# Campaign Spending in Proportional Electoral Systems: Incumbents Versus Challengers Revisited

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## Abstract

This article (a) argues that campaign spending is no more effective for challengers than incumbents in congressional elections using candidate-centered forms of proportional representation (PR), (b) develops a new method to estimate spending effects in poly-candidate elections, and (c) demonstrates that spending benefits incumbents as much as challengers in the congressional elections of three separate countries (Brazil, Ireland, and Finland). This article also offers a theory of campaign spending effectiveness that emphasizes a candidate's relative potential to attract new electoral support as determined by both precampaign familiarity and personal, partisan, and ideological attributes. Challengers and incumbents obtain similar returns to spending under PR because its permissiveness encourages challengers who are limited in their abilities to build electoral support, even when they spend heavily. The theory and findings have important implications for understanding the effects of campaign finance and campaign finance regulations.

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Campaign spending matters—it helps candidates win votes. But how much it matters depends on the candidate running the campaign. For candidates who begin a campaign already well-known by the electorate, campaign spending will matter little. For obscure, unknown candidates, however, campaign finance can make the difference between irrelevance and viability. This was Jacobson's (1978, 1985, 1990) explanation for why challengers in U.S. House elections reap larger returns to spending than incumbents—a finding that has sparked considerable debate (see Green & Krasno, 1988; Levitt, 1994). A similar debate has surfaced with respect to countries using proportional electoral systems, with some studies finding differences between challengers and incumbents (Benoit & Marsh, 2010) and other studies finding no such thing (Benoit & Marsh, 2008; Maddens, Wauters, Noppe, & Fiers, 2006; Samuels, 2001). These debates have important policy implications. In particular, when challenger spending is more effective than incumbent spending, regulations that limit campaign spending will disadvantage challengers and therefore undermine electoral competitiveness and accountability.

An important implication of Jacobson's theory of spending effects—which has gone largely unchallenged—is that nonincumbents who enjoy high levels of familiarity at the beginning of the campaign stand to receive low, incumbent-like returns to spending. However, these are not the only challengers who will find their spending relatively ineffective. Challengers who offer fringe ideological platforms are unlikely to build much electoral support even if they spend heavily. Also, candidates who are lacking in appealing personal attributes (e.g., charisma) may find their campaigns much more effective at building name recognition than electoral support. This implies that campaign spending effectiveness is primarily a function of a candidate's potential to attract new supporters, which relates to a candidate's precampaign familiarity as well as limitations imposed by the electorate's preferences for specific candidate attributes. It further implies that campaign spending benefits challengers more than incumbents only when most challengers have significant potential to build electoral support. In elections that use candidate-centered forms of proportional representation (PR) this is unlikely. The proportionality and electoral fragmentation that accompany multimember districts encourage the entry of many already familiar challengers and politically unattractive challengers, both limited in their abilities to build electoral support. As a result, under PR the average challenger will not gain more from spending than the average incumbent.

This article tests this hypothesis for three separate countries—Brazil, Ireland, and Finland—each chosen for its use of candidate-centered forms of PR and its campaign finance disclosure regulations. The results show that campaign spending does not disproportionately benefit challengers in any

case. The results for the 2002 and 2006 Chamber of Deputies elections in Brazil jibe with Samuels's (2001) analysis of the 1994 elections, which also found no difference between incumbents and challengers. However, the results for Ireland's 2002 and 2007 Dáil elections challenge the findings of Benoit and Marsh (2008, 2010). The estimates for Finnish Eduskunta elections (2003 and 2007) are the first of their kind.

All of the estimates are the product of a new approach to estimating the effects of campaign spending in poly-candidate elections. The main preoccupation in the large literature on spending effects—the present study included—is a methodological one that stems from a reliance on one-shot observational data and a research design that estimates spending effects by comparing spending and votes across candidates. Jacobson (1978) observed that this approach may suggest a negative spending effect for incumbents even when all incumbents benefit from spending, simply because vulnerable incumbents tend to spend more money. The problem is more general and is essentially an omitted variables problem writ large: the failure or inability to control for any factor that is related to both electoral performance and spending leads to biased regression estimates. The instrumental variables approach offers a way out, but it requires variables that are both highly correlated with campaign spending and causally unrelated to the electoral performance of individual candidates—an unlikely combination. The method used here approaches the problem differently, by matching similar-spending copartisan incumbents or nonincumbents and estimating spending effects only within those candidate pairs. In other words, the method refuses to analyze large spending differences—which tend to stem from fund-raising abilities and electoral expectations (i.e., omitted variables)—and focuses only on small spending differences, which are no less relevant to electoral outcomes (on a per unit basis) but much less likely to be systematically related to important omitted variables. As a result, the approach minimizes the potential for simultaneity bias.

The article proceeds as follows. The first section outlines the theory of campaign spending effectiveness and its consequences for candidate-centered PR elections. Next, the article introduces its method for estimating spending effects. The third section discusses the countries and data, and the fourth section provides the spending effects estimates for incumbents and challengers for each country. The final section discusses the implications for campaign finance policy and avenues for further research.

## Who Benefits From Spending?

In his well-known study of campaign spending in U.S. congressional elections, Jacobson (1978) argues that incumbent spending is less effective than

challenger spending because incumbents spend their time in office saturating their districts with information about their accomplishments. As a result, campaigning by incumbents

> produces, at best, very modest gains in support. Challengers, in contrast, typically begin the campaign in obscurity. . . . Their level of campaign activity—largely, if not entirely, a function of campaign spending—thus has a strong influence on how well they do at the polls. (Jacobson, 1990, pp. 334-335)

In this account, spending effectiveness relates to the ability of campaigns to familiarize an electorate with a candidate. Campaign spending can matter in other ways—it can, for example, mobilize voters. However, although the cost of mobilizing a supporter is unlikely to vary much across candidates, it is clear why the familiarity-building effect of campaigns is disproportionately effective for lesser-known candidates—provided, that is, that voters evaluate candidates by more than just their party labels. It follows that if challengers tend to be more obscure than incumbents, the spending effect for the average challenger ($SE_{\bar{c}}$) will exceed the spending effect for the average incumbent ($SE_{\bar{i}}$).

It is important to note, however, that the difference between $SE_{\bar{c}}$ and $SE_{\bar{i}}$ will not be large or meaningful if there are many (a) "already familiar" challengers, who begin the campaign with incumbent-like levels of familiarity, or (b) "politically unattractive" challengers, who begin the campaign in obscurity and yet prove unable to attract supporters no matter how much they spend. Going further, if some obscure challengers do not benefit much from spending, it implies that spending effectiveness relates primarily to a candidate's "room for growth," or potential to attract new supporters. Candidates who are obscure but politically unattractive are similar to already familiar candidates (incumbents included) in that they have little potential for attracting new supporters. Generally speaking, only candidates who are both obscure and politically attractive gain significantly from campaign spending.

Jacobson (1978, pp. 489) made a related point when he acknowledged that a challenger's spending was less effective among voters who support another political party. In my account, the "unattractiveness" that can limit a candidate's room for growth (and thus spending effects) consists of a wide range of candidate attributes. Ideological positions are one type of constraint.[1] In fact, we can identify a candidate's maximum electoral support if we employ spatial voting assumptions and know each candidate's "ideal point" and the distribution of voter preferences. Then, measuring a candidate's room for growth entails subtracting the candidate's precampaign support from his or her spatial

support, and computing the returns to spending becomes an exercise in ascertaining how much of that increase in support is the result of the candidate's campaign spending. These quantities may be difficult to determine empirically, but the theoretical point is straightforward: Differential spending effects stem from differences in candidates' abilities to attract new supporters.

Other relevant elements of candidate (un)attractiveness are captured in Green and Krasno's (1988) notion of challenger "quality." This includes both skill, which is "a candidate's ability to organize a campaign and present himself effectively," and attractiveness, which includes "a full range of characteristics that might be judged appealing in the eyes of the voter: qualifications for office in the form of political experience and occupational background, fame or notoriety, and physical appearance and personality" (Green & Krasno, 1988, p. 887). Green and Krasno argue that challenger quality is an important variable that should be included in regression models of spending effectiveness. However, to understand the differential effectiveness of spending we must employ a notion of attractiveness that does not conflate already popular challengers with attractive but obscure challengers. This is because candidates whose "quality" derives from something that implies precampaign popularity (e.g., fame, previous experience in political office) will obtain low returns to spending,[2] whereas candidates who are "high quality" but obscure will benefit more per expenditure. In other words, although well-known candidates with appealing attributes (personal, partisan, and ideological) are poised to collect votes, their *campaigns* will be less effective than those of attractive but obscure candidates, who stand to benefit much more for a given amount of campaign spending. Therefore, the "attractiveness" that allows for significant returns to spending consists of those attributes that make a candidate appealing to voters but that do not also imply high levels of precampaign familiarity. This argument does not mean that a skillful campaign cannot improve a candidate's appeal to voters. But a candidate's overall attractiveness is less a function of a campaign's ability to "polish" or "market" the candidate than of voters' preferences for particular types of candidates. The effectiveness of campaigns and campaign spending is therefore limited for each candidate, although for some more than others.

These points are especially relevant for elections using candidate-centered versions of PR,[3] where there tend to be many already popular and electorally unattractive challengers. The preponderance of both types stems from the electoral fragmentation and proportionality that accompany multimember districts. This encourages the entry of all kinds of challengers, from those who offer fringe platforms and lack political skill to those who have gained popularity through entertainment or business and who reason that they have

as good a chance at winning a seat as the other strong candidates, incumbents included. The former, unattractive candidates may be able to improve their name recognition by campaigning, but it will not translate into electoral support, so their returns to spending cannot be large. Likewise, challengers who begin the campaign with incumbent-like levels of popularity will find their spending to be as effective as incumbent spending. Of course, in addition to these two types of challengers, PR elections may feature challengers who campaign their way from obscurity to popularity. But with too many other challengers reaping small returns to spending, the average challenger will not "pay" any less per vote than the average incumbent.[4] This motivates a testable hypothesis: In elections using candidate-centered forms of PR, the spending effect for the average challenger will not exceed the spending effect for the average incumbent. This can be written as $H_1$: $SE_{\bar{c}} = SE_{\bar{i}}$.

The purpose of this article is to test this hypothesis. It does not directly test the room for growth theory of spending effects or the connection between candidate attributes and spending effects among obscure challengers. These tests seem doable, although it would be difficult to construct a measure of electoral attractiveness for obscure challengers.

## Estimating Campaign Spending Effects

If one factor influencing the average spending effect is the pool of candidates, another is how we go about measuring spending effects. The common approach to estimate a spending effect—defined as the difference between a candidate's vote total and the counterfactual in which he or she spent nothing—is to use cross-sectional data from the end of the campaign and compare votes and total spending across candidates.[5] The success of this approach depends on how much candidates resemble one another in ways other than spending. To appreciate this, consider the familiar "Jacobson effect"—a negative correlation between spending and votes among incumbents.[6] This occurs not because spending causes incumbents to lose votes. It occurs because we compare threatened incumbents, who spend heavily but still perform poorly, to unthreatened incumbents, who spend little and win. The same problem can occur with challengers: when "electorally unattractive" and "already familiar" challengers both reap small returns on spending but the latter spend much more money, a simple comparison of spending and votes will suggest a large spending effect regardless.

In both cases, there is an omitted variables problem. If we can adequately control for each incumbent's vulnerability and each challenger's viability, then we can "equalize" candidates within each group and isolate the difference

that spending makes.[7] If we fail to control for these variables, however, we are likely to underestimate $SE_i$ and overestimate $SE_{\bar{c}}$, thus creating a bias in favor of the null hypothesis (cf. Jacobson, 1990, pp. 335). However, the problem is more general: The failure to control for any variable that affects both spending and votes will introduce bias. In the regression context, "simultaneity" bias occurs whenever a model contains endogeneity resulting from omitted variables.

When researchers lack the ability to control for relevant variables, they often seek to sidestep the problem with two-stage least squares (2SLS) regression with instrumental variables (IVs). However, for this method to work well in predicting candidates' votes from their spending, a very high hurdle must be met: the IVs must be both (a) highly correlated with candidates' spending in a first stage regression that includes all of the exogenous regressors (i.e., control variables) and (b) causally unrelated to candidates' vote totals.[8] Good instruments are seldom available because any variable that meets (a) is unlikely to satisfy (b), and vice versa. These requirements become still more taxing when one considers issues of modeling: a model intended to predict an individual's votes should include not only his or her own spending but also spending by the individual's competitors, which will depress his or her vote total.[9] But this means more endogenous regressors, and so more hard to find IVs.[10]

## A Different Approach

This article seeks to sidestep the problem a different way, by matching each incumbent and challenger with their nearest spending copartisan competitor of the same type (incumbent or challenger) and then estimating the effect of the small spending differences that remain. On a per unit basis, small spending differences are just as relevant to electoral outcomes as large spending differences. However, by refusing to analyze large spending differences (which are likely to correlate with important omitted variables) we reduce the threat of simultaneity bias.[11] Put differently, although large spending differences are likely to stem from factors that simultaneously affect vote differences between candidates, small spending differences between same party district incumbents (or challengers) will be not be systematically related to omitted variables and hence exogenous in the regression model. Because many factors affect the vote, the difference in votes for spending-matched candidates can vary somewhat, especially in large electorates where candidates can collect many votes. In the aggregate, however, we should find that small spending differences matter for electoral performance.[12]
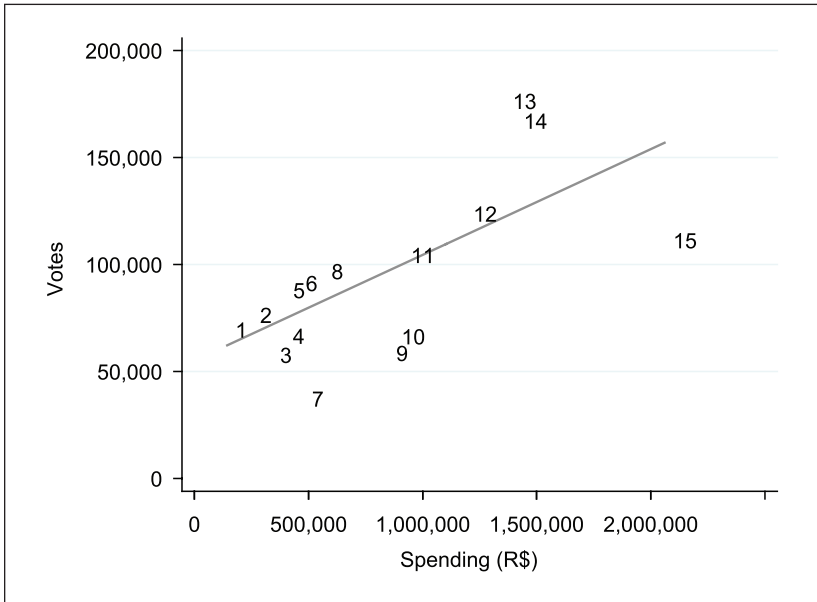
**Figure 1.** Spending and votes for Brazilian São Paulo Workers' Party (PT) incumbents, 2006 Chamber of Deputies elections

To illustrate the approach, consider Figures 1 and 2. The first shows the spending and votes for Workers' Party (PT) incumbents in a single district (São Paulo) in the 2006 Chamber of Deputies elections in Brazil. There, 15 incumbents spent between R$142,788 and R$2,061,634. The numbers that identify the candidates are ordered from 1 (*lowest spender*) to 15 (*highest spender*). With robust standard errors, the regression line has a positive and statistically significant slope of .049, and a constant of 55,209. If we were to interpret this cross-candidate slope as a spending effect, we would say that these candidates won about 50 votes per R$1,000 (or R$20/vote), on top of the approximately 55,000 votes they won for spending nothing. Of course, the slope can be taken as a spending effect only if the two variables are otherwise unrelated to each other. With such wide variation in spending and votes (among incumbents in the same party and district!), this is unlikely.

Figure 2 shows the same data, slightly rearranged. Here, the axes correspond to the *differences* in spending and votes between an incumbent and the next highest-spending incumbent. That is, Incumbent 2 (Iari Bernardi) is positioned on the x-axis according to how much her spending exceeded that
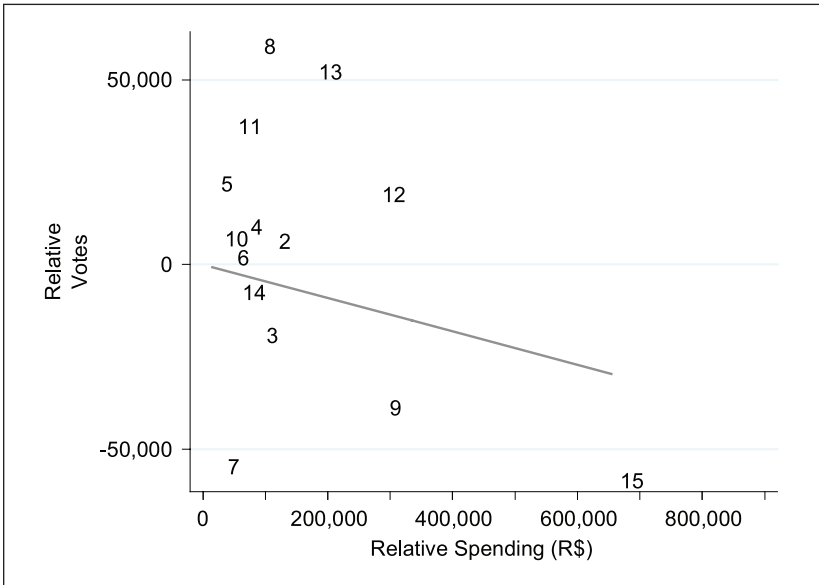
**Figure 2.** Spending and votes of Brazilian São Paulo Workers' Party (PT) incumbents relative to next highest spending incumbent running mate, 2006 Chamber of Deputies elections

of Incumbent 1 (Durval Orlato)—which was about R$150,000—and on the y-axis by the extent to which her votes exceeded Orlato's, which was very little. Similarly, Incumbent 3 (Roberto Gouveia Nascimento) is positioned according to how much his spending and votes exceeded those of Bernardi (2). That he is below zero on the y-axis indicates that he won fewer votes than Bernardi, despite spending more money. All incumbents are positioned in this way, with Orlato (1) not shown since there were no incumbents who spent less than he.

In fitting a line to the dyadic data, the constant has been forced to zero, so that the expected vote difference between two incumbents who spent the same amount of money is zero. This makes theoretical sense and dispenses with an estimated constant term. More significant, because the line now compares spending and votes across similar-spending pairs of candidates, omitted variables are less of a threat to our interpretation of the slope as an incumbent spending effect. Note, however, that the slope is now –.045, indicating that outspending a running mate by R$1,000 should yield 45 fewer votes. Negative spending effects should be treated with suspicion, and in this case it is clear

that it is mostly the result of Observation 15, which has high leverage because of its value on the explanatory variable. In fact, the spending difference is probably too large to be independent from relevant omitted variables, so it should be dropped. Sekhon (2009) notes that it is often "jarring for people to hear that observations are being dropped . . . [but the] intuition against dropping observations comes from what happens with experimental data" (p. 496) where differences between treated (higher spenders) and untreated (lower spenders) are unrelated to the pretreatment covariates (e.g., expected electoral performance). When estimating causal effects with observational data, however, it is advisable to drop observations that do not facilitate good estimates, and the concerns this raises about reduced statistical power are misplaced. There is no obvious way to determine whether a particular spending difference is "too large" or not, but included observations should be defensible on grounds that they are small enough to be independent from expectations about electoral performance. The most permissive defense that is offered below will extend to fewer than half of the dyads in Figure 2.

Excluding large spending differences does not remove the threat of influential observations since ordinary least squares (OLS) remains sensitive to both vertical outliers (i.e., extreme values on the outcome variable) and bad leverage points (i.e., outliers with large values on the explanatory variable). Robust regression is a good alternative to OLS when there are influential observations. Some forms of robust regression, such as median regression or Huber's M-estimator, are well equipped to deal with vertical outliers, but they remain vulnerable to bad leverage points. However, Yohai's (1987) MM-estimator is resistant to both types of influential observations. The MM-estimator minimizes the sum of scaled residuals via Huber's M-estimator, where the scale is determined by a first-stage S-estimator. The S-estimator is more resistant to bad leverage points than other forms of robust regression because it uses a series of preliminary estimates based on subsamples of the data to produce (and minimize) an outlier-resistant scale of the residuals. The estimator is comparatively inefficient, however, which motivates the two-stage approach: abandon the S-estimates but use its scale in the more efficient M-estimator. A full description of the procedure can be found in Verardi and Croux (2009), who also describe their MM-estimator for Stata.

## Model

To test $H_1$, I construct the dyads separately for each party "cohort" (incumbents or challengers) in a given party-district and estimate Equation 1 using both OLS with robust standard errors and the MM-estimator.

$$\text{Relative Votes}_j = \beta_1 * \text{Relative Spending}_j + \beta_2 * \text{Relative Spending}_j * \text{Incumbents}_j + \varepsilon_j (1)$$

The variables are as above, with *Relative Spending* for candidate *j* = (*j*'s campaign spending – *i*'s campaign spending), where the reference candidate *i* is the next-highest spending candidate (relative to *j*) in *j*'s cohort. The variable is strictly positive by construction, whereas the dependent variable, *Relative Votes*, which equals *j*'s vote total less *i*'s vote total, can be positive or negative. The model excludes a constant term because the expected vote difference for *Relative Spending* $_j$ = 0 is exactly zero.

   To differentiate spending effects by candidate type, the model interacts *Relative Spending* with the dummy *Incumbents*, which is 1 for a pair of incumbents and 0 for a pair of challengers. The model excludes *Incumbents* as a stand-alone term because the expected vote difference for both dyad types—incumbents or challengers—is exactly zero for *Relative Spending* = 0. For the same reason, there are no variables to distinguish dyads by party or district. It should also be clear why there are no *candidate*-level controls for party, district, or incumbency status—such differences simply do not exist in the model.[13]

   When *Incumbents* = 0, the estimate is based only on challenger dyads, so $\beta_1$ is the estimate for the average number of votes a challenger can expect over a copartisan challenger for a given unit of greater expenditure. $\beta_2$ is the departure from $\beta_1$ that occurs from switching from two challengers to two incumbents. Therefore, if challenger spending is more influential than incumbent spending, we expect $\beta_2 < 0$. Again, $H_1$ predicts $\beta_2 = 0$. If the model works appropriately, we should also observe $\beta_1 > 0$ and $\beta_1 + \beta_2 > 0$. If instead we observe negative spending effects for either incumbents or challengers, this would suggest that our cross-candidate comparisons are capturing something other than the effect of spending on votes.

## Countries and Data

Spending effects are estimated for three separate countries using data from two legislative elections per country—the 2002 and 2006 Chamber of Deputies elections in Brazil, the 2002 and 2007 Dáil elections in Ireland, and the 2003 and 2007 Eduskunta elections in Finland. These countries were chosen because of their candidate-centered PR systems and the availability of candidate-level campaign finance disclosures. The Brazilian and Finnish chambers elect members via open list proportional representation (OLPR), where voters vote for individual candidates,[14] seats are allocated proportionally to lists of candidates according to the sum of votes obtained by candidates

on each list, and candidates win seats if they are among the top *c* vote-winning candidates on a list that wins *c* seats. Therefore, OLPR list mates run as a team against other lists (though they may not cooperate much in this task), but they also compete against one another. Within-list competition is largely but not exclusively within-party competition since parties in both countries often run joint lists. Finland's districts have magnitudes that range from 6 to 33, whereas Brazil's range from 8 to 70.[15] With many lists competing to fill many seats, there are usually dozens (if not hundreds) of candidates per district. Many of the challengers who run have little chance of either winning or expanding their electoral appeal, whereas others will have significant popularity before the campaign begins, often gained via political office at some other level or some other endeavor, such as business, sport, or television (Ames, 1995; Arter, 2009; Raunio, 2005; Samuels, 2001). As a result, the incumbent versus challenger divide in Brazilian and Finnish elections does not correlate strongly with the factors that make for significantly different spending effects.

Unlike Brazil and Finland, Ireland uses the single transferable vote (STV) system in districts with magnitudes of three to five. On STV ballots, voters rank candidates in decreasing order of preference from one up to as many candidates as there are running in the district. Then, candidates who obtain enough "first preference" votes to surpass the Droop quota are elected and their surplus votes (votes in excess of the quota) are transferred to other candidates based on those votes' second preferences. Subsequently, any candidates who pass the quota based on the vote transfer are declared elected, and surplus votes are transferred again (according to the votes' next preference). If not all winners are yet determined, the candidate with the fewest votes is eliminated, and his or her votes are transferred to candidates still in the running according to the next preference on each vote. This last step is repeated until all the seats are filled for the district.

With small district magnitudes, few Irish parties can hope to win more than one seat per district, so few nominate more than one candidate per district. The parties that can win multiple seats per district (e.g., Fianna Fáil and Fine Gael) do two things to improve their odds of success. First, they refrain from nominating weak candidates, at least for the seats they think they can win. (This contrasts with OLPR, where weak challengers can still help the party list even if they have little chance of winning.) A party that thinks it can win two seats might nominate one weak challenger along with two strong candidates, anticipating that the weak challenger will get eliminated from the "count" and some of his or her votes will transfer to his or her running mates. But the party will not nominate too many candidates (and thus risk splitting the vote too much) and will not nominate weak challengers for the seats they

think they can win. Indeed, Irish parties take care to nominate challengers who are well-known in their districts (see Galligan, 2003), many of whom are popular councilors or Seanad members. As a result, the challengers in multi-candidate slates will closely resemble incumbents in both their precampaign popularity and their returns to spending.[16]

To further ensure that each of their viable candidates collects enough first preference votes, Irish parties sometimes seek to "manage" the vote by encouraging their candidates to concentrate their campaigns in particular subconstituencies that are chosen to furnish each candidate a roughly similar number of votes. To the extent that parties succeed in vote management, they alter electoral outcomes from what would be observed under unfettered intra-party competition (Johnson and Hoyo, 2012). Because vote management will reduce the variance in electoral performance between two same-party-district incumbents, it is likely to create a deflationary bias on estimates for incumbent spending effects. A similar bias is likely for challengers wherever a party seeks to even the vote between two same-party-district challengers, but because parties sometimes nominate an extra challenger (i.e., one more than they think they can elect) who may be less popular and less involved in vote management schemes, the deflationary bias is likely to be less pronounced for challengers. The combination—a greater deflationary bias for incumbents as opposed to challengers—will create a bias against $H_1$.

## Campaign Spending Data

The campaign finance data analyzed here are taken from the official post-electoral disclosures mandated by law in each country.[17] The incentives to lie about spending were low in each of these elections because in Brazil and Finland there are no limits on spending,[18] and Irish incumbents could over-spend their limits as a result of "office spending"—that is, their use of Dáil office resources during the campaign period.[19] Even so, there is no reason to suppose that any inaccuracies would create a bias in favor of $H_1$. For that to occur, they would have to systematically deflate the estimates for challengers or inflate the estimates for incumbents—both unlikely.

When the candidate-level electoral data are transformed into dyadic form, candidates who ran as independents or without copartisan running mates are discarded. With Brazil and Finland, there are additional candidates who do not make the analysis. Many Brazilian candidates simply did not disclose their finances (although the vast majority of nondisclosers won very few votes), and I further exclude those who reported spending less than R$1,000 to ensure that the analysis is not overly affected by very low spenders.[20] However, the

**Table 1.** Campaign Spending Reported by Candidates in Six Legislative Elections

| Election | Number of candidates | Percentage incumbents | Campaign spending | |
|---|---|---|---|---|
| | | | *M* | Max |
| Brazil 2002 | 2,203 | 17 | R$86,468 | R$2,531,874 |
| Brazil 2006 | 3,045 | 12 | R$136,482 | R$2,949,964 |
| Finland 2003 | 267 | 57 | €26,089 | €70,689 |
| Finland 2007 | 273 | 55 | €35,999 | €119,336 |
| Ireland 2002 | 463 | 31 | €10,461 | €36,021 |
| Ireland 2007 | 469 | 32 | €14,126 | €41,619 |

Currencies not adjusted for inflation. Brazilian candidates reported spending less than R$1,000 are excluded.

Brazilian data still consist of thousands of challengers and more than 99% of the 748 incumbents who ran for reelection in 2002 or 2006.[21] The Finnish data do not include candidates who were not elected as either members of parliament or as "deputy members" (substitutes) since only these candidates must adhere to the disclosure regulations. This rule prevents the analysis of many Eduskunta candidates (primarily challengers); but it should not exclude challengers who gained the most from spending, so it should not bias estimates against the null hypothesis. More generally, "missing" data pose no methodological problems since our approach focuses only on small spending differences between spending-matched candidates. Indeed, throughout the analysis, we pursue better estimates by restricting the range on the explanatory variable, discarding more observations. Lest this reduce too much the statistical power brought to bear on the tests, I pool the data by country (i.e., across election years, after adjusting currencies for inflation).[22] Table 1 summarizes the candidate-level spending data for all Irish candidates, all Finnish candidates who were required to disclose, and all Brazilian candidates who reported spending over R$1,000. For the analysis, the data are adjusted for inflation to the more recent election year and then rearranged into dyadic form.

## Results

### Brazil

Table 2 presents regression estimates of Equation 1 using the Brazilian data.[23] There are six regressions, each estimated with both OLS (with robust standard errors) and the MM-estimator. The regressions differ according to

**Table 2.** Spending Effects for Challengers and Incumbents in Brazilian Chamber of Deputies Elections

| Model | Relative spending capped at: | Relative Spending | | Relative Spending × incumbents | | Number of dyads | | | |
|---|---|---|---|---|---|---|---|---|---|
| | | $\beta_1$ | SE | $\beta_2$ | SE | Incumbents | Challengers | $R^2$ | $\beta_1 + \beta_2$ |
| OLS1 | R$100,000 | 0.351*** | (0.073) | −0.276*** | (0.10) | 188 | 3,334 | .04 | 0.075 |
| OLS2 | R$85,000 | 0.364*** | (0.085) | −0.230** | (0.12) | 173 | 3,302 | .03 | 0.081 |
| OLS3 | R$70,000 | 0.255*** | (0.048) | −0.231** | (0.11) | 149 | 3,231 | .02 | 0.024 |
| OLS4 | R$60,000 | 0.283*** | (0.058) | −0.292** | (0.13) | 136 | 3,161 | .02 | −0.009 |
| OLS5 | R$50,000 | 0.329*** | (0.074) | −0.265 | (0.16) | 122 | 3,098 | .02 | 0.064 |
| OLS6 | R$40,000 | 0.353*** | (0.090) | −0.117 | (0.25) | 95 | 3,005 | .02 | 0.236 |
| MM1 | R$100,000 | 0.080*** | (0.018) | −0.070 | (0.11) | 188 | 3,334 | — | 0.010 |
| MM2 | R$85,000 | 0.083*** | (0.021) | −0.069 | (0.06) | 173 | 3,302 | — | 0.014 |
| MM3 | R$70,000 | 0.086*** | (0.018) | −0.005 | (0.07) | 149 | 3,231 | — | 0.081 |
| MM4 | R$60,000 | 0.081*** | (0.018) | −0.014 | (0.10) | 136 | 3,161 | — | 0.067 |
| MM5 | R$50,000 | 0.071*** | (0.021) | −0.253 | (0.48) | 122 | 3,098 | — | −0.182 |
| MM6 | R$40,000 | 0.087*** | (0.022) | 1.320*** | (0.08) | 95 | 3,005 | | 1.407 |

Rows provide estimates of Equation 1 using ordinary least squares (OLS; with robust standard errors) and the MM-estimator. The dependent variable, *Relative Votes,* is the number of votes a candidate received in excess of the next lowest spending candidate in his or her party cohort. *Relative Spending* is in 2006 reais. The total number of observations per regression is the sum of the number of incumbent and challenger dyads.
\*\**p* < .05. \*\*\**p* < .01.

maximum allowed value on the explanatory variable, *Relative Spending*. The estimate for $\beta_1$ indicates how many votes a challenger would receive for spending R$1 more than a copartisan challenger, and $\beta_2$ indicates how much this effect changes by switching to an incumbent dyad. The spending effect for incumbents, $\beta_1 + \beta_2$, is shown in the final column. Therefore, the results of the first regression (OLS1) indicate that each R$1,000 buys a challenger 351 votes and an incumbent 75 votes.

Influential observations seem to be a problem for the OLS regressions. Note, for example, that the estimates given by OLS4—where the cap on *Relative Spending* is R$60,000—suggest a negative spending effect for incumbents. However, if we either (a) add more observations by raising the cap to R$70,000 (OLS3) or (b) remove the largest observations by lowering the cap to R$50,000 (OLS5), $\hat{\beta}_1 + \hat{\beta}_2$ returns to positive territory and $\hat{\beta}_2$ decreases in both magnitude and statistical significance. Put differently, there are a few influential observations with *Relative Spending* = [R$50,000–R$60,000] that have high leverage when the cap is R$60,000.

When the cap is raised (lowered), the influence of these observations diminishes (disappears).

As expected, the MM estimates show less sensitivity to these observations. With the MM-estimator, the differences across the third and fourth regressions are less pronounced, and the influential observations do not produce a negative estimate for incumbent spending effects in MM4. However, the estimates for incumbents become unstable in Regressions 5 and 6, where the number of incumbent dyads is smallest and the MM-estimator is least able to dampen the influence of incumbent outliers. Those regressions aside, the estimates for incumbents are similar to those using OLS, with predicted spending effects ranging from 10 to 81 votes per R$1,000.

The MM-estimator leads to a dramatic change in the estimates for challengers. The estimates are much smaller in magnitude, no longer statistically larger than the estimates for incumbents, and more consistent and stable across regressions (ranging between 71 and 87 votes per R$1,000). It makes sense that influential observations in the Brazilian data would cause OLS to overestimate challenger spending effects. In Brazilian elections, "outlier challengers" are not heavy spenders who underperform but "over-performers" who collect many more votes per R$1 than the average challenger—usually because of popularity gained from some other career. Without the appropriate control variables, these candidates appear as outliers and exert a strong, positive influence on the OLS estimates. However, the MM-estimator dampens the effect of these observations and produces more believable estimates. The change also indicates that challenger spending is not more effective than incumbent spending in Brazilian elections. This finding supports $H_1$ and complements Samuels's (2001) findings for the 1994 Chamber elections.

Two countervailing principles should guide which estimate we select as the best estimate for the effects of campaign spending. On one hand, our choice should be robust and representative of a set of good estimates. On the other hand, we should privilege estimates based on smaller ranges on the spending variable. Because MM5 and MM6 are already unstable because of the small number of incumbent dyads, it makes sense to choose another estimate from Table 2. I choose to average MM2–MM4, which suggests that R$1,000 "buys" an incumbent 54 votes (or R$18.50/vote) and a challenger 83 votes (R$12/vote). If we ignore the fact that it is not statistically meaningful, the difference deserves recognition—it will matter for electoral results. However, that does not mean it will matter much relative to the many other factors that affect electoral outcomes, including office resources (for incumbents), precampaign levels of popularity, ideological positions, and the like.

## Ireland and Finland

Table 3 provides the results of a series of regressions using the Irish and Finnish data. To strengthen the test of $H_1$, I estimate Equation 1 not only for each country-pooled sample but also with a combined Irish–Finnish data set. The MM-estimates remained unstable, so Table 3 only reports the results using OLS. Although OLS may overestimate challenger spending effects—thus biasing against $H_1$—the distortion should be less pronounced for Ireland and Finland than it was for Brazil. The small electorates in Ireland and Finland simply do not allow for extreme differences in the performance of challengers, thus reducing the number of extreme "outlier challengers" who can bias challenger spending effects upward. (There is another reason why large differences in the performance of same-party-district challengers are unlikely to appear in the Finnish data: Few poorly performing candidates disclose their campaign finances.) Therefore, OLS should produce good estimates of challenger spending effects for both European countries—provided, that is, that we attend to the possible influence of bad leverage points.

In the first five OLS regressions for Ireland, two patterns stand out. First, the smallest estimate for incumbent spending effects appears in OLS1, where the cap on *Relative Spending* is highest (€15,000). It was anticipated that large, unexplained spending differences would serve to overstate the differences between incumbents and challengers. And spending differences of €15,000 are indeed large in Irish elections—too large, in fact, to be independent from relevant omitted variables. Second, the subsequent four regressions exhibit considerable stability in estimates for challengers, but not for incumbents. In part, this stems from some high-leverage incumbent dyads with *Relative Spending* > €7,000. I removed these observations and reestimated OLS2–OLS4, the results given by OLS2.b–OLS4.b. Although no more than two observations are dropped per regression, the changes are considerable and the estimates are more stable and believable. (Note that the effect of dropping the observations is a decrease in $\hat{\beta}_2$, which supports the null hypothesis.) If we average the three new estimates with OLS5, the result indicates that challengers collect 122 votes per €1,000, or €8.20/vote. For incumbents, the estimate is 71 votes per €1,000, or €14.08/vote. Although statistically insignificant, the €5.88/vote difference is deserving of recognition, although with the caveat that other factors may affect the vote even more.

With the Finnish data, we again observe that the regression with the most permissive range on *Relative Spending* yields the smallest estimate for incumbent spending effects.[24] In fact, it is negative. As before, however, reducing the

**Table 3.** Spending Effects for Challengers and Incumbents in Irish Dáil and Finnish Eduskunta Elections

| Model | Relative spending capped at: | Relative Spending | | Relative Spending × incumbents | | Number of dyads | | $R^2$ | $\beta_1 + \beta_2$ |
|---|---|---|---|---|---|---|---|---|---|
| | | $\beta_1$ | SE | $\beta_2$ | SE | Incumbents | Model | | |
| Ireland | | | | | | | | | |
| OLS1 | €15,000 | 0.107** | (0.047) | −0.094 | (0.085) | 101 | 68 | .02 | 0.013 |
| OLS2 | €10,000 | 0.102* | (0.059) | −0.013 | (0.130) | 99 | 62 | .02 | 0.089 |
| OLS3 | €9,000 | 0.098 | (0.063) | 0.092 | (0.130) | 98 | 59 | .04 | 0.190 |
| OLS4 | €8,000 | 0.119* | (0.064) | 0.041 | (0.140) | 95 | 58 | .04 | 0.160 |
| OLS5 | €7,000 | 0.139* | (0.075) | −0.105 | (0.130) | 92 | 54 | .02 | 0.034 |
| Influential observations removed | | | | | | | | | |
| OLS2.b | €10,000 | 0.102* | (0.059) | −0.028 | (0.100) | 99 | 60 | .02 | 0.074 |
| OLS3.b | €9,000 | 0.131** | (0.056) | −0.025 | (0.110) | 97 | 58 | .03 | 0.106 |
| OLS4.b | €8,000 | 0.119* | (0.064) | −0.051 | (0.110) | 95 | 57 | .02 | 0.068 |
| Finland | | | | | | | | | |
| OLS1 | €15,000 | 0.093* | (0.056) | −0.120 | (0.079) | 137 | 56 | .01 | −0.027 |
| OLS2 | €10,000 | 0.072 | (0.079) | −0.015 | (0.120) | 117 | 46 | .01 | 0.057 |
| OLS3 | €9,000 | 0.068 | (0.100) | −0.032 | (0.140) | 112 | 40 | .00 | 0.036 |
| OLS4 | €8,000 | 0.109 | (0.100) | −0.017 | (0.170) | 100 | 39 | .01 | 0.092 |
| OLS5 | €7,000 | 0.129 | (0.170) | −0.013 | (0.230) | 92 | 30 | .01 | 0.116 |
| Pooled | | | | | | | | | |
| OLS1 | €15,000 | 0.098** | (0.040) | −0.114* | (0.060) | 203 | 156 | .01 | −0.016 |
| OLS2 | €10,000 | 0.086* | (0.051) | −0.024 | (0.084) | 177 | 144 | .01 | 0.062 |
| OLS3 | €9,000 | 0.102* | (0.056) | −0.047 | (0.090) | 170 | 137 | .01 | 0.055 |
| OLS4 | €8,000 | 0.144* | (0.058) | −0.031 | (0.110) | 157 | 134 | .01 | 0.113 |
| OLS5 | €7,000 | 0.136* | (0.074) | −0.052 | (0.130) | 146 | 122 | .01 | 0.084 |

Rows provide estimates of Equation 1 using ordinary least squares (OLS; with robust standard errors). The dependent variable, *Relative Votes*, is the number of first-preference votes a candidate received in excess of next lowest spending candidate in his or her party cohort. *Relative Spending*, is in 2007 euros. The total number of observations per regression is the sum of the nmber of incumbent and challenger dyads.
*$p$ < .10. **$p$ < .05.

range on the explanatory variable causes incumbent spending effects to increase and the two effects to converge. The estimates in Finland's OLS2–OLS5 are not so unstable as to suggest excessive influence by high-leverage observations. It is interesting that they are also very similar to the Irish estimates: The average of OLS2–OLS5 indicates 95 votes per €1,000 for challengers (or €10.53/vote) and 75 votes per €1,000 for incumbents (€13.33/vote).

The cross-country similarity in the spending-votes relationship allows us to pool the two data sets and evaluate H$_1$ with a stronger test. The estimates using the pooled data exhibit greater precision, but they remain statistically

insignificant. There is one exception—the regression that allows spending differences up to €15,000 (OLS1). This has two possible explanations: either it is an artifact of admitting too many *bad* observations (i.e., dyads with overly large spending differences) or it is the consequence of using a *larger* number of (good) observations. There are already grounds to suspect the former: The regression predicts negative spending effects for incumbents. Another way to choose between these two possibilities is to reestimate the regressions with a data set that includes duplicates of each observation. This will increase the statistical significance of the estimates in every regression, but without changing the estimated coefficients or the range on *Relative Spending*. Therefore, if $\hat{\beta}_2$ becomes statistically significant, it will suggest that a small *N* is responsible for the insignificant results in OLS2–OLS5. However, this does not occur—with the doubled data set, none of the regressions show $\hat{\beta}_2$ to be significant at $p < .10$ (results not shown). Even OLS2, which with the doubled data set uses 151 *more* dyads than the original OLS1, shows a standard error for $\hat{\beta}_2$ of 0.059, which is too large to conclude that challengers enjoy larger returns to spending. Therefore, it is not the number but the type of observations that makes $\hat{\beta}_2$ significant in OLS1, which further illustrates a point that has been made repeatedly: the inclusion of large, unexplained spending differences causes spending effects models to overestimate the difference between incumbents and challengers. Of course, it remains possible that larger data sets would show statistically meaningful differences in the two spending effects. However, note that a larger data set would allow not only more statistical power but also a *better* test of $H_1$ because we can further reduce the range on *Relative Spending*. If the results shown here are any guide, this would only cause the two spending effects to further converge.

## The Results Compared

A full cross-country comparison of spending effects is outside the scope of this article, but since we have estimates for several countries at once a brief comparison is an interesting digression, even if we can only speculate about the differences. Table 4 provides the country-averaged estimates noted above, with Brazil's converted to 2007 euros. The comparison suggests that spending is most effective in Brazilian elections; however, once purchasing power parity is taken into account, it is likely that Brazilian candidates pay *more* per vote than Irish or Finnish candidates. Even so, the relative weakness of spending in Brazilian elections is striking given the oft-noted weakness of Brazil's parties,[26] which ought to diminish the attachment of voters

**Table 4.** The Cost of a Vote Compared (in 2007 euros)

|         | Challengers | Incumbents |
|---------|:-----------:|:----------:|
| Brazil  | 4.12        | 6.73       |
| Ireland | 8.20        | 14.08      |
| Finland | 10.53       | 13.33      |

Brazil's estimates obtained via MM-estimation; Ireland's and Finland's via ordinary least squares.

to certain candidates and in turn increase the effectiveness of campaign spending. To explain the high cost of Brazilian votes, we might point to the types of strategies used to woo voters and the large number of votes that candidates need to secure victory in Brazilian districts.

The similarity between the Irish and Finnish estimates is also striking. But on reflection, that is perhaps not surprising given that Irish and Finnish winners tend to collect similar numbers of votes and that challengers in both countries tend to be comparable to incumbents in their precampaign familiarity (at least within their districts). The vote management sometimes exercised by Irish parties may account for why the Irish estimates show the smallest returns to incumbent spending and the largest difference between incumbents' and challengers' spending effects.

## Conclusion

In elections that use forms of candidate-centered forms of PR, challengers' spending is not more effective than incumbents' spending. Large spending effects may accrue to some candidates, most of whom will be nonincumbents. But in PR elections, many challengers reap only modest returns to spending because, like incumbents, their potential to attract new supporters is limited. As a result, campaign spending limits disadvantage only a small subset of challengers. This does not mean that spending limits are no cause for concern—when any potentially popular challenger is handicapped by campaign finance laws, electoral competitiveness suffers and voters are denied the chance to hear other points of view. But PR elections are already flush with competition and points of view. And, especially with the new types of media that increasingly characterize today's campaigns, clever candidates can often find ways of reaching potential new supporters on the cheap. Therefore, limits on spending or contributions are sensible methods to control some of the problems of money in politics, even if they are difficult to enforce.

Still, the problem could be ameliorated with a public financing system to subsidize candidates' campaigns. In poly-candidate elections, subsidies for each candidate can quickly add up to a significant state expense, but the state need not fully subsidize each candidate's campaign to allow potentially popular candidates the chance to build popularity. Ireland already has such a system. It reimburses campaign expenses up to €8,700, but only for candidates who obtain a reasonable number of votes—specifically, one fourth of the electoral quota at some stage of the count. For elections with larger districts and more expensive campaigns, another approach could be to create a precampaign contest to award subsidies to some challengers (see Ackerman & Ayres, 2002).

Beyond incumbents and challengers, there are several important questions surrounding campaign spending in proportional systems that deserve investigation. For many of these, the methods developed here will prove useful. For example, with some measure to distinguish among various types of challengers, this article's matching-candidates approach could be used to provide a direct test of the connection between spending effects and candidate attributes. Other worthwhile applications may be to ascertain the sensitivity of spending effects to cross-district or cross-party variables. In PR elections, it is possible that factors such as a district's economic environment or a party's ideology have a large and material influence on the effectiveness of campaign spending.

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## Declaration of Conflicting Interests

## Funding

## Notes

1. A *lack* of ideological positions can also limit a candidate's appeal to voters.
2. Indeed, empirical studies that use mostly well-known challengers have found little difference between the two spending effects (Gerber, 1998; Levitt, 1994; Samuels, 2001). Other studies have found little difference on accounting for close versus nonclose races (Erickson & Palfrey, 2000) and marginal versus safe

incumbents (Moon, 2006). In U.S. elections, however, these distinctions may be very similar to distinctions based on challenger familiarity.

3. Specific candidate-centered proportional representation (PR) systems are discussed below. They all allow voters a choice between individual candidates, so individual candidates run their own campaigns. Closed list PR is not one of these systems, as it allows voters only a choice between party lists.

4. This argument emphasizes challengers who have little room for growth, but the incumbent–challenger difference will also decline with the proportion of incumbents that have "challenger-like" growth potential. Under PR, some incumbents may win their first election with a small percentage of the vote and still have considerable room for building electoral support come the following campaign. Using this logic, and noting also that some challengers begin their campaigns with incumbent-like popularity, Samuels (2001) argues that Brazilian Chamber of Deputies elections will feature challengers and incumbents with similar spending effects.

5. Of course, if they were available, experimental data would be preferable. See Jacobson (1990) for an approach using time-varying data.

6. Jacobson (1978) first noticed the phenomenon. Cox and Thies (2000) called it the "Jacobson effect."

7. The Jacobson effect illustrates that a cross-candidate comparison may not yield a good estimate of a spending effect, but a nonnegative correlation across incumbents would not necessarily be much better. Only when all else is equal between the candidates does the correlation capture the effect of spending.

8. Absent a high correlation, "weak instrument bias" can be extreme (Stock & Yogo, 2005).

9. This is perhaps unnecessary for poly-candidate elections, where a candidate's vote can be taken as independent from the spending of the many competitors.

10. Recognizing this problem, Cox and Thies (2000) estimate their two-stage least squares (2SLS) model in two separate stages, with a second stage regression that includes the instrumented spending of both the focal candidate and her or his competitors. The cost of this approach is that the nonsimultaneous estimation biases the standard errors. This is less of a problem when the goal is only a point estimate, rather than a test for differences between two spending effects.

11. A reviewer noted that this approach may be "underpowered" because it restricts the variance in the independent variable. However, this concern is better directed at other approaches: Unless they include controls for all the variables that simultaneously determine both spending and votes, their models will be overpowered (at least for challengers) because they will attribute large vote differences to large spending differences when in fact both stem from omitted variables.

12. Besides party, district, and incumbency status, there are many variables that can affect the vote difference between two candidates. Variables that might appear in a model include celebrity status, terms in other elected offices, and being a relative of a famous politician. Matching by spending is meant to "control" for these and other relevant variables indirectly. (If the data were readily available, these variables could be controlled directly by including them in the matching procedure. However, this would probably greatly reduce the number of matches—i.e., observations to analyze.) It is possible that my matching-by-spending approach does not sufficiently avoid the omitted variables problem because one or more of these excluded variables is systematically related to the small spending differences in the model. If so, however, the likely consequence will be a bias *against* my hypothesis. As mentioned above (and in Jacobson, 1990, and Green & Krasno, 1988), omitted variables are likely to overestimate challenger spending effects because the large differences in spending between various challengers will be taken as exogenous, when in fact they relate to important omitted variables.

13. It is tempting to think that this approach captures a "within-party spending effect" rather than an "across-all-candidates spending effect." But note that the purpose of matching copartisans is to tightly control the comparison of candidates, which helps isolate the effect of spending. It is not to isolate the effect of spending along a within-party dimension.

14. In Brazil there is an option to vote for a list of candidates, an option that has no bearing on how individual candidates perform.

15. In Finland, there is also one single-member district, for the Åland Islands. Its elections are excluded.

16. Still, Benoit and Marsh (2010) contend that challengers will reap significantly larger spending effects, and Benoit and Marsh (2008) argue that the difference is merely an artifact of the unreported use of office resources by incumbents. But there are grounds to reject these accounts. Consider the empirical analyses, which use models that do not separate candidates by anything except spending and incumbency. Among other things, the lack of control variables implies that candidates in parties such as Fianna Fáil and Fine Gael—really the only parties that can hope to elect more than one candidate per district—are identical in electoral strength to the various fringe candidates who run in Irish elections except for their differences in spending. More damaging, these omitted variables (parties) are correlated with spending, causing a severe omitted variables problem. Also, neither analysis demonstrates an effective use of IV-2SLS: the instruments are only weakly correlated with candidates' spending and there are no arguments to establish that the variables used as instruments are unrelated to candidates' votes except through spending. Last, one might question the argument about incumbents' office resources. If we normally observe only a portion of incumbent

spending (with the "office spending" unobserved), then it seems that incumbent spending effects should normally be overestimated—not underestimated—as the analysis will attribute too many votes to each (observed) euro.

17. The Finnish data, compiled by Broberg (2004) and Hyvärinen (2009), are available from the Finnish Social Science Data Archive. They are used here with permission.

18. Brazilian candidates can spend unlimited amounts, but they cannot run their own spots on television or radio (although they usually appear in their parties' state-provided television time). They are also limited in the donations they can accept from business and prohibited from taking contributions from labor unions. In Finland, campaign financing was completely unregulated (aside from the disclosure requirement) through the 2007 elections, the period under study. However, new regulations were put in place after 2008, when it emerged that some 40 candidates, primarily in the largest governing party—the Center Party—deliberately failed to report contributions from a previously unknown organization run by a few business leaders in their 2007 disclosures. The omissions should not systematically affect our analysis, especially since the donations were often the same size. Because we match similar-spending copartisans, missing identically sized contributions within a party will be inconsequential.

19. After the high court ruled that it was unfair for incumbents' campaign use of Dáil office resources to not count toward their spending limits, it was mandated that incumbents report their office spending. However, office spending does not yet apply toward incumbents' spending limits. No candidate exceeded his or her spending limit in either 2002 or 2007 except as a result of office spending. In 2002, the limits were €25,394.76, €31,743.45, and €38,092.14 for three-, four-, and five-seat districts; in 2007, they were €30,150, €37,650, and €45,200. Candidates' spending limits are reduced if they cede part of their allowed spending to their parties, and parties can spend only what has been ceded to them by their candidates. For comparability with Benoit and Marsh (2010), we take a candidate's campaign spending to be the sum of a candidate's normal spending, a candidate's office spending, and whatever a party spent on his or her behalf (seldom more than a small amount, and usually nothing at all), which is disclosed in the party's filing.

20. This drops 906 candidates, only one of whom was elected.

21. For Brazil, candidates were coded as incumbents if they were elected in the previous election. Because of retirements and substitutions, some Brazilian "challengers" may have been serving as deputies in the term before the election. For Finland and Ireland, candidates were coded as incumbents if they served in the previous parliament, a small number of whom had been elected in by-elections since the previous general election. In each country, some "challengers" served legislative terms prior to the immediate term.

22. Estimates did not show statistically significant differences between the two spending effects for any of the six individual elections.
23. Data and replication files are available from the author.
24. Three extreme outliers were dropped from the Finnish data.

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