What Was the Persuasive Effect of Televised Campaign Advertising in the 2016 Presidential Election?

Abstract

Televised campaign advertising remains one of the most significant expenditures in U.S. Presidential elections, yet its persuasive impact on voters continues to be the subject of debate. In this registered report, we propose a new observational design that uses a high-frequency, seventeen-week panel survey of more than 4,497 voters during the 2016 Presidential election. By tracking the same individuals over time, we examine how variations in television ad spending relate to changes in voting intentions using individual fixed effects. The frequency with which voter attitudes are observed and our ability to reject differential survey non-response are two particular strengths of this dataset. Our approach revisits a substantive question of long-standing importance using new data sources and methods.

Introduction

Televised campaign advertising is a central feature of modern political campaigns in the United States, with billions of dollars spent each election cycle to shape voter attitudes and behavior (Shaw, Althaus, and Panagopoulos 2024; Fowler, Franz, and Ridout 2018; Goldstein and Ridout 2004; Goldstein and Freedman 2002). During the 2016 election for instance, Hillary for America paid a single media consultant, GMMB, 308 million dollars for the purpose of buying broadcast media advertisements.[1] This enormous sum amounted to well over half of the campaign's total spending. Indeed, television ads are almost always the biggest single line item for a U.S. Presidential campaign. Non-academic observers commonly presume that this spending has very significant consequences for citizens' decisions about whether and for whom to vote.

Despite the massive financial investment in televised campaign advertising and its widely-presumed efficacy, within the political science discipline questions persist about whether and how much campaign advertising actually influences voter intentions and behaviors. While some studies suggest that advertising often plays a very substantial role in persuading voters (Ansolabehere, Behr, and Iyengar 1993; Ansolabehere and Iyengar 1995; Goldstein and Ridout 2004; Spenkuch and Toniatti 2018; Sides, Vavreck, and Warshaw 2022), especially in down-ballot or primary races, others argue that its effects are minimal, short-lived, or primarily reinforce existing preferences (e.g. Gerber et al. 2011; Broockman and Kalla 2022). Observational studies, even those that are careful about causal identification, have often found bigger impacts than experimental studies (cf. Coppock, Hill, and Vavreck 2020; Sides, Vavreck, and Warshaw 2022), however they are still subject to somewhat greater concerns about identifying assumptions. The literature is one where there is significant room and need for studies that use independent data sources and approaches. A registered report is particularly beneficial in such cases, even if the design is observational, as it encourage researchers to focus on using the most persuasive methods without fear or favor of the direction or significance of their statistical findings.

This paper contributes to this literature by examining the persuasive impact of television ads in the 2016 U.S. presidential election. The most distinguishing aspect of this particular study is its outcome data. To examine voter attitudes, we use a relatively large panel survey that recontacted the same 4,497 individuals every week over a seventeen-week period leading up to the 2016 general election. We link these high-frequency panel survey data to ad spending data through the use of sensitive, low-level geographic identifiers provided by the vendor under rigid access controls, which are nevertheless open to other researchers willing to research under similar conditions. Our paper has a similarity to other media market studies (e.g. Franz and Ridout 2007), however these have not had nearly so many observations over time from the same person. By

1. https://www.fec.gov/data/disbursements/?data_type=processed&committee_id=C00431569&committee_id=C00575795&recipient_name=GMMB&two_year_transaction_period=2016

contrast, the seventeen-week repeated individual observation panel we use allows us to measure how shifts in ad exposure over time correspond to changes in vote intention over time. Through our unique observational design, we provide new insights into the mechanisms and limits of campaign advertising effects in modern elections.

Persuasive Effects of Political Advertising in U.S. Elections

Existing work demonstrates that television advertising can have short-term persuasive effects, though these effects vary depending on factors such as timing, candidate familiarity, and electoral context (Coppock, Hill, and Vavreck 2020; Sides and Vavreck 2013; Huber and Arceneaux 2007; Shaw 2006). While some analyses detect only small changes in voter attitudes due to campaign spending, even modest effects can be decisive in closely contested races (Sides, Tesler, and Vavreck 2018). Advertising's impact also differs by election type, with stronger effects observed in down-ballot races where voters typically have less pre-existing information about candidates (Sides, Vavreck, and Warshaw 2022; Jacobson 2015). The importance of persuasion in these contexts aligns with theories of voter learning, which suggest that advertising provides novel information that voters incorporate into their decision-making (Freedman, Franz, and Goldstein 2004; Carpini and Keeter 1996). And yet in times of increasing polarization and as the media environment has become increasingly fragmented, it is possible that television advertisings effectiveness might have declined.

As mentioned in the introduction, studies of campaign advertising employ methodologically diverse approaches, including randomized experiments, natural experiments, and observational designs. While some experimental work has found small but usually rapidly decaying persuasive effects (Broockman and Kalla 2022; Kalla and Broockman 2022; Coppock, Hill, and Vavreck 2020; Kalla and Broockman 2018; Gerber et al. 2011) others have shown that advertising can indeed durably influence voter preferences under certain conditions (Sides, Vavreck, and Warshaw 2022; Spenkuch and Toniatti 2018; Fowler, Franz, and Ridout 2016; Huber and Arceneaux 2007). The vast majority of observational studies of presidential campaigns advertising effectiveness use correlational designs that are broadly similar to one another. Essentially, these designs focus on whether campaigns receive more votes in places where they spend more money. Typically, causal credibility in these studies derives from the use of geographical discontinuities, for example between neighboring counties that were and were not exposed to advertising. While the findings of such observational analyses do help to rationalize presidential campaign behaviors in some ways, the short-lived nature of advertising effects in experimental contexts raises concerns about how observational approaches have aggregated geographically and temporally. For example, Spenkuch and Toniatti (2018) aggregate advertising exposure over a fixed 60-day period before the election, implicitly assuming that persuasive effects persist over time, despite evidence that they decay within weeks or even days (Gerber et al. 2011). Additionally, the geographic discontinuity approach commonly used in these studies assumes that neighboring counties assigned to different media markets are truly comparable, despite potential differences that may violate identification assumptions.

A High-Frequency Panel Approach to Measuring Ad Effects

By combining a high-frequency individual panel survey with ad spending data, our study overcomes these limitations, capturing within-person changes in voting intentions in response to shifts in local ad exposure. To do this, we draw on the Daybreak Poll, a panel survey that tracked the opinions and vote intentions of 4,497 voters weekly throughout the 2016 U.S. Presidential general election campaign. Although the recontact design allows us to observe non-response for individuals who have taken at least one survey, not all individuals in the Daybreak subject pool were invited to participate immediately, so the set of subjects in this study grows larger over time. The result is an unbalanced panel (n = 50,016), where the number of weeks of observations per individual varies. We then geographically match the voter survey to data on ad spending to obtain our treatment variable of ad spending. The frequency of contact with likely voters allows us to examine how over time variation in ad spending relates to over time variation in attitudes. This is a key innovation as it overcomes the issue of decaying effects faced by many observational studies, and also permits the analysis of attitude changes *within person* in response to changes in the campaign environment. Moreover, we include time fixed effects, which allow us to control for level shifts in aggregate support over time. We thus formulate the following research hypothesis:

- Hypothesis 1: Increased intensity of spending on television ads by a U.S. Presidential candidate is associated with increased likelihood voters will intend to vote for that candidate. This hypothesis is associated with the following three, related null hypotheses.
  - H_{1A}: An increase in observable spending by Clinton has no relationship with vote intentions favoring Clinton.
  - H_{1B}: An increase in observable spending by Trump has no relationship with vote intentions favoring Trump.
  - H_{1C}: An increase in net observable spending by Clinton over Trump has no relationship with net intentions to vote for Clinton over Trump.

The most significant methodological challenge in studies like this one is that the media environment is both messy and partially observed. Spending is also undertaken strategically in response to shifts in the electorate, behaviors of the opposite campaign, and actions undertaken by other non-campaign actors such as PACs and super-PACs. If outside forces influence voter attitudes in a way that are also correlated with campaign spending, there is a possibility of omitted variable bias (OVB). Even experimental studies cannot completely rule out the possibility of such interference, in so far as their "experimental treatment" might draw attention from other campaigns who then respond with counter-acting advertising. We know of no silver bullet addressing this concern on a topic of such great policy importance, however we think the "strongest" threat of OVB arises from the activity of the other campaign. Therefore, we focus on a second set of hypotheses that consider most directly circumstances where the Clinton (Trump) campaign is advertising unopposed, at least on television. To do so, we will subset our data to only look at the effect of various levels of campaign spending by Clinton, for example, on attitudes in geographies where Trump has not spent any money whatsoever. If unobserved counter-active forces are attenuating the estimated effects of campaign advertising, it is reasonable to expect that to become most obvious when we most rigorously exclude television advertising by the opposite campaign.

- Hypothesis 2: Increased intensity of *unopposed* spending on television ads by a U.S. Presidential candidate is associated with increased likelihood voters will intend to vote for that candidate. This substantive theoretical hypothesis gives rise to two related null sub-hypotheses.
  - H_{2A}: An increase in unopposed observable spending by Clinton has no relationship with vote intentions favoring Clinton.
  - H_{2B}: An increase in unopposed observable spending by Trump has no relationship with vote intentions favoring Trump.

Although our primary interest is in persuasive effects, our design can easily be adapted to consider additional outcome variables, including most notably voter turnout. While we cannot directly observe whether respondents voted, the panel survey contains a question about intention to vote, which is measured as the self-reported percent chance of voting. We therefore wish to test the effect of spending on intention to vote. One caveat is that mobilization of supporters and demobilization of opponents are potentially different goals pointing in different directions. We address this in a few ways. We define "probable" Clinton voters as those whose first expressed intention of voting for Clinton exceeds 50% while "probable" Trump voters are those who are more than 50% for Trump on that first contact. In a similar fashion to Hypotheses 1 and 2, we examine variously whether Clinton spending spending mobilizes her probable voters or demobilizes Trump's probable voters. We examine gross Clinton spending in all areas, Clinton spending net of Trump spending in all areas, and separately consider places and times when Clinton spending is unopposed, where gross and net spending are the same. We also do all these things from the perspective of Trump as opposed to Clinton.

It is worth acknowledging that our approach assumes uniform exposure within media markets and does not directly measure whether individual respondents actually saw the ads aired in their area. This is a standard strategy in the campaign advertising literature (e.g., Franz and Ridout 2007; Spenkuch and Toniatti 2018), because of how costly and impracticable it is to observe actual consumption behaviors. Importantly, this limitation does not necessarily undermine our identification strategy. First, to the extent that actual ad exposure varies idiosyncratically across individuals within a media market (for example, due to different viewing habits), this likely introduces classical measurement error, which would attenuate estimated effects toward zero. Second, presidential campaigns generally purchase television ads at the media market level in order to reach large, demographically broad audiences, rather than tailoring ads to specific subgroups within a market. This practice reduces the risk of systematic within-market variation in exposure that could bias our results. It also implies that the effect we examine is often the one that campaigns consider relevant. Third, because we follow the same individuals over time and estimate models with individual fixed effects, we account for stable, time-invariant differences in respondents' media consumption, such as whether they regularly watch television at all. These factors give us confidence that our approach is still worth pursuing even without individual-level viewership data.

To extend and check the robustness of our work, we examine how our estimates would change on a sub-sample of respondents for which we do have some roughly contemporaneous media consumption data. Here, we take advantage of one of the strengths of the survey platform we use, which is that respondents' identities are linked across all surveys in which they participated. We expect a large number of respondents in our study will have also answered questions in 2017 about whether they get their news from social media and how frequently they watch Fox, CNN, and MSNBC. The fact that the media consumption data comes from some months after the conclusion of the 2016 general election and not during or before raises some concerns about post-treatment variables, however we expect most individuals' media habits will remain stable especially over relatively short time horizons. We use these data in two ways. First, we restrict spending to only those channels for which we have consumption information (i.e. Fox, MSNBC, CNN) and interact spending with channel viewership in specifications similar to those we propose for our main study. Second, we examine whether the estimates change for users of social media. In the 2016 election in particular, there are reports that spending on social media platforms started to take off, especially for the Trump campaign. The cover of the December 2016 issue of *Forbes Magazine*, for example, featured a glowing picture of Jared Kushner paired with the headline "This Guy Got Trump Elected" – purportedly because of his extensive use of advertising on Facebook and Twitter (Bertoni 2016). Presumably, if that social media campaign spending was a significant source of omitted variable bias, the estimates of campaign spending efficacy should look different among users and non-users of social media.

Data

Our design takes advantage of two unique data sources. First, for our dependent variable of vote intention in the 2016 presidential election, we use the USC Dornsife/LA Times survey poll, also called the "Daybreak" poll. This survey draws from a pool of respondents who are continuously enrolled by USC's Dornsife Center. Participants are selected via probability sampling (Understanding America Study 2024). Starting on July 4, 2016, the survey asked respondents daily about their voting intentions and perceptions of the race. It was conducted online through the Understanding America Study (UAS), a panel of households put together by the University of Southern California. UAS is an "Internet Panel" meaning that respondents answer surveys on a device like a tablet or smart phone wherever and whenever they wish to participate. Households are recruited to the panel via address-based probability sampling. Households without internet access are provided with a tablet and internet service. A complex series of incentives are put in place to recruit and retain panelists, the rules of which have evolved over time but are well-documented. Participants are paid at the rate of $40 per hour, and various mechanisms are in place to detect panelists who are "straightlining" or pursuing an ordered response strategy. Time to respond to questions is also monitored and a data file flags the identities of occassional respondents whose unreliability may have contaminated past data.

For the 2016 election study, respondents were assigned a participation day each week and could complete the survey at any time online. Not every panelist participated on the day they were invited to do so. If the respondent took more than one week to answer, their response was excluded. The polling response data thus constitutes an unbalanced panel with occasionally missing responses, containing up to 17 responses per individual panelist over the course of the summer and fall of the 2016 presidential election. Of the total of 5,706 invited panelists, 5,007 completed at least one survey (88%), and 4,509 contributed responses used in our analysis. The UAS verifies panelists' identities and links responses to unique household records, minimizing risks of duplicate entries or fraudulent submissions.

Matching on the location and time of the responses, we combine the "Daybreak" poll with data from the Wesleyan Media Project (WMP), which provides comprehensive coverage of political advertisements in the United States. The WMP data is sourced from Kantar/CMAG and tracks ad airings on local broadcast, national broadcast, and national cable television going back to 2010, covering all 210 media markets in the country. The granularity of the WMP data is a significant asset for our study. Each observation corresponds to an ad airing, capturing detailed information including the day, time, cost, and location (media market) alongside content variables such as tone or topic. In addition to the high level of detail in terms of time and place, we are also able to identify exactly who paid for an ad, as sponsors are classified into categories and identified by name. The data also provides us with the type of election an ad was intended to influence (i.e. ”Presidential” or ”Gubernatorial”).

By matching the time and location of campaign ad spending captured by the WMP data to responses in the Daybreak poll, we are able to correlate ad spending with changes in voter intentions using a two-way fixed effect model. Observational studies of campaign spending effects such as ours are typically subject to concerns about omitted variables, i.e. variables that could plausibly have a statistical relationship with both the persuasive effect of ads as well as with the rationale behind running an ad in a specific location (e.g. education level). However, unlike most similar polls, the Daybreak Poll surveys its panel *every week*. This means that we are able to control for such omitted variables using individual fixed effects, assuming that they are time-invariant for the duration of the general election campaign in 2016.

Analysis

Our main models seeks to estimate the impact of television ad spending by 2016 Presidential campaigns on voting intentions.

$$\Pr(\text{Vote Clinton})_{imt} = \beta_1 \cdot \log_{10}\big(\text{Clinton Spending Intensity}_{mt} + 1\big) + \psi_i + \theta_t + \epsilon_{imt} \tag{1}$$

$$\Pr(\text{Vote Trump})_{imt} = \beta_1 \cdot \log_{10}\big(\text{Trump Spending Intensity}_{mt} + 1\big) + \psi_i + \theta_t + \epsilon_{imt} \tag{2}$$

$$\Delta \Pr(\text{Vote Clinton})_{imt} = \beta_1 \cdot \log_{10}\left(\frac{\text{Clinton Spending Intensity}_{mt} + 1}{\text{Trump Spending Intensity}_{mt} + 1}\right) + \psi_i + \theta_t + \epsilon_{imt} \tag{3}$$

where ψ_i is the individual fixed effect, θ_t is the time fixed effect, and ϵ_{imt} is the disturbance term. Although omitted here for brevity, we use analogous models for turnout. Importantly, because ad spending varies at the media market level m rather than the individual level, we denote it as Spending Intensity$_{mt}$. An immediate implication of this structure is that standard errors should be clustered at the media market level, as individuals within the same media market share the same treatment intensity. We implement this correction by clustering standard errors at the media market level (Abadie et al. 2023) to account for potential within-market correlation in the treatment effects. As robustness checks, we will also run the model with a lagged term for opponent spending, which accounts for the strategic spending behavior of campaigns. This adjustment controls for the possibility that a candidate's spending in a given market at time t is a reaction to the opponent's spending in the prior period $t-1$. We also test the number of ad airings instead of spending as an explanatory variable. We expect that specification to produce similar results to our main analysis. Because the range of the outcome variables are constrained in each equation, generalized linear models (GLMs) such as the logit or probit are theoretically appealing, however fixed effects with GLMs are biased if the number of periods is relatively small (Chamberlain 1980).[2]

Further, we note that the key explanatory variable is highly skewed and likely subject to diminishing marginal returns, therefore logging this variable is appropriate in our main specifications. In many locations, the amount of spending observed is 0 in which case the log would be undefined, for this reason we add $1 of spending to every place. The log specification also ties most cleanly to our preferred interpretation of the coefficients, as it may be interpreted as the percentage increase in spending across the board that would be necessary to generate a 1% shift in the outcome variable. In the Appendix, we also consider the non-logged equations.

Most observational studies have so far treated all spending observed for the *entirety* of a Presidential campaign as the treatment, implicitly assuming that resulting treatment effects do not decay. In our approach, we posit that there is a δ discount factor applied to the dollar spending values that produces a variable we call "candidate spending intensity." For example, if $\delta = 0.8$, an ad aired today counts fully, an ad aired yesterday counts as 80% of today's ad, an ad from two days ago counts as 64% (0.8^2), and so on, reflecting a decay in the strength of treatments over time. Prior experimental research suggests that the persuasive effects of television campaign ads decay rapidly. Gerber et al. (2011) estimate that the effect of an ad campaign decays by approximately 50% within a week and is nearly gone after two weeks. Hill et al. (2013) find a half-life of 2–3 days in congressional and gubernatorial elections and about 4–7 days in presidential elections. We initially estimate δ using a maximum likelihood approach, however we recognize the possibility that $\hat{\delta}^{MLE}$ may have implausibly wide confidence intervals or vary widely across specifications.[3] To address this possible issue, we supplement the maximum likelihood approach with a sensitivity analysis where we fix δ across all specifications, which we vary in increments of 0.05 between 0 and 1. Findings from both approaches will be discussed in the manuscript. Tables for all results will be included in the supplementary appendix.

2. Fixed effects with GLMs have an incidental parameters problem with too few periods. Even with remarkable progress in the analysis of difference-in-difference or two-way fixed effect specifications, there is still no solution we are aware of. In supplementary analyses, we will fit a conditional logit specification with individual fixed effects a la Chamberlain (1980) on a dichotomized outcome variable, however for the main analysis we consider the robustness we obtain via two-way fixed effects to be more desirable than a form for the likelihood function with the theoretically correct range.

3. It is hard to justify ex ante an arbitrary cutoff of what would be unreasonable. That said, since prior studies have estimated δ ranging from 0.7 to 0.9, we regard a smaller range of values as reasonable, while a substantially wider range, perhaps two or three times, would be implausible

Results

Descriptive statistics. The analytic sample contains 50,016 respondent-wave observations from 4,497 unique individuals interviewed over 19 weekly waves between August and November 2016. Respondents contribute an average of 11.1 survey waves (median = 12; range = 1–20), yielding an unbalanced panel. Because panel recruitment was staggered, the number of completed responses rises over the field period: weekly responses increase from 1,491 in week 1 to a peak of 3,520 in week 17, before dropping in the final wave (565 responses), consistent with a partially completed last week of interviewing. The survey spans 196 designated market areas (DMAs), with a mean of 23.2 respondents per DMA (median = 16; 10th–90th percentile = 3–45.5), implying broad geographic coverage with a long right tail of large media markets. Geographic inconsistency is rare: 0.9% of respondents ever appear in more than one DMA, and no respondent appears in more than two. Item nonresponse on demographics is low (all below 1.5%: age 1.0%, gender 0.9%, race 1.4%, education 0.9%, income 1.1%, state 1.0%). Advertising exposure varies sharply across DMA-weeks. Mean weekly spending is \$9,293 for Clinton and \$4,153 for Trump, but median spending is zero for both candidates because 71.9% of DMA-weeks contain no Clinton ads and 85.0% contain no Trump ads (both absent in 69.2% of cases). Clinton spends unopposed in 15.7% of DMA-weeks, while Trump spends unopposed in 2.6%. Maximum weekly spending reaches \$387,970 for Clinton and \$469,090 for Trump. Outcomes are measured as vote intention probabilities on a 0–100 scale: mean Clinton support is 42.6 (SD = 44.0) and mean Trump support is 41.7 (SD = 44.0), while mean turnout intention is 87.1 (SD = 28.2). Within-person variation in vote intentions is modest: among respondents with at least three observations, the mean within-respondent standard deviation is 7.8 for Clinton and 7.5 for Trump, with mean within-person ranges of 23.1 and 22.1 points, respectively; 57.0% of respondents report exactly the same Clinton vote intention across all observed waves and 57.8% do so for Trump.

Main effects. Table 1 reports fixed-effects models with respondent and week indicators, standard errors clustered by DMA, and $N = 50{,}016$. In a level-$\log_{10}$ model such as this, the coefficient represents the shift in the outcome induced by increasing the explanatory variable ten-fold. To be sure, that is a big jump in the explanatory variable, but it is a meaningful scale for thinking about campaign spending because it roughly corresponds to the difference in intensity between the first week of the general election and the last week, or from the lower intensity DMAs where campaigns spend money to the higher intensity ones. In some cases, it will be useful to discuss the consequences of doubling the explanatory variable, which produces a shift in the outcome of roughly $\beta/3$, while in others it will be worth thinking about increasing the explanatory variable by 10%, which produces a shift of roughly $\beta/20$. The coefficient of Spending Intensity on the Clinton vote, $\hat{\beta} = 0.076$ ($SE = 0.110$), implies that spending twice as much over the election campaign would move

Table 1: Main effects (H1): Decayed, logged ad spending

	Clinton Vote	Trump Vote	Clinton Vote
Spending intensity	0.076	−0.044	
	(0.110)	(0.067)	
Net spending intensity			0.068
			(0.089)
$\hat{\delta}$	0.865	0.010	0.939
Respondent FE	Yes	Yes	Yes
Week FE	Yes	Yes	Yes
SE clustered by	DMA	DMA	DMA
N	50016	50016	50016

aggregate opinion 2.33% percentage points in her favor. For Trump vote, $\hat{\beta} = -0.044$ ($SE = 0.067$), implying a tenfold increase would have decreased his support 4.4%. The coefficient on net spending intensity implies that doubling the Clinton spending advantage would have increased her support by 6.3% among survey respondents. That said, none of these effects are statistically different from zero at conventional levels. Estimated decay parameters are $\hat{\delta} = 0.865$ for Clinton vote, 0.010 for Trump vote, and 0.870 for Clinton advantage, but the profile-likelihood intervals span the grid $[0.01, 0.99]$, so we treat persistence as weakly identified. Adding a lagged opponent-intensity control leaves signs and inferences unchanged. Detailed estimates are reported in the appendix.

Unopposed windows. Restricting the sample to weeks and DMAs where the opposing campaign spent zero, candidates spending intensity is significantly associated with Clinton vote and imprecisely negative for Trump. For Clinton, $\hat{\beta} = 2.722$ ($SE = 1.084$) is distinguishable from zero at the one percent level using a one-sided test, but slightly shy of that level using a two-sided test ($p = 0.012$). The coefficient implies that Clinton increasing spending 10% more in places where she was unopposed was associated with a roughly 11.3% shift in her favor among respondents in those areas. For Trump, $\hat{\beta} = -1.060$ ($SE = 1.279$) is also substantitively large, but negative, which is implausible. In any event, the coefficient is not statistically different from zero. The estimated decay parameter has a value that is credible based on prior studies and reasonably well-identified on the Clinton-unopposed subset ($\hat{\delta} = 0.953$; CI $[0.88, 0.99]$), but again weakly identified for Trump ($\hat{\delta} = 0.059$; CI $[0.01, 0.99]$). Sample sizes reflect the rarity of unopposed exposure (N = 7,860 Clinton; 1,323 Trump), which likely contributes to the wider uncertainty in the Trump specification. As in the main-specification discussion, adding a lagged opponent term leaves signs and inference unchanged; those estimates appear in the appendix. Finally, we estimate the relationship between unopposed Clinton spending and support for Trump, finding the expected negative relationship, albeit smaller and less statistically surprising ($\hat{\beta} = -1.045$ ($SE = 0.698$)). The one-sided test, which seems reasonable in this case, is somewhat shy of significance ($p = 0.067$). That said, the estimates for the discount parameter suggest any such effect was quite transitory ($\hat{\delta} = 0.015$).

Table 2: Unopposed windows (H2)

	Clinton Vote	Trump Vote
Spending intensity	2.722	−1.060
	(1.084)	(1.279)
$\hat{\delta}$	0.953	0.059
Respondent FE	Yes	Yes
Week FE	Yes	Yes
SE clustered by	DMA	DMA
N	7860	1323

Turnout and demobilization. For Clinton mobilization, $\hat{\beta} = 0.008$ ($SE = 0.138$), implying about 0.002 percentage points for a doubling and 0.008 for a tenfold increase; not statistically different from zero. For Trump mobilization, $\hat{\beta} = -0.053$ ($SE = 0.084$), implying −0.016 for a doubling and −0.053 for a tenfold increase; not statistically different from zero. For Clinton decreasing vote intention for Trump, $\hat{\beta} = -0.083$ ($SE = 0.141$), implying −0.025 for a doubling and −0.083 for a tenfold increase; not statistically different from zero. For Trump decreasing vote intention for Clinton, $\hat{\beta} = -0.061$ ($SE = 0.108$), implying −0.018 for a doubling and −0.061 for a tenfold increase; not statistically different from zero. Persistence is weakly identified in all four specifications ($\hat{\delta} = 0.378, 0.963, 0.319, 0.507$). Sample sizes are $N = 18{,}112$ for Clinton mobilization and demobilization of Clinton supporters by Trump spending, and $N = 18{,}591$ for Trump mobilization and demobilization of Trump supporters by Clinton spending. Specifications with a lagged opponent term yield similar signs and inference; details are in the appendix.

Table 3: Turnout and demobilization

	(1)	(2)	(3)	(4)
Candidate spending intensity	0.008	−0.053		
	(0.138)	(0.084)		
Opponent spending intensity			−0.083	−0.061
			(0.141)	(0.108)
$\hat{\delta}$	0.378	0.963	0.319	0.507
Respondent FE	Yes	Yes	Yes	Yes
Week FE	Yes	Yes	Yes	Yes
SE clustered by	DMA	DMA	DMA	DMA
N	18112	18591	18591	18112

Spending that hits consumers Table 4 shows how the effect of Clinton spending intensity varies across subgroups with different news consumption habits. Here we focus on spending intensity that is *specific* to the channel, so that we are investigating the intensity of Clinton spending on MSNBC for MSNBC viewers, intensity on CNN for CNN viewers, and intensity on Fox for Fox viewers. The tables report the unlogged median intensity among respondents. As one might expect, the intensity is higher for Clinton around CNN and MSNBC than Fox, but the intensity that combines across all news outlets is necessarily higher still. The final model combines across all news channels. The results are very similar to what one finds in Table 1, with the results being positive but not statistically meaningful. Even taking the estimates at face value, huge jumps in intensity would be necessary to produce shifts in opinion that are electorally meaningful.

Table 4: Channel-specific spending intensity among viewers. Outcome: Clinton vote

Channel	Coef.	SE	$\hat{\delta}$	N
CNN	0.137	0.265	0.060	2839
FOX	0.824	0.545	0.678	2305
MSNBC	1.265	0.934	0.823	3088
SE clustered by DMA				

Each row uses only viewers of that channel. Treatment is channel-specific decayed, logged spending. Respondent and week FE included; SE clustered by DMA.

In the Supplementary Appendix, the analogous tables are provided focusing on Trump spending intensity and spending intensity advantage. Just as table 4 looks quite similar to Table 1 for the Clinton spending intensity variable, so too do these supplementary tables look similar to the results that come from looking without considering targeting or media consumption.

Table 5: Decayed, logged spending with social-media interaction

	(1) Clinton vote	(2) Trump vote	(3) Clinton vote
Own spending intensity	−0.302	0.086	–
	(0.467)	(0.237)	–
Own spending intensity × social-media user	0.366	−0.143	–
	(0.484)	(0.238)	–
Net spending intensity (Clinton − Trump)	–	–	−0.242
	–	–	(0.858)
Net spending intensity × social-media user	–	–	0.469
	–	–	(0.894)
$\hat{\delta}$	0.990	0.010	0.990
Respondent FE	Yes	Yes	Yes
Week FE	Yes	Yes	Yes
SE clustered by	DMA	DMA	DMA
N	50016	50016	50016

Columns are different models. Baseline is non-users. All models include respondent and week fixed effects; SEs clustered by DMA.

Social Media and TV Spending - Table 5 shows how the results vary among those who do and those who do not use social media. Those who do not use social media are a relatively small set of individuals. The effects estimated in this group are larger than we have seen before, suggesting that a 10% increase in spending intensity would product a .75% shift in support for Clinton, which is an electorally relevant margin in that election at a more realistic level of spending given the campaign's fundraising capacities. Even so, the sample size is smaller and one cannot rule out that they might occur due to chance. Among social media users, the effects are much more similar to whole sample. None of the δ are well-identified.

Discussion

Most of our analyses fail to find any substantial evidence of Presidential campaigns deriving a meaningful benefit for their television ad spending. That is even despite the fact that ad spending is a very large budget item for many campaigns, particularly these two that we study. The general absence of significant effects was found with respect to persuasion and mobilization. A number of candidate Trump's estimates were even negative, which is far-fetched if not quite unimaginable. Negative effects imply that a candidate could do better by simply not running ads. All of these negative effects were statistically indistinguishable from no effect, however. The sample size of our study was large and some of our earlier prospective power analyses suggested if there were effects such as a 10% increase in spending producing a 1% shift in vote intentions, we likely would have detected it. The fact that the discount factor was poorly identified across so many of these models also suggests that not much actual signal was picked up by these regressions.

Having said that, perhaps the most interesting finding regarded the spending in places and times where a campaign was unopposed. One can dismiss the negative association between Trump's spending and support for Trump as likely due to the imprecision of the estimates. Indeed, there were relatively few areas where Trump spent unopposed. Again, the discount factor being poorly identified tends to support the noise interpretation. But the unopposed Clinton spending had an association with vote intention that was not attributable to chance. It is true that we have looked at a lot of effects. Spurious significance that arises from multiple testing is a potential concern. If we had proposed five one-sided tests originally (three on H1 and two on H2), the Bonferroni-correction suggests that we would be looking for a p-value of less than 0.01, which is what we found. If we proposed five 2-sided tests, however, than we would not reach the stringent Bonferroni-corrected hurdle. Still, Bonferroni-correction is often viewed as overly stringent. If ex ante one had given us the choice to solely examine one regression to find an effect of campaign spending on respondent attitudes, there is little doubt that we would have chosen to look at unopposed Clinton spending, as we presume most other political scientists would. The lack of opposing spending tends toward the expectation that is where spending will be most effective, and there were far more places where Clinton was spending unopposed for longer than Trump spending unopposed. If one has the view that an effect, if it exists, is likely to be found there, then it is not easy to dismiss the result as simply a result of multiple comparisons. The effect we found is well over the significance threshold as one would hope from an appropriately powered design. We also found some evidence that unopposed spending decreased support for Trump as one would expect, although it was weaker.

While few of the discount factors were well-identified or precisely estimated, one exception is in the regression of unopposed Clinton spending on intention to vote for Clinton. The estimate is a bit larger than in Gerber et al. (2011), suggesting possibly more persistence when spending is unopposed. Still, the overall tenor of rapidly diminishing payoffs for spending is similar in both that older study and this one. If the value of prior spending is discounted 5% per day, as we estimate, that implies a dollar spent loses half its value after two weeks. Gerber and co-authors find that "just a week or two later, the advertisement's effects have all but disappeared," but that is not substantively inconsistent with what we found. At the low end of our confidence interval, a 12% haircut every day implies that a dollar spent is only worth about 16 cents after two weeks. Despite heavy discounting over time, the effects are really quite substantial in size, suggesting that 10% more spending in these areas might have moved support around 10-15% in her favor among similar respondents. The concern that unopposed Clinton spending largely targeted places where there were more supportive respondents is unfounded. Individual fixed effects control for background attitudes of each respondent, while the descriptive analysis also suggests no evidence of targeting in this fashion.

The overall shape of these regression results do help to rationalize campaign behavior. In particular, if unopposed spending produces substantial shifts in vote intentions as we find, then that explains why campaigns find it necessary to contest so many different areas. Spending less earlier than later is well-justified by the strong discounting of the value of past spending. If one spends too little, however, so that spending is unopposed or perhaps nearly so, then it creates exploitable opportunities for opponents. Given such a state of affairs, campaigns have incentive to always spend at least something everywhere that is electorally relevant, and then ramp up that spending as one gets closer to the finish line. Although we are reluctant to read too much into non-significant results, we do think it worth mentioning that the estimates on *net* spending intensity were also always considerably larger than gross spending intensity and would have a large substantive significance. If failing to oppose spending is dangerous, then it is reasonable to believe that falling far behind likely has similar effects. In the 2016 campaign, Trump's spending disadvantage was greatest earlier in the campaign and by the end of the campaign he had largely closed the gap and was contesting every battleground state. Clinton was faulted for contesting too many uncontested areas late in the game. Perhaps the bigger issue was not taking full advantage of her early opportunities.

Although it is often presumed that campaigns and candidates operate as single-minded election seekers, it is also worth considering the implications of campaign spending for governance. Winning the popular vote is important for the political capital of candidates when they come into office, which in turn may influence the ability of candidates to achieve their policy priorities, avert mid-term losses, and win reelection. Most states are not electorally competitive and receive little spending. The results here suggest that smaller investments by Presidential campaigns throughout the thirty or so non-battleground states might have substantial returns for winning the popular vote and electoral mandates. To be sure, it is likely that both candidates would value this end. That suggests there might be U.S. Presidential campaign equilibria where both campaigns do actually contest electorally non-competitive areas for the sake of winning the popular vote, even if that is not the equilbrium we have observed recently. The current pattern of coalescing on spending only in the battleground states might be understood as a sort of cooperative arrangement between the campaigns to preserve resources by avoiding costly arms race for the popular vote. A somewhat older literature on resource allocation incentives for parties in elections dealt with similar themes (Snyder 1989, e.g.). It is an interesting question what makes it easier or harder to avoid arms races, in campaigns or otherwise, and a large literature studies the arms race topic. Our work suggests that more theoretical investigations that translate the literature on conflict to US Presidential campaigns could be of considerable interest.

Conclusion

The literature on the persuasive effect of Presidential campaigns is relatively divided, with observational studies often finding small but electorally pivotal effects and experimental designs often finding not much, although questions about the possibility of qualitatively different results to scale and also context remain. In such a context, the registered report format is particularly valuable, even and especially for observational designs, as it focuses research attention and incentives around design rather than outcomes. We admit that we undertook the project with fairly strong priors that we would not find significant or substantial effects of Presidential campaign spending. The expectation that we would not find a significant effect led this research to languish in the file drawer for many years, despite our enthusiasm for the high-frequency survey panel as a data source and its potential to open new avenues for the literature using data that is often getting collected anyway for different reasons. What is more, and also a credit to the registered report format, we admit our results surprised us. The story that emerges from our regressions is one that has been told by others. We find evidence that the effects of spending by Presidential campaigns washes each other out, but there are relatively large effects found in places and times where one campaign is unopposed, suggesting that some level of Presidential campaign spending is necessary to remain competitive. Substantial discounting implies that most of this unopposed spending has little ultimate impact, at least if it occurs early in the election, and it is hard to create a net spending advantage that produces significant traction. Still, the effects of failing to contest areas are big enough that campaigns know not to do that, at least not in places that are electorally pivotal and at least not in the home-stretch of the campaign. Candidate Trump was badly outspent in many places in the early days of the general election, but our results focused on individual attitudes also explain why that would not have mattered very much.

Presidential general elections are the hard case for detecting effects of campaign advertising. Many voters have hardened opinions on the candidates and there is tremendous amounts of information provided through other channels about them. It is true that the ad spending is on a very different scale from other US elections, but it is also up against a lot of other simultaneous treatments and noise in the political marketplace. The methods we adopt here to study spending in this election could with relative ease be adapted to other electoral contests, including and especially primary elections and down-ballot races. In such contexts, we are likely to observe bigger variation in spending between candidates and more places where spending is uncontested. Another very important benefit of surveys relative to administrative outcomes is that one can get much richer set of responses from individuals. In particular, one could have asked about things like name recognition, knowledge about candidate's policies or biographies, and other intermediate outcomes that are surely relevant to vote selection, particularly in campaigns and down-ballot races. The study we used did not ask about these things in 2016, but some others might have and future ones still could. Moreover, as panels like these come into greater use, it is likely that more data of the kind we study are going to become available. Our work here offers a template for conducting many other studies on related topics and perhaps more far-flung ones that can take advantage of what high-frequency surveys have to offer.

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Appendix

Sensitivity of Results to different δ values

In the manuscript, β values reflect the value where δ is maximized. Here we show robustness of estimates to different choices of δ, which may be of particularly interest in those models where δ is poorly identified (generally where β is not significant). Recall that if X_{it} is the spending intensity in place i at time t and S_{it} is the actual spending in that time, then $X_{it} = S_{it} + \delta S_{i(t-1)} + \delta^2 S_{i(t-2)} = S_{it} + \sum_{p=1}^{K(t)} \delta^p S_{i(t-p)}$ where $K(t)$ is the number of periods for which there is spending data available.

Table A1: Sensitivity to discount factor (H1A)

δ	β	SE	p
0.00	0.0097	0.0957	0.919
0.05	0.0073	0.0958	0.939
0.10	0.0068	0.0959	0.943
0.15	0.0074	0.0961	0.939
0.20	0.0088	0.0965	0.927
0.25	0.0112	0.0971	0.909
0.30	0.0140	0.0980	0.886
0.35	0.0170	0.0989	0.864
0.40	0.0199	0.0998	0.842
0.45	0.0230	0.1006	0.819
0.50	0.0272	0.1015	0.789
0.55	0.0329	0.1022	0.748
0.60	0.0396	0.1029	0.701
0.65	0.0464	0.1034	0.654
0.70	0.0532	0.1039	0.609
0.75	0.0611	0.1047	0.560
0.80	0.0692	0.1066	0.517
0.85	0.0754	0.1096	0.492
0.90	0.0729	0.1112	0.513
0.95	0.0577	0.1120	0.607
1.00	0.0294	0.1091	0.788

For reference, using joint **maximum likelihood estimation,** $\beta = 0.0760$ **(0.110) for** $\hat{\delta} = 0.865$**,** although $\hat{\delta}$ was weakly identified.

Table A2: Sensitivity to discount factor (H1B)

δ	β	SE	p
0.00	-0.0445	0.0674	0.510
0.05	-0.0439	0.0676	0.517
0.10	-0.0435	0.0677	0.522
0.15	-0.0421	0.0679	0.535
0.20	-0.0398	0.0682	0.560
0.25	-0.0370	0.0688	0.592
0.30	-0.0341	0.0697	0.625
0.35	-0.0317	0.0707	0.654
0.40	-0.0300	0.0716	0.675
0.45	-0.0288	0.0725	0.692
0.50	-0.0275	0.0736	0.709
0.55	-0.0260	0.0749	0.729
0.60	-0.0243	0.0765	0.751
0.65	-0.0227	0.0785	0.772
0.70	-0.0223	0.0808	0.783
0.75	-0.0238	0.0830	0.774
0.80	-0.0276	0.0849	0.746
0.85	-0.0339	0.0864	0.695
0.90	-0.0405	0.0871	0.643
0.95	-0.0463	0.0864	0.592
1.00	-0.0552	0.0842	0.513

For reference, using joint **maximum likelihood estimation,** $\beta = -0.044$ **for** $\hat{\delta} = 0.010$**,** although $\hat{\delta}$ was weakly identified.

Table A3: Sensitivity to discount factor (H1C)

δ	β	SE	p
0.00	0.0908	0.1434	0.527
0.05	0.0892	0.1435	0.535
0.10	0.0891	0.1434	0.535
0.15	0.0903	0.1435	0.530
0.20	0.0927	0.1438	0.520
0.25	0.0961	0.1444	0.507
0.30	0.1000	0.1454	0.493
0.35	0.1037	0.1464	0.480
0.40	0.1070	0.1474	0.469
0.45	0.1105	0.1483	0.457
0.50	0.1155	0.1491	0.440
0.55	0.1223	0.1497	0.412
0.60	0.1337	0.1503	0.375
0.65	0.1464	0.1512	0.334
0.70	0.1605	0.1526	0.294
0.75	0.1758	0.1545	0.257
0.80	0.1923	0.1580	0.225
0.85	0.2063	0.1639	0.210
0.90	0.2090	0.1647	0.206
0.95	0.2057	0.1631	0.209
1.00	0.1862	0.1590	0.243

For reference, using joint **maximum likelihood estimation,** $\beta = 0.068$ **for** $\hat{\delta} = 0.939$**,** although $\hat{\delta}$ was weakly identified.

Table A4: Sensitivity to discount factor (H2A)

δ	β	SE	p
0.00	1.1941	0.6091	0.052
0.05	1.2021	0.6123	0.052
0.10	1.2106	0.6136	0.050
0.15	1.2206	0.6148	0.049
0.20	1.2319	0.6160	0.047
0.25	1.2445	0.6173	0.046
0.30	1.2582	0.6189	0.044
0.35	1.2730	0.6210	0.042
0.40	1.2891	0.6240	0.039
0.45	1.3075	0.6285	0.038
0.50	1.3301	0.6352	0.037
0.55	1.3594	0.6451	0.036
0.60	1.3985	0.6589	0.034
0.65	1.4512	0.6779	0.032
0.70	1.5230	0.7041	0.030
0.75	1.6218	0.7401	0.027
0.80	1.7636	0.7884	0.022
0.85	1.9791	0.8539	0.016
0.90	2.3113	0.9465	0.013
0.95	2.7024	1.0737	0.041
1.00	2.7031	1.3096	0.039

For reference, using joint **maximum likelihood estimation,** $\beta = 2.722$ **for** $\hat{\delta} = 0.953$ **(95% CI:** $[0.96, 1.00]$**).**

Table A5: Sensitivity to discount factor (H2B)

δ	β	SE	p
0.00	-1.0562	1.2807	0.412
0.05	-1.0597	1.2798	0.410
0.10	-1.0596	1.2777	0.409
0.15	-1.0575	1.2752	0.409
0.20	-1.0540	1.2728	0.410
0.25	-1.0499	1.2716	0.412
0.30	-1.0461	1.2735	0.414
0.35	-1.0444	1.2818	0.418
0.40	-1.0467	1.3007	0.423
0.45	-1.0548	1.3335	0.431
0.50	-1.0691	1.3819	0.442
0.55	-1.0893	1.4464	0.454
0.60	-1.1149	1.5278	0.468
0.65	-1.1459	1.6273	0.483
0.70	-1.1833	1.7462	0.500
0.75	-1.2302	1.8860	0.516
0.80	-1.2940	2.0536	0.530
0.85	-1.3954	2.2736	0.541
0.90	-1.5929	2.6057	0.497
0.95	-2.0537	3.0073	0.419
1.00	-2.6926	3.3108	0.416

For reference, using joint **maximum likelihood estimation,** $\beta = -1.060$ **for** $\hat{\delta} = 0.059$**,** although $\hat{\delta}$ was weakly identified.

Ad-airings Treatment

Table A6: Main effects (H1): Decayed, logged ad airings

	Clinton vote	Trump vote	Clinton vote
Ad-airings intensity	0.364	−0.142	
	(0.254)	(0.193)	
Net ad-airings intensity			−0.038
			(0.204)
$\hat{\delta}$	0.929	0.010	0.664
Respondent FE	Yes	Yes	Yes
Week FE	Yes	Yes	Yes
SE clustered by	DMA	DMA	DMA
N	50016	50016	47407

Table A7: Unopposed windows (H2): Decayed, logged ad airings

	Clinton vote	Trump vote
Ad-airings intensity	3.119	−1.406
	(1.094)	(1.949)
$\hat{\delta}$	0.947	0.010
Respondent FE	Yes	Yes
Week FE	Yes	Yes
SE clustered by	DMA	DMA
N	7860	1323

Non-log Specification

Table A8: Main effects (H1): Decayed ad spending, non-logged

	Clinton vote	Trump vote	Clinton advantage
Spending Intensity	0.000	0.000	0.000
	(0.000)	(0.000)	(0.000)
$\hat{\delta}$	0.010	0.010	0.010
Respondent FE	Yes	Yes	Yes
Week FE	Yes	Yes	Yes
SE clustered by	DMA	DMA	DMA
N	50016	50016	50016

Table A9: Unopposed windows (H2): Decayed ad spending, non-logged

	Clinton vote	Trump vote
Spending Intensity	0.000	0.000
	(0.000)	(0.000)
$\hat{\delta}$	0.943	0.990
Respondent FE	Yes	Yes
Week FE	Yes	Yes
SE clustered by	DMA	DMA
N	7860	1323

Lagged Opponent Spending

Table A10: Robustness: own decayed log spend with lagged opponent (t–1)

	Clinton vote	Trump vote	Clinton advantage
Spending intensity (log)	0.062	0.005	0.168
	(0.119)	(0.074)	(0.174)
Lagged opponent (t–1)	−0.073	−0.341	0.012
	(0.097)	(0.127)	(0.160)
$\hat{\delta}$	0.839	0.310	0.855
Respondent FE	Yes	Yes	Yes
Week FE	Yes	Yes	Yes
SE clustered by	DMA	DMA	DMA
N	45519	45519	45519

Logit Specification (Fractional Model)

Table A11: Main effects (H1): fractional-response logit with decayed spending

	Clinton vote share	Trump vote share
Spending intensity (log)	0.009 (0.016)	−0.007 (0.011)
δ (fixed)	0.865	0.010
Respondent FE	Yes	Yes
Week FE	Yes	Yes
SE clustered by	DMA	DMA
N	28335	27259

Table A12: Unopposed windows (H2): fractional-response logit with decayed spending

	Clinton vote	Trump vote
Spending intensity (log)	0.399 (0.169)	−0.107 (0.186)
δ (fixed)	0.953	0.059
Respondent FE	Yes	Yes
Week FE	Yes	Yes
SE clustered by	DMA	DMA
N	3942	460

Channel specific results

Table A13: Channel-specific net (Clinton) spending intensity among viewers. Outcome: Clinton vote

Channel	Coef.	SE	$\hat{\delta}$	N
CNN	0.385	0.347	0.049	2839
FOX	2.464	1.790	0.891	2305
MSNBC	2.303	1.445	0.854	3088
SE clustered by DMA				

Each row uses only viewers of that channel. Treatment is channel-specific decayed, logged net spending (Clinton minus Trump). Respondent and week FE included; SE clustered by DMA.

Table A14: Channel-specific spending intensity among viewers. Outcome: Trump vote

Channel	Coef.	SE	$\hat{\delta}$	N
CNN	0.112	0.308	0.990	2839
FOX	0.232	0.178	0.114	2305
MSNBC	−0.567	0.413	0.850	3088
SE clustered by DMA				

Each row uses only viewers of that channel. Treatment is channel-specific decayed, logged spending. Respondent and week FE included; SE clustered by DMA.

Unopposed Clinton spending x social media use

Table A15: Decayed spending with social-media interaction. Outcome: Clinton unopposed

	(1) Clinton unopposed
Spending intensity	−0.489
	(2.321)
Spending intensity × social-media user	3.643
	(2.080)
$\hat{\delta}$	0.962
Respondent FE	Yes
Week FE	Yes
SE clustered by	DMA
N	7860

Table A16: Unopposed Clinton spending and Trump vote

	Trump vote
Spending intensity	-1.045
	(0.698)
$\hat{\delta}$	0.015
Respondent FE	Yes
Week FE	Yes
SE clustered by	DMA
N	7860

Sample restricted to respondent-waves in which Clinton advertises unopposed. Respondent and week fixed effects included; SE clustered by DMA.