independently applied the same protocol. We adjudicated their underlying evidence using the procedure described in the appendix. An independent research assistant also coded a random sample of 100 candidates without seeing either the AI classifications or A-list status. All three exercises point to lower local embeddedness among A-list nominees. Table 1 reports the final full-sample classifications.

Table 1: Pre-selection localness and broad parachuting by A-list status

Measure	A-list nominees	Other candidates	Difference (pp)
Enough evidence to classify	94.3% (50/53)	75.1% (434/578)	19.3
<i>Among classified candidates</i>			
Direct pre-selection local tie	26.0% (13/50)	76.7% (333/434)	−50.7
Any direct or strong regional tie	56.0% (28/50)	85.3% (370/434)	−29.3
Broad parachute	44.0% (22/50)	14.7% (64/434)	29.3
Worst-case bounds after assigning unresolved cases	41.5–47.2%	11.1–36.0%	[5.5, 36.1]

Notes: A direct tie is a documented pre-selection connection to the constituency, a predecessor constituency, the same local authority, or an immediately adjacent area. A strong regional tie is a substantial and sustained pre-selection connection to the same county, metropolitan area, city-region, or a meaningfully adjacent area. A broad parachute has neither type of connection when the available evidence is sufficient. Rates in the three classification rows condition on candidates with enough evidence to classify. The final row assigns unresolved A-list and comparison candidates in the directions least and most favorable to a positive A-list difference. These are missing-data bounds, not confidence intervals.

The evidence shows that A-list nominees were indeed less locally embedded than other Conservative candidates. Among candidates with enough information to classify, 44.0% of A-list nominees are broad parachutes, compared with 14.7% of other Conservative candidates, a difference of 29.3 percentage points. Even under the assignment of every unresolved case least favorable to this conclusion, the full-candidate difference remains positive, between 5.5 and 36.1 percentage points.

The result is replicated across independent coding exercises. Applied separately, the two complete AI-assisted searches produce broad-parachute differences of 28.8 and 29.2 percentage points, and both worst-case full-population bounds remain above zero. The human-coded random sample points in the same direction: among the cases the research assistant could classify, 42.9% of sampled A-list nominees and 6.4% of other sampled candidates are broad parachutes.

The sample contains only eight A-list nominees, so the human contrast is a directional check rather than a precise standalone estimate. For the main distinction used here, broad parachute versus any qualifying local or regional tie, the human coder and the adjudicated classification agree in 83.6% of cases classified by both. Appendix OE reports the full search protocol, separate model results, adjudication procedure, missing-data bounds, and human-validation exercise.

Taken together, the two complete searches, their adjudicated combination, and the blinded human validation provide strong evidence that A-list nominees were less locally connected.

4 Results

4.1 The organizational response

Table 2 reports the short-run difference-in-differences estimates of A-list placement on campaign spending and vote share in the 2010 general election. Panel A uses the complete data, and Panel B applies the preferred matching weights. The evidence is consistent with additional campaign resources compensating for weaker local embeddedness, although it does not by itself establish that localness caused the spending response.

Table 2: A-list placement, campaign spending, and vote share in 2010

	Total spending	Personal spending	Party spending	Vote share (percentage points)
<i>Panel A. Raw constituency panel</i>				
A-list constituency $\times$ 2010	0.175*** (0.047)	−0.294 (0.465)	0.190*** (0.051)	0.816 (0.542)
Control pre-treatment mean	8.668	3.362	8.627	31.319
Observations	2,145	2,145	2,144	2,146
Constituency panels	556	556	556	556
<i>Panel B. Preferred probability-matched panel</i>				
A-list constituency $\times$ 2010	0.102*** (0.026)	−0.138 (0.474)	0.102*** (0.028)	0.077 (0.561)
Control pre-treatment mean	8.996	3.670	8.964	38.590
Observations	2,063	2,063	2,062	2,064
Constituency panels	518	518	518	518
Constituency and election FE	Yes	Yes	Yes	Yes
Post-treatment election	2010	2010	2010	2010

Notes: Each column reports the coefficient on A-list $\times$ 2010 from a separate constituency-panel fixed-effects regression using Conservative observations in 1997, 2001, 2005, and 2010. Columns 1–3 use $\log(1 + y)$ spending outcomes; column 4 uses Conservative vote share in percentage points. Party spending is total spending net of candidate personal expenses. Panel B applies ATT Epanechnikov kernel weights from a logit propensity score based on Conservative vote share and log electorate in 1997, 2001, and 2005 and mean campaign intensity in 2001 and 2005 (bandwidth 0.06; 42 treated panels on common support). Standard errors clustered by constituency are in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

In the full panel, A-list placement increases total spending by 0.175 log points (s.e. 0.047) and party spending by 0.190 log points (s.e. 0.051), both significant at the one-percent level. The descriptive exponential-scale translations are 19.1% and 20.9%, respectively. Personal spending is estimated to fall by 0.294 log points but is imprecise. The vote-share estimate is 0.816 percentage points (s.e. 0.542), so the data do not support a precisely estimated short-run vote effect.

The preferred match attenuates the spending estimates but preserves the composition of the response. Total spending rises by 0.102 log points (s.e. 0.026; a descriptive exponential-scale translation of 10.8%) and party spending by 0.102 log points (s.e. 0.028; 10.7%). Personal spending is again imprecisely estimated. The matched vote-share coefficient is 0.077 percentage points (s.e. 0.561), with an approximate 95% confidence interval from −1.02 to 1.18 percentage points. The evidence therefore supports no precisely estimated average short-run vote-share

change; it does not establish a literal zero effect.

The near-equality of the total- and party-spending coefficients indicates that virtually the entire increase in the reported budget comes from resources other than the candidate's own spending. The reform did not simply select candidates who personally financed more expensive campaigns; organizational resources were mobilized around centrally promoted candidates. The reduced form does not reveal whether those resources compensated for a negative direct candidate effect, reflected higher organizational targets for selected candidates, or combined both forces.

Figure 3 displays the preferred matched event study. The pre-treatment coefficients for total spending are 0.012 in 1997 and -0.002 in 2001 (joint $p = 0.867$); for party spending they are 0.020 and 0.012 (joint $p = 0.926$).

After the reform, the resource outcomes separate: the 2010 coefficients are 0.107 for total spending and 0.114 for party spending, and by 2015 they reach 0.380 and 0.401. Personal spending remains imprecise, while the 2015 vote-share point estimate is positive but not precisely estimated. The larger 2015 coefficients should not be read as the isolated effect of the original nomination decision in an unchanged environment. By 2015 some candidates had become incumbents, careers had evolved, and the party had made further strategic choices. The main table therefore treats only 2010 as post-treatment. Appendix Table OB3 reports a pooled 2010–2015 specification.

4.2 Robustness and design checks

The preferred match keeps the largest matching-variable standardized difference close to 10% and reproduces small pre-reform spending differences. Appendix Table OB2 compares the preferred specification with two alternative matching designs. An electoral-history-only match leaves materially greater covariate imbalance, although its joint pre-trend tests do not reject. Adding pre-reform campaign intensity and using either a 0.03 or 0.06 bandwidth produces similar positive 2010 total- and party-spending estimates.

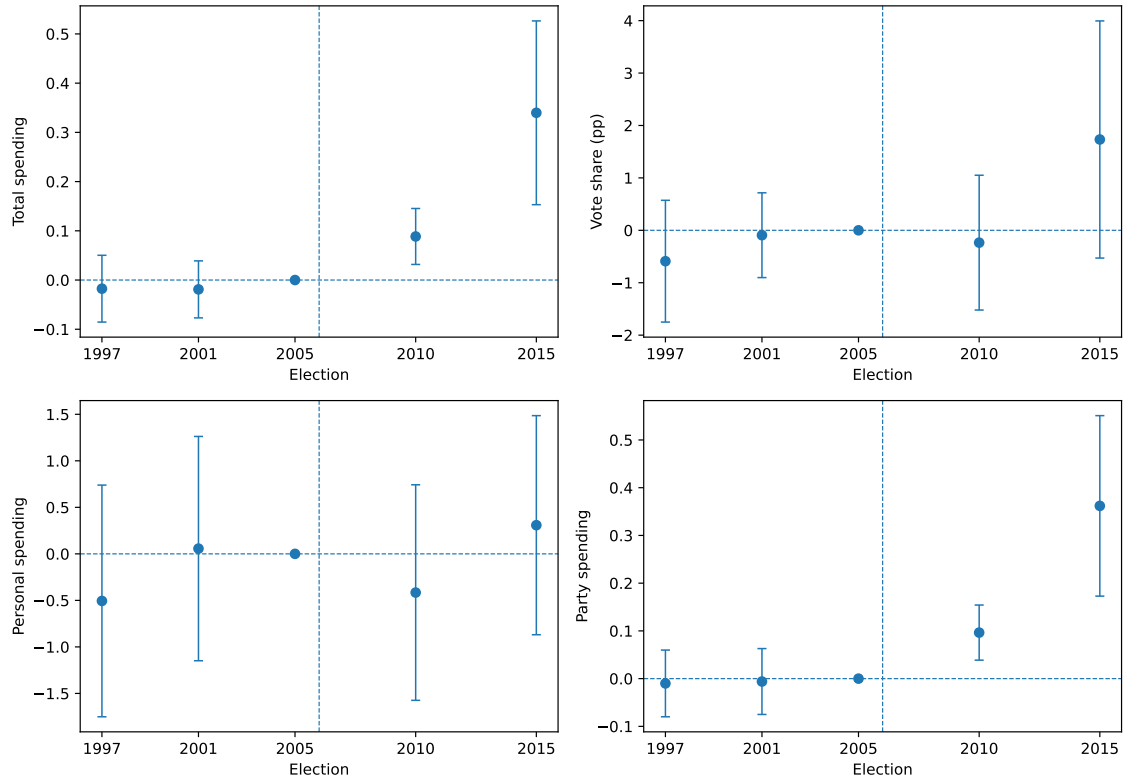


Figure 3: Preferred probability-matched event study

Notes: Dynamic difference-in-differences estimates for total spending, vote share, personal spending, and party spending. Matching uses Conservative vote share and log electorate in 1997, 2001, and 2005 and mean campaign intensity in 2001 and 2005, with an Epanechnikov kernel bandwidth of 0.06. The omitted election is 2005. Bars are 95% confidence intervals based on standard errors clustered by constituency panel.

Because A-list candidates were concentrated in electorally competitive or strategically prioritized seats, Appendix OF reports two additional exercises. First, the analysis is restricted to constituencies within narrow bands of the 2005 Conservative margin. Within these bands, the matched total- and party-spending estimates remain positive. Second, the baseline model is augmented with the 2005 margin interacted with election indicators, allowing constituencies with different pre-reform competitiveness to follow different paths. The matched estimates are essentially unchanged. These results show robustness to flexible controls for the observed relationship between pre-reform competitiveness and subsequent outcomes; they do not prove orthogonality to every unobserved post-reform seat-specific shock.

Appendix Table OC1 assigns the Conservative A-list indicator to Labour, Liberal Democrat,

and UKIP spending outcomes. These are equilibrium-response checks rather than pure placebos: rival parties could genuinely alter spending in response to a centrally promoted Conservative candidate. The estimates do not show a paired rise in both total and party spending resembling the Conservative result. A significant decline in Liberal Democrat personal spending is therefore reported as an observed rival-party response, not dismissed as impossible under the reform.

Appendix Figure OC1 progressively excludes constituencies with the largest changes in registered electorate. The total- and party-spending estimates remain positive across cutoffs, while personal spending and vote share remain imprecisely estimated. Appendix Table OD1 and Figure OD1 restrict the event study to constituencies with a new Conservative candidate in 2010 and reproduce the post-2005 rise in total and party spending. Because newcomer status is downstream of selection, this is mechanism evidence rather than a separate identification strategy.

Across these checks, the evidence consistently shows a Conservative-specific increase in party (non-personal) spending in A-list constituencies and no statistically detectable average short-run vote-share change. All reported inference uses standard errors clustered by constituency panel. The preferred analysis contains 42 treated constituencies.

5 Mechanisms and the price of control

5.1 Was all central spending used to boost female candidates?

Because advancing women’s representation was a central objective of the A-list, Table 3 asks whether the spending response is confined to female candidates. Among candidates in the harmonized 2010 panel, 25 of 42 A-list candidates (59.5%) and 121 of 569 other Conservative candidates (21.3%) were women. We supplemented and verified the gender coding for A-list candidates through a dedicated AI-assisted search. Hill (2013) reports that A-listers and non-A-listers were similar on several education and class markers. Our model permits separate female and male A-list effects and nets out the contemporaneous female–male difference among other Conservative candidates.

Table 3: A-list spending effects by candidate gender

	Total spending	Personal spending	Party spending	Vote share (percentage points)
<i>Panel A. Raw constituency panel</i>				
Female A-list	0.315** (0.141)	−0.345 (0.647)	0.353** (0.145)	1.115 (0.836)
Male A-list	0.143*** (0.042)	−0.255 (0.714)	0.150*** (0.047)	1.026 (0.667)
<i>p</i> -value: female = male	0.244	0.926	0.185	0.933
<i>Panel B. Preferred probability-matched panel</i>				
Female A-list	0.108** (0.055)	−0.133 (0.706)	0.115** (0.056)	0.185 (0.933)
Male A-list	0.110*** (0.026)	−0.110 (0.721)	0.105*** (0.027)	0.370 (0.684)
<i>p</i> -value: female = male	0.978	0.982	0.875	0.873

Notes: Each panel reports one fixed-effects model using 1997–2010 Conservative observations, with separate A-list interactions for female and male 2010 candidates and constituency-panel and election fixed effects. Columns 1–3 use $\log(1 + y)$; column 4 uses vote share in percentage points. Panel B applies the preferred ATT kernel weights. Standard errors clustered by constituency panel are in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

In the full panel, the total-spending estimates are 0.315 for female A-list candidates and 0.143 for male A-list candidates; party-spending estimates are 0.353 and 0.150. The female point estimates are larger, but equality cannot be rejected. Under the preferred match the gender estimates converge: total spending rises by 0.108 for women and 0.110 for men, and party spending by 0.115 and 0.105. Central sponsorship was therefore associated with greater organizational spending for candidates of both genders. This does not diminish the reform’s representational objective; it shows that the estimated budgetary response was not confined to women. Appendix Table OD3 reports the corresponding descriptive comparison by documented minority background. Documented minority background was identified for 4 of 53 A-list nominees and 42 of 578 other Conservative candidates. The spending estimates do not differ significantly between documented-minority and other A-list candidates, although the documented-minority A-list subgroup is small.

5.2 Strategic safety and assignment

A-list nominees were disproportionately placed in constituencies with realistic electoral prospects. In the model, assignment depends jointly on district difficulty, voter appeal, and leadership value. A leadership-preferred candidate can be placed in a valuable or winnable district even when lower local embeddedness raises the resources needed to reach the target. The observed increase in party spending is consistent with such a compensating organizational response, but the assignment pattern alone does not identify the mechanism.

We also examine whether A-list seats were differentially exposed to post-2010 austerity. Using the constituency exposure index of Fetzer (2019), the estimated mean difference is negative but statistically imprecise (Appendix Table OD2). We therefore do not detect a difference in mean austerity exposure, but the estimate is too imprecise to rule out substantively relevant differences.

5.3 Candidate continuation and medium-run recovery

Figure 4 examines candidate continuation, medium-run vote dynamics, and total-spending dynamics. Panel (a) documents the turnover generated by the A-list reform. Same-seat Conservative candidate continuation falls sharply in 2010 and is not precisely different from the weighted comparison group by 2015. In levels, 30 of 42 A-list candidates (71.4%) and 63.5% of the weighted comparison group contested the same constituency again in 2015. The evidence is therefore directionally consistent with greater later continuation, but it does not support a precisely estimated effect on re-running.

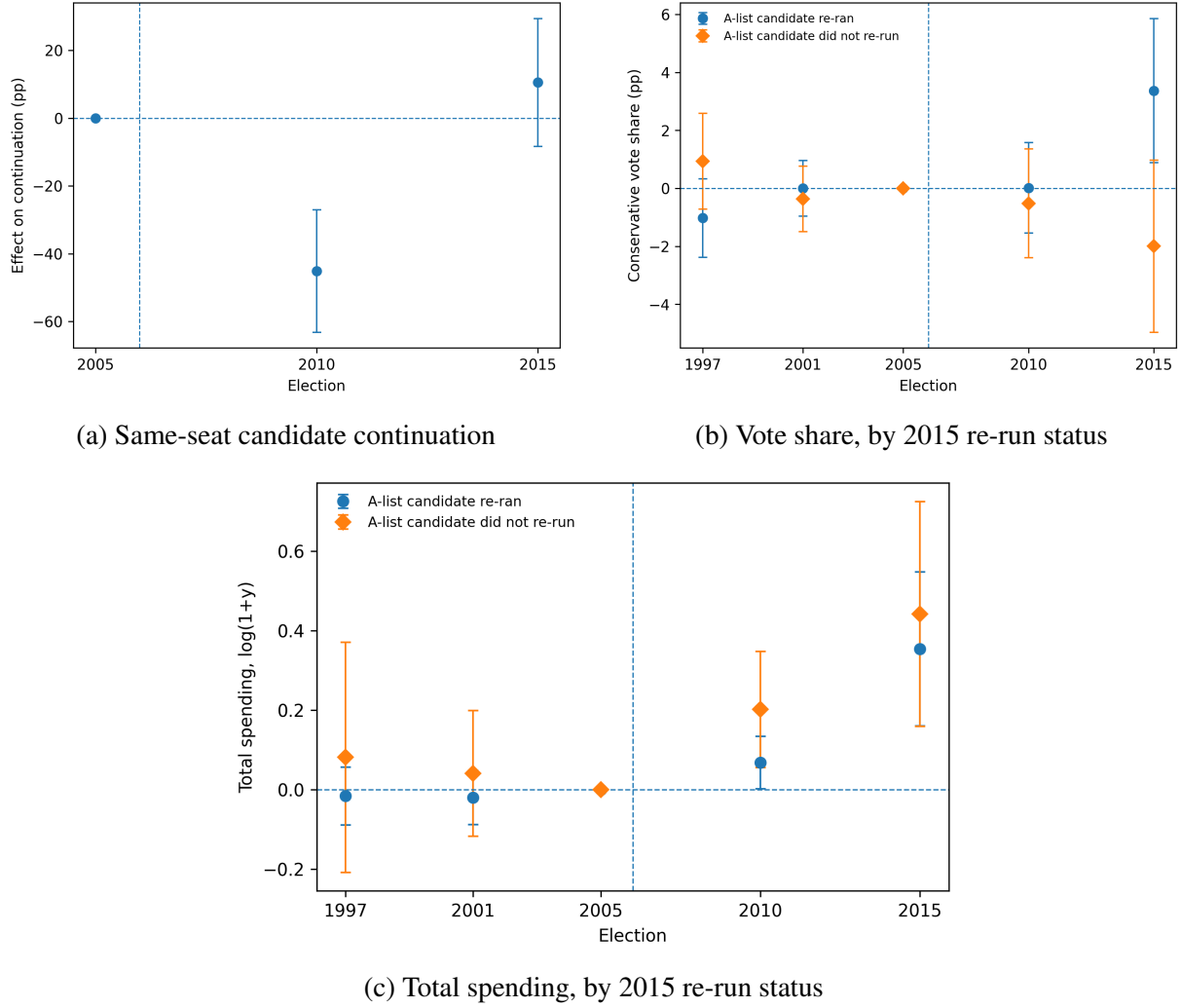


Figure 4: Candidate continuation, medium-run vote-share recovery, and total spending after A-list placement

Notes: Panel (a) reports dynamic estimates for an indicator equal to one when the Conservative candidate also contested the same constituency at the preceding general election. Panels (b)–(c) report event-study estimates for A-list constituencies split by whether the 2010 A-list candidate contested the same seat again in 2015. Panel (b) uses Conservative vote share in percentage points; Panel (c) uses total spending, measured as $\log(1+y)$. The subgroup paths in Panels (b)–(c) are estimated jointly. All specifications include constituency-panel and election fixed effects, use the preferred pre-reform matching weights, and cluster standard errors by constituency panel. The omitted election is 2005. Because 2015 continuation is post-treatment, Panels (b)–(c) show descriptive heterogeneity and do not identify the causal effect of incumbency.

Panel (b) splits treated constituencies according to whether the 2010 A-list candidate contested the same seat again in 2015. The two groups look similar in 2010: the eventual re-runner coefficient is 0.02 percentage points, while the one-off candidate coefficient is -0.52 percentage points. By 2015, however, the paths diverge. The vote-share coefficient is 3.37 percentage

points among re-runners and -1.99 percentage points among one-off candidates; the difference is 5.37 percentage points. This pattern is consistent with recognition, constituency service, organizational presence, and incumbent survival improving effective candidate appeal over time.

This comparison conditions on a post-treatment outcome. In the matched sample, 30 of 42 A-list candidates re-ran in 2015, while 12 did not. One of the 30 re-runners lacks an observed 2010 Conservative result. All 29 re-runners with an observed 2010 result won in 2010, whereas two of the 12 one-off candidates won. The split therefore closely tracks incumbent survival and may also reflect victory, underlying candidate appeal, seat-specific shocks, and subsequent party decisions. Our causal statements remain restricted to the intent-to-treat effect of A-list placement.

5.4 Downstream support for UKIP and Leave

The reform shifted influence over candidate choice from local associations toward the party center. We therefore examine whether treated constituencies later exhibited greater support for UKIP or Leave, two outcomes associated with opposition to centralized political control. These exploratory outcomes are downstream of the 2010 intervention and do not by themselves identify a voter-response mechanism. Appendix Table OC2 reports the results. In the preferred matched UKIP event study, the coefficients are close to zero in both 2001 and 2010. The 2015 estimate is positive at 1.275 percentage points, while the estimated effect on whether UKIP fielded a candidate is 2.2 percentage points. Neither estimate is statistically distinguishable from zero. A Great Britain specification with country-by-election fixed effects produces the same 2015 vote-share estimate. The preferred matched Leave-vote estimate is 2.426 percentage points. The estimate falls to 1.802 points after adjusting for the pre-reform matching covariates and is 2.417 points when the uniform model-based Leave measure is used. All three estimates are positive but imprecisely estimated. The UKIP and Leave results are therefore directionally consistent with a delayed protest response, but they do not establish that voters connected the A-list reform to either outcome.

5.5 Conditional calibration of the price of control

The preceding results establish that A-list constituencies received more party resources without a precisely estimated average short-run vote gain. They do not establish whether the additional spending offset weaker candidate appeal or reflected greater organizational priority. We therefore use the model to ask what combinations of a candidate-specific vote disadvantage and a vote–spending elasticity are consistent with the reduced-form spending and vote-share estimates. The exercise is a conditional calibration, not a structural estimate.

Taking logs of (2), the change associated with A-list placement can be written

$$\Delta \ln s = \alpha \Delta \ln K + \chi, \quad \chi \equiv \gamma \Delta h + \psi \Delta L. \quad (8)$$

The spending response $\Delta \ln K$ and vote-share response are estimated reduced-form objects. The direct candidate channel χ is not separately identified and must be assumed. Equation (8) therefore traces combinations of (α, χ) that are consistent with the reduced forms; it does not recover a unique vote–spending elasticity. The economically relevant compensating–spending cost is $-\chi/\alpha$, not α by itself.

We anchor the exercise on the preferred matched estimates. Party spending rises by 0.102 in the $\log(1+y)$ specification, which corresponds descriptively to 10.7%. Because zero party–spending returns account for fewer than one percent of observations, we treat 0.102 as an approximate log change for this calibration while retaining the caveat that it is not an exact elasticity. The matched vote-share effect is 0.077 percentage points. Given the matched control pre-treatment Conservative vote share of 38.59 points,

$$\Delta \ln s \approx \frac{0.077}{38.59} \approx 0.0020. \quad (9)$$

Let δ denote an assumed candidate-only vote-share change in percentage points. Using

$\chi \approx \delta/\bar{y}$, where $\bar{y} = 38.59$, equation (8) implies

$$\alpha(\delta) \approx \frac{\Delta \ln s - \delta/\bar{y}}{\Delta \ln K}. \quad (10)$$

No candidate disadvantage ($\delta = 0$) implies an elasticity near 0.02. A half-percentage-point disadvantage ($\delta = -0.5$) implies $\alpha \approx 0.15$, a one-point disadvantage implies $\alpha \approx 0.27$, and a two-point disadvantage implies $\alpha \approx 0.53$. These are conditional calibrations, not point-identified structural estimates.

The spending increase required to offset a candidate deficit while holding vote share constant is

$$\Delta \ln K^* = -\frac{\chi}{\alpha} \approx -\frac{\delta}{\alpha \bar{y}}. \quad (11)$$

For a one-percentage-point deficit, the required increase is approximately 0.259 log points, or 30%, when $\alpha = 0.10$; 0.130 log points, or 14%, when $\alpha = 0.20$; and 0.104 log points, or 11%, when $\alpha = 0.25$. The corresponding range is therefore approximately 11–30%. At an elasticity near 0.25, the implied 11% compensating increase is close to the preferred matched spending estimate. This numerical similarity is an internal-consistency comparison, not independent validation, because the observed spending response is itself an input to the calibration.

6 Discussion and Conclusion

Candidate selection is not only a decision about who appears on the ballot; it can also reshape how parties allocate scarce organizational resources. We provide evidence of this connection and the resource cost associated with greater central influence over party nominations in a major democracy. The British Conservative Party’s A-list reform increased party (non-personal) campaign spending in treated constituencies by approximately 11% in our preferred matched DID specification and 21% in the full panel, while we detect no statistically significant change in candidates’ personal outlays, nor short-run vote shares. This finding implies that seizing control

is feasible, but comes at a price. The result survives a demanding probability match on electoral history, constituency size, and prior campaign intensity.

Our calibration gives a conditional interpretation of the short-run reduced form results. Assumed candidate-channel disadvantages of one-half to one percentage point imply vote-spending elasticities of approximately 0.15–0.27. At the matched pre-treatment vote share, offsetting a one-percentage-point deficit requires approximately 11–30% more spending for elasticities between 0.10 and 0.25. These quantities are conditional on assumed candidate penalties and should not be read as a point-identified structural estimate, yet they demonstrate what additional lessons can be learned when empirical estimates are combined with a calibration of a formal model: taking back control over nominations has a price, and we can put a number on it.

The findings add a budget constraint to the study of party organization and specify the monetary costs of changing candidate pools by centralizing control. Candidate-selection research has mapped who controls nominations and how selection affects representation, ideology, and the balance of power among leaders, activists, and members (Gallagher and Marsh 1988; Katz and Mair 1995; Cross and Katz 2013). Our evidence suggests that leadership intervention can also change constituency-level resource allocation and that the leadership must spend funds to offset its intervention.

The broader contribution beyond the specific case is to recast candidate selection as a dual organizational problem. Parties choose not only who contests a constituency but also how campaign resources are distributed. When leaders use greater influence over nominations to pursue attributes they value—including loyalty, ideological fit, or descriptive representation—party finance can serve as a second instrument for sustaining constituency competitiveness. Centralization consequently has an organizational shadow price. Its reach depends not only on formal nomination rules, but also on the party center’s capacity to direct scarce resources. Because resources committed to one constituency cannot be deployed elsewhere, nomination control generates opportunity costs across the electoral map. Candidate selection, party organization, and campaign finance are therefore intertwined: effective control over nominations is also dependent

on resource capacity.

Two separate full-candidate AI searches, their adjudicated combination, and a blinded human sample all point to much lower local embeddedness among A-list nominees. This comparison does not by itself establish that weaker local embeddedness caused lower electoral appeal or additional party spending, rather than the loss of local control itself or other characteristics of centrally promoted candidates. However, existing evidence shows that, in hypothetical settings that abstract from resource allocation, voters are more likely to choose local than non-local candidates in both the United Kingdom (Campbell et al. 2019) and the United States (Hunt and Fontana 2025). We also find no evidence that the additional spending was confined to female or documented-minority A-list candidates.

Our empirical application focuses on one party and one major reform that centralized candidate selection. However, the argument travels beyond Britain under three identifiable conditions: national and local party actors value different candidate attributes; voters respond to candidate-level or local characteristics; and the party center can reallocate campaign resources across constituencies. These conditions are present in many electoral systems in which voters choose their representatives directly and party leaders have incentives to control candidate selection and discretion over finances. These scope conditions yield a broader implication: centralization is not equally available to all parties. Resource-poor party centers may have to delegate more authority to local organizations or accept a larger electoral cost from intervening in candidate selection. Whether resource-poor parties centralize less is a natural question for future research.

Data Availability Statement

Replication materials for the quantitative analyses and candidate-coding exercises will be made available in the APSR Dataverse. The deposit will include the analysis code, prompts, model outputs, source links, adjudication records, and blinded human-validation coding used in the localness analysis. Third-party materials that cannot legally be redistributed will be documented

with their provenance and access instructions.

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Online Appendix

OA Proofs

Lemma 1 (Minimum resources) *Given (2), the minimum campaign resources needed for candidate c to reach target vote share v_i in constituency i are*

$$K_i^*(c) = \left(\frac{v_i}{A_i Q_{ic}} \right)^{1/\alpha} = \left(\frac{\tau_i}{Q_{ic}} \right)^{1/\alpha}, \quad \tau_i \equiv \frac{v_i}{A_i}.$$

Proof. Because $\alpha > 0$, vote share is strictly increasing in K_i . The least-cost allocation therefore reaches the target at equality. Solving $v_i = A_i K_i^\alpha Q_{ic}$ for K_i gives the result. ■

Lemma 2 (Comparative statics) *For $\alpha > 0$,*

$$\frac{\partial \ln K_i^*}{\partial \ln \tau_i} = \frac{1}{\alpha} > 0, \quad \frac{\partial \ln K_i^*}{\partial \ln Q_{ic}} = -\frac{1}{\alpha} < 0.$$

Proof. Differentiate $\ln K_i^* = (1/\alpha)(\ln \tau_i - \ln Q_{ic})$. ■

Lemma 3 (Pairwise assignment) *Let $\tau_i \geq \tau_j > 0$ and $Q^{hi} \geq Q^{lo} > 0$. Then*

$$\left(\frac{\tau_i}{Q^{hi}} \right)^{1/\alpha} + \left(\frac{\tau_j}{Q^{lo}} \right)^{1/\alpha} \leq \left(\frac{\tau_i}{Q^{lo}} \right)^{1/\alpha} + \left(\frac{\tau_j}{Q^{hi}} \right)^{1/\alpha}.$$

Proof. Let $\phi(\tau, Q) = \tau^{1/\alpha} Q^{-1/\alpha}$. The left-hand side minus the right-hand side equals

$$\left(\tau_i^{1/\alpha} - \tau_j^{1/\alpha} \right) \left((Q^{hi})^{-1/\alpha} - (Q^{lo})^{-1/\alpha} \right) \leq 0,$$

because the first factor is nonnegative and the second nonpositive. ■

Proof of Proposition 1. Part (1) follows from Lemma 2; Part (2) follows from Lemma 3 and repeated pairwise exchanges. For Part (3), the local candidate has voter appeal $Q_L = \exp(\gamma h_L +$

ψ) and the leadership-preferred outsider has $Q_A = \exp(\gamma h_A)$. Applying Lemma 1,

$$\frac{K_A^*}{K_L^*} = \left(\frac{Q_L}{Q_A} \right)^{1/\alpha} = \exp\left(\frac{\psi - \gamma(h_A - h_L)}{\alpha} \right).$$

The leadership may nevertheless choose the outsider because the objective in (3) also assigns value λ_{z_c} to leadership-valued candidate attributes. ■

OB Preferred Matching and Extended Estimates

The preferred probability match is estimated once at the constituency-panel level and the resulting weight is applied to every election observation in that panel. The logit propensity score contains Conservative vote share and log electorate in 1997, 2001, and 2005, together with average total spending as a percentage of the legal limit in 2001 and 2005. The first two sets of variables capture the level and evolution of competitiveness and constituency scale; average campaign intensity captures persistent local campaign capacity and prior targeting. The procedure uses Epanechnikov average-treatment-on-the-treated kernel weights, bandwidth 0.06, and common support.

The propensity model deliberately excludes 1997 spending, leaving it as a held-out *outcome* diagnostic. Because 1997 vote share and electorate are included, this is not a wholly out-of-sample exercise. All 42 treated panels with complete covariates remain on support; 476 control panels receive positive weight. The effective control sample size is 309.6, and the largest control weight is 0.179.

Table OB1: Covariate balance under the preferred probability match

Covariate	Treated mean	Full control mean	Matched control mean	Full SMD	Matched SMD
Conservative vote share, 1997	37.514	31.043	37.373	66.4	1.4
Conservative vote share, 2001	39.192	31.915	38.688	68.2	4.7
Conservative vote share, 2005	40.316	32.129	39.710	71.6	5.3
Log electorate, 1997	11.162	11.119	11.154	43.2	8.0
Log electorate, 2001	11.172	11.122	11.163	44.6	8.0
Log electorate, 2005	11.183	11.128	11.171	47.1	10.2
Mean campaign intensity, 2001–2005	85.164	67.316	84.280	85.5	4.2

Notes: SMD is the absolute standardized mean difference in percent, using the pooled standard deviation. Mean campaign intensity is total candidate spending as a percentage of the legal limit, averaged over 2001 and 2005. The match uses ATT Epanechnikov kernel weights with bandwidth 0.06 and common support.

Table OB2: Sensitivity to the matching specification

Matching design	Positive-weight controls	Max. SMD (%)	Total spending 2010	Party spending 2010	Joint pre-trend <i>p</i> -value	
					Total	Party
Electoral history only, bandwidth 0.06	475	19.0	0.076	0.082	0.453	0.413
Campaign intensity added, bandwidth 0.03	476	7.1	0.110	0.116	0.793	0.832
Preferred: campaign intensity added, bandwidth 0.06	476	10.2	0.107	0.114	0.867	0.926

Notes: All specifications use Epanechnikov average-treatment-on-the-treated kernel weights and retain 42 treated constituencies on common support. The 2010 columns report event-study coefficients relative to 2005. The final two columns report joint *p*-values for the 1997 and 2001 treatment interactions. SMD is the largest absolute standardized mean difference across the matching variables.

The principal analysis treats 2010 as the only post-treatment election. Table OB3 instead codes both 2010 and 2015 as post-treatment. The pooled coefficients combine the initial reform response with later incumbency, career, and party-strategy choices, so they are descriptive of the full post-reform period rather than a clean estimate of the initial intervention.

Table OB3: Medium-run effects of A-list placement: pooled difference-in-differences estimates

	Total spending	Personal spending	Party spending	Vote share (percentage points)
<i>Panel A. full constituency panel</i>				
A-list constituency $\times$ Post	0.415*** (0.059)	−0.016 (0.402)	0.431*** (0.061)	1.975*** (0.705)
Observations	2,836	2,836	2,833	2,837
Constituency panels	624	624	623	624
<i>Panel B. Preferred probability-matched panel</i>				
A-list constituency $\times$ Post	0.242*** (0.051)	0.134 (0.414)	0.248*** (0.052)	1.054 (0.743)
Observations	2,580	2,580	2,579	2,581
Constituency panels	518	518	518	518
Constituency and election FE	Yes	Yes	Yes	Yes
Post-treatment elections	2010, 2015	2010, 2015	2010, 2015	2010, 2015

Notes: The interaction captures the average differential change across the 2010 and 2015 elections. Spending outcomes use $\log(1 + y)$; vote share is in percentage points. Standard errors clustered by constituency panel are in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

OC Outcome Robustness

Table OC1 applies the Conservative A-list constituency indicator to rival-party outcomes. These estimates are best understood as equilibrium-response checks: Labour, Liberal Democrat, or UKIP campaigns could respond to an A-list Conservative candidacy. The table does not reproduce the paired increase in total and party spending observed for Conservatives.

Table OC1: Rival-party campaign responses in A-list constituencies

	Total spending	Personal spending	Party spending
Labour	−0.449 (0.311)	−0.048 (0.395)	−0.359 (0.302)
Liberal Democrats	0.080 (0.202)	−1.020*** (0.323)	0.092 (0.202)
UKIP	0.018 (0.443)	−0.069 (0.439)	0.221 (0.436)

Notes: Each row reports separate difference-in-differences regressions over 1997, 2001, 2005, and 2010, with constituency-panel and election fixed effects. The treatment indicator marks constituencies with a Conservative A-list candidate in 2010. Spending outcomes use $\log(1 + y)$. Standard errors clustered by constituency panel are in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table OC2: Downstream UKIP and Leave-vote outcomes

	full England vote share	Matched England vote share	Matched Great Britain vote share	Matched England UKIP entry
<i>Panel A. UKIP event-study estimates, 2005 omitted</i>				
A-list constituency $\times$ 2001	−0.038 (0.260)	−0.086 (0.264)	−0.086 (0.264)	4.514 (7.597)
A-list constituency $\times$ 2010	0.058 (0.312)	−0.037 (0.315)	−0.037 (0.315)	6.416 (6.561)
A-list constituency $\times$ 2015	0.527 (0.847)	1.275 (0.866)	1.275 (0.866)	2.156 (5.281)
Observations	1,970	1,820	2,072	1,820
Constituency panels	523	455	518	455
ATT weights	No	Yes	Yes	Yes
Country $\times$ election fixed effects	No	No	Yes	No
<i>Panel B. 2016 Leave-vote comparisons</i>				
	full preferred estimate	Matched preferred estimate	Matched plus pre-reform controls	Matched uniform model-based estimate
A-list constituency	1.998 (1.755)	2.426 (1.807)	1.802 (1.449)	2.417 (1.822)
Observations	523	455	455	455
ATT weights	No	Yes	Yes	Yes
Pre-reform controls	No	No	Yes	No

Notes: Panel A uses constituency and election fixed effects and clusters standard errors by constituency panel. UKIP vote share is measured in percentage points and is set to zero when UKIP did not field a candidate. The entry outcome in column 4 is multiplied by 100, so its coefficients are also percentage-point effects. The Great Britain model absorbs country-by-election fixed effects. Panel B reports cross-sectional 2016 comparisons for English constituencies with heteroskedasticity-robust standard errors. The preferred House of Commons Library estimate uses a directly observed constituency result where available and a model-based estimate otherwise; the final column applies the same model-based measure to every constituency. The adjusted model includes Conservative vote share and log electorate in 1997, 2001, and 2005 and mean campaign intensity in 2001–2005. Matching uses the preferred pre-reform ATT weights.

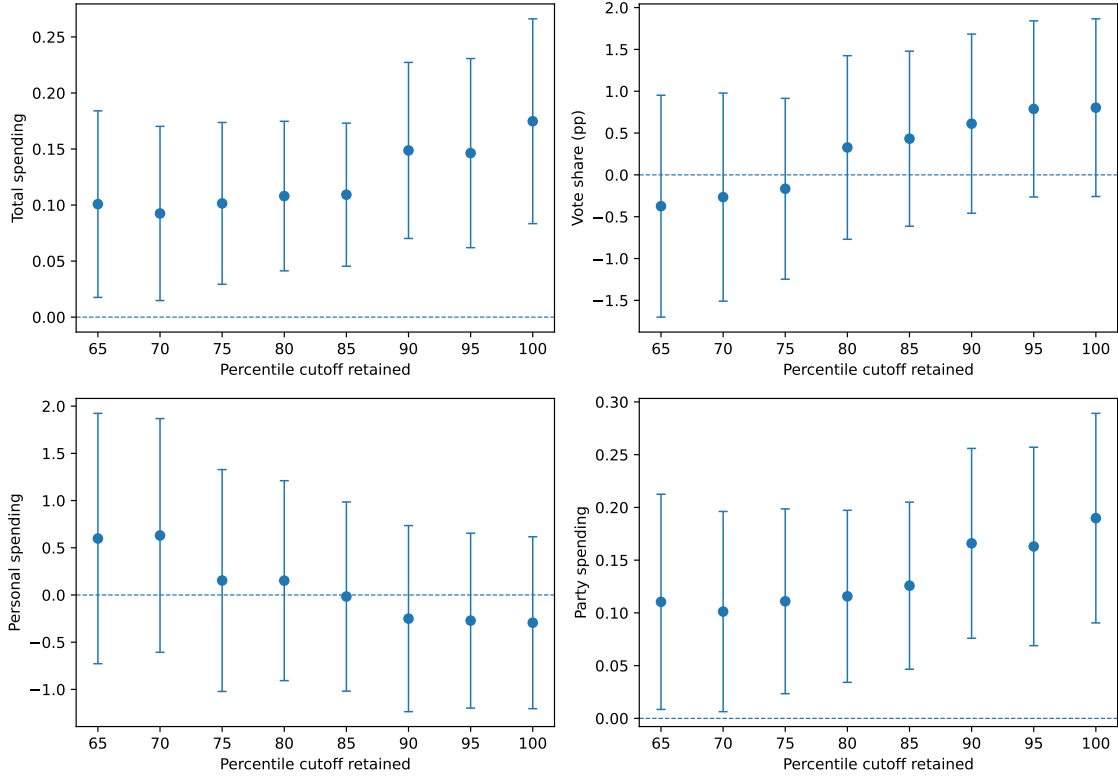


Figure OC1: Sensitivity to large changes in registered electorate

Notes: Each point re-estimates the raw 2010 fixed-effects specification after retaining constituencies below the indicated percentile of the maximum absolute election-to-election change in registered electorate. Spending outcomes use $\log(1 + y)$; vote share is in percentage points. Bars are 95% confidence intervals from standard errors clustered by constituency panel.

Table OC3 compares the baseline $\log(1 + y)$ outcome with the pure logarithm and inverse hyperbolic sine. Total and party spending are zero in fewer than one percent of returns, and the matched estimates are nearly invariant across transformations. Personal spending is zero much more often, so pure-log specifications use a substantially smaller sample.

Table OC3: Sensitivity of the 2010 estimates to the spending transformation

	log(1 + y) baseline	log(y) zeros dropped	asinh(y) zeros retained
<i>Panel A. Raw constituency panel</i>			
Total spending	0.175*** (0.047)	0.101*** (0.031)	0.181*** (0.049)
<i>N</i>	2,145	2,139	2,145
Party spending	0.190*** (0.051)	0.107*** (0.033)	0.197*** (0.053)
<i>N</i>	2,144	2,136	2,144
Personal spending	−0.294 (0.465)	−0.332 (0.231)	−0.315 (0.523)
<i>N</i>	2,145	1,097	2,145
<i>Panel B. Preferred probability-matched panel</i>			
Total spending	0.102*** (0.026)	0.092*** (0.026)	0.103*** (0.026)
<i>N</i>	2,063	2,057	2,063
Party spending	0.102*** (0.028)	0.097*** (0.027)	0.102*** (0.028)
<i>N</i>	2,062	2,055	2,062
Personal spending	−0.138 (0.474)	−0.171 (0.234)	−0.143 (0.534)
<i>N</i>	2,063	1,057	2,063

Notes: Each cell reports the A-list $\times$ 2010 coefficient from the baseline fixed-effects model under the indicated outcome transformation. Panel B applies the preferred ATT kernel weights. Standard errors clustered by constituency panel are in parentheses. *** $p < 0.01$.

OD Candidate-Level Evidence

Table OD1 and Figure OD1 restrict the event study to constituencies whose 2010 Conservative candidate did not contest the same constituency panel in 2005. In the matched specification, the total- and party-spending estimates remain positive after 2005, and the joint pre-treatment tests are not rejected.

Table OD1: Event-study estimates among 2010 newcomer constituencies

Estimator	Total spending		Party spending		Pre-trend p	
	2010	2015	2010	2015	Total	Party
Raw	0.128*	0.759***	0.161**	0.770***	0.004	0.017
	(0.074)	(0.126)	(0.077)	(0.126)		
Probability matched	0.091**	0.440***	0.096**	0.450***	0.997	0.900
	(0.040)	(0.127)	(0.039)	(0.128)		

Notes: Coefficients are event-study estimates relative to 2005. The final columns report the joint p -value for the 1997 and 2001 interactions. Spending outcomes use $\log(1+y)$. All models include constituency-panel and election fixed effects. Standard errors clustered by constituency panel are in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

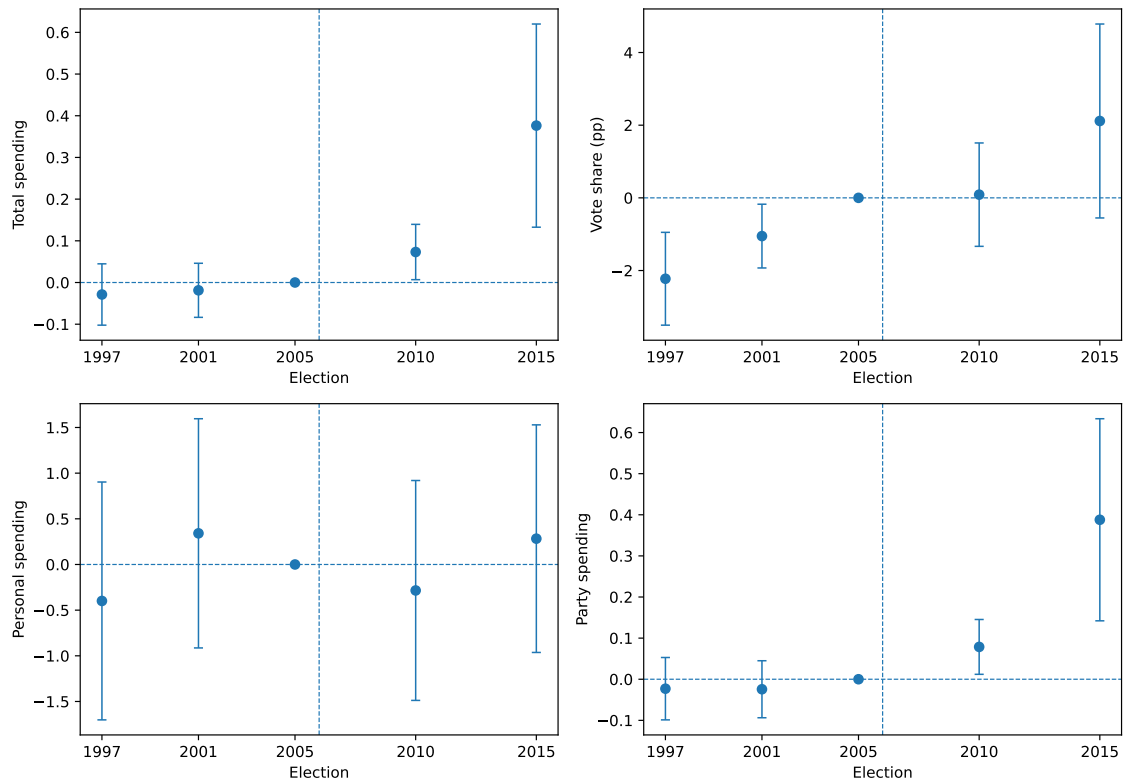


Figure OD1: Preferred matched event study among 2010 newcomer constituencies

Notes: The omitted election is 2005. Bars are 95% confidence intervals. Each panel has its own vertical scale.

Table OD2: Association between A-list constituencies and austerity exposure

	Austerity-exposure index
A-list constituency	−20.802 (16.558)
Observations	570
R^2	0.0028

Notes: OLS regression of constituency exposure to austerity cuts on an indicator for a 2010 A-list candidate. Data on austerity exposure are from Fetzer (2019). Standard error in parentheses.

Table OD3: A-list spending effects by documented minority background

	Total spending	Personal spending	Party spending	Vote share (percentage points)
<i>Panel A. Raw constituency panel</i>				
Documented-minority A-list	0.199 (0.227)	0.859 (1.083)	0.213 (0.230)	1.917 (2.720)
Other A-list	0.173*** (0.047)	−0.384 (0.489)	0.188*** (0.051)	0.721 (0.543)
<i>p</i> -value: documented minority = other	0.909	0.296	0.913	0.666
<i>Panel B. Preferred probability-matched panel</i>				
Documented-minority A-list	0.043 (0.096)	1.426 (1.098)	0.055 (0.104)	1.265 (2.867)
Other A-list	0.105*** (0.027)	−0.258 (0.499)	0.104*** (0.029)	0.039 (0.563)
<i>p</i> -value: documented minority = other	0.532	0.163	0.650	0.675

Notes: Minority background is coded from explicit public evidence collected by AI. The 2010 audit identified minority backgrounds for 4 of 53 A-list nominees and 42 of 578 other Conservative candidates. Three of these A-list constituencies enter the estimation panel. “Other” means that minority background was not documented; it does not mean White. The model estimates separate A-list effects by minority status, adjusting for differences by minority status among non-A-list candidates in each election. Standard errors clustered by constituency panel are in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

The documented-minority subgroup is small, and none of the equality tests rejects a common spending effect. The appendix result therefore provides no evidence that the increase in organizational spending was concentrated among documented-minority A-list candidates.

OE Dual-Model AI-Assisted Candidate Localness Coding and Independent Validation

This appendix documents the localness and parachuting exercise used in Table 1. The candidate universe consists of all 631 official Conservative candidates in the 2010 general election. Two complete candidate-level searches were conducted separately in August 2026, one using Claude Opus 5 and one using OpenAI GPT-5.6 Sol Pro through ChatGPT. Both models received the same prespecified coding protocol and were instructed to check ConservativeHome for every candidate while continuing to other sources where necessary. For each candidate, each search recorded an evidence summary, source URLs, the timing of the connection, confidence, and a classification. The models were used to locate and summarize public-source evidence and to apply the coding protocol; they were not used to define A-list treatment or estimate the regression models. The prompts, candidate-level outputs, source links, adjudication records, and human-validation materials are included in the replication materials. A connection counts against parachuting only when it predates candidate selection. Moving into a constituency or beginning local activity after selection does not establish a pre-existing tie, and a failed search is not treated as evidence that no tie existed.

We adjudicated the two sets of classifications using common rules rather than mechanically choosing one output. When the classifications agree, the adjudication retains the stronger evidence and combines source links where useful. When one search is resolved and the other unresolved, the resolved classification is accepted only when supported by dated positive evidence or sufficiently informative evidence of absence. Dated positive evidence of a tie normally takes precedence over a generic no-tie claim. Direct-versus-regional conflicts retain the most geographically specific supported connection, while balanced conflicts remain unresolved.

Table OE1: Coverage, final classifications, and agreement between model searches

Coding outcome	A-list	Other	All
<i>Panel A. Final adjudicated coding</i>			
Official 2010 Conservative candidates	53	578	631
Complete candidate-level searches	53	578	631
Final classification available	50	434	484
Unresolved	3	144	147
Direct pre-selection local tie	13	333	346
Strong regional tie only	15	37	52
Strict parachute: no direct tie	37	101	138
Broad parachute: no direct or regional tie	22	64	86

Notes: A direct tie is a documented pre-selection connection to the constituency, a predecessor constituency, the same local authority, or an immediately adjacent local area. A strong regional tie is a sustained pre-selection connection to the same county, metropolitan area, city-region, or a meaningfully adjacent area among candidates without a direct tie. A strict parachute has no direct tie; a broad parachute has neither a direct nor a strong regional tie.

Table OE2: A-list broad-parachute contrast across the two model searches

Coding source	A-list, resolved	Other, resolved	Difference (pp)	Worst-case difference (pp)
AI search 1	40.4% (21/52)	11.6% (48/414)	28.8	[2.9, 33.2]
AI search 2	46.9% (23/49)	17.7% (74/417)	29.2	[2.7, 38.1]
Final adjudication	44.0% (22/50)	14.7% (64/434)	29.3	[5.5, 36.1]

Notes: The first two rows use the two complete 631-candidate model searches before adjudication. Rates condition on resolved cases. The final column assigns every unresolved A-list candidate first to non-parachute and every unresolved comparison candidate to broad parachute to obtain the lower endpoint, then reverses those assignments for the upper endpoint. These are missingness bounds, not sampling-confidence intervals.

The two searches sometimes differ on individual categories. For each candidate, the adjudication records both initial classifications, the supporting evidence and sources, timing, confidence, the type of disagreement, and the final rationale.

The individual-category disagreement does not translate into disagreement about the aggregate A-list contrast. As Table OE2 shows, both searches separately place the classified-case difference at about 29 percentage points and both yield positive worst-case full-population bounds. Final coverage is 94.3% among A-list candidates and 75.1% among others. Among classified cases, 22 of 50 A-listers and 64 of 434 other candidates are broad parachutes. The full-candidate worst-case difference is bounded between 5.5 and 36.1 percentage points.

OE.1 Independent human validation

After the two model searches were adjudicated, an independent research assistant coded a simple random sample of 100 candidates drawn from all 631 Conservative candidates. The research assistant received candidate names, constituencies, identifiers, countries, and regions, but not the AI classifications or A-list status. The coder used the same substantive definitions and source hierarchy and could leave a case unresolved when the evidence was insufficient. We compared the classifications only after the human coding was complete, and the human coding was not used to revise the full-sample classifications.

Table OE3: Agreement in the independent human-validation sample

Comparison	Agreement
Broad parachute versus any qualifying local or regional tie, among cases resolved by both	56/67 (83.6%)
Direct tie versus no direct tie, among cases resolved by both	57/67 (85.1%)

Notes: Human-validation sample was drawn without replacement. “Resolved by both” excludes candidates classified as unresolved by either the human coder or the final adjudicated classification.

For the distinction used in the substantive analysis, broad parachute versus any qualifying tie, the human coder and the adjudicated classification agree in 56 of the 67 cases resolved by both, or 83.6%. Agreement is similarly high for direct tie versus no direct tie (85.1%).

Table OE4: A-list contrast in the independent human-validation sample

Human-coded comparison	A-list	Other	Difference (pp)
Broad parachute among resolved candidates	42.9% (3/7)	6.4% (5/78)	36.4
Worst-case interval after assigning unresolved cases	37.5–50.0%	5.4–20.7%	[16.8, 44.6]

Notes: The random sample contains eight A-list candidates and 92 other candidates. Rates condition on cases resolved by the human coder. The worst-case intervals assign unresolved cases in the directions least and most favorable to a positive A-list difference. They are missingness bounds, not confidence intervals.

The independent human coding points in the same aggregate direction as both complete model searches. Among human-classified cases, 42.9% of sampled A-list nominees and 6.4%

of other candidates are classified as broad parachutes. The difference remains positive under worst-case assignments of unresolved cases. Because the random sample contains only eight A-list candidates, this is directional corroboration rather than a precise estimate of the A-list difference. The main analysis therefore retains the unresolved category, the full-sample bounds, and a descriptive interpretation.

OE.2 Coding protocol

The same consolidated protocol was applied to every A-list and non-A-list candidate in both model searches.

Objective. Code candidate localness and possible parachute candidacy in the 2010 United Kingdom general election for every Conservative candidate.

Research question. Determine whether each candidate had a meaningful connection to the constituency or surrounding region before being selected as the Conservative parliamentary candidate.

Direct local tie. Code a direct tie when pre-selection evidence documents birth or upbringing, residence, schooling, sustained employment or business activity, local council service, regular local-party or community activity, or previous candidacy or representation in the constituency, a predecessor constituency, the same local authority, or an immediately adjacent local area.

Strong regional tie. Code a strong regional tie when the candidate lacks a direct constituency-level tie but has a substantial and sustained pre-selection connection to the same county, metropolitan area, city-region, or a meaningfully adjacent area. A broadly defined region alone is not sufficient.

Weak tie. Record limited links—for example, attending a university in a broadly defined region, a distant family association, or short-term activity—as weak ties. A weak tie alone does not count as a strong local connection.

Parachute classifications. A strict parachute has no direct local tie when evidence is sufficient. A broad parachute has neither a direct nor a strong regional tie when evidence is sufficient. Cases with insufficient evidence remain unresolved. The absence of a biography or a failed search is not, by itself, evidence of parachuting.

Temporal rule. Only ties that predate candidate selection establish localness. When timing cannot be established, lower confidence and record the uncertainty.

Search procedure and sources. Use Google to research each candidate and explicitly check ConservativeHome, but treat it as one important source among several rather than the only source. Where it contains no relevant information, continue with official election, council, parliamentary, and party records; contemporaneous local newspapers and candidate profiles; archived campaign material; reputable national newspapers; local Conservative association pages; and reliable retrospective biographies. Use Wikipedia only as a supplementary lead or cross-check. Apply the same search effort, source standards, and coding criteria to every candidate.

Dual-model adjudication. When both searches agree, retain the classification and the stronger evidence record. When one search is unresolved, accept the resolved result only when supported by dated or otherwise sufficiently informative evidence. Give priority to dated positive evidence of local ties over unsupported absence claims. Resolve direct-versus-regional conflicts using the most geographically specific supported tie, and leave balanced conflicts unresolved.

Confidence and review. High confidence requires strong dated evidence or strong agreement; medium confidence reflects one adequate evidence stream; low confidence indicates an unresolved case or material conflict. Do not force low-confidence cases into binary parachute categories.

Audit fields. For each candidate, record a paraphrased evidence summary, source URLs, evidence timing, confidence, coding method, limitations, both initial classifications, conflict type, decision provenance, and review notes. Distinguish “no documented connection found” from “insufficient information.”

The human coding is used only to assess reliability and directional external validity; it does not alter the full-sample classifications or missingness bounds.

OF Strategic Targeting

The preferred match conditions on pre-reform campaign intensity and electoral history, but matching cannot eliminate all concern about changing strategic priorities. Table OF1 restricts the analysis to increasingly narrow bands of the 2005 Conservative margin. Within these observed competitiveness bands, the matched total- and party-spending estimates remain positive.

Table OF1: A-list effect within marginal seats in 2010

Absolute 2005 margin cutoff	Total spending	Party spending	Personal spending	Vote share	Treated / panels
<i>Panel A. Raw panel</i>					
≤ 20 percentage points	0.022 (0.028)	0.013 (0.033)	−0.068 (0.524)	0.294 (0.649)	34 / 270
≤ 15 percentage points	0.032 (0.027)	0.016 (0.034)	−0.119 (0.589)	0.557 (0.683)	29 / 199
≤ 10 percentage points	0.061** (0.027)	0.036 (0.042)	−0.281 (0.693)	0.702 (0.765)	23 / 128
<i>Panel B. Preferred probability-matched panel</i>					
≤ 20 percentage points	0.053* (0.027)	0.048 (0.029)	−0.076 (0.529)	0.154 (0.660)	34 / 262
≤ 15 percentage points	0.053** (0.027)	0.044 (0.030)	−0.088 (0.596)	0.529 (0.699)	29 / 192
≤ 10 percentage points	0.072*** (0.026)	0.060* (0.033)	−0.164 (0.702)	0.757 (0.775)	23 / 123

Notes: Each block restricts the baseline fixed-effects model to constituencies whose absolute 2005 Conservative margin does not exceed the stated cutoff. Spending outcomes use $\log(1 + y)$ and vote share is in percentage points. Panel B applies the preferred ATT weights. Standard errors clustered by constituency panel are in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table OF2 augments the baseline model with the 2005 Conservative margin interacted with election indicators, allowing constituencies with different pre-reform competitiveness to follow different election-specific paths. This is a robustness check for the observed competitiveness gradient, not proof that A-list assignment is orthogonal to every unobserved strategic change.

Table OF2: A-list effect with flexible controls for pre-reform competitiveness

	Total spending	Party spending	Personal spending	Vote share
<i>Panel A. Raw panel</i>				
Baseline A-list $\times$ 2010	0.175*** (0.047)	0.190*** (0.051)	−0.294 (0.465)	0.816 (0.542)
With margin $\times$ election	0.103*** (0.037)	0.104*** (0.040)	−0.214 (0.473)	0.176 (0.590)
<i>Panel B. Preferred probability-matched panel</i>				
Baseline A-list $\times$ 2010	0.102*** (0.026)	0.102*** (0.028)	−0.138 (0.474)	0.077 (0.561)
With margin $\times$ election	0.103*** (0.025)	0.102*** (0.027)	−0.136 (0.472)	0.077 (0.565)

Notes: The augmented model interacts the 2005 Conservative margin with election indicators, with 2005 omitted. Both rows in a panel use the same estimation sample. Spending outcomes use $\log(1 + y)$ and vote share is in percentage points. Standard errors clustered by constituency panel are in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

OG Data Sources and Measurement

OG.1 Spending and election returns

For 2001 onward, candidate spending comes from Electoral Commission candidate returns. The 1997 general election is added from the long-run candidate-level campaign-finance data assembled by Cagé and Dewitte (2025). Election results, vote shares, and electorates come from the House of Commons Library constituency-results series (House of Commons Library 2020). The analysis uses the 1997, 2001, 2005, 2010, and 2015 elections.

Total spending is the amount reported for the short regulated campaign period. Personal spending is reported separately, and party (non-personal) spending is total spending minus personal spending. For 2010 and 2015, expenditure reported for the separate long campaign period is not added to the outcome.

Electoral Commission reports and guidance document the applicable candidate spending limits and, for 2010 and 2015, distinguish separate long and short regulated periods (Electoral Commission 2006, 2011, 2015, 2016). The short-campaign limit was £5,483 plus 6.2p per

elector in county constituencies or 4.6p in borough constituencies in 2001; £7,150 plus 7p or 5p per elector in 2005 and 2010; and £8,700 plus 9p or 6p per elector in 2015. The 2010 and 2015 elections also had separate long-period limits, but those amounts do not enter the spending outcomes.

The party-spending specification has one fewer usable observation than the total- and personal-spending specifications. For one candidate return, total spending minus personal spending is at or below -1 , so the $\log(1 + y)$ transformation is undefined.

OG.2 UKIP and referendum outcomes

UKIP vote shares and candidate entry come from the constituency election results for 2001, 2005, 2010, and 2015. When UKIP did not field a candidate, both vote share and entry are coded as zero. This avoids conditioning the vote-share analysis on the party's potentially endogenous entry decision. The event study uses 2005, the final pre-reform election, as the omitted category.

The House of Commons Library provides constituency-level estimates of the 2016 Leave vote. The preferred measure uses a directly observed constituency result where available and a model-based estimate otherwise. The robustness specification applies the same model-based measure to every constituency. All shares are expressed in percentage points. We do not restrict the analysis to constituencies with directly observed results because their availability is not random across counting areas.

OG.3 Treatment and panel coding

The treatment indicator equals one for a constituency whose official 2010 Conservative candidate is on the compiled A-list roster. Auxiliary name matches that do not correspond to an official Conservative candidate do not define treatment. Constituency-panel identifiers harmonize capitalization, punctuation, and non-substantive spelling variants. The baseline retains treated panels with an observed pre-reform Conservative history and excludes treated 2010 constituencies without such a history rather than creating discretionary synthetic predecessor observations.

The treatment-coding flow is 103 rostered individuals, 53 official prospective parliamentary candidates, 43 constituencies that can be assigned to the harmonized panel, and 42 constituencies with the pre-reform observations and complete covariates required for the preferred match. Constituencies newly created or substantially reconfigured for 2010 are excluded when they cannot be linked consistently to a pre-reform series. Thirsk and Malton can be identified in the 2010 data but has no pre-reform Conservative observation and therefore does not enter the preferred matched analysis.

Table OG1: Summary statistics for the short-run Conservative panel

Variable	<i>N</i>	Mean	Std. dev.	Min.	Max.
Total spending	2,145	7,056.8	3,056.0	0	23,831.6
Personal spending	2,145	174.8	274.0	0	7,694.6
Party (non-personal) spending	2,145	6,882.0	3,049.8	−904.3	23,831.6
Conservative vote share	2,146	32.798	13.339	1.41	62.80

Notes: The table summarizes Conservative candidate–constituency–election observations in 1997, 2001, 2005, and 2010 from the raw short-run estimation sample. The sample contains 556 constituency panels observed in at least two elections. Spending is measured in pounds. Party spending is total spending minus personal spending. One party-spending residual is at or below −1, so the transformed party-spending regression contains one fewer usable observation.

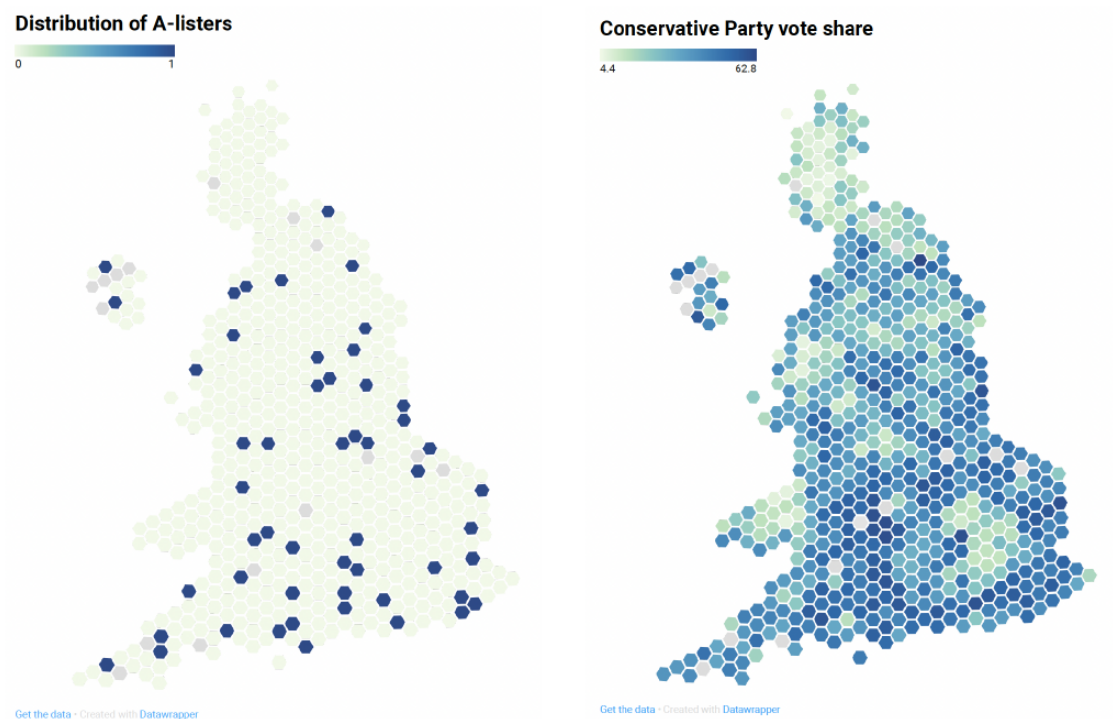


Figure OG1: Geographic distribution of Conservative constituencies and vote share in 2010

Notes: The left panel identifies constituencies with an A-list Conservative candidate in 2010; the right panel shows Conservative vote share. The map is descriptive and does not define the matched comparison group.

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ISSN 1796-3133