

# Does Campaign Spending Affect Election Outcomes? New Evidence from Transaction-Level Disbursement Data

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This article uses detailed, transaction-level data on candidate disbursements and panel survey data to estimate the effect of candidate spending. Transaction-level data allow me to isolate only spending that is being used on messages to voters, while panel survey data enable me to control for unobserved candidate characteristics. I find that spending on messages to voters has a statistically significant effect on voter support for candidates. Spending is especially effective in changing the composition of voters, instead of convincing potential voters to switch their vote. Not all voters are equally affected by spending; low-information voters, members of a political party and the economically dissatisfied respond strongly to candidate spending. Finally, I provide evidence that the most commonly used measure of candidate spending overestimates the amount of money that candidates use on their own campaigns, and regressions using this measure are less likely to find a statistically significant effect of spending.

**D**espite the large space that campaign spending occupies in the public debate on American politics, there are still large gaps in our understanding of how (or even if) spending helps candidates achieve their goals. “Virtually everything we have learned by examining the effects of campaign-specific variables on election results tells us that, in one way or another, campaigns do matter. What remains in question is how campaigns matter” (Jacobson 2006). A large body of literature has explored the effects of candidate spending, but little consensus has been reached on the causal effect of money on election outcomes (Milyo 1999). Likewise, there has been no discernible pattern in the behavior of lawmakers with regard to campaign contribution laws. Over the past thirty years, numerous states (Nevada, Ohio, Tennessee) have introduced caps on contributions to candidates in state elections, while others (Florida, Maryland, Wyoming) have raised the cap, and still others (California, Missouri) have first imposed and later eliminated caps.

This article aims to advance our understanding of how campaigns matter in three ways. First, by using repeated surveys of voters both before and after elections, and pairing it with detailed candidate transaction data, I am able to estimate

the effect of candidate spending on messages to voters with relatively weak identifying assumptions. Second, since my outcome variable consists of voter-level data (instead of district-level vote shares), I can disaggregate the causal effect across different types of voters. Finally, I show that the most frequently used method of measuring candidate spending overstates the amount of money that is being spent to increase a candidate’s vote share or odds of victory, and the magnitude of this differs systematically across candidates based on election competition and incumbency status. Incumbents and candidates with a low probability of losing spend a larger fraction of their total disbursements on things that cannot feasibly help them win an election, such as contributions to other candidates, loan repayments, and donation refunds. Therefore, even unbiased estimates of candidate spending can differ across electorates and candidates. There is likely no single “true” effect of candidate spending, but data limitations have made it impossible to systematically evaluate different types of candidate spending and estimate their effect on different types of potential voters.

By using detailed, transaction-level candidate disbursement data and new questions asked in the 2012 American

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National Election Study (ANES), I am able to link changes in voter preferences during a campaign to changes in candidate spending. This framework improves on studies that rely on cross-election variation, and allows me to test theories of which voters are persuaded by candidate messages. I find that candidate spending on advertising and campaign events has a positive, statistically significant effect on voter support. Spending changes the composition of those who vote (turn-out), but does not lead to voters switching from one party to another in large numbers. Campaign spending appears to primarily affect low-information voters, those who identified with a political party, and people who are unhappy with the state of the economy. Finally, I find that when total aggregate spending (the most common measure used in the literature) is used to create my measure of spending, I fail to find a statistically significant effect, suggesting a possible solution for the empirical puzzle of ineffective candidate spending. When studies find candidate spending to have little effect, it could be due to the fact that candidate spending measurements often include millions of dollars of useless spending (at least in terms of increasing a candidate's vote share), which could drive the resulting point estimates toward zero or increase the noise of those estimates.

### MOTIVATION

Although empirical research has frequently addressed the impact of campaign spending, little consensus has been reached. While some studies (Gerber 1998; Goldstien and Freedman 2000) have found large effects of campaign spending on candidate success, others (Levitt 1994; Welch 1981) have found small or insignificant effects. The importance of understanding the role of money in elections relates directly to determining optimal campaign finance policy. In order to anticipate the impact of any change in campaign finance laws, we must first understand the extent to which candidates are able to use money to achieve their election goals.

Numerous papers have attempted to resolve the issue of apparently ineffective campaign spending. Stratmann (2009) shows that when advertising prices are accounted for, the estimated effect of candidate spending increases (especially for incumbents). Gerber (2004) finds that the effectiveness of spending changes with the competitiveness of elections (which is also found by Green and Krasno 1988) and argues that candidates use spending to increase the likelihood of being elected, not necessarily to increase their vote share. Erikson and Palfrey (2000) show that a simultaneity problem exists since both candidates will increase their spending if they expect an election to be close. Despite these insights, the empirical literature has failed to reach a consensus on the magnitude of the effect of candidate spending.

In this article, I highlight several important factors that severely complicate attempts to determine causal effects. First, not all candidate spending is used on a candidate's own campaign. A candidate may increase their spending by purchasing ad time or by refunding a contribution. Both appear as disbursements, but only ad spending could impact a candidate's success. There are systematic differences across candidates in how they spend money, especially based on their incumbency status and the competitiveness of their election. Unfortunately, detailed spending data have only recently become easily available for US elections, meaning that most estimates in the literature cannot disentangle a dollar of ad spending from a dollar of refunds, or any other type of spending that is not being used directly on a campaign.<sup>1</sup>

Second, the composition of the electorate will influence the effect of candidate spending. Not all voters change their preferences, and many people's voting behavior will not be influenced by candidate messages. The effect of candidate spending will be greater in districts where more voters can be persuaded by candidate messages to enter or leave an election or switch their party allegiance. Unfortunately, most studies have been forced to use a candidate's vote share as the dependent variable, and cannot determine which specific potential voters are being influenced by spending.

By using individual-level panel data on potential voters, I can measure changes in individual voter preferences. Therefore, my estimates rely on weaker identifying assumptions than do papers using cross-election variation in candidate spending and voting behavior. Few previous studies have used individual-level variation. Jacobson (1990) uses a panel survey data taken during the 1986 elections measures the effect of candidate spending on changes in voter preferences during the last weeks of the campaign, finding that challenger spending has a larger effect on voter preferences. The author's findings are limited by the use of aggregate measures of candidate spending, instead of being able to track candidate spending over time. Goldstein and Freedman (2000) combine data on advertising exposure with the 1996 ANES and find that candidate spending on advertising is largely effective. However, the authors use cross-sectional variation in voting behavior and therefore cannot control for time-invariant unobserved characteristics.

Another contribution is achieved by pairing individual changes in voter preferences with detailed changes in the pattern of candidate spending. Jacobson (2006, 217) identified the need for more detailed data, stating that "it would

1. While transaction reports have long been publicly available, researchers wishing to use the full population of transactions needed to digitize hundreds of thousands items for every election cycle.

also be useful to get as detailed as possible a breakdown on the timing of campaign spending . . . the more fully we can measure campaign spending the better.” To my knowledge, only one paper, Ansolabehere and Gerber (1994), has specifically addressed the issues that might arise from using aggregate spending data as a measure of candidate spending. Using a fully itemized data set of 1990 congressional election candidates, the authors compare estimates of causal effect under three different measures of campaign spending. The authors consider all spending (which they call “disbursements”), only spending that will likely be used directly on the candidate’s own campaign (“spending”), and finally “communications,” which is advertising, direct voter contact, phone banks, and campaign rallies. However, the authors cannot control for unobserved candidate characteristics, which highlights the fact that a precise measure of campaign spending does not resolve the underlying endogeneity issues plaguing estimates of causal effect.

To this end, this article compliments the existing literature that focuses on directed campaign messages. Unlike studies on the effect of campaign spending, these studies have consistently found candidate messaging to be effective in improving candidate outcomes (Da Silveria and De Mello 2011; Kendall, Nannicini, and Trebbi 2015; Spenkuch and Toniatti 2018). By focusing on the type of candidate spending, this article provides one way to reconcile the lack of consensus on the effect of candidate spending with the results of the effect of candidate messaging.

This work also builds on papers that have explored the role of campaigns on voter mobilization and demobilization, as I find that candidate spending is effective in discouraging potential voters for voting for opposing candidates. Since voting in the United States is voluntary, the impact of mobilization has been extensively studied, particularly with regards to negative advertising (Ansolabehere, Iyengar, and Simon 1999; Kahn and Kinney 1999). This article expands this work by broadening the focus to campaign spending, while individual survey data allow me to identify which voters are most influenced by politicians’ campaigns.

### CANDIDATE SPENDING DATA

One of the central goals of this article is to better understand different types of candidate spending, and to isolate only those that are most likely to be used to sway voter. Candidate spending data comes from the Federal Election Commission (FEC) candidate disbursement data set, which details each individual transaction in support of a candidate of the 2012 congressional elections.<sup>2</sup> In support of the 2012 election,

2. So-called memo line transactions were dropped from the data, as these disbursements represent double-payments (i.e., credit card payments, where the original purchase had already been itemized).

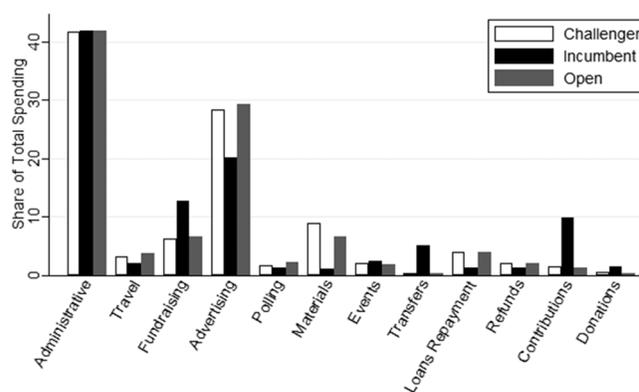


Figure 1. Distribution of spending. Share of candidate spending by each of the 12 defined Federal Election Commission (FEC) category codes. Sample is all candidates for the 2012 House of Representatives elections that filed a report with the FEC.

House of Representatives candidates spent almost \$1.0966 billion consisting of 533,715 disbursements. This measure only includes candidates’ own committees and therefore does not include spending by political action committees and independent committees. By focusing on only spending by candidates, I obtain a measure of spending that is clearly defined (committee spending may be used to simultaneously support numerous candidates or causes) and also avoid a significant potential threat to my identification strategy, as I will discuss later.

FEC filing rules separate disbursement into 12 categories.<sup>3</sup> Only 223,106 (41.8%) of disbursements, constituting \$475 million (43.3%) of spending have this filing code explicitly provided. However, 530,853 (99.5%) of disbursements, constituting \$1.089 billion (99.3%) of spending have a description detailing the type of spending, allowing me to assign spending codes to observations for the vast majority of disbursements.

To assign spending to the correct codes, I used the following procedure. First, I assigned codes to observations where the attached description was identical to one of the 12 codes (i.e., “Donation,” “Advertisement”), were phrases that contained those words, similar variants (such as “TV Spot”), or a misspelling of a phrase.<sup>4</sup> Finally, I determined, for all identical descriptions in the data, the most frequent disbursement code to which it was assigned. This code was assigned for any remaining missing values with the same description. Figure 1

3. The categories are (1) Administrative/Salary/Overhead Expenses, (2) Travel, (3) Fund-raising, (4) Advertising, (5) Polling Expenses, (6) Campaign Materials, (7) Campaign Events, (8) Transfers to other committees of same candidate, (9) Loan Repayments, (10) Refund of Contributions, (11) Political Contributions, and (12) Donations.

4. This included abbreviations (“ads,” “contr.,” “trvl.”) and other variations.

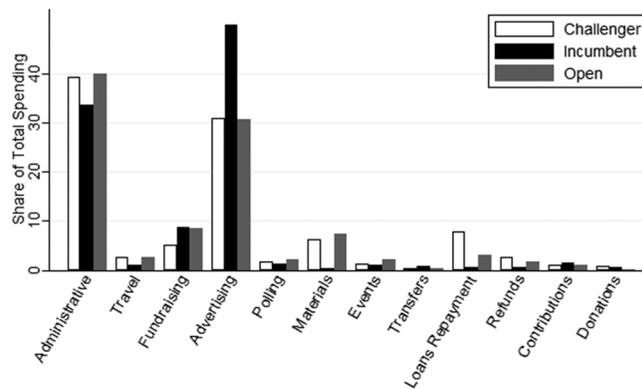


Figure 2. Distribution of spending: close elections. Share of candidate spending by each of the 12 defined Federal Election Commission (FEC) category codes. Sample is all candidates for the 2012 House of Representatives elections that filed a report with the FEC and who were in a district defined as “Toss-ups,” “Lean D,” or “Lean R” by the Cook Political Report.

shows the distribution of spending across these categories for incumbents, challengers, and candidates for open seats.

Figure 2 shows the distribution of spending for all elections that were rated as “Toss-ups,” “Lean D,” or “Lean R” by the Cook Political Report. The largest change is the increase in advertisement spending, especially for incumbents, who spend three times as much on advertising when they are in competitive elections compared to those in noncompetitive ones. When candidates are most in need of convincing voters, they increasingly turn to spending on messages to the electorate. This insight builds on the work of Erikson and Palfrey (2000), who show that challengers and incumbents increase their total spending when the election is expected to be close. Detailed disbursement data show that politicians also change the nature of their spending when they expect a tight race, and suggest that aggregate measures of candidate spending miss important variation across elections.

To my knowledge, all studies of candidate spending in US elections, with the exception of Ansolabehere and Gerber (1994), have used candidate spending aggregates as the measure of candidate spending.<sup>5</sup> This measure includes all disbursements by candidates, regardless of the timing or type of spending. By relying on candidate summaries, previous research implicitly assumes that all candidate disbursements, regardless of the nature or timing, are being used to help a candidate win an election.

Figure 3A illustrates a drawback to that assumption. It shows the spending throughout the election cycle of candidates who ultimately won in the 2012 House of Representa-

5. This measure excludes any disbursements that are labeled as transfers to other committees.

tatives general election, grouped by contested and uncontested candidates.<sup>6</sup> Candidates who faced no contest in their general election spent more than those who were in contested ones. If the only purpose of all campaign spending was to persuade voters, it is difficult to conceive of circumstances under which those who have no need to sway voters spend the greatest amount of money doing so. Instead, safe incumbents are likely to maintain large staffs and begin building war chests to prepare for future political battles, raise funds for their party, and donate to candidates and committees. Throughout this article, I focus on spending by candidates in the form of messages to voters, and therefore use advertising and events (categories 4 and 7) as the main measure of candidate spending.<sup>7</sup> Of all candidate disbursements, about 30% consists of advertising or events.<sup>8</sup> The motivation for this decision is grounded in literature that models the relationship between candidates and potential voters. Several models (Austen-Smith 1987; Baron 1994) describe the function of candidate spending as a way of informing voters of candidates’ true policy position. In others (Coate 2004), candidates use spending to inform voters about their quality. Throughout, messaging is the primary mechanism through which candidates persuade voters.

Figure 3B shows the same comparison as figure 3A, using only spending on advertising and campaign events. Though uncontested candidates have spent more on these activities than contested candidates before the beginning of the year, almost all of this spending occurs before the beginning of the general election. Once the general election starts, spending all but ceases for uncontested candidates, while contested candidates increase their rate of spending. It is spending on messages, and not aggregate spending, that appears to follow patterns we would expect if the goal of spending is to change voter opinion during the general election.

## EMPIRICAL SPECIFICATION

The 2012 ANES surveyed potential voters twice: once in the weeks before the election, and once after the election. During

6. Candidates are considered to be uncontested if they face either no challenger in the general election or challengers who failed to earn 5% of the vote share.

7. It could be argued that materials also fall into this category, since this includes items like yard signs and bumper stickers. However, I do not include them in my measure, as materials is an ambiguous definition, and may include things such as office supplies.

8. As examples of the disbursements that appear under the category of ads and events, the five most common descriptions for “event” disbursements are Catering Expense, Event Catering, Event Supplies, Event Ticket, and Event Expense. The five most common advertising disbursement descriptions are Advertisement, Advertising, Direct Mail, Media, and Media Buy.

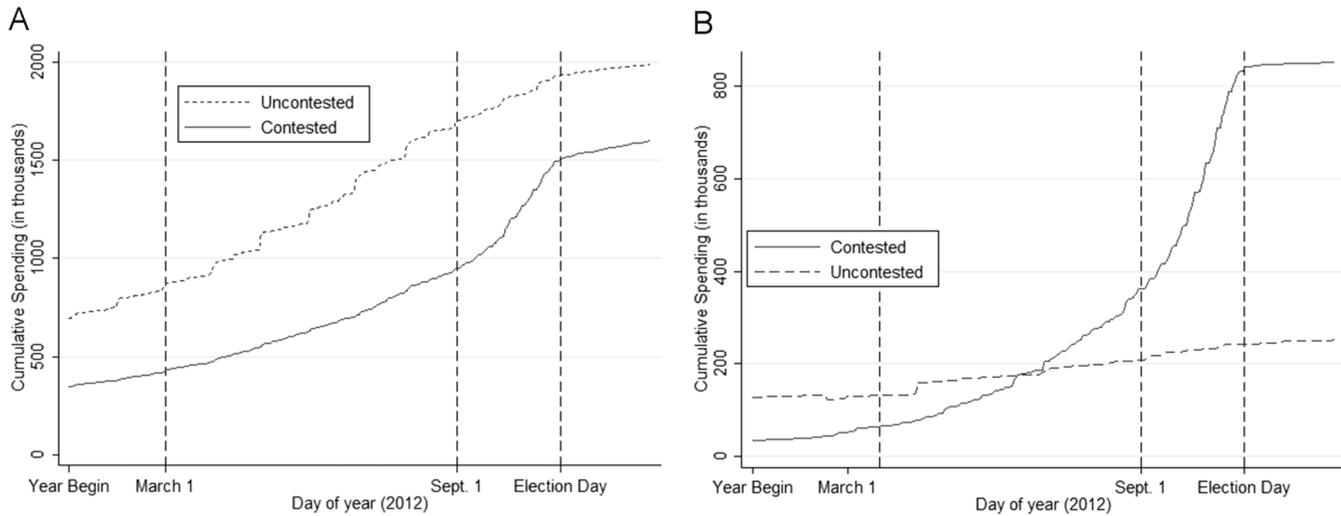


Figure 3. Cumulative spending by all election winners for 2012 House of Representative elections. A, All spending, regardless of nature; B, only spending on advertising or events (Federal Election Commission category codes 4 and 7).

the preelection survey, respondents were asked for whom they intended to vote in Congressional elections, and during the postelection survey they reported for whom they cast their ballot. Aside from 1996, the preelection question had not previously been asked with regards to Congressional elections. The panel of individual data allows me to control for unobserved individual and district-specific effects. Because preelection voter opinion is only measured in the month before the election, I am therefore estimating the effect of late-campaign election spending.

Individual voter behavior on Election Day is described by the following linear model:

$$\text{NetVotes}_{id} = \phi \mathbf{X}_i + \beta(\ln(\text{Republican Spending}) - \ln(\text{Democratic Spending}))_d + \delta_i + \zeta_d + \epsilon_{id},$$

where  $\text{NetVotes}_{id}$  is the net votes of individual  $i$  in district  $d$  for the Republican candidate in a two-candidate race. A vote for the Republican is +1, a vote for the Democrat is -1, and a vote for neither is 0. This is determined by a set of observed voter characteristics  $\mathbf{X}_i$ ; unobserved, time-invariant individual and district characteristics,  $\delta_i$  and  $\zeta_d$ .<sup>9</sup> The candidate spending variable is constructed as the difference in the log of spending by the Republican and Democratic candidates. Candidate spending is determined by cumulative spending of candidates between the beginning of the general election campaign (as measured by the end of the primary election) and the date of the general election. Candidate quality, political climate and unobserved voter preferences are captured

9. These characteristics include age, marital status, and sex.

by  $\delta_i$  and  $\zeta_d$  but are unobserved and lead to a biased estimate. Popular candidates will be better able to raise (and therefore spend) money, and will also be preferred by more individuals.

Preelection voting intentions are modeled in an almost identical way:

$$\text{NetVotes}_{idt} = \gamma_t + \phi \mathbf{X}_i + \beta(\ln(\text{Republican Spending}) - \ln(\text{Democratic Spending}))_{dt} + \delta_i + \zeta_d + \epsilon_{idt},$$

where the dependent variable is determined by the candidate for whom the individual states they intend to vote at time  $t$ .  $\text{NetVotes}_{idt}$  is +1 if a potential voter intended to vote for the Republican candidate, -1 if they intended to vote for the Democratic candidate, and 0 if they did not know for whom they would vote, if they intended to vote for a different candidate, or intended not to vote.<sup>10</sup> Now, candidate spending is determined by the cumulative sum of spending between the beginning of the general election campaign and the date of the preelection survey. Since the preelection survey was conducted over several weeks, I include time fixed effects,  $\gamma_t$ , which control for common, time-variant partisan shocks.

By taking the difference between these two values, unobserved, time-invariant characteristics of elections and voters are fully captured:

$$\Delta \text{NetVotes}_{idt} = \Delta \gamma_t + \beta \Delta(\ln(\text{Republican Spending}) - \ln(\text{Democratic Spending}))_{dt} + \epsilon_{idt}.$$

10. The ANES survey asks "For whom does respondent intend to vote," with a list of candidate names.

The dependent variable is the change in net votes for the Republican candidate. The variable is equal to +2 if the voter switches from Democrat to Republican, +1 if they switch from Democrat to no vote or from no vote to Republican, and so forth. By constructing the variable in this way, I can simultaneously capture movements along several margins (since a measure of  $-1$  can denote Republican to “no vote” or “no vote” to Democrat, as they have the same impact on the vote share of a candidate). This allows for a natural comparison to research using vote share as the dependent variable, since the vote share for a candidate can be affected by voters either switching their vote or by changing their decision on whether to vote at all. Since state-level elections are also occurring during this period, standard errors are clustered at the state level.<sup>11</sup>

As mentioned, for most of the estimates in this article I limit my measures of spending to only those which are direct communications from politicians to potential voters (codes 4 and 7 in the FEC data). These types of spending are most suitable for the specification above for several reasons. First, candidates spend significant amounts on advertising and campaign events in the weeks immediately before the election, as seen in figure 3B. Second, since the measure of candidate spending is time sensitive, I want to focus on high-frequency spending and eliminate administrative expenses, as well as spending that does not directly support a candidate’s own campaign.

By observing changes in voter opinion within an election, the first-difference specification will capture any unobserved candidate quality that will otherwise bias the estimates. Additionally, the time dummy will allow me to control for common, partisan time shocks. Since 2012 was a presidential election year, and congressional and presidential fates are correlated, shocks occurring to the presidential election could affect outcomes for all congressional candidates.

An alternative specification to a linear model would be an ordered logit model. The dependent variable takes on values  $-2$  to  $+2$  and is monotonic in the sense that any one-unit increase represents a change of one net vote for the Republican candidate. Therefore, interpreting the coefficients for an ordered logit regression is relatively straightforward.<sup>12</sup>

11. The vector of observable, fixed voter characteristics disappears from this specification, since it is differenced out and fall into the error term. However, I can still include these variables in the regression to test whether they predict changes in voter behavior, which would indicate that the first-difference regression is not appropriate. Therefore, I chose variables that are (1) objective and easy to verify and (2) correlated with partisan leanings in the cross-section of my data.

12. To perform this regression, the dependent variable is converted from a  $-2$  to  $2$  scale to a  $0$ – $5$  scale.

The results from this regression are presented alongside those from the linear specifications, and show that the sign and significance of the findings are robust to using this model.

The primary identifying assumption of this framework is that election and individual-specific effects remain time invariant during the general election campaign. Specifically, the assumption is that the unobserved factors that affect both candidate spending and voter preferences are fixed between the pre- and postelection surveys. This assumption would be violated if, for instance, candidate-specific, time-variant shocks occurred during the election cycle that affected both the spending patterns of candidates, and the behavior of voters.

Three characteristics of this framework help address these concerns. First, the preelection survey occurred after most of the fund-raising activity for the election had taken place. In 2012, only 7% of all fund-raising spending occurred during the final month of the campaign, and much of this was done by unchallenged incumbents. This means that even if, for example, a negative shock to candidate likability reduced the likelihood that a potential voter would vote for that candidate, the effect it would have on candidate spending would be mitigated by the fact that most funds are acquired early in the election cycle.

Second, the measure of spending used here captures only spending by candidates and excludes spending by political action committees. However, transfers from committees directly to candidates are common, and must be addressed. Though FEC data of transfers of funds into candidates’ campaigns from committees are not disaggregated to the day, I am able to isolate any transfers that occur after the 20th day prior to the general election, which compares favorably to the timing of the preelection surveys. As shown in the Results section, my findings are robust to controlling for these late-campaign transfers.

Finally, as there is variation in the timing of preelection survey, I can use district fixed effects. This is an important control, as it addresses many lingering concerns about the specification strategy. Though fixed effects reduce the precision of my estimates, the point estimates and most of the statistical significance do not change when they are included.

Since my measure of voter behavior is constructed using survey data, there are concerns of mismeasurement and response bias. Specifically, it has been noted that respondents may incorrectly report to have voted for incumbents or election winners (Atkinson 1999; Bloom 1991). Incumbency bias would not pose a threat to my identification strategy, since the identity of the incumbent does not change throughout the election cycle. Therefore, any incumbency bias would exist in the preelection survey as well and would be controlled for in the first-difference regression. However, a winner bias

could lead to biased estimates. If candidates who ultimately win the election also increase their spending late in the campaign cycle, and survey respondents erroneously report to have voted for the winner after the election, the first-difference regression would yield biased estimates. I test for this by regressing a winner dummy variable on changes in voter preferences, without controlling for candidate spending. The results, presented in the appendix (available online), fail to indicate that a winner bias plagues my results.

## DATA

Data on candidate spending come from the FEC and are outlined in the Candidate Spending Data section. Unfortunately, spending data cannot be obtained for all candidates. Candidates spending less than \$5,000 throughout the entire election cycle do not have to file detailed reports with the FEC. Any district with missing candidate spending data is dropped. Though these are mostly symbolic candidates who do not actively campaign, some of these candidates may have engaged in campaign spending which cannot be observed.

Data on potential voters come from the 2012 ANES Time Series Study, using questions regarding candidates for the House of Representatives. Preelection surveys took place between September 9 and November 5 (election day was No-

vember 6), with more than half taking place between October 16 and October 20. Despite the late timing of the pre-election surveys, a significant percentage of respondents change their stated preferences between the pre- and postelection survey. The most active margin is people who stated no preference (or who intended to abstain from voting) in the pre-election survey and shifting their vote to one of the candidates. More than 30% of these respondents ended up voting for one of the two candidates. Of the respondents who intended to vote for one of the candidates, about 25% changed their preference, but only about one-fifth of this group did so by switching to the other candidate. Republicans are slightly less likely (22%) than Democrats (26%) to change their preference.

Summary statistics are shown in table 1. Of the 435 congressional districts, only 320 were used in my sample, as I dropped uncontested elections, elections where either only one candidate from a major party was participating, or two candidates from the same major party were participating in the general election (which makes voter behavior impossible to determine through the ANES). Also, this sample only includes elections in which both candidates filed disbursement reports. By election day, the average respondent was in a district where a candidate spend \$599,589, with \$186,283 occurring between the preelection survey and election day.

Table 1. Summary Statistics

Variable	Mean	Min	Max
Cumulative disbursements (preelection)	413,306	0	6,729,638
Cumulative disbursements (election day)	599,589	1	10,856,336
Cumulative ad and event spending (preelection)	196,170	0	4,496,435
Cumulative ad and event spending (election day)	298,202	0	6,922,603
<i>N</i>	3,139		
Number of districts	320		
	ANES Sample (Full Response)	ANES Sample (Full Sample)	Demographics of Voters
% male	48.8	48.26	48.1
% married	50.7	53.12	52.3
Age	50.4	54.5	49
Net votes (preelection)	-.0895	-.116	...
Net votes (November 5)	-.0580	...	...
% Democratic vote	54.15	...	50.6
<i>N</i>	3,139	5,410	

Note. ANES characteristics of potential voters from American National Election Study 2012 Time Series Study. The "Full Response" group, which is the sample used for this article, includes potential voters for House of Representative races who completed both rounds of the survey, had not voted before the preelection survey, and were in districts in which both candidates filed Federal Election Commission (FEC) reports. The "Full Sample" is everyone who participated in the first round of the survey. Data on candidate spending are from the FEC. Demographics of voters come from the US Census. Age is approximated from age group averages.

To focus on spending only of ads and events, candidates spent \$298,202 by election day, \$102,032 of which occurred after the preelection survey. Candidates are therefore shifting their spending patterns toward ads and event spending as the election draws near. Before the preelection survey, 47.5% of all spending is devoted to ads and events. After the preelection survey, 54.8% of all spending consists of ads and events. Compared to figure 1, the candidates in my sample are spending more than the average race on ads and events. This is due to the exclusion of uncontested elections, where candidates are unlikely to spend much on ads.

Individuals are only included in the data set if they participated in both the pre- and postelection survey, and therefore there is the potential for nonresponse or attrition bias to affect the sample (Peress 2010).<sup>13</sup> However, individual characteristics of my sample are representative of both the full ANES sample and of eligible voters in general. Individual characteristics appear to be roughly representative of the voting-age population, with ratios of males and average age similar to the average reported by the US Census Bureau. While the average age within my sample is slightly higher than the average age from the US Census Bureau, this could be because both the ANES and Census only report broad age groups, instead of actual ages, which forces me to impute estimated ages for respondents. Of the 5,410 responders of the ANES for which demographic data are available, 3,139 are used for my analysis, and this group differs slightly from the full ANES sample. They are more likely to be male, unmarried, and slightly younger, but these differences are not large.<sup>14</sup>

Finally, the net vote variable shows that the average voter will, on election day, cast net votes of  $-0.058$  for the Republican, relative to the Democrat. This means that respondents are more likely to have voted for Democrats. The positive shift in net votes between pre- and postelection surveys means that respondents shift toward Republican candidates. Of those who voted for either the Republican or Democrat, 54.2% voted for the Democrat. Though this difference is significant compared to the preferences of all voters (where 50.6% of the two-party vote went to Democrats), this is mostly due to the elimination of uncontested elections, which were more likely to be won by Republican candidates.

13. I eliminate from all regressions anyone who had already voted before the preelection survey. I also drop any extreme outliers, which are any observations for which the independent variable has a magnitude greater than 10, which occurs when a campaign has reported no or very little spending by the preelection survey, but follows that with moderate or high amounts of spending in the weeks leading up to the election. This eliminates fewer than 20 observations. My main results do not change when these observations are included.

14. The regression results are also robust to the inclusion of additional demographic covariates.

**RESULTS**

First, I consider the effect of candidate spending on advertising and events, pooling all contested elections for which I have spending data. Table 2 presents these findings. Column 1 comes from a simple cross-section, using individual voting decisions on election day as the dependent variable. Unsurprisingly, a strong, positive correlation is observed between voter preferences and the spending of candidates. However, spending is endogenous to voter preferences. Unobserved candidate quality will likely increase both the funds candidates can raise (and therefore spend) and the likelihood that an individual will vote for them.

Columns 2–4 provide the results for various specifications of the first-difference regression. In columns 2 and 4, I include a set of dummies for when the preelection survey occurred, and control for committee transfers to candidates. Column 3 includes a full set of time and district dummies, as

Table 2. Effect of Campaign Spending on Voter Behavior

Variable	Cross-Section (1)	First Difference (2)	First Difference (3)	O-Logit (4)
$\Delta \log(\text{spending})$	.0232*** (.00451)	.0116** (.00564)	.0202* (.0107)	.0458** (.0191)
Male (= 1)	.0452 (.0179)	...	-.255 (.0206)	...
Married (= 1)	.205* (.0294)	...	-.0183 (.0189)	...
Age	.000388 (.000910)	...	-.000580 (.000789)	...
R <sup>2</sup>	.028	.022	.120	.014
Observations	3,561	3,139	3,112	3,139
Time fixed effects	No	Yes	Yes	Yes
Committee transfers	No	Yes	No	Yes
District fixed effects	No	No	Yes	No

Note. Coefficients for a cross-sectional regression (col. 1) and a first-difference regression (cols. 2–4). The dependent variable is the net votes for the Republican candidate (col. 1) or the change in net votes for the Republican candidate (cols. 2–4). The  $\log(\text{spending})$  variable measures (in col. 1) the difference between the natural log of spending by the Republican candidate and the Democratic candidate on advertisements and events. In cols. 2–4,  $\log(\text{spending})$  measures the change in the natural log of Republican candidate spending on advertisements and events relative to the natural log of the Democrat’s spending. Robust standard errors clustered at the state level in parentheses.

\*  $p < .1$ .  
 \*\*  $p < .05$ .  
 \*\*\*  $p < .01$ .

well as the inclusion of demographic characteristics.<sup>15</sup> Regardless of the specification, I find the effect of spending on a candidate's success to be positive and statistically significant. Using table 2 columns 2 and 3, I find that a one-unit change in the log of spending leads to a 0.0116–0.0202 increase in the net votes for a candidate, depending on the covariates selected. This means that a 10% increase in the spending for one candidate relative to the spending of the other, would increase in the net votes an individual casts for that candidate by 0.00116–0.00202. Consider an election with 500,000 voters, the average number of eligible voters in a congressional district. A 0.00116–0.00202 change in the net votes for all voters would mean a change in the net votes of 580–1,010. A 580-unit change in net votes would occur if 580 potential voters entered an election to vote for a candidate or if 290 voters switching from the opponent to a candidate. Column 4 presents the results from an ordered logit regression, which indicates similar (though larger) causal effect.

To put these results in context, consider a \$100,000 increase in spending on advertising and events for a Republican candidate. Using the estimates in columns 2 and 3, this infusion would lead to a swing in the net vote of 0.0200–0.0349. In a two-candidate race where everyone votes for one of the two candidates, this means that the candidate spending the extra \$100,000 would see their vote share increase by 1%–1.745%, with a corresponding drop in the opponent's vote share.<sup>16</sup> This shift would change the winner in between 12 and 20 of the 2012 House of Representatives races.

Table 3 shows the estimates of the effect of all other spending categories. One of the primary claims of this article is that only some categories of spending are used in support of a candidate's own success; therefore, spending in several other categories should not be associated with an increase in voters' opinions. The category-specific estimates support this claim. The most important results are those for categories of spending for which there can be no possible electoral benefit, such as loan repayments, refunds, transfers,

15. This forces me to drop the committee transfer measure, as it does not vary within districts. Also, by including observable characteristics, I am also providing a test for the validity of the first-difference regression. Since the identifying assumption of the first-difference specification is that by subtracting the two equations, I am fully capturing the effect of time-invariant, unobserved characteristics, I certainly hope that it will also capture the effect of time-invariant observable characteristics. The results in col. 3 of table 2 provide evidence that I have achieved this, as none of the coefficients on voter characteristic variables are statistically significant.

16. Because my sample includes voters and nonvoters, this effect is larger when turnout is lower. If voter turnout were 70%, a 0.02 swing in net votes for every potential voter would lead to a 2.86% shift (0.02/0.70) in the vote share for a candidate, or an increase of 1.43% for that candidate with a 1.43% decrease for the other candidate.

Table 3. Effect of Spending by Category

	$\Delta \log(\text{Spending})$	SE
Administrative	.0119	.0107
Travel	−.000823	.00548
Fund-raising	.00693	.00555
Polling	−.00834	.00571
Materials	.00150	.00513
Transfers	.00306	.00515
Loan repayments	.00227	.00421
Refunds	−.00285	.00617
Contributions	.00103	.00365
Donations	.000691	.00727
All spending	.0306	.0204

Note. Coefficients for a first-difference regression, with the same specifications as that in col. 2 of table 2. The dependent variable is the change in net votes (for the Republican candidate). The  $\log(\text{spending})$  variable measures the change in the difference of the natural log of Republican candidate spending on advertisements and events relative to the natural log of the Democrat's spending. Time dummy and committee transfer variables included. Robust standard errors clustered at the state level.

\*  $p < .1$ .

\*\*  $p < .05$ .

\*\*\*  $p < .01$ .

contributions, and donations, because they provide a test of the validity of my identification strategy. If voter opinion was changing due to some unobserved factor that also changed candidate spending on ads and events, instead of due to causal effect of spending itself, this factor would also likely affect candidate spending in these categories.

The coefficient on administrative expenditures, which constitute the largest portion of candidate spending, is positive and somewhat large. Some campaign activity that is used to persuade voters (such as payroll for Get Out the Vote staffers) may fall into this category. By focusing only on ads and events, my goal was to use categories that were most likely to have a majority of spending that was being used to persuade voters, not to capture all possible methods of voter persuasion. As I show in the appendix, my main estimates of causal effect are robust to the inclusion of additional categories of candidate spending, such as administrative, travel, or materials. The direction and significance of the results shown in table 2 do not rely on my decision to not include these types of candidate spending. But I also show in the appendix how the inclusion of just one category that constitutes spending that is not used to persuade voters (fund-raising) leads to statistically insignificant estimates.

Finally, I estimate the effect of candidate spending if I use all disbursements, regardless of purpose, as my measure of candidate spending. This measure is most analogous to the

measures that are most often used in the literature. The results are shown in the final row of table 3.<sup>17</sup> The point estimate increases, but so does the standard error, and I cannot reject the null hypothesis that spending has no effect on voter opinion. This suggests that including all categories of candidate disbursement in the measure of candidate spending introduces noise to empirical estimates. The larger point estimate (compared to that in table 2) further loses importance when I estimate the impact of a \$100,000 increase in candidate spending, which is actually smaller than when I use the results from table 2. This is because the independent variable now includes more disbursements, and a \$100,000 increase therefore has a smaller impact on the independent variable.

Erikson and Palfrey (2000) find that incumbent spending in close elections has a much larger estimated marginal effect than in safe elections. While the authors argue that the marginal effect of close elections is the true unbiased estimate (while that for safe elections suffers from simultaneity bias), Gerber (2004) argues that the marginal effect of spending may change with the competitiveness of an election. These results offer a mechanism for this argument: candidates (especially incumbents) spend much more on advertising, and less on disbursements that have no marginal effect, when the election is expected to be close. Since this type of spending has a larger marginal effect, the resulting point estimates will similarly be larger.

### HETEROGENEOUS EFFECTS

Individual-level data allow me to look at certain margins along which people can be affected by candidate spending.<sup>18</sup> Are the observed changes seen in table 2 occurring because turnout is changing, people are switching their vote from one candidate to another, or a combination of the two? First, I look at people who in both the pre- and postelection survey stated a preference for a candidate. The only possible variation in this group comes from party switchers. The results are shown in column 1 of table 4 (which I call the “Party” margin). The point estimate is less than half that of the full estimate, and is not statistically significant. Column 2 shows the estimates when I only include people who either did not intend to vote for either the Republican or Democrat or ultimately did not vote for either (the “Turnout” margin). The positive, significant coefficient indicates that candidate spending is particularly effective in changing the composition of voters in their favor. Even though a single voter switching parties has a larger impact on the net votes for a candidate (from  $-1$  to  $1$ ) than

Table 4. Effects along Different Margins

Margin	Party (1)	Turnout (2)	Turnout: Nonvoters (3)
$\Delta \log(\text{spending})$	.00445 (.00847)	.0209** (.00794)	.0333*** (.0108)
Observations	1,967	1,172	947
$R^2$	.032	.049	.063

Note. Coefficients for a first-difference regression. The dependent variable is the change in net votes (for the Republican candidate). The  $\log(\text{spending})$  variable measures the change in the difference of the natural log of Republican candidate spending on advertisements and events relative to the natural log of the Democrat’s spending. Column 1 restricts the sample to respondents who stated a preference for one of the candidates in both the preelection and postelection survey. Column 2 restricts the sample to respondents who either did not have a preference in the preelection survey or stated during the postelection survey that they did not vote. Column 3 restricts the sample to people who stated during the postelection survey that they did not vote. Time dummy and committee transfer variables included. Robust standard errors clustered at the state level in parentheses.

\*  $p < .1$ .

\*\*  $p < .05$ .

\*\*\*  $p < .01$ .

a voter leaving an election (a one-unit swing), candidate spending has the greatest effect along the turnout margin.

Column 3 shows that within the turnout margin, spending is most effective in driving voters out of the election. By restricting the sample to people who did not vote, the only variation in the dependent variable is coming from people who initially intended to vote, but ultimately abstained. This margin is the most active one along which voters move in response to candidate spending on messages. These results are consistent with the demobilization hypothesis (Ansolabehere et al. 1994, 1999; Krupnikov 2011) and also support the conclusions of Spenkuch and Toniatti (2018), who find that advertising affects election outcomes by changing the composition of the electorate.

Given the rich set of questions asked as part of the ANES, it is also possible to test for heterogeneous treatment effects in order to identify characteristics of persuadable voters, and I find significant differences across different types of voters in their responsiveness to candidate spending.<sup>19</sup> Most of my findings find their theoretical motivation in extensions of the median voter theorem with voter abstention (Adams and Merrill 2003; Hirich and Ordershook 1969; Ledyard 1984) or with poorly informed voters. In Baron’s (1994) model of

17. Estimates for all specifications shown in table 2 are in the appendix.

18. Unless stated otherwise, I use the specification from col. 2 of table 2, but the results are robust to the alternative specifications.

19. Persuadable voters are distinct from swing voters, who may change their minds for any reason and may not respond to candidate spending.

electoral competition, candidates use costly campaign messages (which they pay for with contributions from special interest groups) to attract low-information voters, while high-information voters are unaffected. In equilibrium, candidates can refrain from converging to the median voter while using campaign spending to attract low-information voters.

Candidate persuasion can also play a role in models with voter abstention. When voters can decline to vote at all, candidates must weight the benefit of moving toward the median voter against the cost of losing ideologically extreme voters to abstention. If candidates can use campaigns to sway voters, they may try to either appeal to ideological moderate or extremes on their side, or try to convince extreme voters of the opposing ideology to abstain from voting for their opponent. Therefore, whether they are ideologically moderate or extreme, partisan or independent, potential voters who are more affected by candidate spending provide an important empirical test of the theoretical framework.

First, I compare the causal effects across individuals' displayed level of information. Low-information voters may have less-developed opinions of the candidates and therefore be more responsive to the candidate persuasion; numerous studies have found low-information voters to be more persuaded by candidate messaging (Franz and Ridout 2007; Valentino, Hutchings, and Williams 2004). I measure voter information through six questions asked during the post-election survey. I assign each respondent a score based on how many questions they get correct and divided that score by 6 to create an index that goes from 0 to 1. I then interacted that variable with the measure of candidate spending.

Panel A of table 5 reports the results of this regression. The large coefficient on  $\Delta\log(\text{spending})$  shows that low-information voters (for whom the variable is equal to 0) are highly affected by candidate spending. The negative coefficient on the interaction term indicates that high-information voters are significantly less likely than their low-information counterparts to be affected by candidate spending. The similar magnitudes of the two coefficients suggests that high-information voters (who have index scores of 1) are unaffected by spending.

Next, I consider a person's political party affiliation by comparing the effect of candidate spending on independents and nonindependents. Party identification is taken from the preelection survey, so responses are not the result of swayed voters stating their party membership to be that of the candidate for whom they voted. The results, shown in panel B of table 5, indicate that independent voters are less influenced by candidate spending than people who identify with a political party. The coefficient on  $\Delta\log(\text{spending})$  shows the estimated effect on nonindependents, and the point esti-

Table 5. Differential Effects of Campaign Spending

Variable	First Difference (1)	First Difference (2)	Logit (3)
A. Effect by Knowledge			
$\Delta\log(\text{spending})$	.0310*** (.0103)	.0317** (.0137)	.131*** (.0387)
Knowledge index	-.0285 (.0346)	.00716 (.0431)	-.187 (.128)
Knowledge index $\times$ $\Delta\log(\text{spending})$	-.0379** (.0147)	-.0258 (.0210)	-.166*** (.0586)
Observations	3,139	3,112	3,139
R <sup>2</sup>	.022	.12	.015
B. Effect on Independents			
$\Delta\log(\text{spending})$	.0230*** (.00694)	.0376** (.0144)	.0908*** (.0264)
Independent (= 1)	-.00720 (.0233)	-.0207 (.0263)	-.0790 (.0918)
Independent $\times$ $\Delta\log(\text{spending})$	-.0331** (.0150)	-.0440*** (.0151)	-.130** (.00557)
Observations	3,139	3,112	3,139
R <sup>2</sup>	.025	.124	.016
Time fixed effects	Yes	Yes	Yes
Committee transfers	Yes	No	Yes
District fixed effects	No	Yes	No
Individual characteristics	No	Yes	No

Note. Coefficients for a first-difference regression. The dependent variable is the change in net votes (for the Republican candidate). Knowledge index is a variable between 0 and 1 that captures what ratio of questions (out of 6) the respondent answered correctly. Independent is a variable equal to 1 if the respondent did not identify as a member of either the Republican or Democratic party. The  $\log(\text{spending})$  variable measures the change in the difference of the natural log of Republican candidate spending on advertisements and events relative to the natural log of the Democrat's spending. Robust standard errors clustered at the state level in parentheses.

\*  $p < .1$ .

\*\*  $p < .05$ .

\*\*\*  $p < .01$ .

mates indicate that all of the effect observed in table 2 is coming from Republicans or Democrats, instead of independents. This finding is similar to that of Franz and Ridout (2007) in their study of advertising during the 2004 election. They found that independents and moderates were not more affected by messages from candidates, and the strong Democrats were more affected than others (although strong Republicans were not). Along with the results from table 4,

these results show that candidate spending is effective on party identifiers by changing the composition of the electorate (instead of convincing people to vote for a different candidate). Candidates use spending to decrease turnout in ways that change the pool of voters in their favor.

Finally, I find one last difference across voters, though one that cannot be explained by spacial models of candidate and voter preference. Economic voting appears to play an important role in the effect of candidate spending. When potential voters were asked about the current state of the economy, about 60% identified it as bad or very bad. Table 6 shows the results from a regression that interacts a dissatisfaction dummy with candidate spending. The coefficient on  $\Delta \log(\text{spending})$  means that voters who believed the economy was in good shape were unaffected by candidate spending. However, the interaction term shows that economically dissatisfied voters are especially receptive to candidate spending. If a district consisted of only economically dissatisfied voters, I estimate that a \$100,000 change in spending by the Republican would lead to a swing in the net vote of 0.0401 to 0.0558 in favor of that candidate. In a two-candidate race, this

would result in a 2%–2.79% increase in the vote share for the candidate. These results introduce a nuance to classical economic voting theories, which argue that voters respond to good (bad) economic conditions by rewarding (punishing) the incumbent’s party (Kramer 1971; Powell and Whitten 1993). In this framework, any such punishment of an incumbent is fully captured by the first-difference specification. Instead, the results indicate that negative economic shocks push affected voters onto a margin that can be influenced by candidate messages.

These results show that average estimated effects of candidate messages mask a large diversity of voters across different margins and characteristics. It is important to note that these results do not indicate that high-information, independent, and economically satisfied voters are necessarily less likely to change their votes than others. For instance, independent voters are slightly more likely than nonindependents to change their preferences after the preelection survey. Instead, changes in voting behavior of some voters are predicted strongly by candidate spending, while changes in preferences among other voters is not.

Table 6. Differential Effects of Campaign Spending: Effect by Economic Satisfaction

Variable	First Difference (1)	First Difference (2)	Logit (3)
$\Delta \log(\text{spending})$	-.00758 (.00886)	-.00222 (.017)	-.0306 (.0324)
Dissatisfied (= 1)	-.0816*** (.0233)	-.0809*** (.0270)	-.342*** (.0900)
Dissatisfied × $\Delta \log(\text{spending})$	.0303*** (.0110)	.0345** (.0136)	.119*** (.0423)
Observations	3,139	3,112	3,139
R <sup>2</sup>	.028	.126	.019
Time fixed effects	Yes	Yes	Yes
Committee transfers	Yes	No	Yes
District fixed effects	No	Yes	No
Individual characteristics	No	Yes	No

Note. Coefficients for a first-difference regression. The dependent variable is the change in net votes (for the Republican candidate). Dissatisfied is equal to 1 if the respondent said that the current state of the economy was either “bad” or “very bad.” The  $\log(\text{spending})$  variable measures the change in the difference of the natural log of Republican candidate spending on advertisements and events relative to the natural log of the Democrat’s spending. Robust standard errors clustered at the state level in parentheses.

\*  $p < .1$ .  
 \*\*  $p < .05$ .  
 \*\*\*  $p < .01$ .

**CONCLUSION**

This article contributes to the large literature on the effect on spending on political outcomes by introducing an empirical specification that relies on within-election variation in spending and voter preferences. Though the setting studied here is the final weeks before an election, when many voters may have already made up their minds, I find that candidate spending has a strong effect on the decisions of voters. Individual-level survey data allow me to estimate the effect of spending across groups, and I find that low-information voters, party members, and economically dissatisfied voters are highly affected by candidate spending, and that spending changes election outcomes by dissuading people from voting who would otherwise support a candidate’s opponent.

The marginal effect of spending found in my results is within the range of effects found in the literature, despite the fact that I can only estimate the effect of late-campaign spending, after hundreds of millions of dollars has been spent. This result has several possible implications. First, campaign spending may not exhibit a strong decay in marginal effects; if it did, I would expect smaller marginal effect in the setting studied here. Alternatively, the effect of campaign spending, and especially messaging, may be short-lived. Numerous studies have found campaign messages to be most effective in short-term persuasion (Hill, Lo, and Vavreck 2013; Kalla and Broockman 2018). Given the focus by policy makers on late-campaign activity, as seen in rules such as lowest unit rates or blackout periods, further study on the role of money in

campaigns during the final weeks before elections could be especially illuminating.

These results also suggest that the spending patterns of candidates in close elections (whereby most of their expenditures on messages occur in the final month of the campaign) could be rational responses to a heavily politicized electorate. Bartels (2014) found that during the 2012 presidential election exposure to Obama ads had a significant but decaying effect on Romney supporters. Given that one of the most active margins in my sample was party members who were discouraged from voting, those findings from the presidential campaign suggest that congressional campaigns may be devoting a large portion of their spending on persuading voters late in the campaign so as to minimize the decay of that persuasion. In a polarized political environment, these findings suggest that campaigns are important, especially since nonmandatory voting makes discouragement a feasible option.

Perhaps most importantly, the results suggest that researchers should focus not only on the amount, but also on the nature of spending. Even in an environment where candidates can exert significant influence on their electoral outcomes, unbiased estimates of the marginal effect of spending may find zero effect if those candidates are spending their resources on disbursements that will not change vote shares. Levitt (1994), using a sample of repeated challengers, finds only a small effect of spending on vote shares, while Erikson and Palfrey (2000) find a large marginal effect in their study of close elections. Does one of these papers offer a less biased estimate of the same causal effect, or do systematic differences in the composition of their populations of elections lead to two separate, unbiased estimates? Given the large amount of literature that has (out of necessity) used aggregate spending as the primary right-hand-side variable, a useful vein of future research would be to revisit the literature's findings using detailed disbursement data.

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