Estimating the Returns to Expenditures in Canadian Elections: Evidence from a Regression-Discontinuity Design

Political Science Senior Essay
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I. Introduction

The regulation of political spending remains an enduring target of public and academic attention in the major democracies. Controversy emerges not merely from conflicting normative theories of democracy, but also from deep uncertainty about the impact of spending on election outcomes. Political liberals in the United States, for instance, regularly premise their critiques of court decisions such as *Citizens United v. Federal Election Commission* (2010) on the empirical proposition that spending disparities drive election results, as do defenders of the public financing schemes so prevalent in other leading democracies (Scarrow 2007). And while formal models differ on whether spending improves welfare by more efficiently translating public goods preferences into votes (Coate 2004), or diminishes it by undermining the median voter theorem (Grossman and Helpman 1994), the significance for public policy of either view hinges on the causal relationship between money and votes.

Indeed, despite the importance of the research question, the growing worldwide academic literature on the subject reveals profound disagreement among experts on spending effects. Some authors find that expenditure by both incumbents and challengers raises a candidate's vote share (Green and Krasno 1988; Palda 1994; Gerber 2004). Others argue that spending aids challengers, but is of almost no benefit to incumbents (Jacobson 1978, 1985). Lastly, some—including, most famously, Steven Levitt (1994)—argue that once properly estimated, the influence of spending is nearly zero for both incumbents and challengers. This uncertainty is unsurprising: measuring the causal effect of spending on election outcomes is a thorny undertaking. The data-generating process for both variables is nested within a confluence of other causal relationships. If a candidate's spending level and vote share are simultaneously determined, a simple regression of his vote share on his spending level is likely to be biased. Moreover, any apparent statistical relationship between expenditure and vote share is easily the product, in part, of the effects of a host of unobserved explanatory variables correlated with the regression error. Perhaps the most obvious example is the candidate's quality. Excellent candidates might disproportionately attract both votes and donations (Mebane, Ratkovic, and Tofias 2001)—leading to an upward bias in the estimated effect of spending. On the other hand, a downward bias arises if funds are systematically directed toward weaker districts, and away from areas where an easy victory is expected. Further examples of omitted variables are easy to imagine.

Recent studies have also increasingly wrestled with a second problem: in addition to the threat of bias, researchers must contend with the oft-unobserved heterogeneity of spending itself. A possible explanation for the literature's diverse findings is that different studies measure the ultimate impact of different mixtures of political activity. If a particular budget comprises several distinct forms of expenditure with unequal returns, then *how* this budget is deployed contributes to the overall impact of spending. In the United States, for example, funds spent on communication with voters are roughly three times as effective as the remainder of a candidate's budget (Ansolabehere and Gerber 1994). This fact suggests that researchers need to look toward the underlying mixture of expenditures whose impact is measured, as well as how practitioners might use additional funding under various proposed policy reforms.

In this essay, I develop a regression-discontinuity design to circumvent the endogeneity of spending levels, taking advantage of Canada's wealth of disaggregated data and a natural experiment generated by its public financing system. Regression-discontinuity (RD) designs exploit cutoff points in the rules governing the assignment of real-world treatments to identify treatment effects in the immediate neighborhood of these thresholds (Thistlethwaite and

Campbell 1960). Here, I exploit the following cutoff point: under the *Canada Elections Act* (Elections Canada 2004), federal legislative candidates who receive at least ten percent of the vote in their district receive a reimbursement of sixty percent of their campaign expenses following the election, the bulk of which arrives in the coffers of the local district association of their party shortly thereafter. Under the assumption that candidates on either side of the ten percent threshold are otherwise not systematically different prior to the election, the event of reimbursement generates a "downstream experiment" that permits the unbiased estimation of the treatment effect of this additional spending on other variables (Gerber and Green 2003). Using administrative data on recent elections to the Canadian House of Commons, I first estimate the effect of this additional funding on the party's local spending decisions—both in total and disaggregated by category—during the interval prior to the following election. I conclude that receiving a reimbursement increases the spending level of a local party association dramatically, but that this increase overwhelmingly takes the form of greater overhead, rather than additional contact with the electorate. I then estimate the impact of this spending grant on the party's local electoral success in the following election. The overall impact of reimbursement is negligible, suggesting that the additional overhead spending represents inefficient decisions on the part of local party operatives.

The remainder of this essay unfolds as follows. In Part II, I review the literature on the effects of spending on electoral outcomes. Part III outlines the relevant features of the Canadian system of political financing. Part IV introduces the dataset, offering summary statistics and describing the procedures I followed to construct it from publicly available sources. In Part V, I first present a simple model relating spending to election outcomes. I then develop the regression-discontinuity design, explaining how estimating the conditional expectation function at the cutoff point identifies the spending effect for units in the immediate vicinity of the threshold, while avoiding the biases endemic to existing research. Part VI presents my main results, which I subject to further tests in Part VII. Part VIII concludes, speaking to the validity of my findings and their broader implications for public policy. Throughout, I integrate my analysis with perspectives gleaned from a series of interviews with practitioners.

II. Literature

Ever since the public availability of comprehensive data on political expenditures in the late 1970s, the classic analysis has been an ordinary least squares (OLS) regression of a candidate's vote share on his spending level, or a transformation thereof, typically with a suite of other covariates thought to affect election outcomes. Jacobson inaugurates the genre with his study of elections to the U.S. House of Representatives (1978), yielding the remarkable finding that while the effect of challenger spending is large, the effect of incumbent spending is miniscule—approaching zero, and negative under some statistical specifications. Since Jacobson, scholars of American politics have debated these influential conclusions. Abramowitz's analysis of Senate elections buttresses them (1988), but other authors arrive at a range of divergent results after applying more sophisticated econometric models. Green and Krasno (1988) find substantial and significant spending effects for both incumbents and challengers in the U.S. House after employing lagged spending as an instrumental variable and introducing controls for the quality of the challenger. Gerber's (1998) study of elections to the U.S. Senate mirrors this result: he also exploits an array of instrumental variables, such as candidate wealth, as exogenous determinants of spending. Erikson and Palfrey (2000) account for the simultaneity of spending and vote share by introducing assumptions regarding the relationship between their covariances. They find the spending of both challengers and incumbents to be impactful, but that an incumbent's returns to spending diminish with his time in office. Steven Levitt (1994), on the other hand, uses first-differencing to compare just those pairs of U.S. House candidates who repeatedly faced each other in the same district and thus account for district- and candidate-specific effects, after which his estimates of the influence of spending dwindle to nearly zero for both challengers and incumbents.

Research on spending in other democracies—almost exclusively OLS—parallels this wide range of findings. Early work on the United Kingdom found almost no impact to local advertising spending (Johnston 1983). A later study suggests that a parliamentary candidate who increases his spending level by one percentage point increases his vote share by 0.1 percentage points (Pattie, Johnston, and Fieldhouse 1996). In his study of France, Palda uncovers what appears to be a strong spending effect, and also replicates Jacobson's discrepancy between incumbent and challenger spending (1998). Palda's work on Canada, meanwhile, estimates a return of 0.62 votes for each 1979 Canadian dollar spent by a challenger (1985). Carty and Eagles (1999, 85) reiterate his result, turning up "unequivocal evidence" of a "distinctive" effect using an OLS regression of vote share in the 1988 Canadian election on local spending levels, a finding they reprise when they reanalyze the data using various instrumental variables (2004). Taken together, the bevy of prior studies on American and international spending effects implies—though by no means confirms—an impact, particularly for challengers. Nonetheless, the range of findings is extremely wide. Table 1 reveals the divergence in the challenger's estimated cost per vote, from $5 to well over $100.[2] Without a strong sense of the weight to

[2] Throughout the text of this essay, I convert cost per vote figures to 2004 Canadian dollars as best I can.

assign to each finding, the social planner choosing among alternative regimes of political financing faces an impasse.

Indeed, disparate answers are to be expected, for two reasons. First, to the extent that spending is heterogeneous, each study evaluates a distinct treatment. As I suggest in the Introduction, different forms of expenditure may vary markedly in effectiveness (Ansolabehere and Gerber 1994). Second, variability in the coefficients reported in observational work reflects both modeling uncertainty and sampling uncertainty (Gerber, Green, and Kaplan 2002). Since the corpus of scholarship on spending is almost wholly observational, nearly every previous estimate depends upon the reader's acceptance of strong statistical assumptions for its veracity. We should be particularly cautious with respect to the weight we place on studies that use only OLS, because unobserved explanatory variables may severely bias the findings, even in the presence of suitable covariates. The instrumental variables studies are on sounder methodological footing, but still face the threat of bias because of the difficulty of locating spending instruments that meet the exclusion restriction.[3] Commonly used instruments such as lagged spending and personal wealth might still be associated with unobserved variables, such as a candidate's quality, that undermine their exogeneity.

Perhaps in response to these problems, a field experimental literature has bloomed that evaluates the impact of specific forms of voter contact on a candidate's vote share, such as telephone calls and direct mail. Because of randomization, these studies escape the threat of bias that plagues observational work; because they evaluate specific treatments, these studies avoid the problem of heterogeneous spending. Instead of drawing generalizations from evaluations of indiscrete summations of expenditures, analysts might use a stockpile of experimental

[3] A secondary issue is that the bias arising from a less-than-strong correlation between the instruments and the explanatory variable may be dramatic (Bound, Jaeger, and Baker 1995).

evaluations to predict the effectiveness of various hypothetical budgets, where the overall effectiveness of a budget is the weighted average of its components. Gerber (2004) synthesizes a body of these experiments and concludes that mailings sent by the challenger are effective, but that mailings sent by the incumbent are much less so. A recent study of radio advertising echoes this discovery: nonpartisan messages listing information about upcoming local elections appear to boost the challenger's vote share by raising his profile within the electorate (Panagopoulos and Green 2008). Two recent Canadian field experiments, on the other hand, point in conflicting directions. One study finds that leafleting during a nomination contest has no effect on vote share (Loewen and Rubenson 2007). In another, a Green Party literature drop was effective among voters of high socioeconomic status, although the effect was not significant among the entire treated sample (Brown, Perrella, and Kay 2010). These efforts are encouraging, and further research promises us an increasingly nuanced understanding of the relationship between political advertising and vote share. Yet all such studies face a limitation: if the effectiveness of the remainder of a budget (such as staff salaries, consulting fees, or office expenses) is not tested experimentally, uncertainty persists regarding the overall impact of spending.

This essay extends the tradition of some of the best existing work through careful attention to the twin problems of endogeneity and unobserved heterogeneity. By exploiting a naturally-occurring threshold for reimbursements, the RD design permits the clean identification of both the effect these grants on the spending decisions of practitioners and the downstream consequences of these choices for election outcomes. Computing the ratio of additional dollars to votes produces an estimate of the marginal returns to spending for candidates in the neighborhood of the point of discontinuity.

III. Public Financing of Canadian Campaigns

Several political systems publicly finance campaigns using a reimbursement scheme. Reimbursements based on local vote share are paid in France and Italy, among other European nations, and in the U.S. states of Minnesota and until recently, Connecticut.[4] Canada's system of public financing is unique, however, in combining payments to candidates, not just parties, with a robust system of local constituency associations, so that receiving a reimbursement plausibly increases future local spending. The scheme functions in the following way: after each of the ten national elections since 1979, every federal legislative candidate who wins or receives a particular fraction of the vote in his local district draws a reimbursement of his campaign expenditures from the Canadian federal treasury. The precise rules originate in the 1974 *Election Expenses Act*, and are subject to periodic revision. From 1974 to 1983, the reimbursement threshold was fifteen percent of the vote in each electoral district. The reimbursement's size varied according to a formula that included several variables, including the number of voters, the price of postage, and the geographic area of the district.[5] From 1984 through 2000, the fifteen percent threshold remained, but the size was set at fifty percent of the candidate's total campaign expenses—the sum of polling, travel, salaries, and so forth. Finally, since the 2004 election, the threshold has been lowered to ten percent, and the expense fraction increased to sixty percent. Figure 1 reveals a useful histogram of the magnitudes of reimbursements paid to the candidates of the major parties after the 2004 and 2006 federal elections. The mean reimbursement, among reimbursed candidates, was \$29,235.71, with a standard deviation of \$14,326.41 (n = 1,392).

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[4] In August 2009, Connecticut's public financing scheme was ruled in violation of the U.S. Constitution by the 2nd Circuit Court of Appeals.

[5] For details, see the *Statutes of Canada, 1973-1974*, p. 761-763.

The Canadian system of local district associations also merits further attention. Unusual in their strength and autonomy, the parties' local associations account for the bulk of Canadian grassroots political activity, both during the six-week campaign period and in between elections (Carty and Eagles 2004). These local district, or "riding," associations solicit volunteers and contributions, nominate the local candidate, and spend heavily. Table 2 reveals several summary statistics for the finances of these organizations. These totals—nearly $30,000 per year, for districts with an average population of 110,000—may appear small in light of the recent explosion in political spending in the United States. But they are closer to spending levels in elections to the U.S. House of Representatives during an earlier era. In 1972, candidates spent an average of about $500,000[6] (Ansolabehere, Gerber, and Snyder 2001) in legislative districts averaging four times the size of Canadian ridings. These figures are also at par with spending levels in other advanced democracies, such as the United Kingdom, where spending levels average about $20,000 per district in districts of a similar population to Canada's (Pattie, Johnston, and Fieldhouse 1996).

This paper's identification strategy relies on the following fact: after a candidate is reimbursed, he returns his surplus campaign funds to his local association to be spent over the years to follow, sometimes less a small tax to the national party. The local association uses the funds in the intervening years to support grassroots political activity. In order to explore the potential for these funds to affect election outcomes, I asked several politicians and district leaders to describe the role the associations play in election campaigns. They told me there is little distinction in practice between campaign activities that formally endorse a candidate and association activities that focus on building local support for the party. Speaking of his district association, for instance, an anonymous New Democratic Party leader in Ontario explained: "we

[6] This figure, as well the others in this section, is inflated to 2004 dollars from $330,000 (1990 dollars).

can literally have a campaign event during a campaign that was just 'building membership.'" Leaders also identified reimbursement as a crucial variable driving the vigor of an association's activities. In the words of a Conservative leader in a district where the Conservative Party received 9.9% of the vote during the 2008 election, for example, "it [not receiving reimbursement] has left us without any room to maneuver or put on any activities within the associations…you find that you're not as organized." In short, the structure of the Canadian political system supports the hypothesis that receipt of a reimbursement after one election increases the local association's spending level during the years before the next election.

IV. Data

The quantitative data analyzed in this paper are administrative records collected by the Canadian federal government. *History of Federal Ridings since 1867* (Parliament of Canada 2010), a website maintained by the Canadian Parliament, contains information on the results of elections for Canada's House of Commons. I collected data for every candidate from each of the ten elections from 1979 to 2008, which I coded by party, district, province, year, vote share, and absolute number of votes received. For each case, I matched the party and district in one election with the party and district in the succeeding election, leaving nine pairs of elections. Redistricting occurs in Canada about every ten years; during the time period covered by my dataset, redistricting has occurred in 1985, 1996, and 2003, leading me to drop pairs of elections bridging these years. Six pairs of elections remained: 1979 to 1980, 1980 to 1984, 1988 to 1993, 1997 to 2000, 2004 to 2006, and 2006 to 2008. Table 3 offers informative summary statistics regarding the results of recent elections.

Within pairs, I removed observations lacking a party affiliation, those designated as "Independent," or those affiliated with the "Rhino" party, a spoof party that often runs multiple candidates in one district. Using *History of Federal Ridings* listings of the dates and results of special elections, I dropped candidates who did not serve a full term. I also eliminated the handful of candidates who won an election but did not meet the reimbursement cutoff, because under Canadian election law, the winner of an election draws a reimbursement regardless of vote share. On several occasions, two parties merged, or a party's name changed; in these situations, I recoded the party's earlier name as its later name. On about 150 occasions during my time period, districts' names were changed by Parliament between elections, even as their boundaries remained unchanged, a problem I resolved by constructing a correspondence table from the legislative history of the House of Commons (Parliament of Canada 2010). Lastly, I inspected the dataset to locate districts that could not be matched between elections. In approximately 30 of these situations, the failure to match was a product of misspellings, which I corrected. The full dataset contains 8,339 cases.

Elections Canada maintains files with the annual financial returns of each party's local district associations from 2004 to 2008 (Elections Canada 2010). These returns reveal the contribution, expense, and transfer totals of each association. Although associations need not report each specific expense, Elections Canada requires that they list their totals by category: salaries, office expenses, fundraising, contributions to candidates, outgoing transfers, advertising, travel, polling, professional services, bank charges, depreciation, and "other." Rules for disclosure are strict, a fact which should mitigate any worries about measurement error. Each of the 5,387 returns is labeled with the legal name of the party association, from which I extracted the party and district via a series of string operations. From this dataset, I constructed a variable for inter-election spending for each district association. For the interval between the 2004 and 2006 elections, I took the sum of spending in 2004 and 2005; for 2006 and 2008, I used 2006, 2007, and 2008.[7] In each case, I omitted bank charges, depreciation, and outgoing transfers and contributions.[8] Elections Canada also maintains electronic records of the spending level of each candidate during the six-week campaign period (since 1997[9]). I lacked lagged values of association financing because records are only available from 2004 onward, so I matched a lag of local campaign spending and contributions with my dataset instead. All dollar amounts from 2005-2008 were adjusted for inflation since 2004 (Bank of Canada 2008).

Since the availability of spending data from the riding associations is limited, estimating the impact of spending across the entire thirty-year period is impossible.[10] The vote shares of minor party candidates lacking local associations could also not be matched. For these reasons, 1,876 cases remained after merging the collapsed financial records with the voting results. At this stage, the number of cases where a candidate ran in one election, but not the following election, dwindled to a handful, because the major parties usually run a candidate in every district. In these situations, I chose to designate the party's vote share in the following election as zero to capture the effect of its failure to field a candidate in a district.[11]

[7] The discrepancy in coding arises from the fact that the 2004 and 2006 elections were held during the first half of the year, while the 2008 election was held in late October. Unfortunately, local association spending is only reported for each calendar year.

[8] These variables do not plausibly affect vote share within a district, but their removal substantially reduces my standard errors. In exactly one case, the reported total spending level was less than the sum of these variables, producing a negative result. Assuming reporting error, I dropped the case from the dataset.

[9] Campaign spending data for earlier elections are unavailable electronically, but exist in printed, bound form at the offices of Elections Canada.

[10] One *could* study the simple impact of reimbursement on vote share in the following election for the entire sample from 1979 to 2008. When I conducted this analysis (omitted here), my results were consistent with the findings of this paper: under a range of specifications, I was unable to reject the null hypothesis that the effect of reimbursement on vote share was zero ($p < .10$).

[11] Analysis under the alternative practice of removing these thirteen cases does not substantively alter the estimates reported later in this essay.

V. Identification Strategy

Before introducing the regression discontinuity design, I will first conceive of a relation between a local party association's spending and its vote share:

(1) $$V_2 = \alpha + \beta_1(SPEND) + C + u$$

Denote t, $t + 1$, $t + 2$, … as a time series of elections (Lee 2008). Here, V_2 is the party's vote share in a given district during election $t + 1$, α is a constant, and *SPEND* is the spending level of the party's local association between elections t and $t + 1$. β_1 is the partial effect of *SPEND* with respect to V_2, or the marginal effect of an additional dollar of spending. C is a vector of explanatory variables for election outcomes, such as lagged values of the party's local spending and vote share. ε is a disturbance term.

Our goal is to identify β_1, the marginal effect of spending—the rate at which V_2 changes in response to changes in *SPEND*, holding other factors fixed. If we assume that the mean of u is zero conditional on the explanatory variables, we can derive an unbiased estimate of the causal parameter β_1 by regressing sample values of V_2 on *SPEND*—in effect, estimating the relationship between changes in spending and changes in vote share across our entire sample. *SPEND*, however, is likely to be endogenous, so we should not be comfortable with this assumption. Instead, I use an alternative strategy: I measure how a change in *SPEND* known to be unrelated to the explanatory variables leads to a change in V_2. These two changes are exactly those that I hypothesize are caused by the event of reimbursement.

To estimate these changes at the ten percent threshold—where they are exogenous—I estimate the coefficients of *T* within the following two regression models:

(2) $$V_2 = \alpha + [\beta_1 V_1 + \beta_2 V * + \cdots] + \beta_T T + C + u$$

(3) $$SPEND = \alpha + [\beta_1 V_1 + \beta_2 V * + \cdots] + \beta_T T + C + u$$

$$T = 0 \; if \; V_1 < 0$$
$$T = 1 \; if \; V_1 \geq 0$$

Here, *T* is an indicator variable for treatment status determined entirely by the value of V_1, whose score is the sole determinant of *T*. In this application, V_1 is the party's local vote share in election *t*, with ten percentage points subtracted for ease of analysis. $[\beta_2 V * + \cdots]$ leaves room for arbitrary transformations of V_1, depending upon its underlying functional form, as well as the possibility of interactions between these transformations and *T*, where this interaction permits the parameters of the underlying model to vary across either side the cutoff point. Again, *C* is a vector of explanatory variables, and *u* is a disturbance term.

Lee and Lemieux (2009) specify the assumption necessary to draw an unbiased causal inference about *T* from models of this form: the density of units below and above the cut point must be continuous, conditional on the explanatory variables, *C*, and the error term, *u*. Although I do not claim to break new statistical ground, a reprise of their analysis should illuminate important features of the identification strategy. Formally, imagine a data-generating process in which we model *V*, the forcing variable, as a random variable observable in V_1. For simplicity, introduce *W*, one dimension of the unit's identity, as an unobservable random variable, and *G* as the marginal cumulative distribution function of *W*. Let $F(V_1 \mid w)$ be the cumulative distribution function of *V* conditional on *W* and *f* the corresponding marginal density function. Also define

Y(w, v) as an outcome function, and the functions $y^{-}(w) = \lim_{\varepsilon \to 0^{-}} y(w, \varepsilon)$ and $y^{+}(w) = y(w, 0)$ relating the dependent variable to each unit *w* and the value of the forcing variable V_1. The key assumption is that *F* is continuously differentiable in V_1 at the point of discontinuity. Three implications follow:

(4) $$\Pr[W \leq w \mid V = V_1] \textit{ is continuous in } V_1 \textit{ at } V_1 = 0 \textit{ for all } w$$

(5) $$E[Y|V = 0] - \lim_{d \to 0^{-}} E[Y|V = d]$$

$$= \int_{-\infty}^{\infty} [y^{+}(w) - y^{-}(w)] \frac{f(0|w)}{f(0)} dG(w)$$

(6) $$\Pr[C \leq c \mid V = V_1] \textit{ is continuous in } V_1 \textit{ at } V_1 = 0 \textit{ for all } c$$

We derive (4) and (5) from the application of Bayes' theorem to the continuity assumption about *F*.[12] In (4), we have an indication that the identity of units does not systematically differ between the left and the right sides of the threshold. Statement (5) establishes that estimating the conditional expectation function at the threshold is equivalent to the average of the impacts of interest for each unit *w*, $y^{+}(w) - y^{-}(w)$, where this average is weighted by a unit's probability of appearing close to the cutoff point. The intuition behind this weight is clear: the marginal density function for *V*, evaluated at the cutoff point $V_1 = 0$, will be low for individuals far from the cutoff point. Lastly, (6) is a generalization of (5) to all remaining baseline characteristics *C*.

These statistical properties imply that a properly executed RD design enjoys two important features. First, although it is tempting to conceive of an RD estimator as informative only about a treatment effect only at the point of discontinuity—and, accordingly, as promising

[12] Formal proofs of these propositions are available in Lee (2008).

very limited external validity—it is more accurate to state that the RD design estimates a weighted average treatment effect, where this weight is a function of a unit's proximity to the point of discontinuity. Second, the baseline characteristics of units will not differ, on average, across either side of the discontinuity. Just like the experimentalist, the RD analyst is free to include these controls with an eye toward greater statistical efficiency, but choices about the inclusion or exclusion of covariates will not lead to bias. Moreover, researchers are free to test this implication—and, by extension, the continuity assumption—by comparing these baseline variables across the cutoff point. An example is Caughey (2009), who casts doubt on the identification strategy of Lee's earlier RD analysis of the incumbency advantage in U.S. House elections (2008). Caughey discovers that the baseline covariates of the candidates in Lee's dataset are not balanced across the threshold of victory, perhaps because incumbents enjoy disproportionate influence over official recounts.

More generally, we may ask: in what types of situations is this continuity assumption breached, and can we expect it to hold in the application of the Canadian public financing system? In nearly every real world setting, individuals possess some control over the forcing variable; candidates, for instance, could conceivably put forth greater effort as polls show them nearing a significant cutoff in V_1. By itself, this phenomenon does not invalidate the RD design. Instead, the continuity assumption is violated in any situation where individuals possess *precise* control over V_1, but holds if individuals possess imprecise control. Lee (2008) formalizes this notion: if $V_1 = Z + e$, where Z is a systematic component and e is drawn from a continuous random variable, F remains continuously differentiable at the cutoff point, and RD inferences are on solid footing. In any particular RD application, then, the analyst is left to establish the appropriateness of the design by arguing that e is always substantial. In the case of Canadian parliamentary vote shares, e is likely to be large. It is particularly implausible that candidates are able to sort themselves precisely around the ten percent cutoff during election t. Candidates with vote shares in this neighborhood are overwhelmingly challengers, so there is no room for political manipulation, as in the United States. Moreover, in the neighborhood of $V_1 = 0$, random factors such as weather, counting errors, and other election day dynamics are likely to play a major role in determining final vote share.

Estimating the OLS partial effect of a reimbursement indicator variable, then, also estimates the *causal* effect of reimbursement on the dependent variables, just as in a randomized experiment. What might these variables be? My interviews with practitioners suggested both several effects. First, the reimbursement could by itself boost political support, regardless of its size, by instilling supporters with new resolve. Second, reimbursement has a dramatic effect on an association's finances in several ways. Not only will reimbursed associations receive larger surpluses from their local candidates, but according to an anonymous leader of a Conservative constituency in Newfoundland, the failure to receive a reimbursement may reduce an association's ability to receive aid from other associations:

> I think it will make it more difficult to get help from other associations and the main reason for that is that they target districts that have a fairly good chance of winning. If they write off your district, if they say 'Why give money to a seat that isn't going to win anyway?' It's a psychological barrier… Riding associations next time around are going to be very reluctant to loan money to an association that may not get its ten percent.

Cutting against this phenomenon is the possibility that districts unexpectedly flush with cash might give those funds to other, weaker associations. Similarly, an unreimbursed association might solicit additional private contributions to make up the shortfall; at the same time, individuals might be less likely to contribute to an association they perceive as a failure. Although the magnitude of the overall effect on spending levels is unclear, it still seems likely that reimbursement will boost the association's overall spending level, and perhaps the local candidate's spending level. Changes in spending levels, meanwhile, could affect electoral outcomes.

It is worth specifying that this paper's exploitation of reimbursement-induced spending as a downstream experiment to measure the effect of spending on election outcomes depends on a further assumption about this causal chain: that none of these intervening variables, *besides* the association's spending level, influence election outcomes. If these variables influence election outcomes, then the total effect of reimbursement would no longer be identical to the partial effect of this additional spending. Is it the case that reimbursement has *no* downstream effect on other intervening explanatory variables? Although this assumption is probably strictly untrue, the deviation from it is likely to be small. During my interviews, practitioners dismissed the idea that the psychological component of reimbursement matters to ordinary voters because few voters are even familiar with the reimbursement system. In principle, reimbursement could boost the local candidate's spending level during the six-week campaign period if associations donate directly to his account, but this appears not to occur.[13]

A final issue remains: how should the analyst specify the regression? The researcher must first decide upon the "bandwidth" of the regression—which data points to include or exclude. One would be suspicious of findings that depended heavily on information about cases far from the discontinuity. Yet there is a trade-off: while a tight bandwidth reduces bias, such a window also increases sampling variability, perhaps to the point where the results are uninformative. Second, the underlying functional form of the estimated model is unknown. In some applications, the relationship between the forcing variable and the dependent variable is clearly

[13] I have separately produced regression-discontinuity estimates of the impact of reimbursement on campaign spending in the following election and have found the influence to be both statistically insignificant and substantively miniscule. In interviews with practitioners, this was expected: there are restrictions on an association's contributions to its local candidate, and a large fraction of candidates spend close to their maximum so as to obtain the largest possible reimbursement.

linear. In others, it may be curvilinear. In still others, it may be impossible to adequately reproduce the underlying model that generated the data. Third, one must make a decision about weights. It seems intuitive that observations close to the cutoff point should bear more on the final estimates than those farther afield, but there is no obvious basis for this kernel, and many plausible specifications present themselves. This range of possibilities recommends that the researcher explore several possible models, so as to instill greater confidence in the recovery of the true causal effect.

VI. Main Results

To motivate the quasi-experimental analysis, I first estimate the parameter of interest using traditional methods. Suppose we were to assume, as basic OLS analyses implicitly do, that the contents of u, the disturbance term, are independent of the explanatory variables. We would conduct a regression of vote share on riding association spending, perhaps with controls for several observables, such as lagged spending. Table 4 presents the results of this regression. There is a large, highly significant ($p < .01$) partial effect for riding association spending—in levels as well as under a logarithmic transformation. With covariates, we would expect an additional $10,000 in local association spending to boost vote share by 0.27 percentage points, for a cost per vote of roughly $70.[14] The literature on political finance in Canada supports a similar prior belief. Carty and Eagles (2004) declare:

> It seems evident that the financial health and capacity of local party associations has a measurable and significant impact on a party's local electoral fortunes. Even within the framework of province-wide election campaigns centered on party leaders defending or

[14] The average number of votes cast per district is about 50,000. An 0.27 percentage point increase in vote share, then, is equivalent to about 140 votes, which leads us to an estimate of $71 per vote.

> attacking a government's record, local parties can win votes by spending more. Money matters for grassroots political organizations and their electoral success. (p. 572)

But as I have argued, the procedures that have led researchers to this conclusion in the past are insufficiently rigorous. Since our credence in the OLS results is justified only by unverifiable assumptions, we must look to an alternative identification strategy.

Tables 5 through 10 report the results of estimation using the regression-discontinuity model. Three data-analytic choices here deserve attention. First, I limit most of my analyses to cases where the local candidate spent more than $20,000 in the preceding election, corresponding to a reimbursement of at least $12,000. This practice eliminates 714 of the 1,836 cases in the dataset. A subset makes sense because small reimbursements do not plausibly affect spending, but introduce undesirable statistical noise and render the quasi-experimental strategy I outline in Part V more difficult. Although the $20,000 cutoff is arbitrary, this arbitrariness will not lead to bias. Because the spending level of the association's candidate in election *t* is determined prior to his vote share, it is not systematically related to reimbursement status for candidates in the neighborhood of the ten percent threshold. I apply this subset to both the vote share estimates and the spending estimates. Second, following Green and Krasno (1988), I model spending as the natural logarithm of total spending plus a constant—here $1,000.[15] This transformation is attractive because it allows the returns of spending to diminish: a $20,000 reimbursement might be just as beneficial to a weak organization as a $40,000 reimbursement to a stronger one. Third, to increase statistical precision, I include several covariates in the regression model: lagged levels of campaign spending and contributions, a year dummy, and dummies for parties, provinces, and party-province interactions. Although I save space in my

[15] A constant is typically used here in the spending literature because of the problem of very small values: one would not expect an increase from, say, $10 to $11 to increase vote share in any meaningful sense. Experimentation with several other small constants (omitted here) did not substantively affect these results.

tables by omitting their coefficients, the lagged financial variables enter with the expected sign. I exclude district population,[16] the number of parties fielding a candidate, incumbency status,[17] and party-province-year interactions because their inclusion fails to enhance the model's fit.

Imbens and Kalyanaraman (2010) have devised an RD algorithm that uses local linear regression and an automated process of bandwidth selection to estimate the parameter on the treatment dummy. The algorithm uses asymptotic theory to strike a balance between bias and sampling variability that aims to minimize mean squared error by taking into account the variability of the dependent variable and the density of the forcing variable. Tables 5 and 8 display the algorithm's regression output regarding the impact of reimbursement on the party's local spending level and vote share in the next election. Each of these tables reports my results in three panels, corresponding to three possible samples: associations whose candidate spent at least $20,000, associations whose candidate spent at least $15,000, and the full set of all associations. The upper panel of Table 5 indicates that reimbursement leads to an increase in spending of about $19,497 for associations whose candidate spent at least $20,000 in the preceding election. This finding is statistically significant ($p < .05$ with covariates, although their omission leaves the result slightly outside the zone of significance). This conclusion reaches even greater significance under a logarithmic specification: reimbursement causes spending to rise roughly threefold ($p < .01$ with covariates, $p < .10$ without covariates). These funds, however, are profoundly ineffective. The coefficient on the treatment dummy estimated with covariates in Table 9 is negative, rendering the local association's cost per vote impossible to compute under my preferred specification. Although this estimate becomes positive under a less-precise

[16] Periodic redistricting ensures that population sizes vary only moderately across ridings.

[17] Moreover, in many of the specifications, the number of incumbent candidates is zero; parties with a vote share of about ten percent are overwhelmingly challengers.

alternative specification without covariates, the coefficient remains insignificant, even at the ten percent level.

As I anticipate in Part V, however, I will investigate several other potential models. Indeed, although the appeal of RD depends upon its analogy to a classical experiment, the affinity between RD designs and experiments belies an essential distinction. Whereas a classical experiment demands few substantive assumptions of the researcher, the RD analyst can choose among a multitude of possible econometric specifications. Like any observational study, the true standard error of his point estimate is the sum of the standard error arising from sampling uncertainty and that arising from modeling uncertainty (Green et. al. 2009). By itself, a determination of statistical significance achieves little more than informing the reader that an author can locate at least one specification that supports his pet conclusion. For this reason, I first explore the sensitivity of my findings to the chosen subset. The lower two panels of Table 5 reveal my estimates of the impact of reimbursement on spending for the sample of associations whose candidate spent at least $15,000, as well as the full sample of all associations. Using a $15,000 cutoff, the results hover at the border of significance ($p < .05$, two-tailed test), but remain qualitatively similar. In the estimates produced from the entire sample, the effect disappears in levels, but remains significant in logarithms. This pattern should not surprise us: the full sample includes outlying associations whose candidate may have spent very little on his campaign. For these low-spending organizations, receipt of a reimbursement can lead to a large proportional increase in spending even if the absolute size of this increase is small.

Second, I explore, in Tables 6, 7, 9 and 10, the sensitivity of the main propositions of this paper to alternative bandwidths and functional forms, focusing here on the subset of associations whose candidate spent at least \$20,000 in election *t*.[18] Table 6 estimates the parameter of the treatment dummy, *T*, on the natural logarithm of the local association's spending level during the interval prior to the next election at the IK optimal bandwidth *h*, across several possible polynomials up to the quartic functional form. Table 7 reports the results of this same range of specifications for a bandwidth of twice this size. The impact on spending is highly significant ($p < .01$) across nearly every specification in each table; even the lowest estimate, 0.772, implies that reimbursement causes spending to nearly double. Tables 9 and 10 reveal the robustness of the lack of impact on vote share to a similar array of specifications. The coefficients hover around zero and are frequently negative; under no modeling choice is the impact of reimbursement on vote share significant, even at the ten percent level.

Two other steps should raise our confidence in these findings. First, although graphs alone cannot confirm an effect, Imbens and Lee (2009) suggest that a plot of the forcing variable against the dependent variable is a useful means of examining a putative discontinuity. Figure 2 displays such a plot with the logarithm of spending on the *y*-axis with a window of ten percentage points on either side of the threshold, exposing a jump in the overlaid fourth-degree polynomial at the ten percent threshold. Lest readers think this discontinuity is an artifact of the chosen subset, Figure 3 reveals the same plot for the entire sample of associations; a similar, if slightly muted, gap appears here as well. Figure 4 exposes a similar pattern with just those points within the Imbens-Kalyanaraman optimal bandwidth, each weighted by a triangular kernel. Figures 5, 6, and 7 plot vote share in election $t + 1$ on the *y*-axis against vote share in election *t*. Again, first the sample is limited to the subset of cases eligible for a large reimbursement, then includes across all cases, and then limited to those points used to produce the main results, sized

[18] Following Imbens and Kalyanaraman, I employ a triangular kernel throughout. The alternative of a rectangular kernel (not shown) produced qualitatively similar results.

by their kernel weights. No appreciable discontinuity appears on any graph. Second, Imbens and Lee suggest, as a robustness check, that the analyst regress *pretreatment* variables on the forcing variable; the RD method should fail to identify a treatment effect. Table 11 reports the results of this test using lagged values of several financial variables; none of the estimates are significant ($p < .10$).

Finally, even though the results discussed so far are modestly robust, readers must still make sense of the differences among specifications. I offer two suggestions. First, after viewing the noisy data of Figures 2 through 10, it should not surprise us that the largest differences from the preferred specification emerge following the omission of covariates. Because lagged spending levels and province-level political phenomena might reasonably diverge across the discontinuity in our sample, even if they do not diverge on expectation, relying on a less efficient estimate that fails to exploit these variables makes little sense. Second, among the remaining estimates, the output of the IK algorithm merits greater weight than those produced using a wider bandwidth or polynomial functional form, for two reasons. First, an automated procedure minimizes the chance that a researcher's preference for a particular conclusion will distort the process of model selection—consciously or unconsciously. Second, our willingness to base conclusions on predictions about data far from the cutoff point should depend on our confidence regarding the form of the underlying population model. In the extreme case where the researcher holds full information about the correct specification, for example, any bandwidth less than the entire dataset would waste statistical efficiency for no gain. That this application offers us little reason to endorse one specification over another argues in favor of a non-parametric model that enables us to draw our inferences chiefly from units close to the point of discontinuity, such as the Imbens-Kalyanaraman algorithm. Taken together, the full array of specifications thus confirms the basic finding that reimbursement boosts the spending level of the local party association—as expected—but has almost no effect on the vote share of the local candidate.

VII. Further Tests

As I have argued, different expenditures may differ in their effectiveness. To explore the importance of this fact, I produced separate RD estimates for spending, disaggregated by the categories with which I label specifications (1) through (8) in Table 12. Associations appear to put most of the additional funds toward administrative costs and little of them toward engaging with and energizing voters. The point estimates on transportation, salaries, and office expenses are several thousand dollars each; the coefficients for travel expenses ($p < .01$) and office expenses ($p < .05$) are each statistically significant. In contrast, none of the remaining estimates are significant, and the point estimate for advertising—the only form of spending whose effectiveness is supported by field experiments—is negative. I also compute, as a crude measure of total overhead, the sum of transit, salary, and office expenses, as well as the natural logarithm of this amount plus $1,000. Specifications (9) through (12) report RD estimates of the impact of reimbursement on this measure. The effect is highly significant and comprises the bulk of the additional spending.

The predominance of administrative expenditures is quite robust to modeling decisions. This finding still holds when covariates are excluded in specifications (10) and (12). Moreover, Tables 13 and 14 confirm that it persists across an assortment of polynomial forms and data windows: in logarithms, the increase is at least twofold under even the most conservative specifications, which produce a coefficient of about 0.7. Graphs support this conclusion as well.

In Figure 8, I plot the logarithm of overhead spending in the neighborhood of the ten percent threshold, exposing the expected discontinuity. Figure 9 reveals the same pattern in the full sample of associations, although the effect is perhaps diluted by the presence of associations receiving very small reimbursements. Figure 10 displays exclusively the data points within the Imbens-Kalyanaraman bandwidth, sized by their weights. Overwhelmingly, local party operatives elect to deploy the additional funds toward infrastructural expenses rather than engagement with voters.

It is worth pausing for a moment over this discovery. As Ansolabehere and Gerber (1994) observe, much political spending is administrative, and administrative spending appears to be markedly less effective than other forms. My results extend this conclusion to Canada and show its robustness to a new identification strategy. Why might the local leadership of a party decide to spend a reimbursement so inefficiently? Three possible explanations present themselves. First, it is conceivable that larger offices and staff rosters might actually be the most efficient means of translating dollars into votes. Mailings or television spots are likely to be forgotten well in advance of an election. Meanwhile, greater spending on salaries and transportation costs may reflect the costs, not just of overhead, but also of a more rigorous procedure for selecting a local candidate and deploying additional canvassers or volunteer coordinators on his behalf. This explanation, however, seems unable to account for the magnitude of the findings. Reimbursement appears not to translate into *any* increase in advertising or fundraising activity, even though several field experiments—including at least one in Canada—suggest that voter contact is an effective means of increasing vote share. Meanwhile, this study's results suggest that the marginal decision to spend on infrastructure is spectacularly ineffective. A second explanation, which likely contributes to my findings, is that local operatives are uninformed about the effectiveness of their own spending decisions. (Not one practitioner predicted the null effect of the additional spending when I asked about the likely impact of reimbursements on election outcomes, although several said they were unsure.) The third—and most persuasive—explanation is the emergence of a principal-agent problem between local leaders and the local candidate in the presence of asymmetric information and diverging objectives. If local leaders stand to benefit financially from a salary increase, for example, it is unsurprising that they would allocate a large fraction of the reimbursement grant in this direction.

Finally, I attempted to test one other hypothesis. Might it be that the spending is effective, but other parties respond to reimbursement via strategic mobilization, blunting this effect? If other parties divert financial or volunteer resources to districts where opponents were reimbursed, then the event of reimbursement may increase the party's local vote total, even if this event fails to affect its vote *share*. To explore the potential for this form of strategic behavior, I asked an anonymous Green Party leader whether this pattern occurs. She said it was not the policy of the Greens, although she speculated that "other parties may think like that." Indeed, this hypothesis appears not to be true. In specifications (3) and (4) of Table 8, I report the estimates of the RD model with votes cast for an association's local candidate as the dependent variable. The coefficient on the treatment dummy is never significant. The event of reimbursement—and the overhead expenditures it leads to—buys neither votes nor vote share.

VIII. Conclusion

In sum, this essay leads us to two important conclusions. First, I use an RD design to generate a quasi-experimental estimate of the average impact of campaign reimbursements to local party associations in Canada on their spending level in the following election, finding it to be about $20,000. Remarkably, these grassroots political practitioners elect to spend the bulk of these additional funds on overhead, such as their own salaries, rather than on voter contact. Second, the estimated effect of this spending on vote share is not significantly different from zero, and in fact the coefficient is negative.

These findings are the product of two methodological innovations. First, my usage of RD to measure the total effect of an increase in campaign spending in a natural setting is, to my knowledge, novel. The methodology represents a signal advance over alternative identification strategies, such as OLS or instrumental variables estimation, whose conclusions are subject to serious methodological criticisms. It is instructive to compare the RD estimate of the ratio I introduce in Part V to the OLS estimate (with covariates) from Table 4. Under my preferred specification, the expected impact on vote share of a grant of $20,000 is zero. For simplicity, compare this prediction to the OLS estimate in levels, where every $1000 in district association spending leads to an increase of .027 in vote share. A grant of $20,000 would lead to an increase of 0.54 percentage points in a candidate's vote share—different, but still well within a 95% confidence interval. OLS, with suitable controls, performs adequately in this application. As learners, however, we obtain information about the size of its biases only *after* comparing OLS estimates to those produced using quasi-experimental methods. Before acquiring an estimate that approximates a randomized experiment, our ignorance of the bias would lead us to assign zero weight to the OLS results (Gerber, Green, and Kaplan 2002). As quasi-experimental estimates accumulate, rational researchers and policymakers move from a stance of profound uncertainty to one of bounded uncertainty.

Second, this paper's usage of a disaggregated dataset calls attention to the diverse tasks toward which spending is directed—a phenomenon that often goes unappreciated by authors of both observational studies and experimental studies. Experimentalists such as Gerber (2004) find that voter contact is effective, and extrapolate from this discovery the broader claim that campaign spending is effective. From the finding that the spending of a particular set of candidates is related to their electoral performance in a particular way, observational researchers generalize about the effectiveness of other budgets and other candidates. In either case, authors implicitly assume that practitioners optimize their budgets with an eye toward maximizing the challenger's vote share, but this assumption is erroneous. Comparing this paper's results to a recent Canadian experiment on Green Party voter contact suggests that the gap between the marginal effectiveness of money spent on infrastructure and money spent on voter engagement is enormous (Brown, Perrella, and Kay 2010). Policy decisions that fail to heed this fact may go quite wrong.

The validity of my findings, of course, is subject to critique. The first threat to validity concerns the RD design itself. As I acknowledge in Part V, RD estimates are best understood as a *local* or *weighted* average treatment effect: within the chosen bandwidth, observations close to the threshold contribute more to inference than those farther away, while outside the bandwidth, observations do not contribute to inference at all. As a result, this paper's findings are of diminished validity for candidates more than five or ten percentage points away from the ten percent cutoff. This problem, though, is less severe than it appears. For several important policy questions, such as altering Canada's reimbursement cutoff or evaluating the prospects of public financing schemes and differing contribution limits for political competition, candidates in the neighborhood of ten percent are precisely the population of interest.

A second problem concerns the context of spending. In Canada, spending by a grassroots party organization necessarily takes place in a different setting than spending by the candidate himself during the official six-week campaigning period. Although I have suggested that the activities engaged in by candidates and district associations are quite similar, our willingness to extrapolate our findings to campaign spending as such depends importantly on assumptions about the comparability of these alternative settings. Moreover, the estimates may not be easily generalized to different electoral systems. In particular, they are more readily generalized to other parliamentary systems than presidential systems, such as the United States, where voters are thought to be more sensitive to campaign activity. Nonetheless, we should not overstate this point. If spending is primarily effective through its facilitation of voter contact, and this contact is of similar effectiveness in Canada and the United States (Brown, Perrella, and Kay 2010), it is difficult to imagine why the impact of administrative spending would differ drastically between the two systems.

Lastly, I will trace two implications of my conclusions for public policy. First, my findings about the behavior of grassroots practitioners are by themselves an intriguing examination of how local operatives choose to spend an exogenous grant. Further research is needed on the determinants of the budgets of candidates and party organizations. One possibility is that these decisions depend heavily on the misalignment of incentives between candidates and other practitioners. Another is that staffs choose to spend funds received in block grants in systematically different ways than funds gleaned from individual contributions. In any case, that they choose to spend federal funds so inefficiently argues that public financing systems should be carefully structured to prevent practitioners from spending government grants in wasteful ways.

Second, my results provide strong evidence that increased administrative spending by local party associations offers little net benefit to a local candidate. If this overhead is socially wasteful, this fact supplies an argument for tight limits on spending and contributions, especially because the most consequential activities are attended to with the first expenditures (Gerber 1998). This evidence also counsels the architects of public financing schemes to look carefully at how money is spent. The celebrated advantages—and alleged ills—of these systems may be much smaller than they appear. If funds are primarily deployed to overhead, government grants will do less to enhance the viability of marginal challengers than we might assume. At the same time, there is less threat that public financing systems will exclude those ineligible for their assistance from the political process, such as Canadian party associations whose candidate does not meet the ten percent cutoff, or that incumbents will manipulate the rules of a public financing scheme to reduce electoral competition.

This paper uses a unique Canadian policy discontinuity to confirm the importance of the distinction between spending on voter contact and spending on overhead, a distinction rarely appreciated in the widely divergent literature on political spending. Canadian party operatives spend reimbursement grants overwhelmingly on administrative expenditures, but even enormous increases in these expenditures have small effects on a candidate's vote share. Previous observational work has hinted at similar results for campaigns in the United States (Ansolabehere and Gerber 1994), but this is the first study to estimate the impact of these forms of spending using a rigorous quasi-experimental design. Future research should both explore the conditions under which these expenditures are likely to predominate and offer more precise estimates of their impact on political competition.

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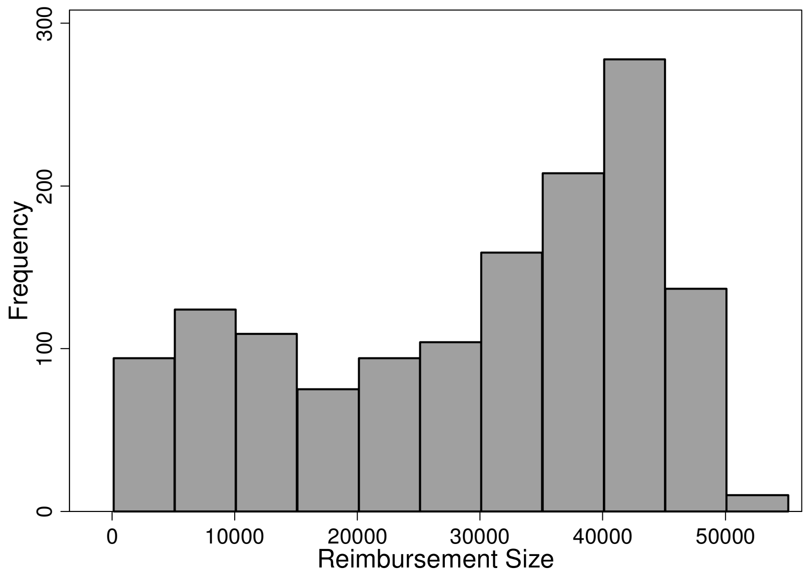
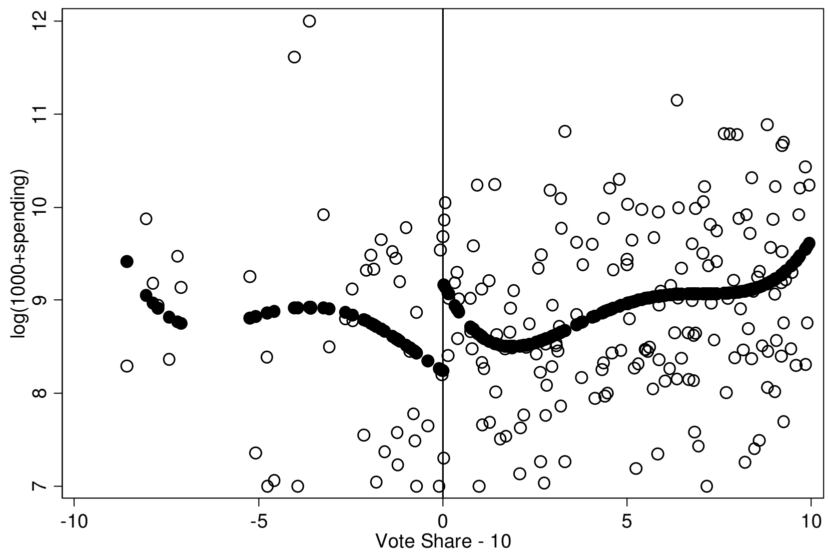
X. Figures

Figure 1: Distribution of Major-Party Reimbursement Sizes, 2004-2006



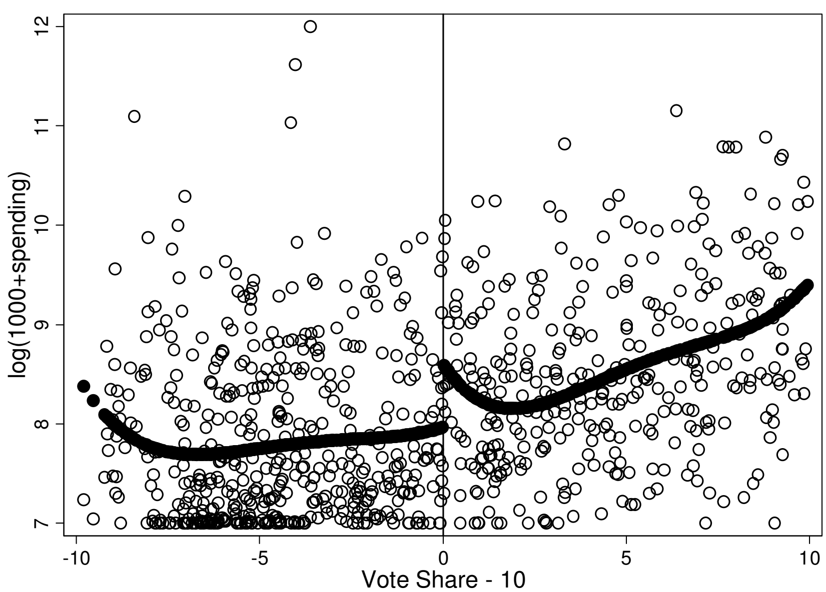
N = 1394. Bins correspond to ranges of $5000. Because the sample is limited to combinations of district and party with a local constituency association, only the Liberal Party, Conservative Party, New Democratic Party, Green Party, and Bloc Québécois are represented.

Figure 2: Polynomial Plot of Log Spending vs. Vote Share, for Associations Whose Candidate Spent at Least $20,000 in Election *t*



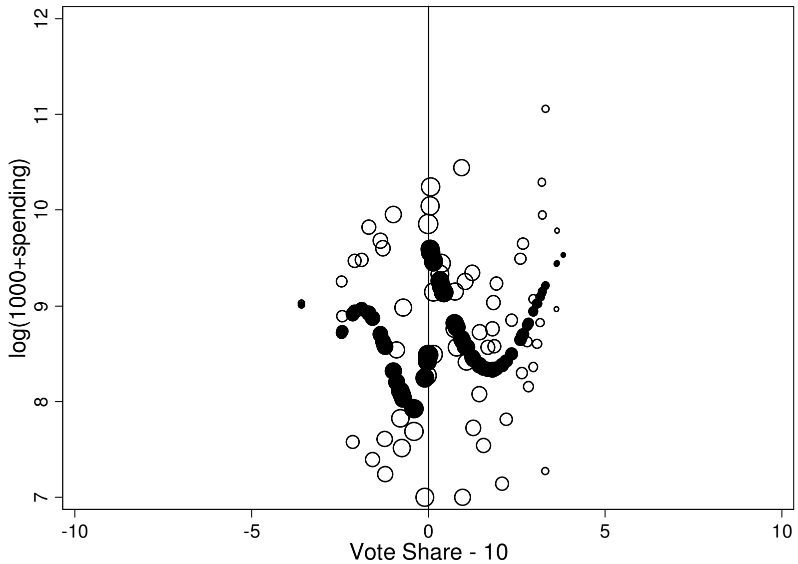
N = 157. *y* is the natural logarithm of association spending during the inter-election interval plus 1000, in 2004 Canadian dollars. Each open circle corresponds to a single observation. Solid circles are predictions from a fourth-order polynomial, estimated separately for each side of the cutoff point.

Figure 3: Polynomial Plot of Log Spending vs. Vote Share, for All Associations



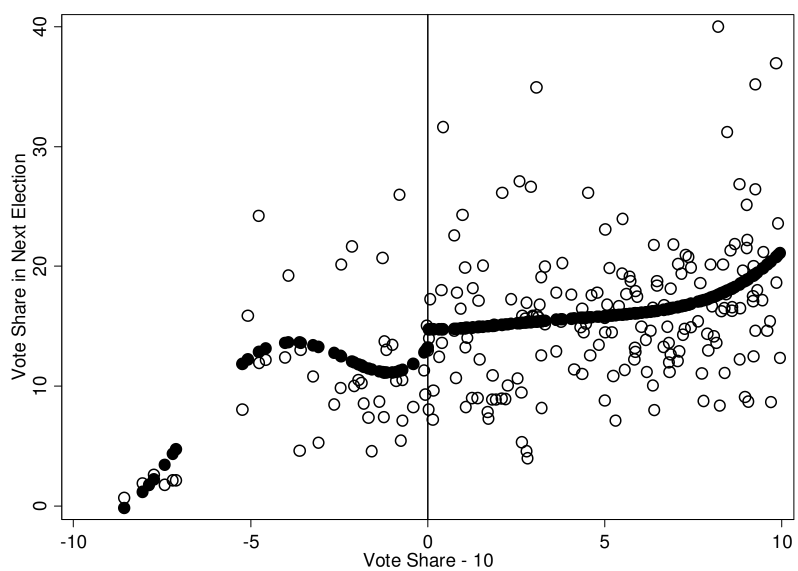
N = 822. *y* is the natural logarithm of association spending during the inter-election interval plus 1000, in 2004 Canadian dollars. Each open circle corresponds to a single observation. Solid circles are predictions from a fourth-order polynomial, estimated separately for each side of the cutoff point.

Figure 4: Weighted Polynomial Plot of Log Spending vs. Vote Share, for Associations Whose Candidate Spent at Least $20,000 in Election *t*



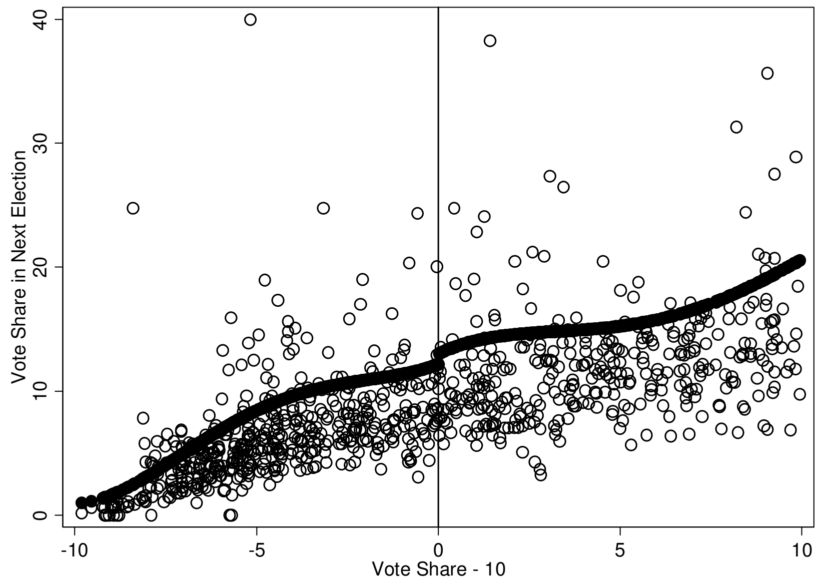
N = 64. The size of a circle corresponds to its weight using a triangular kernel and the Imbens-Kalyanaraman optimal bandwidth of *h* = 3.95. *y* is the natural logarithm of association spending during the inter-election interval plus 1000, in 2004 Canadian dollars. Each open circle corresponds to a single observation. Solid circles are predictions from a fourth-order polynomial, estimated separately for each side of the cutoff point.

Figure 5: Polynomial Plot of Vote Share in Election *t + 1* vs. Vote Share in Election *t*, for Local Associations Whose Candidate Spent at Least $20,000 in the Previous Election



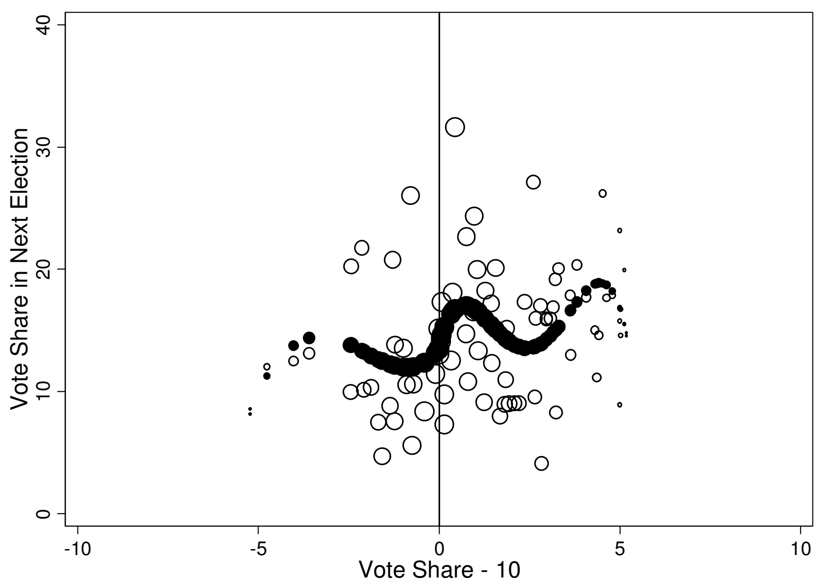
N = 157. *y* is the party's vote share in a particular district during the following election. Each open circle corresponds to a single observation. Solid circles are predictions from a fourth-order polynomial, estimated separately for each side of the cutoff point.

Figure 6: Polynomial Plot of Vote Share in Election *t + 1* vs. Vote Share in Election *t*, for All Local Associations



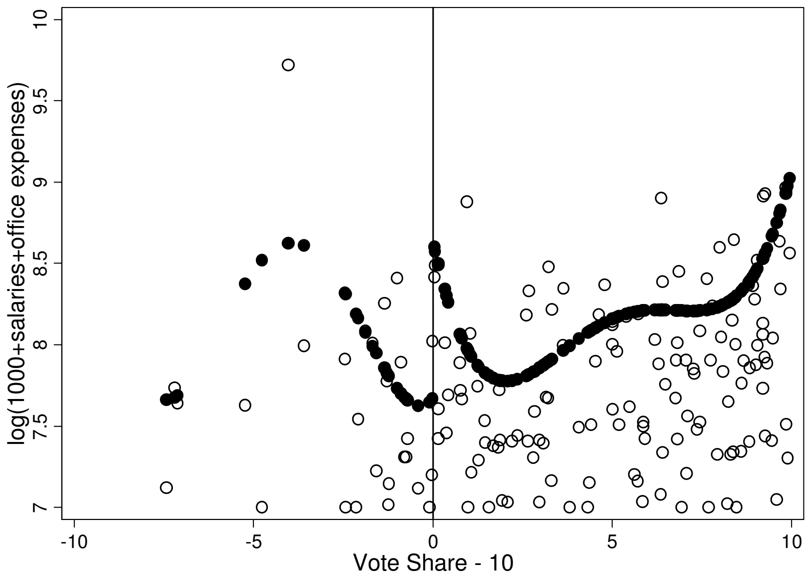
$N = 822$. *y* is the party's vote share in a particular district during the following election. Each open circle corresponds to a single observation. Solid circles are predictions from a fourth-order polynomial, estimated separately for each side of the cutoff point.

Figure 7: Weighted Polynomial Plot of Vote Share in Election *t + 1* vs. Vote Share in Election *t*, for Local Associations Whose Candidate Spent at Least $20,000 in the Previous Election



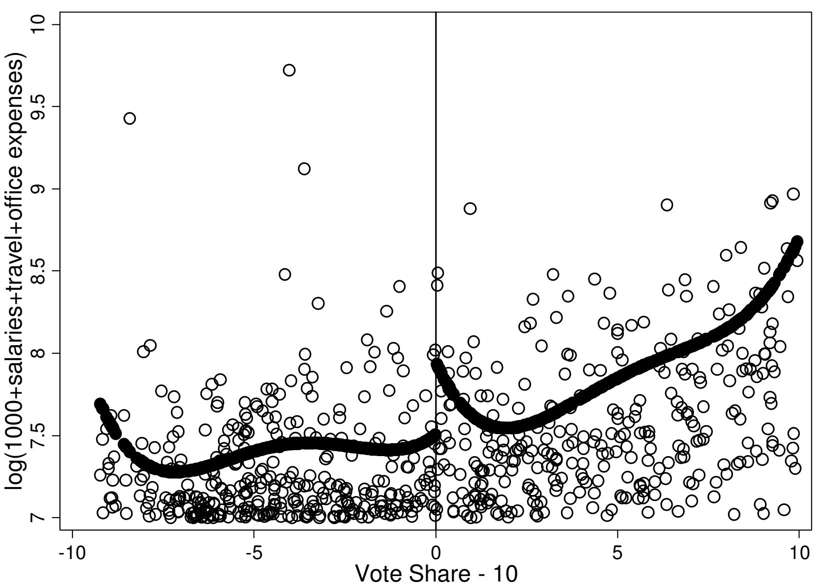
$N = 80$. The size of each circle represents its weight in a triangular kernel centered at zero with an Imbens-Kalyanaraman optimal bandwidth *h* of 5.28. *y* is the party's vote share in a particular district during the following election. Each open circle corresponds to a single observation. Solid circles are predictions from a fourth-order polynomial, estimated separately for each side of the cutoff point.

Figure 8: Polynomial Plot of the Log of Overhead Spending vs. Vote Share, for Associations Whose Candidate Spent at Least $20,000 in the Election *t*



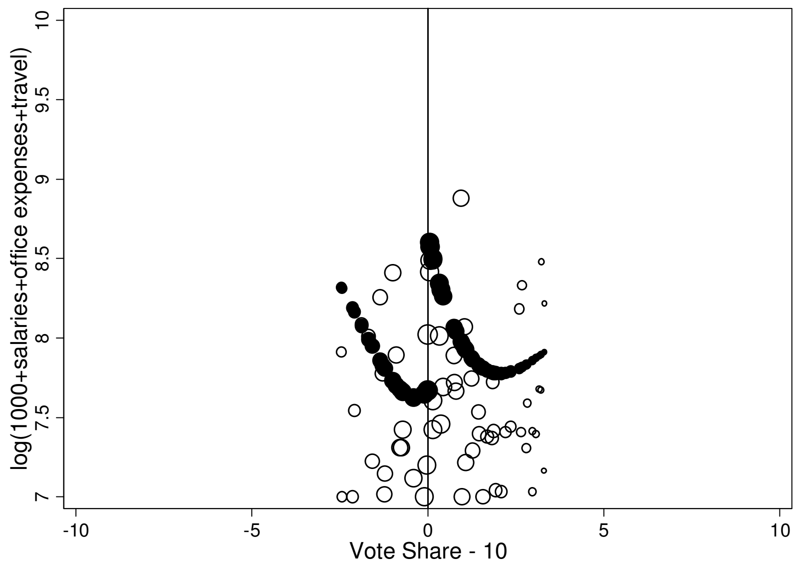
N = 157. *y* is the natural logarithm of 1000 plus the sum of salaries, office expenses, and transit costs in 2004 Canadian dollars. Each open circle corresponds to a single observation. Solid circles are predictions from a fourth-order polynomial, estimated separately for each side of the cutoff point.

Figure 9: Polynomial Plot of the Log of Overhead Spending vs. Vote Share, for All Associations



N = 822. *y* is the natural logarithm of 1000 plus the sum of salaries, office expenses, and transit costs in 2004 Canadian dollars. Each open circle corresponds to a single observation. Solid circles are predictions from a fourth-order polynomial, estimated separately for each side of the cutoff point.

Figure 10: Weighted Polynomial Plot of the Log of Overhead Spending vs. Vote Share, for Associations Whose Candidate Spent at Least $20,000 in the Election *t*



N = 60. The size of each circle represents its weight in a triangular kernel centered at zero with an Imbens-Kalyanaraman optimal bandwidth *h* of 3.56. *y* is natural logarithm of 1000 plus the sum of salary, office, and transit expenses, in 2004 Canadian dollars. Each open circle corresponds to a single observation. Solid circles are predictions from a fourth-order polynomial, estimated separately for each side of the cutoff point.

XI. Tables

Table 1: Prior Estimates of the Amount of Challenger Spending Necessary for an Additional Vote

Study	Cost Per Vote	Office Sought
Jacobson (1985)	$12	U.S. House of Representatives
Green and Krasno (1988)	$13	U.S. House of Representatives
Levitt (1994)	$110	U.S. House of Representatives
Palda (1994)	$10	French National Assembly
Pattie, Johnston, and Fieldhouse (1996)	$5	U.K. Parliament
Erikson and Palfrey (2000)	$24	U.S. House of Representatives
Carty and Eagles (2004)	$8	Canadian Parliament
Gerber (2004)	$24	U.S. Mayor

For consistency with Gerber (2004), figures are deflated, converted, and rounded to the nearest 1998 U.S. dollar.

Table 2: District Association Finances' Summary Statistics, 2004-2008

Statistic	Mean (Std. Dev.)
Outgoing transfers ($)	12,688 (20,582)
Incoming transfers	10,255 (15,746)
Bank charges	156 (285)
Depreciation	43 (241)
Professional Services	722 (2,797)
Outgoing Donations	348 (2,627)
Fundraising Expenses	2287 (5,417)
Office Expenses	2497 (5,172)
Polling	333 (2,319)
Salaries	262 (2,759)
Advertising	1716 (4,597)
Travel	927 (2,908)
Other	793 (3,923)
Total spending ($)	24,754 (33,024)

Values deflated and rounded to 2004 Canadian dollars. *N* = 5387.

Table 3: Canadian House Candidates' Summary Statistics, 2004-2006

Statistic	Mean (Std. Dev.)
Vote share	26.06 (18.08)
Campaign contributions ($)	18,504 (21,381)
Reimbursement (*if* reimbursed)	29,244 (14,325)
Campaign spending (all candidates)	24,754 (33,024)
District population	102,639 (21,855)

N = 1876. District population is an average of districts, not candidates.

Table 4: Ordinary Least-Squares Estimates of the Effect of Spending by Challenger District Associations on their Party's Local Vote Share

Independent Variables	(1)	(2)	(3)	(4)
SPENDING	0.027*** (.007)		0.020*** (.008)	
log(*SPENDING* + 1000)		0.871*** (0.181)		5.284*** (0.165)
v	0.477 (0.66)	0.401 (0.068)		
v^2	-0.022** (0.009)	-0.198** (0.009)		
v^3	0.003*** (0.001)	-0.003*** (0.001)		
v^4	-.0001*** (0.00001)	-0.001*** (0.00001)		
Covariates	*Yes*	*Yes*	*No*	*No*
N	1,364	1,364	1,364	1,364
R-squared	0.783	0.784	0.292	0.429

The first and second rows reveal the estimated impact of spending. Spending figures are in thousands of 2004 Canadian dollars. Sample restricted to elections where *t* is 2004 or 2006 and associations whose candidate did not win in election *t*. The dependent variable is the party's vote share in a district in election $t + 1$. Coefficients on lagged spending, lagged contributions, dummy variables for year, party, province, and party-province interactions omitted.

Standard errors in parentheses: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$ (two-tailed test).

Table 5: RD Estimates of the Impact of Reimbursement on Local Party Association Expenditures, Using the Imbens-Kalyanaraman Algorithm

	Linear Spending		Log Spending	
	(1)	(2)	(3)	(4)
	A. Associations Whose Candidate Spent Over $20,000 in Election *t*			
SPENDING GRANT	19,497.65** (9,630.55)	9,167.39 (6,634.18)	1.641*** (0.376)	0.965* (0.516)
Optimal bandwidth *h*	5.14	5.14	3.95	3.95
N	78	78	64	64
Covariates	*Yes*	*No*	*Yes*	*No*
	B. Associations Whose Candidate Spent Over $15,000 in Election *t*			
SPENDING GRANT	20868.23* (11497.75)	9,400.61 (6,315.80)	0.951* (0.492)	0.703 (0.485)
Optimal bandwidth *h*	5.35	5.35	3.95	3.95
N	119	119	92	92
Covariates	*Yes*	*No*	*Yes*	*No*
	C. All Associations			
SPENDING GRANT	1,159.19 (1,354.03)	789.45 (1,050.95)	0.332* (0.187)	0.472** (0.230)
Optimal bandwidth *h*	7.34	7.34	3.13	3.13
N	697	697	234	234
Covariates	*Yes*	*No*	*Yes*	*No*

Estimates of the treatment effect appear in the first row of each panel. In specifications (3) and (4), the dependent variable is the natural logarithm of 1000 plus spending, in 2004 Canadian dollars. Coefficients on lagged spending, lagged contributions, and dummy variables for year, party, province, and party-province interactions omitted. All values were computed using the Imbens-Kalyanaraman algorithm.

Standard errors in parentheses: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$ (two-tailed test).

Table 6: RD Estimates of the Impact of Reimbursement on Local Party Association Expenditures, by Polynomial Specification with Bandwidth *h*

Independent Variables	(1)	(2)	(3)	(4)
SPENDING GRANT	1.312***	2.137***	2.251***	2.305**
	(0.472)	(0.604)	(0.744)	(0.899)
v	-0.517*	-1.115	-1.057	0.222
	(0.295)	(0.755)	(1.489)	(3.308)
v * *SPENDING GRANT*	0.239	-0.361	-0.922	-3.684
	(0.321)	(0.902)	(1.917)	(3.948)
v^2		-0.206	-0.145	1.814
		(0.270)	(1.141)	(4.542)
v^2 * *SPENDING GRANT*		0.584*	0.934	1.064
		(0.318)	(1.478)	(5.442)
v^3			0.013	0.976
			(0.247)	(2.156)
v^3 * *SPENDING GRANT*			-0.096	-2.020
			(0.312)	(2.472)
v^4				0.145
				(0.321)
v^4 * *SPENDING GRANT*				-0.006
				(0.373)
N	64	64	64	64
R-squared	0.404	0.474	0.477	0.487

The dependent variable is the natural logarithm of 1000 plus spending, in 2004 Canadian dollars. Sample restricted to associations whose local candidate spent at least $20,000 during election *t*. Tables are least-squares regression estimates weighted using a triangular kernel. The model is estimated with covariates; coefficients on lagged spending, lagged contributions, and dummy variables for year, party, province, and party-province interactions omitted. These four specifications are based on an optimal bandwidth of 3.95 percentage points, as computed by the Imbens-Kalyanaraman algorithm.

Standard errors in parentheses: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$ (two-tailed test).

Table 7: RD Estimates of the Impact of Reimbursement on Local Party Association Expenditures, by Polynomial Specification with Bandwidth *2h*

Independent Variables	(1)	(2)	(3)	(4)
SPENDING GRANT	0.772**	1.368***	1.593***	1.933***
	(0.345)	(0.487)	(0.570)	(0.632)
V	-0.324**	-0.816*	-0.262	-0.349
	(0.155)	(0.438)	(0.875)	(1.508)
*V * SPENDING GRANT*	0.355**	0.561	-0.819	-1.878
	(0.164)	(0.464)	(0.963)	(1.723)
V^2		-0.103	0.242	0.134
		(0.089)	(0.464)	(1.380)
V^2 * *SPENDING GRANT*		0.147	0.128	1.020
		(0.096)	(0.491)	(1.447)
V^3			0.050	0.008
			(0.0649)	(0.443)
V^3 * *SPENDING GRANT*			-0.0825	-0.218
			(0.067)	(0.459)
V^4				-0.005
				(0.045)
V^4 * *SPENDING GRANT*				0.018
				(0.046)
N	117	117	117	117
R-squared	0.292	0.316	0.353	0.375

Estimates of the treatment effect appear in the first row. The dependent variable is the natural logarithm of 1000 plus spending, in 2004 Canadian dollars. Sample restricted to associations whose local candidate spent at least $20,000 during election *t*. Tables are least-squares regression estimates weighted using a triangular kernel. The model is estimated with covariates; coefficients on lagged spending, lagged contributions, and dummy variables for year, party, province, and party-province interactions omitted. These four specifications are based on a bandwidth of 7.91 percentage points—two times the Imbens-Kalyanaraman optimal bandwidth—centered at the point of discontinuity

Standard errors in parentheses: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$ (two-tailed test).

Table 8: RD Estimates of the Impact of Reimbursement on Election Outcomes, Using the Imbens-Kalyanaraman Algorithm

	Vote Share		Absolute Votes Cast	
	(1)	(2)	(3)	(4)
	A. Associations Whose Candidate Spent Over $20,000 in Election *t*			
SPENDING GRANT	-0.32 (2.11)	3.16 (2.36)	-1,385.47 (1,098.24)	1,223.07 (1,350.53)
Optimal bandwidth *h*	5.28	5.28	5.15	5.15
N	80	80	78	78
Covariates	*Yes*	*No*	*Yes*	*No*
	B. Associations Whose Candidate Spent Over $15,000 in Election *t*			
SPENDING GRANT	0.55 (1.89)	2.76 (1.97)	658.51 (987.70)	750.69 (1,103.81)
Optimal bandwidth *h*	5.48	5.48	5.22	5.22
N	120	120	116	116
Covariates	*Yes*	*No*	*Yes*	*No*
	C. All Associations			
SPENDING GRANT	0.55 (0.85)	1.47 (1.05)	-124.39 (483.40)	404.67 (584.29)
Optimal bandwidth *h*	5.24	5.24	5.51	5.51
N	485	485	512	512
Covariates	*Yes*	*No*	*Yes*	*No*

Estimates of the treatment effect appear in the first row of each panel. Coefficients on lagged spending, lagged contributions, and dummy variables for year, party, province, and party-province interactions omitted. All output computed using the Imbens-Kalyanaraman algorithm; *N* reflects the relevant bandwidth.

Standard errors in parentheses: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$ (two-tailed test).

Table 9: RD Estimates of the Impact of Reimbursement on Vote Share, by Polynomial Specification with Bandwidth *h*

Independent Variables	(1)	(2)	(3)	(4)
SPENDING GRANT	-0.201	-0.307	-2.089	-5.043
	(2.130)	(2.929)	(3.487)	(3.875)
V	1.457	1.246	2.616	0.680
	(1.127)	(2.875)	(5.834)	(9.389)
*V * SPENDING GRANT*	-0.695	-0.103	2.840	20.810*
	(1.230)	(3.318)	(7.209)	(12.41)
V^2		-0.063	0.816	-1.706
		(0.723)	(3.512)	(9.444)
V^2 * *SPENDING GRANT*		-0.032	-3.419	-17.79
		(0.875)	(4.142)	(12.13)
V^3			0.142	-0.772
			(0.560)	(3.400)
V^3 * *SPENDING GRANT*			0.230	6.926
			(0.662)	(4.281)
V^4				-0.101
				(0.390)
V^4 * *SPENDING GRANT*				-0.516
				(0.474)
N	80	80	80	80
R-squared	0.523	0.523	0.534	0.576

Estimates of the treatment effect appear in the first row. The dependent variable is the party's district-level vote share in the following election. Sample restricted to associations whose local candidate spent at least $20,000 during election *t*. Tables are least-squares regression estimates weighted using a triangular kernel. The model is estimated using covariates; coefficients on lagged spending, lagged contributions, and dummy variables for year, party, province, and party-province interactions omitted. These four specifications are based on a bandwidth of 5.28 percentage points, which is the Imbens-Kalyanaraman optimal bandwidth.

Standard errors in parentheses: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$ (two-tailed test).

Table 10**:** RD Estimates of the Impact of Reimbursement on Vote Share, by Polynomial Specification with Bandwidth *2h*

Independent Variables	(1)	(2)	(3)	(4)
SPENDING GRANT	0.548	2.179	1.520	2.008
	(1.404)	(1.963)	(2.401)	(2.701)
V	1.195*	-1.189	0.075	-2.313
	(0.613)	(1.825)	(3.741)	(6.563)
*V * SPENDING GRANT*	-0.618	1.743	0.855	3.007
	(0.627)	(1.876)	(3.859)	(6.939)
V^2		-0.503	0.225	-2.278
		(0.363)	(1.955)	(5.972)
V^2 * *SPENDING GRANT*		0.505	-0.326	2.294
		(0.369)	(1.998)	(6.062)
V^3			0.101	-0.732
			(0.270)	(1.895)
V^3 * *SPENDING GRANT*			-0.094	0.720
			(0.270)	(1.904)
V^4				-0.085
				(0.191)
V^4 * *SPENDING GRANT*				0.086
				(0.191)
N	171	171	171	171
R-squared	0.525	0.532	0.533	0.533

Estimates of the treatment effect appear in the first row. The dependent variable is the party's district-level vote share in the following election. Sample restricted to associations whose local candidate spent at least $20,000 during election *t*. Tables are least-squares regression estimates weighted using a triangular kernel. The model is estimated with covariates; coefficients on dummy variables for year, party, province, and party-province interactions omitted. These four specifications are based on a bandwidth of 10.57 percentage points, which is twice the Imbens-Kalyanaraman optimal bandwidth.

Standard errors in parentheses: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$ (two-tailed test).

Table 11: RD Estimates of the "Impact" of Reimbursement on Pretreatment Variables, Using the Imbens-Kalyanaraman Algorithm

	(1) Lagged donations	(2) Lagged spending	(3) Lagged log donations	(4) Lagged log spending
SPENDING GRANT	4,578.87 (3,063.68)	-1,577.96 (7,642.00)	0.150 (0.363)	0.081 (0.192)
Optimal bandwidth *h*	14.83	7.24	5.24	3.82
N	285	107	80	64
Covariates	*Yes*	*Yes*	*Yes*	*Yes*

Estimates of the treatment effect appear in the first row. Sample restricted to associations whose candidate spent at least $20,000 in election *t*. Coefficients on lagged spending, lagged contributions, and dummy variables for year, party, province, and party-province interactions omitted. All values were computed using the Imbens-Kalyanaraman algorithm.

Standard errors in parentheses: * $p < 0.1$ (two-tailed test).

Table 12: RD Estimates of the Impact of Reimbursement on Specific Categories of Association Expenditures, Using the Imbens-Kalyanaraman Algorithm

	(1) Fundraising	(2) Office Expenses	(3) Salaries	(4) Travel
SPENDING GRANT	-88.627 (1161.52)	4522.765** (1984.93)	6251.49 (4485.71)	1630.608*** (344.13)
Optimal bandwidth *h*	11.20	5.28	4.65	8.35
N	182	80	71	125
Covariates	*Yes*	*Yes*	*Yes*	*Yes*

Variables	(5) Polls	(6) Professional Services	(7) Advertising	(8) Other
SPENDING GRANT	108.69 (79.93)	364.86 (347.03)	914.49 (471.88)	-133.21 (1,076.05)
Optimal bandwidth *h*	8.32	12.23	6.10	6.93
N	124	208	89	101
Covariates	*Yes*	*Yes*	*Yes*	*Yes*

Variables	Linear Overhead Spending		Log Overhead Spending	
	(9)	(10)	(11)	(12)
SPENDING GRANT	13,195.19** (6,299.17)	6,255.33* (3,687.82)	1.433*** (0.351)	0.784* (0.402)
Optimal bandwidth *h*	4.74	4.74	3.56	3.56
N	71	71	60	60
Covariates	*Yes*	*No*	*Yes*	*No*

Estimates of the treatment effect appear in the first row of each panel. Sample limited to associations whose candidate spent at least $20,000 in election *t*. Overhead spending is defined as the sum of transit, salary, and office expenses. In specifications (11) and (12), the dependent variable is the natural logarithm of 1000 plus spending, in 2004 Canadian dollars. Coefficients on lagged spending, lagged contributions, and dummy variables for year, party, province, and party-province interactions omitted. All values were computed using the Imbens-Kalyanaraman algorithm; *N* reflects the cases within the relevant bandwidth.

Standard errors in parentheses: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$ (two-tailed test).

Table 13: RD Estimates of the Impact of Reimbursement on the Sum of Salary, Transit, and Office Expenditures, by Polynomial Specification with Bandwidth *h*

Independent Variables	(1)	(2)	(3)	(4)
SPENDING GRANT	1.160***	2.073***	2.320***	2.520***
	(0.389)	(0.497)	(0.658)	(0.709)
V	-0.384	-1.550**	-1.320	1.451
	(0.254)	(0.707)	(1.883)	(3.537)
*V * SPENDING GRANT*	0.009	0.150	-1.308	-6.567
	(0.279)	(0.818)	(2.101)	(3.924)
v^2		-0.470	-0.181	5.738
		(0.283)	(1.894)	(6.474)
v^2 * *SPENDING GRANT*		0.821**	1.616	-0.522
		(0.325)	(2.106)	(7.266)
v^3			0.081	4.073
			(0.520)	(4.144)
v^3 * *SPENDING GRANT*			-0.317	-6.229
			(0.553)	(4.241)
v^4				0.837
				(0.854)
v^4 * *SPENDING GRANT*				-0.531
				(0.902)
N	60	60	60	60
R-squared	0.495	0.579	0.596	0.630

Estimates of the treatment effect appear in the first row. Sample restricted to associations whose local candidate spent at least $20,000 during election *t*. The dependent variable is the natural logarithm of 1000 plus salaries, transit, and office expenditures in 2004 Canadian dollars. Tables are least-squares regression estimates weighted using a triangular kernel. The model is estimated with covariates; coefficients on lagged spending, lagged contributions, and dummy variables for year, party, province, and party-province interactions omitted. These four specifications are based on a bandwidth of 3.57 percentage points, which is the Imbens-Kalyanaraman optimal bandwidth.

Standard errors in parentheses: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$ (two-tailed test).

Table 14: RD Estimates of the Impact of Reimbursement on the Sum of Salary, Transit and Office Expenditures, by Polynomial Specification, with Bandwidth *2h*

Independent Variables	(1)	(2)	(3)	(4)
SPENDING GRANT	0.746**	1.322***	1.500***	1.966***
	(0.295)	(0.408)	(0.473)	(0.521)
V	-0.317**	-0.700*	-0.129	-2.013
	(0.135)	(0.368)	(0.727)	(1.234)
*V * SPENDING GRANT*	0.272*	0.276	-1.130	0.204
	(0.145)	(0.395)	(0.826)	(1.449)
V^2		-0.081	0.276	-1.731
		(0.0762)	(0.390)	(1.135)
V^2 * *SPENDING GRANT*		0.146*	0.154	2.572**
		(0.0847)	(0.421)	(1.224)
V^3			0.052	-0.632*
			(0.055)	(0.367)
V^3 * *SPENDING GRANT*			-0.091	0.489
			(0.058)	(0.390)
V^4				-0.071*
				(0.038)
V^4 * *SPENDING GRANT*				0.080**
				(0.039)
N	105	105	105	105
R-squared	0.331	0.371	0.414	0.446

Estimates of the treatment effect appear in the first row. Sample restricted to associations whose local candidate spent at least $20,000 during election *t*. The dependent variable is the natural logarithm of 1000 plus salaries, transit, and office expenditures in 2004 Canadian dollars. Tables are least-squares regression estimates weighted using a triangular kernel. The model is estimated with covariates; coefficients on lagged spending, lagged contributions, and dummy variables for year, party, province, and party-province interactions omitted. These four specifications are based on a bandwidth of 7.14 percentage points on either side of the point of discontinuity—two times the Imbens-Kalyanaraman optimal bandwidth.

Standard errors in parentheses: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$ (two-tailed test).