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 4,509 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 too particular strengths of this dataset. This approach aims to provide an orthogonal replication of prior observational studies using different methods and data sources.

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 4,509 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 4,509 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 = 50,163), 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 are able to use time fixed-effects, which allow us to control for unobservable temporal variables that create linear shocks to voter attitudes. 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 hypothesis is associated with the following three, related null 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' identites 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. Overall retention was high: 64% of panelists remained active nearly every week over the seventeen-week period. 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 model seeks to estimate the impact of television ad spending by 2016 Presidential campaigns on voting intentions. The model for the turnout outcome is analogous and omitted here for brevity:

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

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

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

where ψ_i is the individual fixed effect, θ_t is the time fixed effect, and ϵ_{imt} is the disturbance term. Importantly, because ad spending varies at the media market level m rather than the individual level, we denote it as $\text{SpendingIntensity}_{mt}$. 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

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			0.208
			(0.165)
$\hat{\delta}$	0.865	0.010	0.870
CI for δ	[0.01, 0.99]	[0.01, 0.99]	[0.01, 0.99]
Respondent FE	Yes	Yes	Yes
Week FE	Yes	Yes	Yes
SE clustered by	DMA	DMA	DMA
N	50016	50016	50016

Results

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 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.

Table 2: Unopposed windows (H2)

	Clinton unopposed	Trump unopposed
Candidate Spending Intensity	2.722	−1.060
	(1.084)	(1.279)
$\hat{\delta}$	0.953	0.059
CI for δ	[0.88, 0.99]	[0.01, 0.99]
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→Trump demobilization, $\hat{\beta} = -0.083$ ($SE = 0.141$), implying −0.025 for a doubling and −0.083 for a tenfold increase; not statistically different from zero. For Trump→Clinton demobilization, $\hat{\beta} = -0.061$ ($SE = 0.108$), 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 ($\hat{\delta} = 0.378, 0.963, 0.319, 0.507$; CIs $[0.01, 0.99]$). Sample sizes are $N =$ 18,112 for Clinton mobilization and Trump→Clinton demobilization, and $N =$ 18,591 for Trump mobilization and Clinton→Trump demobilization. Specifications with a lagged opponent term yield similar signs and inference; details are in the appendix.

Table 3: Viewer-only (CNN/Fox/MSNBC): Channel-matched decayed, logged ad spending

	CNN viewers	Fox viewers	MSNBC viewers
Outcome = Clinton vote			
Spending Intensity	0.066	0.063	0.063
	(0.121)	(0.121)	(0.121)
Net Spending Intensity	0.218	0.218	0.216
	(0.178)	(0.178)	(0.178)
$\hat{\delta}$ (Spending Intensity)	0.866	0.866	0.866
CI for δ (Spending Intensity)	[0.01, 0.99]	[0.01, 0.99]	[0.01, 0.99]
$\hat{\delta}$ (Net Spending Intensity)	0.990	0.990	0.990
CI for δ (Net Spending Intensity)	[0.01, 0.99]	[0.01, 0.99]	[0.01, 0.99]
Outcome = Trump vote			
Spending Intensity	−0.064	−0.066	−0.063
	(0.078)	(0.078)	(0.078)
$\hat{\delta}$ (Spending Intensity)	0.077	0.077	0.077
CI for δ (Spending Intensity)	[0.01, 0.99]	[0.01, 0.99]	[0.01, 0.99]
Respondent FE	Yes	Yes	Yes
Week FE	Yes	Yes	Yes
SE clustered by	DMA	DMA	DMA
N	43663	43680	43649

Spending that hits consumers Table 3 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 logically the intensity that combines across all news outlets is 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.

In the Supplementary Appendix, the analogous tables are provided focusing on Trump spending intensity and spending intensity advantage. Just as table 3 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.

Social Media and TV Spending - Table TOCOME2 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 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. The most notable difference from prior results is that Trump's spending on those who do not use social media has a similar sign and magnitude as what one finds with Clinton in Table TOCOME, but it is also not significant. None of the δ are well-identified.

Table 4: H1 within subgroup: nosoc

	Clinton vote	Trump vote	Clinton advantage
Own decayed log spend	0.180	0.231	0.415
	(0.465)	(0.282)	(0.780)
$\hat{\delta}$	0.884	0.570	0.824
CI for δ	[0.01, 0.99]	[0.01, 0.99]	[0.01, 0.99]
Respondent FE	Yes	Yes	Yes
Week FE	Yes	Yes	Yes
SE clustered by	DMA	DMA	DMA
N	4706	4706	4706

Discussion

Most of our analyses fail to find evidence suggesting Presidential campaigns derive 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 Trump result here as also due to the imprecision of the estimates, likely due to the fact that 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 we found, but we would not reach the right level with a 2-sided test. That said, the Bonferroni-correction is rightly argued as being too conservative. If ex ante one had given us the choice to only examine one regression in order to find an effect, there is little doubt that we would have looked there, 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, and the effect we found is well over the significance threshold as one would hope from an appropriately powered design.

Moreover, the discount factor in that particular regression was also (for once) reasonably well-identified. 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 suggests that falling far behind is likely similar. 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, however perhaps the bigger issue was not taking full advantage of what happened early.

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 an electoral mandate. 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). 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. The effect of spending by Presidential campaigns wash each other out, but if one spends nothing then one is going to get blown out of the water. Substantial discounting ensures much of this spending has little ultimate impact, while it is hard to assemble a net spending advantage that leads to 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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Table 5: H1 within subgroup: cnn

	Clinton vote	Trump vote	Clinton advantage
Own decayed log spend	0.066	-0.064	0.218
	(0.121)	(0.078)	(0.178)
$\hat{\delta}$	0.866	0.077	0.990
CI for δ	[0.01, 0.99]	[0.01, 0.99]	[0.01, 0.99]
Respondent FE	Yes	Yes	Yes
Week FE	Yes	Yes	Yes
SE clustered by	DMA	DMA	DMA
N	43663	43663	43663

Coefficients for ad-intensity terms; respondent week FE included. SE clustered by DMA.

Table 6: H1 within subgroup: fox

	Clinton vote	Trump vote	Clinton advantage
Own decayed log spend	0.063	-0.066	0.218
	(0.121)	(0.078)	(0.178)
$\hat{\delta}$	0.866	0.077	0.990
CI for δ	[0.01, 0.99]	[0.01, 0.99]	[0.01, 0.99]
Respondent FE	Yes	Yes	Yes
Week FE	Yes	Yes	Yes
SE clustered by	DMA	DMA	DMA
N	43680	43680	43680

Coefficients for ad-intensity terms; respondent week FE included. SE clustered by DMA.

Table 7: H1 within subgroup: msnbc

	Clinton vote	Trump vote	Clinton advantage
Own decayed log spend	0.063	-0.063	0.216
	(0.121)	(0.078)	(0.178)
$\hat{\delta}$	0.866	0.077	0.990
CI for δ	[0.01, 0.99]	[0.01, 0.99]	[0.01, 0.99]
Respondent FE	Yes	Yes	Yes
Week FE	Yes	Yes	Yes
SE clustered by	DMA	DMA	DMA
N	43649	43649	43649

Coefficients for ad-intensity terms; respondent week FE included. SE clustered by DMA.

Table 8: H1 within subgroup: nosoc

	Clinton vote	Trump vote	Clinton advantage
Own decayed log spend	0.180	0.231	0.415
	(0.465)	(0.282)	(0.780)
$\hat{\delta}$	0.884	0.570	0.824
CI for δ	[0.01, 0.99]	[0.01, 0.99]	[0.01, 0.99]
Respondent FE	Yes	Yes	Yes
Week FE	Yes	Yes	Yes
SE clustered by	DMA	DMA	DMA
N	4706	4706	4706

Coefficients for ad-intensity terms; respondent week FE included. SE clustered by DMA.

Table 9: H1 within subgroup: tvnews

	Clinton vote	Trump vote	Clinton advantage
Own decayed log spend	0.062	−0.064	0.213
	(0.121)	(0.078)	(0.178)
$\hat{\delta}$	0.866	0.077	0.990
CI for δ	[0.01, 0.99]	[0.01, 0.99]	[0.01, 0.99]
Respondent FE	Yes	Yes	Yes
Week FE	Yes	Yes	Yes
SE clustered by	DMA	DMA	DMA
N	43748	43748	43748

Coefficients for ad-intensity terms; respondent week FE included. SE clustered by DMA.