

Uncovering the Persuasive Effects of Presidential Advertising

ABSTRACT

Do presidential campaign advertisements mobilize, inform, or persuade citizens? To answer this question we exploit a natural experiment, the accidental treatment of some individuals living in non-battleground states during the 2000 presidential election to high levels or one-sided barrages of campaign advertisements simply because they lived in a media market adjoining a competitive state. Unlike previous efforts, this approach remedies the observed correlation between advertising and both other campaign activities and previous election outcomes. By comparing National Annenberg Election Survey respondents matched with records of locally broadcast presidential advertising within individual non-contested states, we can isolate the effects of advertising. In contrast to previous research, we find little evidence that citizens are mobilized by or learn from presidential advertisements, but strong evidence that they are persuaded by them. This research supports the contention that political communication is best conceived of as propaganda rather than a vehicle to enhance democratic accountability.

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Political scientists and media observers offer starkly contrasting evaluations of contemporary presidential campaign advertisements. On the one hand, Freedman, Franz, and Goldstein describe political commercials as “rich in information and laden with emotional content,” which “can inform and mobilize the citizenry” (2004: 734). On the other hand, advertising mogul David Ogilvy argues that “[p]olitical advertising ought to be stopped. It’s the only really dishonest kind of advertising left” (1982). This view echoes the common perception that political advertisements are deliberately crafted to win votes by self-aggrandizement, besmirching one’s opponent, or simply confusing voters with misleading “facts” (Rosenstiel 1993; Fletcher, Ross, and Schweitzer 2002). In popular accounts, campaign advertisements are little more than propaganda able to cause voters to act against their own self-interest.

Aside from evidence showing that campaign advertisements improve voter knowledge and interest in campaigns (Atkin and Heald 1976; Brians and Wattenberg 1996; Freedman, Franz, and Goldstein 2004; Hillygus 2005; Zhao and Chaffee 1995), however, few studies relying on actual campaign activity have been able to document evidence of individual persuasion from campaign advertisements (Goldstein and Ridout 2004; but see Shaw 1999; Johnston, Hagen, and Jamieson 2004). Yet the finding that campaign commercials mobilize and inform voters seems inconsonant with a direct evaluation of the content of most presidential advertisements. In general, political advertisements seem crafted more to influence citizens’ emotional evaluations of candidates (Brader 2005) than to facilitate a normatively desirable model of democratic citizenship. If so, why have studies of advertising in the field not found an effect on candidate affect and vote choice?

We argue that previous observational studies have not documented the persuasive effects of political advertisements due to limitations of data and research design. Post-election studies of advertising effects are limited both by the near balance of campaign advertisements in competitive presidential campaigns and the correlation between strategically targeted advertising streams and underlying voter proclivities. At the same time, the finding that commercials inform and motivate citizens is most likely spurious, arising because of the geographic correlation between exposure to political communication and other campaign activities and also because of the individual-level correlation between self-reported media

consumption and political knowledge.

To overcome these barriers to accurately measuring the effects of campaign advertisements, we exploit a natural experiment created during the 2000 presidential campaign by the interaction between the Electoral College, state boundaries, and the transmission of broadcast television signals. Specifically, we use the fact that while only certain states were actively contested by the two major campaigns, citizens of many non-“battleground” states nonetheless received high levels of campaign commercials because they resided in a broadcast media market that crossed into a “battleground” state.¹ Other scholars have taken advantage of this quasi-experimental setting, but cannot isolate the effects of campaign commercials from other campaign activities because they do not account for the strategic targeting of advertising and other campaign activities toward and within battleground states (e.g., Freedman, Franz, and Goldstein 2004; Johnston, Hagen, and Jamieson 2004). In contrast, we are able to isolate the effects of campaign commercials by measuring changes in people’s attitudes over the course of the campaign within individual non-battleground states. In these non-battleground states, the presidential campaigns are largely inactive on the ground and advertising volume and partisan balance are uncorrelated with previous voting behavior.

Our analysis provides an assessment of the effects of presidential advertisements that is starkly

¹ Throughout this paper we define the set of twenty battleground states as AZ, AR, DE, FL, IL, IA, LA, ME, MI, MO, NV, NH, NM, OH, OR, PA, TN, WA, WV, and WI (Source CNN). These twenty states, identified in the summer of 2000, were those in which the campaign was perceived as likely to be competitive in the fall. Because we desire to avoid misattributing to advertising the effects of campaign field activity in contested states, we validated this list by examining national party transfers to each state (to state and local party committees) through Federal Elections Commission (FEC) filings. In these 20 states, the average transfer to state and local parties was \$1.83 per resident from 1/1/1999 to 12/31/2000, while it was only \$.30 in the remaining 30 states. Examining the FEC data suggest that perhaps Kentucky (\$1.08 transfers per citizen) and Minnesota (\$1.00) should be included in our list, while Arizona (\$.25) and West Virginia (\$.14) should be excluded. All of the subsequent analyses reported in this paper are robust to these alternative specifications of the set of battleground states.

different from previous observational research (but far more consistent with laboratory studies). We show there are small and inconsistent engagement and informational effects that result from presidential advertisements. Instead, we find that campaign advertisements are able to persuade voters to support one candidate over another. Furthermore, we validate these survey results with an analysis of county-level vote returns in the 2000 election. These findings suggest that popular concerns that presidential advertisements are uninformative and misleading are, in all likelihood, more appropriate than scholarly claims that they enhance democratic accountability.

The remainder of this paper is organized in six sections. First, we begin by reviewing previous arguments about the effects of political advertisements on political opinion and behavior. Second, we identify a series of methodological limitations in previous research relying on natural variation in advertising that undercuts the inferences one can draw from these works. In the third section, we describe our natural experiment design that overcomes these problems and thereby provides a means to estimate the effects of political advertising in isolation from other campaign activities. We next implement this research design and find strong evidence for the persuasive effects of presidential advertising, but only limited support for the argument that campaign advertisements convey factual information. The fifth section extends our analysis to alternative model specifications and applies a similar design to an analysis of election outcomes. Finally, the last section is a conclusion.

I. The Effects of Political Advertisements

Despite concerns in the early 20th century that the increasing reach of mass communication would enhance the use of propaganda to manipulate public opinion (e.g., Lippmann 1922), post-war studies found no evidence that election campaigns could use the mass media to persuade voters (Berelson, Lazarsfeld, and McPhee 1954; Klapper 1960; Lazarsfeld, Berelson, and Gaudet 1948). Outside the laboratory setting, the vast majority of contemporary scholarship continues to find little evidence that campaigns do much to persuade voters to alter their support for a particular candidate.² Rather, there is a

² Like much of the work in the study of campaigns, we conceptualize *persuasion* as broadly referring to

great deal of evidence that campaigns mobilize voters and disseminate information that allows voters to make “enlightened” choices (Alvarez 2001; Campbell 2000; Finkel 1993; Gelman and King 1993; Holbrook 1994, 1996; Wlezien and Erikson 2002).

Scholars have localized these mobilization and informational effects of campaigns in television advertisements, finding that campaign commercials enhance viewers’ knowledge of the candidates, lead voters’ to form opinions about issues addressed by the competing camps, and reinforce citizens’ underlying partisan predispositions (e.g., Atkin and Heald 1976; Brians and Wattenberg 1996; Freedman, Franz, and Goldstein 2004; Zhao and Chaffee 1995).³ Although it is highly likely that campaigns defined in a broad sense (news coverage, campaign events, etc.) motivate and inform, we argue that the content of campaign commercials suggests they are far more likely to simply persuade viewers. Contemporary campaign advertisements typically make cursory and tendentious references to issues, and citizens are rightly skeptical of the “factual” content in paid political advertisements (Vavreck 2001). Furthermore, advertisements rarely urge viewers to participate in politics. Instead, viewing political advertisements makes clear that they are explicitly designed to be persuasive, often by evoking an emotional response from the viewer (Ansolabehere and Iyengar 1995; Brader 2005).

Additionally, there are two critical methodological limitations in previous observational work that instances of attitude change. We remain agnostic about the particular psychological mechanism that cause changes in attitudes.

³ There is also ongoing debate about whether the tone of political advertising matters. While some scholars have found evidence that the tone of advertising is related to turnout and persuasion, a systematic meta-analysis suggests there are no pertinent differences (Lau et al. 1999). Moreover, randomized field experiments have validated this conclusion (e.g. Clinton and Lapinski 2004). While we are agnostic about the potential effects of variation in advertising tone, we note that we find no correlation between the tone of advertising (attack, contrast, or promote) and the volume or partisan balance of advertising in our analysis (see below). Thus, our estimates do not conflate volume or one candidate’s advertising advantage with a particular type of advertising. More generally, because the tone of advertising appears relatively constant across media markets, these data are not particularly useful for testing the relative effects of different types of advertising.

finds advertisements are informative but not directly persuasive. First, the failure of previous studies to find persuasion effects from presidential advertising likely reflects the fact that presidential campaigns are highly competitive environments in which the overall quality and volume of campaign messages tend to be balanced (Campbell 2000: 45). It would therefore be surprising to uncover much evidence of persuasion in simple post-election studies. Rather, it is necessary to find variance in the relative balance of disseminated campaign messages before one can expect to observe whether persuasion effects exist (Zaller 1996).

Second, individual-level studies that find campaign advertisements inform citizens about issues and candidate positions use self-reported media consumption to measure advertising exposure (Atkin and Heald 1976; Brians and Wattenberg 1996; Freedman, Franz, and Goldstein 2004; Wattenberg and Brians 1999; Zhao and Chaffee 1995).⁴ But individuals who report having viewed advertisements are substantially different along a number of salient dimensions, including interest in campaigns and knowledge of politics, from individuals who do not report having viewed advertisements. Additionally, campaigns are likely to target non-advertising activities at those individuals who are also most likely to report high levels of attention to political advertising. Consequently, it is likely that the findings in previous research reflect these unobserved factors, including personal attentiveness and receptivity to other campaign activities, which are correlated with reported exposure, rather the effects of the campaign advertisements themselves (Ansolabehere, Iyengar, and Simon 1999; Iyengar and Simon 2000: 152; Vavreck 2000).⁵

⁴ Freedman, Franz, and Goldstein (2004) weight objective measures of presidential advertisements broadcast in respondents' media markets by self-reported exposure to television news programs and shows. As we explain in the text below, this approach also creates bias. See also footnotes 5 and 15.

⁵ For example, being at home to watch the evening news or the early-evening television programs on which presidential advertisements are frequently run increases one's probability of encountering other forms of campaign activity (e.g., door-to-door canvassing) that influence voting behavior. Note too that controlling for whether a state is competitive will not remove this source of bias, since it is only attentive

II. Previous Efforts to Exploit Natural Experiments

In light of these persistent barriers to identifying the effects of presidential campaign advertising from survey data alone, recent scholarship has sought to use the “natural experiment” of variation in presidential advertising across media markets to discern its effects. In laboratory experiments, random manipulation of exposure to political commercials allows researchers to estimate the effects of campaign message exposure without bias. Laboratory studies have provided evidence that campaign advertisements are persuasive (Iyengar and Simon 2000), but researchers who rely on observational data are often skeptical of these findings because even the most sophisticated laboratory experiments cannot fully duplicate the larger context in which individuals receive political communication (Goldstein and Ridout 2004; Shaw 1999). Consequently, even advocates of experimental work argue that, “no matter how realistic their designs, experimenters must strive to replicate their results using alternative sources of evidence” (Iyengar and Simon 2000: 164).

The pioneering work of Johnston, Hagen, and Jamieson (2004, hereafter JHL) embodies this model by attempting to exploit the “natural” variation in advertising exposure across time and geographic areas. JHL match respondents in the 2000 National Annenberg Election Survey (NAES) to actual records of the campaign advertisements broadcast in their local media market. While this approach does not allow JHL to ascertain whether an individual survey participant actually viewed any presidential advertisements, only those who lived in areas saturated by advertisements could have experienced high levels of advertising exposure.

JHL can therefore estimate the effects of broadcasting campaign commercials, even though they reach only a portion of a media market’s population.⁶ As long as individual differences in receptivity to advertising (e.g. attentiveness, etc.) are uncorrelated with geographic variation in advertising, this approach obviates the need to measure those individual characteristics directly. JHL’s approach therefore

individuals *in competitive states* who are receptive to both advertisements and other campaign activity.

⁶ This estimand is equivalent to an intent-to-treat effect.

avoids the aforementioned bias associated with using self-reported media exposure. Unlike the bulk of the observational literature, they find evidence that campaign advertisements are persuasive: In media markets where Bush's (Gore's) campaign commercials were more prominent than Gore's (Bush's), the proportion of survey respondents indicating they would vote for Bush (Gore) increased.

JHL's work, while innovative, may nonetheless present an inaccurate account of the effects of presidential advertising because of the systematic non-random variation in campaign activity across media markets. A crucial assumption in "natural experiments" is that treatment is random and therefore uncorrelated with characteristics of individuals that would otherwise affect the behavior of interest. Yet by comparing individuals living in battleground states to one another as well as to those living in non-battleground states, JHL assume that only advertising varies across these contexts. We find, however, that this assumption is likely violated because of the strategic targeting of campaign advertising *across and within* states.

First, across all of the media markets and state combinations⁷ in JHL's sample, there is a strong correlation between the partisan balance of local advertising streams in the 2000 election and previous election outcomes. Specifically, Table 1A reports a series of regression models predicting Bush's margin over Gore in the rating-adjusted volume of presidential advertising for the nation's 75 largest media market and state combinations as a function of the 1996 election outcome in that geographic area.⁸ (Advertising volume is measured in Gross Rating Points, or GRPs, which we scale by dividing by 1000. 25 GRPs, or .025 GRPs/1000, equals an estimated viewership of 25% of a media market's households.⁹)

⁷ For example, the Philadelphia media market is divided into separate parts for each of the three states it serves, Delaware, Pennsylvania, and New Jersey.

⁸ We calculate Republican vote share as $\text{Republican vote} / (\text{Democratic} + \text{Republican vote})$ in the media market and state combination using vote returns from the area's constituent counties. Because of Perot's presence on the ballot in some states in 1996, we also estimated these models using Republican share of the overall vote. The results are highly similar.

⁹ GRPs are measured using local Nielsen programming data. Note that 100 GRPs, .1 GRPs/1000, would

These results show that in areas where Dole (Clinton) did better in 1996, Bush (Gore) outspent his opponent, whether one includes an indicator variable for battleground states (column 1) or not (column 2). Furthermore, the relationship between previous election outcomes and advertising persists *even if one accounts for differences across states by using state fixed effects* (this effect is significant at $p < 0.1$ for a one-tailed test, Column 3). As a consequence of this relationship between previous voter behavior and the current advertising stream, it is impossible to disentangle whether differences in candidate support between media markets are due to the baseline characteristics of voters or the persuasive effects of advertising. Furthermore, because we know neither the magnitude nor direction of this bias, one cannot remedy it by merely including the previous election outcome in a statistical analysis (King, Keohane, and Verba 1994: 133).¹⁰

Second, there is also a strong positive correlation between advertising volume in the 2000 campaign and whether an area was closely contested in the previous presidential election or was part of a state deemed “competitive” in 2000. Specifically, in Table 1B we report a series of regression models predicting the overall rating-adjusted volume of presidential advertising for the same areas as in Table 1A. Column (1) shows that advertising in 2000 was much higher in highly contested “battleground” states, and those in column (2) demonstrate that, ignoring battleground status, those areas that were closely contested in 1996 also experienced higher levels of advertising.¹¹ As above, this correlation persists even if one accounts for state-specific differences ($p < 0.1$ with a one-tailed test, Column 3). As a

indicate either 100% of households viewing an advertisement once or 50% viewing it twice.

¹⁰ On the one hand, if the 1996 vote were inflated by a similarly partisan advertising stream in that election, then including it will make the current election’s advertisements appear less effective. On the other hand, if the 1996 election result proxies underlying voter preferences, excluding it could also artificially inflate the estimated effect of advertising. Additionally, JHL include the 1996 state-level presidential vote share in their analysis, rather than local vote results. The state-level vote, however, will not account for the systematic correlation between *local* partisanship and *local* advertising in these states.

¹¹ Closeness is defined as .5 minus the absolute value of (.5 minus Republican Vote Share in 1996). Higher values indicate more competitive areas.

consequence of this systematic variation in advertising volume, estimates of the effects of presidential advertising, both between battleground and non-battleground states and within individual states, may misattribute to advertising the consequences of other campaign activity (e.g. local partisan and non-partisan GOTV efforts, candidate visits, etc.) correlated with advertising in battleground states. Once again, however, we do not know the precise nature of this potential bias ahead of time.

This correlation between advertising and previous voter behavior is a particular concern because, as we show in Table 2, advertising volume is correlated with reported campaign contact both across (column 1) and within (column 2) states. If this occurred because Republicans both purchased more television advertisements and had more aggressive field activities in certain areas (while Democrats did the same in others), then an analysis of the effects of the partisan balance of advertising that ignored field activity would inappropriately attribute to advertisements the effects of these similarly partisan field activities. If the correlation were negative, perhaps if campaigns substituted for weak field organizations with aggressive advertising, they it would understate the effects of television advertising. Absent a true account of the partisan nature of local campaign field activities *within individual states*, however, one cannot know ahead of time.¹² Furthermore, including reported campaign contact as a proxy for these field activities raises additional threats to inference for the reasons discussed in the previous section.¹³

III. Uncovering the Persuasive Effects of Political Advertising

This discussion suggests that previous efforts to use advertising broadcast records to discern advertising

¹² These concerns also apply to Shaw (1999), who uses aggregate state advertising purchases and non-advertising campaign expenditures to discern the effects of advertising.

¹³ In addition to the general problem of unobserved field activity, if field activity were actually relatively constant throughout a state but television advertising stimulated individuals to misreport having been contacted by the campaign, then including reported contact in an analysis of the effects of advertising could lead to either understating or overstating the effects of advertising on behavior. For instance, if individuals who misremembered advertisements were most affected by the advertisements, it would lead to understating the effects of advertisements, but if they were less affected relative to those who did not misreport campaign contact, it would lead to overstating the effects of advertisements.

effects, while a substantial advancement over the use of self-reported media exposure, are nonetheless limited. These concerns apply to JHL's attempt to gauge the persuasive effects of advertising, as well as to efforts to measure the effects of advertising volume on citizen engagement and knowledge (e.g., Freedman, Franz, and Goldstein [2004], who also study the 2000 presidential election). To overcome these limitations in non-random advertising partisan balance and volume, we instead identify a different and more appropriate natural experiment: The "accidental" treatment during the 2000 presidential campaign of some individuals in non-battleground states to high-levels of, or one-sided partisan streams of, presidential advertising simply because they lived in a media market adjoining a battleground state.

In these states, individuals living in areas where presidential advertisements were broadcast were subject to advertising not because the campaigns were seeking their votes, but because purchasing broadcast time for the adjoining battleground state unavoidably made the advertisements available in their media market. Thus, by setting aside battleground states where advertising volume is correlated with previous voter behavior and field campaign activity is unobserved, we can use this natural experiment to isolate the effects of advertising. Our work therefore builds on the pioneering work of JHL and Freedman, Franz, and Goldstein (2004) who link survey data to broadcast advertisement data. Like JHL, our primary data source is created by merging the 2000 National Annenberg Election Survey (NAES, Romer et al. 2004), which includes a measure of the media market in which an individual survey participant resided, and records of actual campaign advertisement broadcasts collected by the Campaign Analysis Media Group (CMAG) and coded for content by the Wisconsin Advertising Project (WAP, Goldstein, Franz, and Ridout 2002).¹⁴ (Like previous work, we include all advertisements, regardless of sponsor,

¹⁴ CMAG uses a technology that records which advertisements are broadcast in individual media markets. The 2000 CMAG data include advertising data from the nation's 75 largest media markets, which reach about 78% of the nation's population. These are the same data used by Friedman, Franz, and Goldstein (2004) and Johnston, Hagen, and Jamieson (2004). For further details about this technology and validation of the CMAG data, see Goldstein and Ridout (2004). Because these are all large media markets centering on relatively populated areas, we need not be concerned that our results are driven by intrinsic

advocating for one presidential candidate. Thus, we include both party- and candidate-sponsored advertisements.)

Unlike these important earlier works, however, our analysis of the CMAG-linked survey data differs in three important respects.¹⁵ First, we restrict our focus to non-battleground states where the presidential campaigns are not active on the ground and we can avoid concerns about whether advertising volume is correlated with unobserved or misreported field activity. Second, we compare respondents *within* individual non-battleground states (that have multiple media markets) by including fixed effects for each state in our analysis to account for *all* other contextual differences at the state level, including state registration laws, whether the state had a competitive Senate or Governor's race, geographic affinity for a particular candidate, etc.¹⁶ Third, we rely on the NAES's embedded panels, so that we can compare individuals before and after exposure to different levels and types of advertising while accounting for individual level differences prior to advertising exposure (e.g. the tendency to over report voting [Vavreck

differences between rural and urban areas. Confirming this intuition, we find no differences between those NAES respondents in non-battleground states living in these media markets and the remaining NAES respondents in these states.

¹⁵ Additionally, unlike Friedman, Franz, and Goldstein, we do not rely on self-reported media consumption to weight the broadcast advertisement data because campaign activity may lead to distorted reports of media consumption and because being at home to watch the early evening shows on which many advertisements were broadcast is also likely to be correlated with watching television news or being contacted by phone bank mobilization efforts that may also affect behavior. In our research design, advertising volume and tone is uncorrelated with other campaign activity or observed education levels, so treatment variation is uncorrelated with likely viewership.

¹⁶ Previous studies assume that there are no differences between and within states. In contrast, we make the *less* restrictive assumption that unobserved heterogeneity is only identical within states. Note that it is unlikely that unmeasured differences in within-state characteristics are driving our results since we are only comparing individuals living in large media markets that encompass both rural and urban areas. Moreover, we include covariates for a host of individual-level and within-state characteristics (age, gender, ideology, party identification, education, union membership, income, competitive house race), which will control for any remaining imbalances.

2005] or have knowledge of political issues). In contrast to previous observational studies that rely solely on average differences in advertising across geographic units, we study whether different advertising streams during the campaign cause individuals to change their attitudes after this specific exposure.¹⁷ (Like Hillygus and Jackman [2003], this design also permits us to estimate treatment effects across cohorts that vary in their initial preferences, a topic we address directly in Section V.)

A direct randomization check confirms that our design remedies the previously observed correlations between prior election outcomes and advertising in 2000. Examining columns (4) and (5) of Table 1A show that the partisan balance of the advertising stream within media markets in battleground states is highly affected by the 1996 election results in the area even if one accounts for intrinsic differences across states. In the non-battleground states that we use in our analysis, however, columns (6) and (7) demonstrate there is no relationship between the 1996 vote share and the partisan balance of the advertising stream in 2000.¹⁸ Similarly, columns (4) and (5) of Table 1A shows that across media markets in battleground states, more competitive areas in 1996 had substantially higher advertising volumes during the 2000 campaign (although neither coefficient is statistically significant). Among non-battleground states, however, this effect is much smaller (column 6) and not statistically significant,

¹⁷ We discard NAES respondents not living in the nation's 75 largest media markets on the grounds that no accurate measure of their advertising exposure exists. Respondents in the NAES panels were first interviewed in the NAES cross-sectional survey between June 1 and November 6 (24% in July, 29% August, 20% September, and 22% in October). They were interviewed for the second time beginning in August. (22% of respondents in our sample were re-interviewed in August; the remainders were re-interviewed after October 1.)

¹⁸ We find no evidence that the imbalance of presidential advertisements is correlated with visits by the presidential and/or vice presidential candidates to the abutting battleground state's media market. Therefore, it is unlikely that this measure is confounded with the impact of news reports on candidate visits. Understandably, the *overall* volume of presidential advertisements is correlated with candidate visits, but this should lead us to overestimate the effects that presidential advertisements have on learning, providing a conservative test of our hypothesis that presidential advertisements do more to affect people's feelings about the candidates than their understanding of issues

especially after one includes state-specific effects (column 7) as we do in our analysis. Because current advertising is uncorrelated with prior vote choice or electoral competitiveness, our research design is unlikely to be confounded by the endogeneity and selection biases that have undermined previous work.

To give clear sense of the substantial variation in advertisement balance in our data, Figure 1 plots Bush's advertising margin in the previous week's advertisements (measured as [Pro-Bush GRPs minus Pro-Gore GRPs]/1000) by state and media market for nine selected states in our sample (of 23 non-battleground states with multiple media markets¹⁹) between July 1, 2000 and Election Day. (Each line is a separate media market.) Note how, within a media market, different candidates had the edge at different times and that variation within a state is not identical over time (for example, one market in Indiana has a clear Bush advertising advantage in the last week of the election, one has a Gore advantage, and three are closely contested). Thus, our data from the NAES's embedded panels allow us to overcome the tendency toward evenness in the partisan balance of advertisements in previous observational studies. When linked to the NAES panels, we continue to find substantial variation in Bush's advertising margin between respondents' interview dates. Across all observation in our dataset, Bush's advertising margin (GRPs/1000) ranges from -5.6 to 12.2, with a mean of .53 and a standard deviation of 1.5. Similarly, Figure 2 plots the overall presidential advertising volume (GRPs/1000) for the previous week in the same states and media markets. Note again that within each state there is some variation in treatment over time, and that, for any given date, there is also variation in advertising quantity across media markets within a state. In the NAES panels the average advertising volume (GRPs/1000) between interview dates is about 1.2 with a standard deviation of 3.1.

Overall, our approach therefore closely replicates the laboratory manipulation of advertising exposure without abandoning the "real-world" context of political behavior. Insofar as disagreements

¹⁹ Seven non-battleground states have only a single media market in our data: AK, HI, MN, ND, RI, SD, and VT. Of the remaining 23 states, we also have to drop ID, MS, MT, NE, VT, and WY due to small sample sizes in the NAES panel. The analysis of county-level vote returns in Section V includes all non-battleground states with multiple media markets.

persist between field and laboratory studies, this research design bridges the gap by isolating the effects of advertising using a large-N natural-experiment.

IV. Findings

Do paid political advertisements inform, mobilize, or persuade citizens to support a particular candidate?

In this section we implement the research design introduced above to estimate the independent effects of political advertisements on citizen opinions and behavior. Contrary to earlier observational research, we find relatively little evidence that campaign advertisements motivate or inform citizens. Instead, we find much stronger evidence that campaign advertisements persuade voters to support a particular candidate.

Do Presidential Advertisements Inform and Mobilize Citizens?

We consider three specific mechanisms have been proposed as to how advertisements can influence political behavior without directly changing which candidate a citizen supports. First, advertisements may generate *interest* in the campaign. In this view, the frequent airing of commercials reminds viewers that there is an election afoot whose outcome may have important implications for their livelihoods.

Consequently, respondents in areas with high advertising volume should be more likely to express both an interest in the campaign and that they are planning to vote. Second, even if advertisements do little to shape the content of citizens' issue opinions, they may nonetheless *educate* voters about the candidates' positions. Advertisements, by making voters more knowledgeable about what each candidate stands for, may therefore allow voters to choose the candidate with whom they most agree. Third, advertisements may *reinforce partisan positions*. As a result, advertisements will polarize the electorate by reminding Democrats that they, like their candidate, prefer spending on health care and Democratic stewardship of the economy. Likewise, Republicans are reminded of the benefits of defense spending and Republican financial policies.²⁰ If reinforcement takes place, we would expect that respondents in areas saturated

²⁰ We also tested for whether the volume (generally, and by advertising subject) or partisan balance of the advertising stream affected the direction of citizen opinions on specific issues (e.g. Social Security Privatization) and found no cases in which it did. Thus, there is little evidence that advertising is

with advertisements would be more likely to express personal policy opinions and candidate preferences in line with their underlying partisan identities than in areas without high volumes of advertising.

In each case, these arguments are built on the assumption that the presence of campaign advertising, rather than its partisan balance, is the means by which citizens are affected by the campaign.²¹ Thus, we follow the practice in previous research on this topic and estimate the effects of the *volume* of presidential advertisements broadcast in a media market on the political attitudes of survey respondents who reside in that market. Advertising exposure is measured as the ratings-adjusted number of presidential advertisements (in GRPs/1000) broadcast in the local media market between each NAES panelists' first and second interview.²² In addition to the volume of presidential advertising, each estimated model includes the respondent's expressed opinion on the relevant dependent variable of interest during the first interview.²³ We also include a measure of all campaign advertising not advocating a presidential candidate (measured on the same scale as the presidential advertising variables) and a set of individual and contextual control variables to account for any other natural variation in responses over time.

At the individual level, we include first-interview measures of strength of partisanship, ideological extremity, church attendance, union membership, income, employment status, educational attainment, race (white) and ethnicity (Hispanic), gender, and age (and age squared to account for any non-linear effects). To account for all state-level contextual effects we include indicator variables for each

persuasive on the issues.

²¹ Indeed, in a simple signaling model, voters can learn about what they do and do not support by observing the positions of their opponent (Lupia 1994).

²² We also experimented with different specifications for advertising exposure and found highly similar results. In particular, we estimated a logarithmic transformation of advertising volume (to test for diminishing effects) and a measure of advertising volume in each of the 4 weeks prior to a respondent's re-interview (to test for short-term effects, see JHL 2004).

²³ Where the dependent variable includes more than 2 categories, we enter each first-interview response category as a separate indicator variable to allow for scaling differences in item response across time.

state of residence. Additionally, because local non-presidential campaign activity might bias our results if it were correlated with presidential campaign advertising, we include an indicator variable for whether a respondent lived in a competitive House district. Each model also includes indicator variables for whether one of five salient events occurred between the respondents' interviews (The Democratic or Republican conventions and each of the three presidential debates) and the week of the second interview to account for national variation in news coverage and events unrelated to presidential advertising. Finally, because our treatment, campaign advertising, varies at the media market level, we cluster our standard error estimates across individuals within a state and media market combination to allow for geographically correlated errors.

Table 3 displays the estimates of the effects of advertising for two measures of campaign interest.²⁴ The first (column 1) is constructed from a survey item asking respondents to describe how interested they were in the presidential campaign on a three-point scale. Higher values indicate greater interest. The second (column 2) asked respondents whether they were likely to vote in the upcoming presidential election. A positive response denotes planning to vote. Contrary to Freedman, Franz, and Goldstein (2004), we find no evidence that campaign advertising increases either interest in the campaign or stated intention to vote. In fact, we find that advertising has a negative and statistically significant effect on stated likelihood of voting.

Table 4A displays estimates of the effects of presidential advertisements on citizen knowledge of candidate positions. Respondents who could correctly place both candidates on each of eight campaign issues for which both candidates took clear positions were coded as knowledgeable (1=Knowledgeable, 0=Not).²⁵ Respondents were also asked to place Bush and Gore on a simple right-left ideology scale, and were coded as knowledgeable if they placed Bush to the right of Gore. Finally, we created a scale that

²⁴ To save space, we report only the coefficient for the measure of presidential advertising in all models. Full model results are available upon request from the authors.

²⁵ All other response patterns (don't know, misplacement, etc.) were coded as unknowledgeable.

summed the number of issues for which a respondent could accurately place both candidates.²⁶ Across the nine individual knowledge items and the summary scale, there are only three cases (Social Security privatization [column 1], death penalty [6], and tax cuts[9]) for which the effect of advertising volume has a positive and statistically significant effect on issue knowledge.

Of course, it is possible that we are understating the effects of advertisements by pooling all advertisements together irrespective of their content. Consequently, Table 5A reports the results of models in which we measured the volume of presidential advertisements relevant to the issue position about which respondents were asked (Advertising subject was coded by the Wisconsin Advertising Project). For only five topics, Social Security, school vouchers, universal health care for children, lawsuits against HMOs, and tax cuts, were there a substantial number of advertisements. Interestingly, the results here are less favorable than in the analysis using all advertisements. Contrary to the results shown in 4A, advertisements on the relevant topic have no statistically significant effect on knowledge of the candidates' positions on Social Security reform and actually depress knowledge about candidate positions regarding tax cuts (column 5). To put these results most succinctly, for those cases for which we can compare the generic and specific effects of advertisement on citizen knowledge, we find greater evidence that advertisements diminish citizen knowledge of candidate positions on the topics they address.

Second, to measure reinforcement, we examined whether advertising caused Democrats and Republicans to *personally* take positions in agreement with their party's candidate on each of seven issues and for their expressed vote choice. Because identifying the appropriate candidate required assessing a respondent's partisan affiliation, we included only those respondents who affiliated with either of the major political parties.²⁷ Individuals scored a 1 if their opinion on an issue matched their party's

²⁶ Because the NAES rotated questions, not all individuals were asked the same battery of questions. Therefore, for all of the scale analyses reported in this paper, the dependent variable is the sum of the separately answered items and the independent variables include indicator variables for whether the respondent was asked each of the constituent items.

²⁷ None of the findings reported here change substantively if we exclude independents who said they

candidate's position (e.g., a Democrat who supports universal health care) and a 0 if their opinion did not. (Individuals were not asked their personal opinions about the size of the tax cut or ideology.) Once again, a scale summarizes this measure across the policy items. Results are shown in Table 4B. For two items, privatizing Social Security (column 1) and providing health care to children (3), we find positive and statistically significant reinforcement effects, although neither effect is statistically significant when we confine our analysis to advertisements on those particular topics as in Table 5B. With regards to candidate choice, we observe a negative and statistically significant coefficient on advertising volume, indicating that greater advertising actually diminished the probability Democrats supported Gore or Republicans supported Bush.

Overall, these results provide little evidence to support the contention that presidential campaign advertisements mobilize or inform citizens. We find no evidence that advertisement increase interest in the campaign or increase the propensity to turnout to vote. Additionally, of the 28 models estimated for tables 4 and 5 to test for knowledge and reinforcement effects, the number of positive and statistically significant coefficients are about what one would expect by chance (18% are positive at $p < .10$, 10% at $p < .05$, and 4% at $p < .01$). This distribution of coefficients suggest that the informational effects of presidential advertisements is likely close to zero with random sampling error accounting for these apparent effects. Furthermore, the general lack of positive results does not appear to be a result of measurement error since supportive findings are apparent in neither the cumulative scales nor when advertisements are broken down by subject. This point aside, we still find occasional positive and statistically significant coefficients. Yet, it is still essential to recognize that this finding is substantially different from the apparently large and persistent knowledge and partisan reinforcement effects that are often attributed to political advertisements in the current literature (and are readily apparent in macro-studies of overall campaigns). At best, presidential advertisements have limited and inconsistent effects in this regard.

“leaned” toward one of the major political parties.

Do Presidential Advertisements Persuade?

While presidential advertisements appear to do relatively little to enlighten citizens, perhaps instead they persuade citizens about which candidate to support. To test this argument, we shift from a simple measure of advertising volume to an assessment of the partisan balance of the advertisements shown in a respondent's media market. We measure the partisan balance of the advertising stream both as Bush's advertising margin vis-à-vis Gore (in GRPs/1000) and separately as the Pro-Bush and Pro-Gore advertising volumes (GRPs/1000). We continue the model specification from the previous section, adding the overall volume of presidential advertisements shown in the market as an additional control variable. Additionally, here we include ideology and partisanship not as absolute values, but as directional variables because Democrats and liberals should behave differently than Republicans and conservatives in evaluating each candidate.

Since the goal of campaign advertisements is to persuade individuals to support the sponsoring candidate while denigrating her opposition, we use three measures to identify these effects. The first measure, *Bush favorability margin*, is calculated as the difference in Bush and Gore's standard thermometer rating (on a 0 to 100 scale). The second measure, *Bush likability margin*, is the difference between Bush and Gore on a scale of four items that asked respondents to rate specific candidate traits (cares, honest, inspires, and knowledgeable). Finally, *Bush vote preference* is a three category measure of expressed vote preference, with undecided voters coded as the midpoint between an expressed preference for Bush or Gore.

The results of the models estimated with these measures are shown in Table 6. As a reminder, because all models are estimated *after controlling for the response to the same measure during the first interview*, the models effectively measure the effects of advertising on changes in the relative favorability, likability, and vote choice across interviews. In columns (1) and (2) we observe the effect of the partisan balance of the advertising stream on candidate favorability. While Bush's advertising margin has a positive effect on Bush's relative favorability, this effect is not statistically significant. In the column (2) specification, more advertising by Bush is associated with an increase in Bush's relative favorability and

more advertising by Gore is associated with a decrease in Bush's relative favorability. Only the latter effect is statistically significant, however. Per the column (2) results, a shift in Gore's advertising volume from one standard deviation below the mean to one standard deviation above the mean (a change of about 3 GRPs/1000) is associated with a 2.45 decrease in Bush's relative favorability (Simulated 95% confidence interval -5.59 to 1.01).²⁸ Given that the average change in Bush's favorability margin from the first to second interview is a paltry .15, this is a substantial difference. The results shown in columns (3) and (4) suggest that advertising alters the likability of the candidates, with a more pro-Bush advertising stream associated with greater relative likability for Bush. None of the coefficients is statistically significant, however.

Finally, columns (5) and (6) show that a greater Bush advertising margin, more Bush advertisements, and fewer Gore advertisements are all associated with an increase in the likelihood of supporting Bush and a decrease in the likelihood of supporting Gore. All three coefficients are statistically significant at the $p < .001$ level. In real terms, assuming that the respondent was initially undecided, a shift in Bush's advertising margin from one standard deviation below the mean to one standard deviation above the mean (about 3 GRPs/1000) is predicted to increase the likelihood that the respondent will vote for Bush by 14% (Simulated 95% confidence interval 3% to 27%), from 22% to 36%. Per the column (6) specification, a shift from one standard deviation below the mean to one above it in Bush's advertising volume (about 3.8 GRPs/1000) is associated with a 21% increase (4% to 39%) in the probability of expressing a preference for Bush while a similar increase in Gore's advertising volume (about 3 GRPs/1000) decreases the probability of preferring Bush by 11% (0 to 23%).

These findings are particularly compelling because 85% of respondents in our sample express the same vote choice in both rounds of the survey. Moreover, the highly similar coefficients across columns (5) and (6) suggests that these results are not merely the result of this model specification coupled with

²⁸ Marginal effects calculated using Clarify (Tomz, Whittenberg, and King 2003) with all continuous variables held constant at their sample means, categorical variables at their sample medians, and indicator variables set to the dominant category.

particular patterns of advertising. In other words, increasing Bush's advertising margin has about the same effect (.157) as an additional unit of Bush advertising (.184) or a unit decrease in Gore's advertising (.132) considered separately. (Additionally, we cannot reject [$p < .05$] the null hypothesis that the coefficient on Bush Advertising is equal in magnitude to the negative coefficient on Gore Advertising, suggesting that Bush's advertising helped Bush as much as Gore's advertising hurt him.) Overall, these are substantively large and statistically significant persuasion effects that are far larger than those detected, if at all, in most previous research.

V. Discussion

The preceding analysis shows that campaign advertisements have substantial persuasive effects. When these effects are properly measured, they appear to dwarf the mobilization and informational effects that previous observational studies have ascribed to televised campaign commercials. Rather, in the most stark analysis, paid campaign advertisements are propaganda that are successful in causing citizens to shift their expressed preferences toward the sponsoring candidate. Of course, campaign commercials are not broadcast in isolation. While our analysis allows us to ascertain the effects of advertising while controlling for other campaign events and underlying differences across voters, these micro-level analyses may overstate the effects of advertising relative to what one would observe in the aggregate population, including among those citizens who would not answer a telephone survey.

We address these concerns in two ways. First, we consider alternative specifications of our analysis of survey data from the previous section and find that our results persist under a broad range of conditions. Second, we show that the advertising effects we identify are apparent not just in survey data, but also in county-level vote returns.

Robustness of model specification

Our analysis in the previous section combined the logic of random treatment in a natural experiment with an analysis of panel survey data. One might wonder, however, whether these results are merely the

artifact of this particular model specification.²⁹ First, what if we abandon the panel specification and instead rely fully on the assumption of random treatment in the natural experimental setting we identify? In this case, we can substantially expand the number of observations (to N=6737) in our analysis by including individuals who were interviewed in the NAES sample only once. In this cross-sectional analysis, we continue to find positive and statistically significant ($p < .01$) effects of Bush's advertising margin (measured from June 30 to the date of interview), Bush's advertising volume, and (a decrease in) Gore's advertising volume on respondent's preference for Bush.

Second, our panel analysis assumes that there are no differences other than advertising across media markets within individual non-battleground states. What if we test this assumption, by reanalyzing our data by including fixed-effects at the media market and state level rather than at the state level? In this case, we once again find positive and statistically significant ($p < .01$) effects of Bush's advertising volume on expressed preference for Bush. Conversely, if we simply ignore differences across states by excluding state or media market fixed effects (thus ignoring potential heterogeneity across states), we still continue to find positive and statistically significant persuasive effects ($p < .01$).

Third, perhaps our results are due to some correlation between advertising volume and partisan imbalance. In other words, we may be estimating persuasion effects based on relatively uncontested media markets, when in fact these persuasion effects may be substantially smaller when overall advertising volume increases. This might occur if, for example, advertising were persuasive when only one side's message was prevalent, whereas a similar margin in advertising in the face of frequent advertising could have less of an effect. To ascertain whether the effect of partisan imbalance was decreasing in overall advertising volume, we interacted Bush's advertising margin with the overall volume of advertising and repeated our analysis of vote preference. In fact, we find that both the main effect of Bush's advertising margin and the interaction between Bush's advertising margin and advertising volume are positive and statistically significant, suggesting that advertising effects are larger,

²⁹ Full results for all alternative model specifications are available from the authors.

not smaller, in more contested markets.

Fourth, while our model controls for prior vote preference, it does not consider the interaction between initial preferences and subsequent treatment. Prior work suggests that campaign effects may be heterogeneous, with the largest impact occurring both for those with weak partisan identities and those individuals whose vote choice is out of line with their underlying partisan proclivities (Hillygus and Jackman 2003). To test this hypothesis, we re-estimated the vote choice model shown in columns (5) of Table 6 in two different ways. Initially, we segmented the data based on initial expressed vote choice (Bush, Gore, Other/Undecided). In this specification, our results are largely unchanged for individuals who initially supported Bush or Gore: Bush's advertising margin appears to substantially improve the probability of supporting Bush, although this effect is larger for Gore supporters than it is for Bush supporters. Among initially undecided respondents (about 10% of our sample), however, we find no evidence that advertising is persuasive. Perhaps this is not surprising, since individuals who are undecided during such a contested election may be generally unengaged in the campaign. At the very least, it does suggest that advertising effects are not limited to influencing individuals who are initially unwilling to support either candidate.

We also examined whether the effects of advertising were different for individuals whose initial vote choice was in or out of line with their expressed partisan attachment. The results among individuals who already supported their party's candidate are nearly identical to the full sample, but the effect for individuals who supported the opposite party's candidate are more mixed. The point estimate of the effect of Bush's advertising margin is much larger than for the remainder of the sample, but because of the extremely small number of individuals in this category (N=175), this result is accompanied by an extremely large standard error that renders the estimate not statistically significant. Thus, we can reject neither the hypothesis that is larger nor smaller than the effect in the full sample. Importantly, however, we continue to find persuasion effects even among individuals who were already "properly" aligned with their party.

Advertising and Vote Returns

Up to now, our analysis of persuasion has focused on changes in respondents' expressed preferences in the context of a telephone survey. Individuals who participate in a survey may not be representative of the larger population, however, and the process of deciding upon and expressing a hypothetical vote choice may actually differ substantially from how voters behave when deciding how (and if) to vote on Election Day. Thus, while our survey analysis allows us to show that survey respondents are not mobilized or informed by campaign advertising, it may nonetheless be the case that the persuasion effects we observe in the NAES panel do not manifest in actual voting data. If they do, however, this is compelling evidence of the external validity of these persuasion effects.

Thus, we turn to an examination of election data to test whether advertising changes actual voter behavior. (We focus on vote choice here. Krasno and Green [2006] conduct a similar analysis of voter turnout and, confirming our survey results, find no evidence that advertising increases turnout.) Our dependent variable for this analysis is the *Shift in the Republican Proportion of the Two-Party Presidential Vote* from 1996 to 2000 for all counties in non-battleground states served by the nation's 75 largest media markets. We again measure the partisan balance of the advertising stream using the CMAG data, and calculate Bush's Advertising Margin (GRPs/1000), as well as Bush and Gore's advertising separately, for the period September 1, 2000 to Election Day. To account for other differences across states, including whether Perot was on the ballot in 1996, we again include state fixed effects in our analysis. Results of the estimated models are shown in Table 7. The results shown in columns (1) and (2) validate our earlier findings from the NAES survey. A larger Bush advertising margin, more Bush advertisements, and fewer Gore advertisements are all associated with a statistically significant increase in Bush's share of the 2-party vote at the county level.

Because we analyze county data and smaller counties exhibit substantially greater shifts in party vote shares, we also report in columns (3) and (4) the same models after reweighting the data by observed county votes to reduce this problem of heteroskedasticity. The earlier results persist, and the magnitude and statistical significance of the persuasive effects of advertising increase. Overall, by relying on the natural experiment of accidental treatment of individuals in non-battleground states, we continue to find

strong evidence of the persuasive effects of advertising even in aggregate vote returns.³⁰

VI. Conclusion

Overall, advertising does a little to inform, next to nothing to mobilize, and a great deal to persuade potential voters. By exploiting the natural experiment of accidental treatment of individuals in non-battleground states to presidential advertising during the 2000 campaign, we are able to generate unbiased estimates of the effects of campaign advertising on citizens. Because of this design, we are able to isolate the effects of advertising relative to other campaign activities, events surrounding the campaign, and the inherent biases in work that relies on self-reported media or advertising consumption.

This work has broad implications for political science and politics more broadly. For political science it suggests the necessity of adopting research designs that overcome the correlation between advertising and other campaign activities. Unlike previous work that does not isolate advertising from either individual receptivity to campaign field activities or the geographic allocation of those partisan campaign field activities, we find little evidence that television advertising either mobilizes or informs citizens.

In contrast, like Johnston, Hagen, and Jamieson (2004), we find evidence that advertising is persuasive. But comparing our results to theirs, most directly by repeating our analysis of persuasion *within* battleground states in the NAES panel (Table 6) and for county level vote returns (Table 7), illustrates the danger of assuming that non-advertising campaign activity is constant within individual battleground states. If we use our model specification in these battleground states, we obtain estimates of advertising's persuasive effects that are larger than those shown here, likely indicative of the fact that the campaigns are strategic about how they target *other* campaign resources within battleground states. No research design that does not measure directly the partisan variation in local field activities within battleground states can ever overcome this simultaneity bias, however. Only an appropriate natural

³⁰ In full candor, if we aggregate the county votes to the appropriate state-media market combination, we are left with only 77 observations. In this restrictive context, the effect of Bush's advertising margin, while still positive, is not statistically significant.

experiment (or field or laboratory experiments with direct random assignment) can disentangle these factors, as we do by looking at variation within non-contested states where campaigns are inactive except on the airwaves (and in an effort to reach other states' voters).

Moreover, we believe these findings also have important normative implications that have been dismissed by the prevailing wisdom about campaigns. If advertising only informed voters and brought them to the polls, the sanguine tone of earlier scholarship might be appropriate. In this case, advertising would simply allow voters to better align their decisions with their policy preferences and little would be at stake in whether one candidate systematically outspent another in a given area.

But, because we find a strong persuasive effect independent of underlying policy opinions, it is clear that more advertising alone will not produce a "better" democratic result. By manipulating voter's expressed candidate preference and voting behavior, the partisan balance of the advertising stream has a direct, important, and under-documented effect on election outcomes. Coupled with the unequal distribution of wealth in society and relatively lax rules for campaign spending, our findings validate the fears of many campaign finance reform advocates that television advertising has the potential to distort the democratic process. Because these distortionary effects originate in the relative partisan imbalance of the advertising stream, only reforms that would equalize the amount of money available to each side could reduce this bias. Consequently, these findings bolster arguments for creating mechanisms that equalize funding for major candidates in the presidential race. Absent such a mechanism, our research suggests that the presidential candidate who can raise and spend the most money and target it to those media markets where the most votes are at stake will do better at the polls, independent of his or her merits. Presidential advertising should not be regarded as a benign tool for improving democracy or as an ineffectual sideshow, but as paid propaganda that distorts the democratic process.

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Appendix 1: Data codebook for Table 1

Dependent variables:

Bush advertising margin, June 30 to Election Day in media market. Calculated as Pro-Bush GRPs/1000 minus Pro-Gore GRPs/1000. Source: CMAG data, coding by WAP. Media markets linked to counties using Nielsen (2000).

Presidential Advertisements volume, June 30 to Election Day in media market. Calculated as Pro-Bush GRPs/1000 plus Pro-Gore GRPs/1000.

Independent variables:

Battleground state indicator (1=yes for Arizona, Arkansas, Delaware, Florida, Illinois, Iowa, Louisiana, Maine, Michigan, Missouri, Nevada, New Hampshire, New Mexico, Ohio, Oregon, Pennsylvania, Tennessee, Washington, West Virginia, and Wisconsin.) Source: CNN.

1996 Republican Vote Share in media market, calculated as Dole votes/(Dole+Clinton votes). Source: USA Counties, Census Bureau.

1996 election closeness in media market, calculated as .5 minus the absolute value of (.5 minus Republican Vote Share in 1996). Source: USA Counties, Census Bureau.

Appendix 2: Data codebook for Table 2

(Unless otherwise noted, data are from Hillygus [2004])

Dependent variable:

Reported Presidential Campaign Contact (0 = no, 1 = yes)

Independent Variables:

Presidential Advertisements Broadcast (GRPs/1000) in media market from September 1, 2000 to interview date.

Non-Presidential Campaign Advertisements Broadcast (GRPs/1000) in media market from September 1, 2000 to interview date.

Battleground state indicator.

Days between September 1, 2000 and interview.

Absolute value of ideology (3 categories, 0 to 1)

Absolute value of partisanship (4 categories, 0 to 1)

Age and Age squared (years)

Black (1=yes)

Education (5 categories, 0 to 1)

Female (1=yes)

Hispanic (1=yes)

Political Interest (4 categories, 0 to 1)

Union member (1=yes)

Appendix 3: Data codebook for Tables 3-6

(Unless otherwise noted, data are from NAES.)

Political Engagement Models, Dependent Variables

Interested in Presidential Campaign: (0 = not much, 1 = somewhat, 2 = very much)

Intention to Vote: (0 = no, 1 = yes)

Knowledge models, Dependent Variables

Respondents were asked to place candidates on following issues:

Privatize Social Security (1 = Bush supports, Gore opposes, 0 = otherwise)

Support School Vouchers (1 = Bush supports, Gore opposes, 0 = otherwise)
Support Universal Health Care for Children (1 = Bush opposes, Gore supports, 0 = otherwise)
Support Suing HMOs (1 = Bush opposes, Gore supports, 0 = otherwise)
Favor Restricting Abortion (1 = Bush supports, Gore opposes, 0 = otherwise)
Favor Death Penalty (1 = Bush and Gore supports, 0 = otherwise)
Favor Handgun Licenses (1 = Bush opposes, Gore supports, 0 = otherwise)
Placement of Bush and Gore on Ideology Scale (1 = Bush to right of Gore, 0 = otherwise)
Bush or Gore Favors Biggest Tax Cut (1 = Bush favors, 0 = otherwise)

Reinforcement Models, Dependent Variables

Respondents were asked to express *their* positions on the following issues:

Privatize Social Security (1 = Republicans support, Democrats oppose, 0 = otherwise)
Support School Vouchers (1 = Republicans support, Democrats oppose, 0 = otherwise)
Support Universal Health Care for Children (1 = Republicans oppose, Democrats support, 0 = otherwise)
Support Suing HMOs (1 = Republicans oppose, Democrats support, 0 = otherwise)
Favor Restricting Abortion (1 = Republicans support, Democrats oppose, 0 = otherwise)
Favor Death Penalty (1 = Republicans support, Democrats oppose, 0 = otherwise)
Favor Handgun Licenses (1 = Republicans oppose, Democrats support, 0 = otherwise)
Vote Choice (1=Republicans support Bush, Democrats support Gore, 0 = otherwise)

Persuasion Models, Dependent Variables

Bush Favorability margin: Bush favorability (0 to 100 thermometer scale) minus Gore Favorability.

Bush Likability margin: Bush Likes (Cumulative scale of four items asking respondents how well the following traits characterized Bush: caring, honest, inspiring, and knowledgeable. Scaled 0 to 1) minus Gore Likes

Bush Vote Preference: (1= would vote for Bush if election held today, 0 = undecided, -1 = would vote for Gore if election held today)

Independent Variables

First interview response to dependent variable

Presidential Advertisement broadcast (GRPs/1000) in media market between interview dates.

Non-Presidential Advertisements broadcast (GRPs/1000) in media market between interview dates.

Absolute value of ideology (3 categories, 0 to 1)

Ideology (-2 = very liberal, -1 = somewhat liberal, 0 = moderate, 1 = somewhat conservative, 2 = very conservative)

Absolute value of party identification (4 categories, 0 to 1)

Party identification (-3 = strong Democrat, -2 = weak Democrat, -1 = Independent leans Democrat, 0 = Pure Independent, 1 = Independent leans Republican, 2 = weak Republican, 3 = strong Republican)

Attend Church (1 = yes)

Union Member (1 = yes)

Income (1 = less than 10K, 2 = 10K-15K, 3 = 15K-25K, 4 = 25K-35K, 5 = 35K-50K, 6 = 50-75K, 7 = 75K-100K, 8 = 100K-150K, 9 = 150K+)

Employed (1 = yes)

Education (1 = 8th grade or less, 2 = some high school, 3 = high school degree, 4 = vocational school, 5 = some college, 6 = associates degree, 7 = college degree, 8 = some graduate school, 9 = graduate degree)

Hispanic (1 = yes), White (1 = yes), and Female (1 = yes)

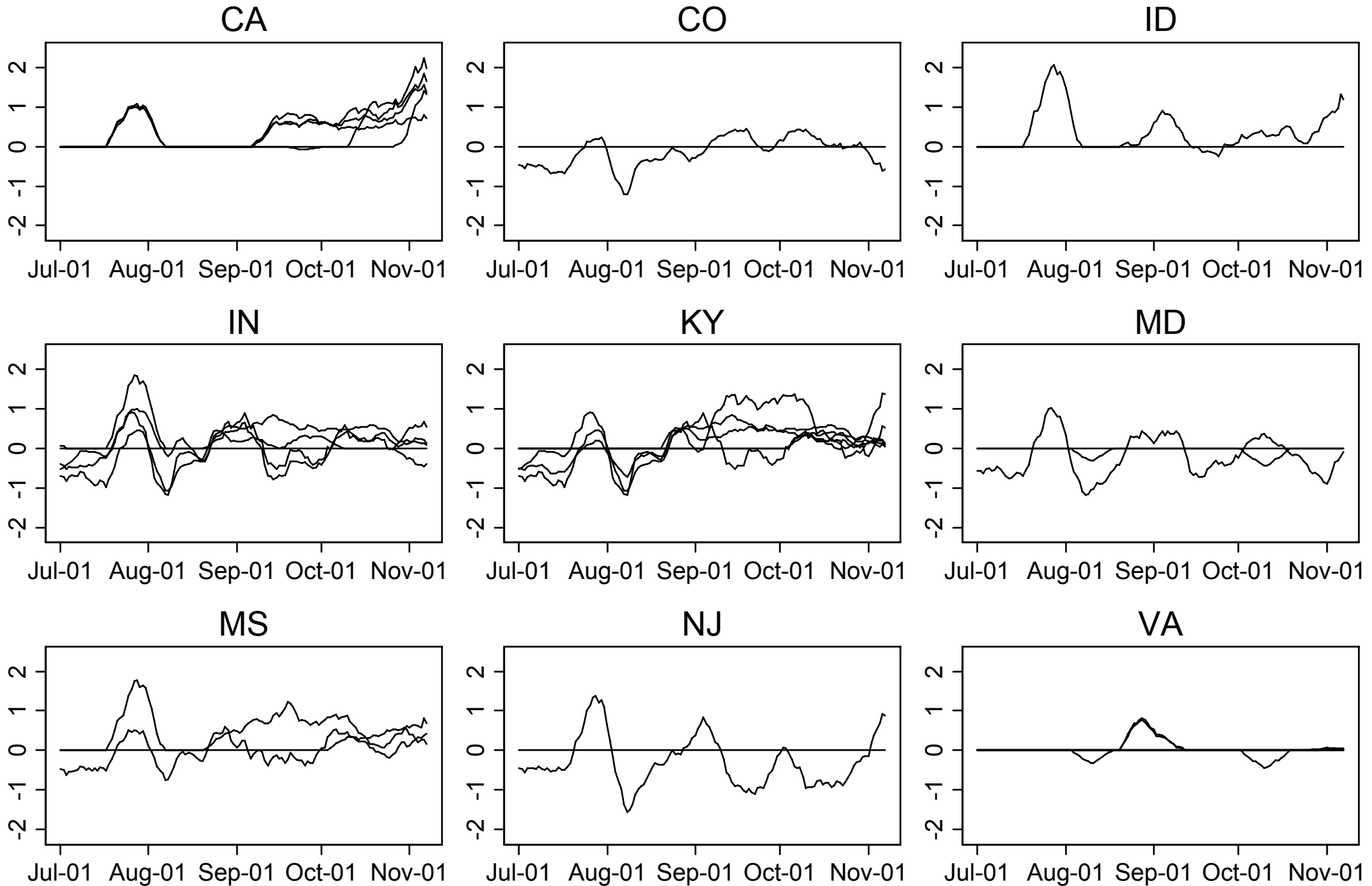
Age and Age squared (in years)

Competitive House Race (see Freedman, Franz, and Goldstein 2004)

State and week fixed effects

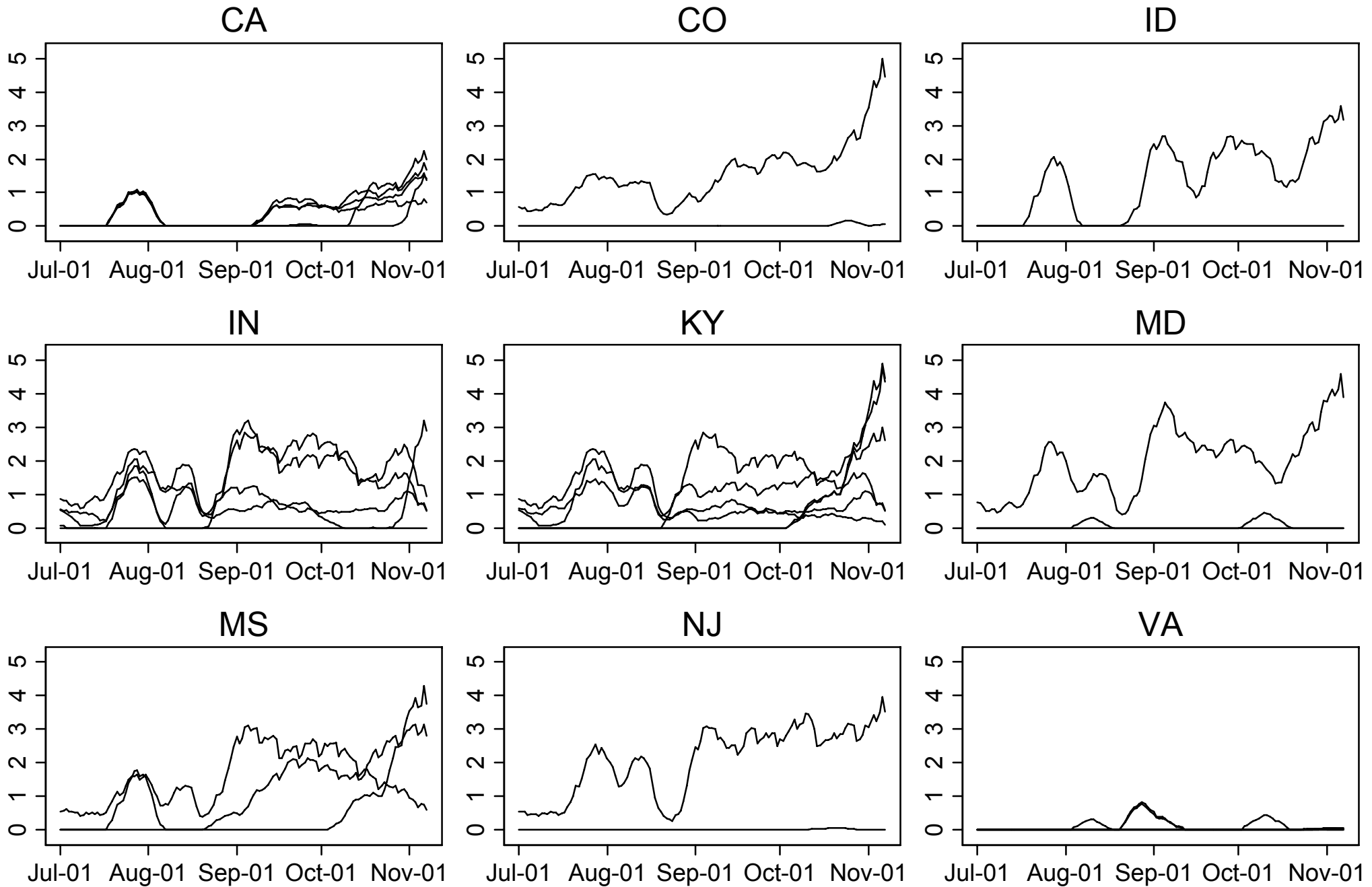
Fixed effects for party conventions and debates

Figure 1: Pro-Bush Advertisement Margin (Previous Week, GRPs/1000) by Media Market and State



Note: Each line is a separate media market

Figure 2: Presidential Advertisements (Previous Week, GRPs/1000) by Media Market and State



Note: Each line is a separate media market

Table 1: The 1996 Vote and Advertising in the 2000 Election

1A) Bush Advertising Margin (GRPs/1000) June 30 to November 7, 2000 by Media Market and State

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	All States			Battleground States		Non-Battleground States	
Fixed Effects for State?	No	No	Yes	No	Yes	No	Yes
Battleground State 2000	-0.88 (0.590)	N/A	N/A	N/A	N/A	N/A	N/A
Republican Vote Share in 1996	4.66 * (2.546)	5.33 ** (2.509)	6.11 (4.244)	13.25 * (7.530)	12.13 (7.376)	1.72 (2.059)	0.56 (4.847)
Constant	-1.17 (1.227)	-1.86 (1.228)	-2.21 (2.113)	-6.09 * (3.615)	-5.55 (3.533)	0.30 (0.958)	0.94 (2.476)
Observations	138	138	130	57	53	81	77
R-squared	0.04	0.03	0.02	0.07	0.07	0.00	0.00

OLS coefficients with robust standard errors in parentheses in columns (1), (2), (4), and (6). See Appendix 1 for data sources and data coding. Fixed effects models include only states with multiple media markets. * denotes $p < .10$, ** denotes $p < .05$, *** denotes $p < .01$. R-squared excludes fixed effects.

1B) Presidential Advertising Volume (GRPs/1000) June 30 to November 7, 2000 by Media Market and State

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	All States			Battleground States		Non-Battleground States	
Fixed Effects for State?	No	No	Yes	No	Yes	No	Yes
Battleground State 2000	16.77 *** (1.987)	N/A	N/A	N/A	N/A	N/A	N/A
Election Closeness 1996	16.28 (10.707)	31.87 ** (14.510)	29.47 (23.312)	25.05 (37.925)	70.79 (50.163)	13.93 (9.068)	10.61 (23.292)
Constant	-0.76 (4.529)	-0.41 (6.021)	0.36 (9.981)	12.24 (16.234)	-7.93 (21.749)	0.22 (3.858)	1.74 (9.886)
Observations	138	138	130	57	53	81	77
R-squared	0.38	0.02	0.02	0.01	0.05	0.01	0.00

OLS coefficients with robust standard errors in parentheses in columns (1), (2), (4), and (6). See Appendix 1 for data sources and data coding. Fixed effects models include only states with multiple media markets. * denotes $p < .10$, ** denotes $p < .05$, *** denotes $p < .01$. R-squared excludes fixed effects.

Table 2: Reported Campaign Contact and Advertising Volume

	(1)	(2)	(3)	(4)
	All States		Battleground States	Non-Battleground States
Fixed Effects for State?	No	Yes	Yes	Yes
Presidential Advertising Exposure (GRPs/1000) September 1 to November 7, 2000	0.013 * (0.0080)	0.017 * (0.0100)	0.052 * (0.0270)	-0.005 (0.0130)
Battleground State 2000	0.238 * (0.1340)	N/A	N/A	N/A
Absolute value of ideology score	0.158 (0.1110)	0.186 * (0.1070)	-0.091 (0.1630)	0.324 *** (0.1230)
Absolute value of partisanship score	0.366 *** (0.1020)	0.360 *** (0.1080)	0.552 *** (0.1960)	0.227 ** (0.1150)
Union member (1=yes)	0.203 ** (0.0890)	0.255 ** (0.1050)	0.438 ** (0.2050)	0.161 (0.1140)
Education	0.490 *** (0.1440)	0.462 *** (0.1600)	0.839 *** (0.2790)	0.250 (0.1860)
Female (1=yes)	0.121 ** (0.0590)	0.084 (0.0680)	0.069 (0.1120)	0.127 (0.0820)
African-American (1=yes)	-0.135 (0.1370)	-0.071 (0.1320)	-0.275 (0.2540)	0.079 (0.1650)
Hispanic (1=yes)	-0.316 (0.2090)	-0.315 (0.2210)	-1.329 ** (0.5810)	-0.088 (0.1850)
Age (years)	0.016 (0.0120)	0.017 (0.0130)	0.036 * (0.0210)	0.001 (0.0150)
Age squared	0.000 0.0000	0.000 0.0000	0.000 0.0000	0.000 0.0000
Constant	-2.137 *** (0.3310)	-2.028 *** (0.3550)	-3.572 *** (0.9470)	-1.440 *** (0.3640)
Observations	1544	1264	515	749
Log-likelihood	-778.06	-642.93	-279.19	-352.51

Probit coefficients with robust standard errors in parentheses, clustered by media zone. Coefficients for fixed effects (columns 2-4) not reported. See Appendix 2 for data sources, data coding, and full model specification. * denotes $p < .10$, ** denotes $p < .05$, *** denotes $p < .01$.

Table 3: Advertising Effects on Campaign Interest and Vote Intention
Controlling for Prior Levels

	(1)	(2)
	Interest in Campaign	Intention to Vote
Presidential Advertising Exposure (GRPs/1000)	0.007 (0.010)	-0.028 ** (0.011)
Other Campaign Advertising Exposure (GRPs/1000)	0.000 (0.004)	0.000 (0.017)
Previous campaign Interest="Not Much"	-2.265 *** (0.127)	N/A
Previous campaign Interest="Somewhat"	-1.259 *** (0.079)	N/A
Previous vote intention="Yes"	N/A	3.432 *** (0.224)
Absolute value of ideology score	0.002 (0.034)	0.212 (0.170)
Absolute value of partisanship score	0.086 ** (0.037)	0.128 * (0.070)
Attend Church (1=yes)	0.188 (0.130)	0.154 (0.253)
Union member (1=yes)	-0.032 (0.088)	-0.338 (0.228)
Income	0.044 ** (0.021)	0.116 ** (0.048)
Employed (1=yes)	-0.082 (0.078)	-0.038 (0.285)
Education	0.051 *** (0.014)	0.020 (0.042)
Female (1=yes)	-0.109 * (0.061)	0.136 (0.173)
Hispanic (1=yes)	0.073 (0.141)	-0.782 (0.557)
White (1=yes)	-0.009 (0.063)	0.165 (0.182)
Age (years)	0.000 (0.013)	0.054 ** (0.022)
Age squared	0.000 0.000	-0.001 ** 0.000
Competitive House District (1=yes)	-0.183 * (0.095)	0.731 (0.497)
N	1868	2051
Log-likelihood	-1444.49	-131.27
Model Specification	Ordered Probit	Probit

Robust standard errors in parentheses, clustered by media zone. Advertising exposure is between first and second interview date. State, week of reinterview, and significant campaign event indicators not reported to save space. See Appendix 3 for data sources, data coding, and full model specification. * denotes $p < .10$, ** denotes $p < .05$, *** denotes $p < .01$.

Table 4: Advertising Effects on Knowledge of Candidate Positions and Partisan Reinforcement
Controlling for Prior Levels

4A) Knowledge of Candidate Positions

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	Privatize Social Security	Support School Vouchers	Support Universal Health Care for Children	Support Suing HMOs	Favor Restricting Abortion	Favor Death Penalty	Favor Gun Control	Placement on Ideology Scale	Candidate Favors Bigger Tax cut	Total Scale
Presidential Advertising Exposure (GRPs/1000)	0.039 ** (0.017)	0.007 (0.014)	-0.028 * (0.014)	-0.008 (0.018)	0.018 (0.030)	0.017 ** (0.008)	-0.007 (0.014)	-0.011 (0.011)	0.019 * (0.010)	0.000 (0.003)
Other Campaign Advertising Exposure (GRPs/1000)	-0.003 (0.006)	-0.011 ** (0.005)	-0.004 (0.005)	-0.004 (0.004)	-0.004 (0.005)	-0.008 ** (0.004)	-0.006 (0.005)	0.001 (0.003)	-0.001 (0.004)	-0.001 (0.001)
Previous Knowledge (1=Knowledgable)	1.126 *** (0.067)	1.416 *** (0.092)	0.876 *** (0.072)	0.958 *** (0.092)	0.912 *** (0.095)	1.254 *** (0.074)	1.243 *** (0.081)	1.378 *** (0.056)	1.232 *** (0.065)	0.159 *** (0.005)
N	1494	1559	1494	1494	1494	1559	1494	2817	2254	3701
Log-likelihood	-760.07	-801.40	-882.81	-840.65	-828.47	-825.33	-798.94	-1130.94	-1003.90	-5903.73
Model Specification	Probit	Probit	Probit	Probit	Probit	Probit	Probit	Probit	Probit	Poisson

4B) Partisan Reinforcement (Alignment between respondent's party's candidate's positions and respondent's positions)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Privatize Social Security	Support School Vouchers	Support Universal Health Care for Children	Support Suing HMOs	Favor Restricting Abortion	Favor Death Penalty	Favor Gun Control	Total Scale	Vote Choice
Presidential Advertising Exposure (GRPs/1000)	0.025 *** (0.007)	0.010 (0.009)	0.023 * (0.012)	-0.002 (0.010)	0.000 (0.008)	0.012 (0.010)	0.014 (0.010)	0.002 (0.002)	-0.031 ** (0.013)
Other Campaign Advertising Exposure (GRPs/1000)	-0.004 (0.003)	-0.002 (0.003)	-0.007 * (0.004)	-0.002 (0.003)	0.001 (0.003)	0.003 (0.003)	0.000 (0.004)	0.000 (0.000)	0.009 (0.006)
Previous Alignment Between Partisanship and Opinion (1=Aligned)	1.436 *** (0.063)	1.874 *** (0.068)	2.432 *** (0.071)	2.177 *** (0.066)	2.165 *** (0.072)	2.426 *** (0.091)	2.158 *** (0.106)	0.183 *** (0.004)	1.800 *** (0.097)
N	2375	3349	2375	2375	3349	2391	2375	3349	1964
Log-likelihood	-1161.58	-1460.55	-781.71	-918.72	-1280.10	-822.82	-931.08	-5958.91	-607.54
Model Specification	Probit	Probit	Probit	Probit	Probit	Probit	Probit	Poisson	Probit

Robust standard errors in parentheses, clustered by media zone. Advertising exposure is between first and second interview date. State, week of reinterview, and significant campaign event indicators not reported to save space. See Appendix 3 for data sources, data coding, and full model specification. * denotes p<.10, ** denotes p<.05, *** denotes p<.01.

Table 5: Advertising Effects on Knowledge of Candidate Positions and Partisan Reinforcement
Controlling for Prior Levels by Advertising Topic

5A) Knowledge of Candidate Positions

	(1)	(2)	(3)	(4)	(5)
	Privatize Social Security	Support School Vouchers	Support Universal Health Care for Children	Support Suing HMOs	Candidate Favors Bigger Tax cut
Presidential Advertising Exposure (GRPs/1000)	0.064 (0.102)	-0.082 (0.123)	-0.098 (0.203)	-0.069 (0.111)	-0.468 *** (0.122)
Other Campaign Advertising Exposure (GRPs/1000)	-0.001 (0.006)	-0.011 ** (0.005)	-0.004 (0.005)	-0.005 (0.004)	-0.002 (0.004)
Previous Knowledge (1=Knowledgable)	1.127 *** (0.068)	1.416 *** (0.090)	0.877 *** (0.072)	0.958 *** (0.092)	1.245 *** (0.065)
N	1494	1559	1494	1494	2254
Log-likelihood	-758.86	-801.15	-882.71	-840.19	-999.83
Model Specification	Probit	Probit	Probit	Probit	Probit

5B) Partisan Reinforcement (Alignment between respondent's party's candidate's positions and respondent's positions)

	(1)	(2)	(3)	(4)
	Privatize Social Security	Support School Vouchers	Support Universal Health Care for Children	Support Suing HMOs
Presidential Advertising Exposure (GRPs/1000)	0.094 (0.190)	0.003 (0.085)	-0.125 (0.101)	0.133 (0.102)
Other Campaign Advertising Exposure (GRPs/1000)	-0.003 (0.003)	-0.002 (0.003)	-0.007 * (0.004)	-0.003 (0.003)
Previous Alignment Between Partisanship and Opinion (1=Aligned)	1.436 *** (0.063)	1.874 *** (0.068)	2.435 *** (0.071)	2.176 *** (0.066)
N	2375	3349	2375	2375
Log-likelihood	-1161.50	-1460.54	-781.05	-918.14
Model Specification	Probit	Probit	Probit	Probit

Robust standard errors in parentheses, clustered by media zone. Advertising exposure is between first and second interview date. State, week of reinterview, and significant campaign event indicators not reported to save space. See Appendix 3 for data sources, data coding, and full model specification. * denotes $p < .10$, ** denotes $p < .05$, *** denotes $p < .01$.

Table 6: Advertising Effects on Assessments of Candidate Traits and Vote Choice
Controlling for Prior Opinions

	(1)	(2)	(3)	(4)	(5)	(6)
	Bush Favorability Margin (Bush minus Gore Favorability)		Bush Likability Margin (Bush minus Gore Likability)		Bush Vote Preference (Bush, undecided, Gore)	
Bush Advertising Margin (GRPs/1000)	0.499 (0.378)	N/A	0.002 (0.011)	N/A	0.157 *** (0.042)	N/A
Pro-Bush Advertising (GRPs/1000)	N/A	0.157 (0.381)	N/A	0.002 (0.011)	N/A	0.184 *** (0.048)
Pro-Gore Advertising (GRPs/1000)	N/A	-0.816 * (0.425)	N/A	-0.002 (0.011)	N/A	-0.132 *** (0.040)
Total Presidential Advertising Exposure (GRPs/1000)	-0.323 ** (0.149)		0.000 (0.003)	N/A	0.025 * (0.014)	N/A
Other Campaign Advertising Exposure (GRPs/1000)	0.101 (0.062)	0.100 (0.062)	0.000 (0.002)	0.000 (0.002)	-0.004 (0.006)	-0.005 (0.006)
Previous Bush Favorability	0.660 *** (0.021)	0.660 *** (0.021)	N/A	N/A	N/A	N/A
Previous Gore Favorability	-0.723 *** (0.027)	-0.723 *** (0.027)	N/A	N/A	N/A	N/A
Previous Bush Likes	N/A	N/A	0.772 *** (0.026)	0.772 *** (0.026)	N/A	N/A
Previous Gore Likes	N/A	N/A	-0.756 *** (0.032)	-0.756 *** (0.032)	N/A	N/A
Previous Bush Vote (1=yes)	N/A	N/A	N/A	N/A	1.221 *** (0.102)	1.221 *** (0.102)
Previous Gore Vote (1=yes)	N/A	N/A	N/A	N/A	-1.146 *** (0.093)	-1.146 *** (0.093)
Ideology score (+ indicates Conservative)	3.408 *** (0.946)	3.408 *** (0.946)	0.076 *** (0.027)	0.076 *** (0.027)	0.127 ** (0.063)	0.127 ** (0.063)
Partisanship score (+ indicates Republican)	4.304 *** (0.544)	4.304 *** (0.544)	0.143 *** (0.013)	0.143 *** (0.013)	0.243 *** (0.027)	0.243 *** (0.027)
Attend Church (1=yes)	2.986 * (1.562)	2.984 * (1.562)	0.029 (0.048)	0.029 (0.048)	0.366 *** (0.125)	0.366 *** (0.125)
Union member (1=yes)	0.245 (1.653)	0.243 (1.653)	0.059 (0.050)	0.059 (0.050)	-0.051 (0.134)	-0.051 (0.134)
Income	-0.322 (0.313)	-0.321 (0.313)	-0.001 (0.010)	-0.001 (0.010)	0.014 (0.024)	0.014 (0.024)
Employed (1=yes)	0.702 (1.567)	0.701 (1.567)	-0.021 (0.052)	-0.021 (0.052)	0.100 (0.114)	0.100 (0.114)
Education	0.087 (0.264)	0.087 (0.264)	0.012 (0.007)	0.012 (0.007)	-0.031 ** (0.016)	-0.031 ** (0.016)
Female (1=yes)	-2.609 ** (1.184)	-2.608 ** (1.184)	-0.057 ** (0.028)	-0.057 ** (0.028)	-0.262 *** (0.070)	-0.262 *** (0.070)
Hispanic (1=yes)	-0.056 (1.690)	-0.055 (1.690)	0.004 (0.061)	0.004 (0.061)	0.074 (0.164)	0.074 (0.164)
White (1=yes)	4.813 ** (1.984)	4.815 ** (1.984)	0.184 *** (0.042)	0.184 *** (0.042)	0.401 *** (0.128)	0.401 *** (0.128)
Age	0.113 (0.147)	0.113 (0.147)	0.004 (0.005)	0.004 (0.005)	0.011 (0.010)	0.011 (0.010)
Age squared	-0.001 (0.002)	-0.001 (0.002)	0.000 (0.000)	0.000 (0.000)	0.000 (0.000)	0.000 (0.000)
Competitive House District Indicator	-0.865 (2.251)	-0.867 (2.250)	0.062 (0.091)	0.062 (0.091)	-0.135 (0.168)	-0.135 (0.168)
N	3541	3541	3232	3232	2142	2142
R-squared/Log-likelihood	0.75	0.75	0.76	0.76	-896.13	-896.11
Model Specification	OLS	OLS	OLS	OLS	Ordered Probit	Ordered Probit

Robust standard errors in parentheses, clustered by media zone. Advertising exposure is between first and second interview date. State, week of reinterview, and significant campaign event indicators not reported to save space. See Appendix 3 for data sources, data coding, and full model specification. * denotes $p < .10$, ** denotes $p < .05$, *** denotes $p < .01$.

Table 7: Advertising Effects on 2000 County-Level Presidential Election Results in non-Battleground States

	Shift in Republican Proportion of Two-party county vote, 1996 to 2000			
	(1)	(2)	(3)	(4)
	Unweighted Analysis		Weighted by County Population	
Bush Advertising Margin (GRPs/1000), September 1-November 7, 2000	0.0032 *** (0.0011)		0.0037 *** (0.0012)	
Pro-Bush Advertising (GRPs/1000), September 1-November 7, 2000		0.0035 *** (0.0013)		0.0042 *** (0.0013)
Pro-Gore Advertising (GRPs/1000), September 1-November 7, 2000		-0.0023 * (0.0011)		-0.0031 *** (0.0011)
N	880	880	880	880
R-squared	0.52	0.52	0.48	0.49

OLS coefficients with robust standard errors in parentheses, clustered by media zone. Constant and state fixed-effect coefficients not reported. See Appendix 4 for data sources and full model specification. * denotes $p < .10$, ** denotes $p < .05$, *** denotes $p < .01$.