Identifying the Persuasive Effects of Presidential Advertising

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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 nonbattleground states during the 2000 presidential election to either high levels or one-sided barrages of campaign advertisements simply because they resided in a media market adjoining a competitive state. We isolate the effects of advertising by matching records of locally broadcast presidential advertising with the opinions of National Annenberg Election Survey respondents living in these uncontested states. This approach remedies the observed correlation between advertising and both other campaign activities and previous election outcomes. 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. We also consider the causal mechanisms that facilitate persuasion and investigate whether some individuals are more susceptible to persuasion than others.

What effect do campaign advertisements have on those viewing them? Aside from evidence showing that these messages improve voter knowledge and interest in campaigns (e.g., Brians and Wattenberg 1996; Freedman, Franz, and Goldstein 2004; Hillygus 2005), few studies that analyze actual campaigns have been able to demonstrate that advertisements persuade individuals to change their minds (cf., Goldstein and Ridout 2004; but see Johnston, Hagen, and Jamieson 2004; Shaw 1999). Yet the finding that campaign commercials mobilize and inform voters seems inconsonant with their content. Political advertisements are crafted to influence citizens' affective evaluations of candidates (Brader 2005), rather than to inform or facilitate a normatively desirable model of democratic citizenship. If so, why have studies of advertising in the field not found effects 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. Postelection studies of advertising effects are limited both by the near overall 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 apparent mobilization effects of presidential campaign advertising may emerge because advertising exposure is strongly correlated with unobserved on-the-ground get-out-the-vote (GOTV) efforts. Similarly, there is a correlation between self-reported media consumption and political knowledge (Zaller 1992), suggesting that the reported informational effects of advertising may instead reflect that informed people more frequently report watching political commercials.

To overcome these barriers to identifying accurately the effects of campaign advertisements, we exploit a natural experiment which occurred during the 2000 presidential campaign. The Electoral College and winner-take-all system cause the major party presidential candidates to strategically focus their campaigning in a handful

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Author order is reverse-alphabetical; both authors contributed fully to this work. Earlier versions of this article circulated under the title "Uncovering the Persuasive Effects of Presidential Advertising." We thank Larry Bartels, Barry Burden, Bob Erikson, Michael Hagen, D. Sunshine Hillygus, Richard Johnston, Jon Krasno, Skip Lupia, David Moore, Becky Morton, David Nickerson, Adam Simon, Lynn Vavreck, Chris Wlezien, John Zaller, and the reviewers, as well as seminar participants at Columbia, Dartmouth, NYU, UCLA, and Yale, for assistance and helpful comments. Appendices, a replication archive, and additional analyses discussed in the text are available at http://research.yale.edu/huber.

American Journal of Political Science, Vol. 51, No. 4, October 2007, Pp. 961–981

 ISSN 0092-5853

of contested states. Despite this targeting, media markets sometimes cross state boundaries. Consequently, citizens of many nonbattleground states receive high levels of campaign commercials because they reside in a broadcast media market that crosses into a battleground state.[1] Other scholars have taken advantage of "natural" variation in advertising to study its effects (e.g., Freedman, Franz, and Goldstein 2004; Johnston, Hagen, and Jamieson 2004), but in contrast to their efforts, we measure attitudes only for individuals from nonbattleground states, thereby breaking the link between political preferences and advertising and isolating the effects of campaign commercials from other campaign activities.

Our analysis provides an assessment of the effects of presidential advertisements that is starkly 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. At best, it appears campaign advertising is only informative on highly salient campaign issues. Instead, campaign advertisements are able to persuade voters to support one candidate over another. We also investigate two causal mechanisms underlying this persuasive effect. First, we find little evidence that advertising persuades by reinforcing the effects of partisan divergence on key policy issues in the campaign. Second, in contrast to most prior observational studies, we find that a one-sided campaign advertising stream alters people's assessments of the presidential candidates' personal characteristics and that this change in candidate affect partially accounts for changes in candidate support.

This persuasion effect is not homogenously distributed throughout the electorate, however. Consistent with Zaller's (1992) Receive-Accept-Sample model, we demonstrate that moderately aware individuals are most susceptible to advertising-induced opinion change. Finally, we validate these survey results with an analysis of county-level vote returns in the 2000 election. Cumulatively, these findings suggest that popular concerns about presidential advertisements being uninformative and misleading may be more appropriate than scholarly claims that they purely enhance democratic accountability.

[1]Throughout this article we define the set of 21 battleground states as AR, DE, FL, IL, IA, KY, LA, ME, MI, MN, MO, MT, NV, NH, NM, OH, OR, PA, TN, WA, and WI. We describe our method for identifying these states in Appendix A1. Roughly, because we seek to differentiate advertising from underlying voter preferences and field-level campaigning, we focus on states where the field campaigns were inactive and variation in advertising treatment is "accidental" (e.g., uncorrelated with prior patterns of voting behavior).

The Effects of Political Advertisements

Despite concerns in the early twentieth century that the increasing reach of mass communication would enhance the use of propaganda to manipulate public opinion (e.g., Lippmann 1922), postwar studies found little evidence that election campaigns succeeded in using the mass media to persuade voters (e.g., Klapper 1960). This early finding has generally stood the test of time: Outside the laboratory, the vast majority of contemporary scholarship continues to find little evidence that campaigns persuade voters to alter their support for a particular candidate. Rather, there is a great deal of evidence that campaigns mobilize voters and disseminate information that allows voters to make "enlightened" choices (e.g., Alvarez 2001; Gelman and King 1993; but see Wlezien and Erikson 2002).

Scholars have localized these mobilization and informational effects of campaigns in television advertisements, finding that campaign commercials increase reported turnout propensities, enhance viewers' knowledge of the candidates, and reinforce citizens' underlying partisan predispositions (e.g., Atkin and Heald 1976; Brians and Wattenberg 1996). Although it is highly likely that campaigns defined broadly (news coverage, campaign events, etc.) motivate and inform, we argue that the content of campaign commercials suggests they are far more likely simply to persuade viewers. Contemporary campaign advertisements often make cursory and tendentious references to issues, and citizens are rightly skeptical of the "factual" content in these messages (Vavreck 2001). Furthermore, advertisements rarely urge viewers to participate in politics. Instead, viewing contemporary 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 research measuring advertising effects. First, the failure of previous studies to find presidential advertising persuasive likely reflects the fact that these campaigns are highly competitive contests in which the overall volume of campaign messages tends to be balanced (Campbell 2000, 45). It would therefore be surprising to uncover much evidence of persuasion in simple postelection studies. Rather, it is necessary to find variation in the relative balance of disseminated campaign messages before one can expect to observe persuasion (Zaller 1996).

Second, individual-level studies that find campaign advertisements inform citizens about issues and candidate positions often use self-reported media consumption to measure advertising exposure (e.g., Atkin and Heald 1976; Zhao and Chaffee 1995).[2] But individuals who report viewing advertisements are substantially different along a number of salient dimensions, including interest in campaigns and knowledge of politics, from individuals who do not report viewing advertisements (Vavreck n.d.). Because interest in politics is positively correlated with receiving political messages, individuals who do pay attention to political advertising are also more likely to be targeted by campaigns for GOTV efforts and direct mail. Consequently, previous research may conflate these unobserved factors, including personal attentiveness and receptivity to other campaign activities, with the effects of campaign advertising (Ansolabehere, Iyengar, and Simon 1999; Vavreck 2000).[3]

In light of these barriers to identifying the effects of presidential campaign advertising from survey data alone, recent scholarship has sought to exploit variation in presidential advertising *across* media markets to discern its effects. In laboratory experiments, random manipulation of exposure to political communication allows researchers to estimate the effects of advertising exposure without bias. Laboratory studies have provided evidence that campaign advertisements are persuasive (Brader 2005; 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 context in which political communication must compete with other viewer interests (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 JHJ) exemplifies this approach. JHJ match respondents in the 2000 National Annenberg Election Survey (NAES) to records of campaign advertisements broadcast in local media markets. While this approach does not allow JHJ 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. JHJ can therefore estimate the (intent-to-treat) effect of broadcasting campaign commercials, even though they reach only a portion of a media market's population. As long as individual differences in receptivity to advertising (e.g., partisanship, attentiveness, etc.) are uncorrelated with geographic variation in advertising, this approach obviates the need to measure those individual characteristics directly and avoids the bias introduced by self-reported media exposure. Unlike most prior observational studies, JHJ find evidence that campaign advertisements are persuasive: In media markets where more pro-Bush (pro-Gore) commercials were broadcast, the proportion of survey respondents indicating they would vote for Bush increased (decreased).

JHJ's work, while innovative, may nonetheless present an inaccurate account of the effects of presidential advertising because variation in campaign activity across media markets is nonrandom. A crucial assumption in naturalistic experiments is that treatment is uncorrelated with other characteristics affecting the outcome of interest. Yet by comparing individuals living in battleground states to one another as well as to those living in nonbattleground states, JHJ assume that only advertising varies across these contexts. As we document more fully in Appendix A1, this assumption is violated because of the strategic targeting of television advertising and other campaign activities. Specifically, across competitive states, and within those states across media markets, *the presidential campaigns target their advertising to areas where their party's candidates have done better in the past*. Similarly, *the volume of presidential campaign advertising is systematically higher in competitive states, and, within those states, in media markets that were close in the previous election, than elsewhere*. It is, therefore, impossible to discern whether differences in candidate support between media markets are caused by the characteristics of their 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). If, for example, the 1996 vote outcome was inflated by a similarly partisan advertising stream in that election, then including it in a statistical model will make the current election's advertisements appear less effective than they actually are. Alternatively, if the 1996 election result proxies underlying voter preferences, excluding it would

[2] Freedman, Franz, and Goldstein (2004) weight objective measures of presidential advertisements broadcast in respondents' media markets by self-reported exposure to television programs. As we explain in Appendix A1, this approach also risks introducing bias.

[3] 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 (Arceneaux, Gerber, and Green 2006). Note too that accounting for whether a state is competitive will not remedy this bias, since it is only attentive individuals *in competitive states* who are receptive to both advertisements and other campaign activity.

artificially inflate the estimated effect of contemporary advertising.[4]

Lastly, while we know field-level campaigns are more active, on average, in competitive states than elsewhere, we cannot accurately measure the targeting or partisan leanings of those efforts across media markets within contested states. (On the partisan targeting of campaign field efforts, see Wielhouwer 2003.) This is a particular concern because, as we document in Appendix A1, advertising volume is correlated with reported nonadvertising campaign contact within contested states. Analyzing advertising effects without accounting for these field-level campaigns therefore risks misattributing to advertising the well-documented effects of those efforts.

Analysis

The foregoing discussion suggests that previous efforts to use broadcast records to discern advertising effects, while superior to the use of self-reported media exposure, are nonetheless limited. In this section we introduce an alternative research design to isolate the effects of advertising from underlying differences in voter characteristics and unmeasured campaign field activity. This research design is then used to test competing theories about the effects of campaign advertising, the mechanism by which advertising might affect candidate choice, and whether advertising has differential effects on separate components of the population.

Research Design

In order to identify the effects of campaign advertising on changes in reported behavior and beliefs, we adopt a research design that both avoids potential biases in reported campaign exposure and differentiates the effects of advertising from field-level campaign activity and underlying differences in voter characteristics. To remedy the former concern, we measure campaign advertising using the Campaign Media Analysis Group's (CMAG) record of ratings-adjusted advertising broadcasts (these advertisements were subsequently coded for content by the Wisconsin Advertising Project [WAP; Goldstein, Franz, and Ridout 2002]).[5] Local ratings for the show during which the advertisement was broadcast are measured in units of Gross Ratings Points, or GRPs.[6] GRPs measure the percentage of households viewing the show on which an advertisement was shown, e.g., an advertisement with a score of 50 GRPs is viewed in half of a media market's households. This measure of advertising exposure is then merged with individual-level survey data collected by the National Annenberg Election Survey (NAES; Romer et al. 2004) over the course of the 2000 campaign. Our basic design therefore builds on the pioneering work of JHJ and Freedman, Franz, and Goldstein (2004), who link survey data to records of broadcast advertisements.[7]

As we discuss above (and document further in Appendix A1), however, exploiting observed variation in advertising neither mitigates our inability to accurately measure campaign field activity nor overcomes the fact that both the volume and partisan balance of advertising are correlated with voter preferences and prior election outcomes. Fortunately, these concerns arise because of the strategic allocation of advertising and nonadvertising campaign resources *to* and *within* contested or battleground states. The presidential campaigns (and their allied national parties) commit substantial resources to GOTV and on-the-ground persuasion efforts in competitive states, but largely ignore the remaining states. If one simply sets aside these contested states and instead focuses only on nonbattleground states, our inability to measure

[4]JHJ include the 1996 state-level vote outcome 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. Nor would simply controlling for local partisanship account for the interaction between citizen beliefs and the effects of advertising (e.g., greater or lesser effects among partisan allies).

[5]CMAG uses a technology that records which advertisements are broadcast in individual media markets. The 2000 CMAG data include advertising measures from the nation's 75 largest media markets, which reach about 78% of the nation's population. These are the same data used by Freedman, 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). Like previous work, we include both party- and candidate-sponsored advertisements advocating for one presidential candidate. Additionally, like JHJ, 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. The NAES dataset includes the media market in which each survey participant resided.

[6]Nielson Media Research uses local viewing habits measured by both electronic monitoring of television tuning habits and self-reported viewing diaries to calculate ratings. These data are matched to the CMAG data using time and channel of airing.

[7]Friedman, Franz, and Goldstein rely on self-reported media consumption to weight the broadcast advertising data. Like JHJ, we do not take this step 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 other campaign mobilization efforts that may also affect behavior. In our research design, advertising volume and tone is uncorrelated with education levels or other campaign activity, so treatment variation is uncorrelated with viewing habits.

field activity is moot; because the presidential field campaigns are inactive in nonbattleground states, we need not be concerned that advertising is correlated with personalized campaigning.

Of course, if campaigns could strategically target television advertisements as precisely as they target field activities, examining uncontested states would provide us with little leverage for understanding the effects of these messages. Fortuitously, television broadcast signals, unlike campaign workers, have little regard for state boundaries. If a campaign purchases advertising in the Philadelphia media market to target voters in Pennsylvania, these broadcasts also appear on televisions in parts of Delaware and New Jersey. Our identification strategy exploits the "accidental" treatment during the 2000 campaign of some individuals in nonbattleground 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. (A list of included state and media-market combinations appears in Appendix A1.) Individuals living in these areas received television 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.[8] Confirming this intuition, our analysis in Appendix A1 shows that both the volume and partisan balance of advertising are uncorrelated with previous voter behavior in nonbattleground states. We can therefore exploit this natural experiment to properly isolate the effects of advertising.

The dependent variables in our analyses are NAES individual-level survey measures of expressed preferences and beliefs. We model them as a function of unsystematic variation in broadcast advertisements in nonbattleground states, an approach which allows us to minimize the bias introduced by self-reported advertising exposure, unobserved field activity, and the strategic targeting of advertising to underlying voter preferences. Several additional features of our approach are also worth emphasizing.

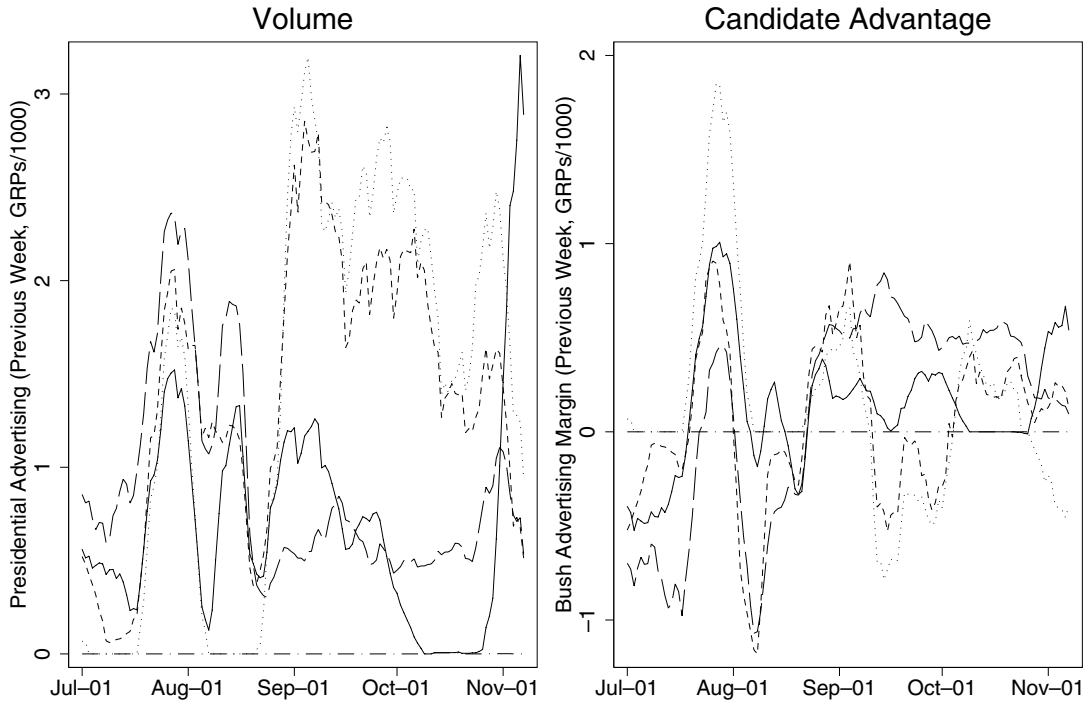
First, the NAES was fielded as a rolling cross-section with embedded panels (a portion of the cross-sectional respondents were selected for subsequent reinterview). We analyze separately both sets of data. In the cross-section, we have approximately 8,300 observations dispersed evenly over the postconvention active election period between September 1, 2000, and Election Day (about 120 per day). In the panel we have approximately 4,300 respondents, but to obtain a sufficient sample for statistical analysis we must include all panel respondents who were first interviewed between June and Election Day (3% in June, 23% in July, 26% in August, 19% in September, 26% in October, and 2% in November), and reinterviewed after August 1, 2000 (22% in August, 37% in October, 41% after the election).

The advantage of the cross-sectional data is that we obtain regular and large samples throughout the active fall campaign. Although we lack first-interview information on expressed beliefs for these respondents, our research design focusing on nonbattleground states ensures there is no systematic relationship between prior beliefs and advertising. In the panel, we measure initial opinions during the first interview, which allows us both to conduct tests of the effects of advertising on changes in opinions and to examine the effects of changes in those opinions on candidate preference. At the same time, the period of data collection is larger (including periods before the active advertising campaigns and after the election) and less regular. For example, the NAES did not reinterview any panel respondents in September, and the postelection interviews extended into December (20% were gathered more than two weeks after the election). Accordingly, the number of days respondents were in the panel varies widely, with a median of 28 days and a mean of 49.

Second, these differences in survey data lead us to measure advertising differently in our cross-sectional and panel analyses. In the panel, advertising is simply measured in cumulative GRPs between the first and second interview date. In the cross-section, however, we must decide on an appropriate time window. We experimented with six-, four-, and two-week lags in broadcast advertisements, but settled on the cumulative GRPs of advertisements broadcast in the four weeks prior to a respondent's interview because that measure captures what might feasibly have been on a respondent's mind by the time she was interviewed without overweighting perhaps temporary short-term effects. To give a clear sense of the substantial variation in advertising in our data, Figure 1 plots both the volume and partisan balance of advertising in a representative nonbattleground state, Indiana. The left panel displays the volume of advertisements (measured in GRPs/1000) in the previous week in each of the five media markets serving Indiana, while the right panel displays Bush's advertising margin (measured as [Pro-Bush GRPs minus Pro-Gore GRPs]/1000) for the same period. Note how, within a media market, advertising volumes fluctuate widely over time and that each candidate had the edge at different times. Similarly, across media markets (even within a single state), advertising volumes and candidate advantage vary substantially. Thus, our data allow us to overcome the tendency toward evenness in the

[8] A notable exception is some markets in California, where the Bush campaign spent large amounts of money in the last few weeks of the campaign (Marks 2000). The persuasion effects reported below persist if we exclude California or any other state.

FIGURE 1 Presidential Advertising Volume and Candidate Advantage in Indiana Media Markets, 2000



Note: Each line is a separate media market

volume and partisan balance of advertising apparent in many previous observational studies.

When linked to the NAES survey data, we continue to observe substantial variation in advertising volume and tone. In the cross-section, the average volume of advertising in the previous four weeks is .69 GRPs/1000 (S.D. 1.80, Min. 0, Max. 11.36), while Bush's average advertising margin is .25 GRPs/1000 (S.D. .82, Min. −2.46, Max. 4.82). In the panel, the comparable figure for between-interview volume is 1.11 GRPs/1000 (S.D. 3.21, Min. 0, Max. 34.92), and for Bush's advertising margin it is .48 GRPs/1000 (S.D. 1.47, Min. −5.56, Max. 12.22).

Third, we seek to avoid misattributing to advertising the effects of other contextual differences that may explain observed opinions. These include differences across states (e.g., whether a state had a gubernatorial race, geographic affinity for a particular candidate, registration laws, etc.), differences over time (e.g., how the televised candidate debates and national news coverage altered evaluations of each candidate), and differences associated with whether respondents lived in an area that was subject to advertising or campaign activity associated with other political contests (e.g., a competitive House race). We account for all contextual differences at the state level by including fixed effects for each state in our analysis. To control for nationwide differences over time, we include indicator variables for each week in which a respondent was interviewed in the cross-sectional analysis and reinterviewed in the panel analysis. (Because panel respondents were in the panels for different periods of time, we also include both a measure of the number of days they were in the panel and indicators for significant campaign events, including the conventions and debates, occurring between their interviews.) We also measure directly whether respondents lived in a district with a competitive U.S. House race and the amount of nonpresidential campaign advertisements broadcast in the area (see Appendix A2 for these and other coding details).

Finally, our treatment, advertising exposure, is systematically correlated across respondents in a single media market. For this reason, our econometric approach is to cluster standard errors within state and media market combinations. Overall, our approach attempts to closely replicate the laboratory manipulation of advertising volume and partisan tone without abandoning the naturalistic context in which this communication is received over time, includes multiple and competing messages, and must compete with other distractions for viewer

attention. Insofar as disagreements persist between field and laboratory studies, this research design should help to bridge the gap by isolating the effects of advertising in the field.

Data Analysis

We begin our empirical analysis by addressing three questions left unresolved in the current literature: (1) Does advertising engage citizens in the campaign, (2) does it inform them (or enable them to adopt appropriate partisan positions on the issues), and (3) does it directly alter their evaluation of the candidates (persuade them)? We then examine the mechanism by which campaign advertising alters reported vote choice. Finally, we test whether advertising effects are moderated by individual characteristics that affect exposure and receptivity.

Does advertising engage? We first consider the argument, advanced most forcefully by Freedman, Franz, and Goldstein (2004), that advertising engages citizens in the campaign. This might occur if, for example, advertising reminds viewers that there is an election afoot, communicates that the substantive stakes of remaining on the sidelines are large, or, if by using emotional imagery and language, it induces viewers to care about the outcome of the election. The core empirical hypothesis is that the frequent airing of commercials will cause citizens to be more likely to express interest in the campaign and to turn out to vote. We test this prediction by determining whether campaign interest and intention to vote are higher among NAES respondents from areas saturated with presidential campaign advertisements. Our independent variable of interest is therefore advertising exposure, measured as the ratings-adjusted number of presidential advertisements (in GRPs/1000) broadcast in the local media market in which each NAES respondent lives (between interviews in the panel, in the prior four weeks in the cross section).

Before proceeding to discuss our estimates, we provide a full description of our model specification in both the panel and cross-sectional analyses because we use a similar design throughout the remainder of this article. (Summary statistics, coding rules, and full model specifications for all estimates appear in Appendix A2.) In our panel specifications we include (in addition to the volume of presidential advertising) first-interview measures of the dependent variable (measured as category indicators to allow for nonlinear effects in cases with more than two response categories), a standard array of individual characteristics that might independently alter engagement (first-interview strength of ideological position and partisanship, church attendance, union membership, income, employment status, education, gender, race and ethnicity, and age), measures of nonadvertising features of the presidential campaign environment (the number of days the respondent was in the panel, indicators for whether the respondent was in the panel during either of the conventions or each of the debates, and an indicator for the week in which the respondent was reinterviewed), and measures of other salient features of the political environment (whether the individual lived in a district with a competitive race for the U.S. House, the volume of nonpresidential campaign advertising, and an indicator for the respondent's state of residence). In the cross-sectional specifications we lack prior responses for the relevant dependent variable question, include the same demographic controls (but measured at the same time as the dependent variable), use week of interview indicators to account for nonadvertising features of the presidential campaign environment, and include the identical measures for other salient features of the political environment.

Table 1 displays the results of our analysis. Columns (1) and (2) are the results for ordered probit models of the effects of advertising volume on reported interest in the campaign (2 = "A Lot", 1 = "Somewhat", or 0 = "Not Much") using, respectively, the panel and cross-sectional data. Columns (3) and (4) are probit estimates of reported turnout plans (1 = "Yes" or 0 = "No"), again estimated separately for the panel and cross-section. If advertising enhances interest or turnout, one would expect positive coefficients on the presidential advertising exposure variable. Across specifications, however, we do not find evidence that advertising increases interest in the campaign or plans to vote. In general, the coefficients are small and statistically insignificant, the one exception being a marginal *decrease* in reported turnout propensity (significant only at the $p < .10$ level) among panel respondents exposed to high levels of advertising, although this effect is not found in the cross-sectional analysis. In short, there is little reason to believe that advertising alone engages citizens in the campaign (for a similar conclusion, see Ashworth and Clinton 2007 and Krasno and Green n.d.).

This finding illustrates the importance of the earlier discussion of the correlation between advertising and other campaign activity in contested states. While frequent advertising is, on average, associated with greater engagement in the campaign, this pattern arises because of the correlation between advertising and grassroots campaigning. When we isolate the effects of advertising from field campaigns, our results diverge sharply from those of prior studies.

TABLE 1 Advertising Effects on Interest in Campaign and Intention to Vote

	Interest in Campaign		Intention to Vote	
	Panel (1)	Cross-Section (2)	Panel (3)	Cross-Section (4)
Presidential Advertising Exposure (GRPs/1000)	0.004	0.008	−0.019*	0.017
	(0.008)	(0.007)	(0.010)	(0.018)
Other Campaign Advertising Exposure (GRPs/1000)	−0.001	0.000	−0.014	−0.006
	(0.004)	(0.004)	(0.017)	(0.006)
Previous Interest in Campaign = "Not Much"	−2.186***			
	(0.118)			
Previous Interest in Campaign = "Somewhat"	−1.225***			
	(0.079)			
Previous Intention to Vote = "Yes"			3.359***	
			(0.196)	
Absolute value ideology score	−0.049	0.084***	0.173	0.070*
	(0.038)	(0.030)	(0.160)	(0.038)
Absolute value of partisanship score	0.224***	0.458***	0.317***	0.535***
	(0.056)	(0.038)	(0.110)	(0.033)
Religious services attendance (Scale, 0 to 4)	0.046	0.042***	0.004	0.105***
	(0.031)	(0.013)	(0.057)	(0.017)
Union Household (1 = yes)	−0.050	0.101**	−0.311	0.150***
	(0.080)	(0.051)	(0.203)	(0.058)
Income (Scale, 1 to 9, 10 = DK/NA)	0.039**	0.044***	0.090**	0.070***
	(0.019)	(0.009)	(0.045)	(0.012)
Income Don't Know/No Answer	0.119	−0.146*	−0.603*	−0.422***
	(0.143)	(0.082)	(0.325)	(0.113)
Employed (1 = yes)	−0.051	−0.025	−0.113	0.032
	(0.074)	(0.058)	(0.269)	(0.065)
Education (Scale, 1 to 9)	0.052***	0.057***	0.044	0.166***
	(0.014)	(0.009)	(0.040)	(0.009)
Hispanic (1 = yes)	−0.156	−0.078	0.404	0.021
	(0.133)	(0.082)	(0.359)	(0.075)
White (1 = yes)	0.008	0.036	0.217	0.015
	(0.056)	(0.063)	(0.179)	(0.050)
Female (1 = yes)	−0.127**	−0.118***	0.096	0.070
	(0.056)	(0.044)	(0.157)	(0.048)
Age (years)	0.000	0.007	0.058***	0.030***
	(0.011)	(0.007)	(0.020)	(0.008)
Age Squared	0.000	0.000	−0.001***	0.000
	(0.000)	(0.000)	(0.000)	(0.000)
Competitive House District (1 = yes)	−0.065	0.116	0.427	−0.027
	(0.086)	(0.080)	(0.403)	(0.084)
Observations	2099	3972	2265	7477
Log-likelihood	−1624.543	−3905.542	−149.021	−1799.500
Model Specification	Ordered Probit	Ordered Probit	Probit	Probit

Coefficients with robust standard errors in parentheses, clustered by state and media market combination. In panel analyses, advertising exposure is between first and second interview date. In cross-section, it is four weeks before interview date. Constant, state and week of (re)interview indicators, and, in panel only, days in panel and significant campaign event indicators, not reported to save space. See Appendix A2 for data coding and full model specification. *denotes $p < .10$, **denotes $p < .05$, ***denotes $p < .01$, two-tailed tests.

Does advertising inform? If advertising does not engage citizens, it may nonetheless inform them about the positions of the candidates on important issues. This effect might manifest in two ways. First, more accurate information about candidate positions could make citizens better able to identify their positions. Alternatively, the effect could be more indirect, with more information allowing partisan voters to express personal policy positions in line with their party's candidate's expressed positions (e.g., Finkel 1993). In other words, advertising may reinforce the correlation between personal and partisan positions on the issues (e.g., Democrats will prefer maintaining the status quo Social Security system, while Republicans will prefer greater shifts to personal accounts). In either case, both perspectives predict that knowledge of candidate positions and the alignment between a respondent's opinions and those of her party's candidate will be greater in areas with more frequent advertisements. Once again, it is therefore the presence of campaign advertising, rather than its partisan balance, that is the means by which citizens are affected by the campaign.[9]

Table 2 displays the results of our analysis of knowledge and reinforcement effects. (Because of space limitations, we do not report full model results for the remaining tables.) In Panel 2A we focus on whether high levels of exposure to presidential campaign advertising are associated with knowledge of candidate positions on five salient issues and a simple left-right ideology scale.[10] Respondents were coded as knowledgeable (1 = "Yes" or 0 = "No") if they could accurately place both candidates on the issue or could place Bush to the right of Gore on the ideology scale. The only evidence of citizen learning emerges in the case of Social Security reform, for which presidential advertising is associated with a greater ability to place Bush (in favor of personal accounts) and Gore (opposed to personal accounts) in both the panel and cross-sectional analyses. Per the column (1) specification relying on panel data, for a respondent who was previously unknowledgeable and had been exposed to the average volume of advertising in the median state, a one standard deviation increase in advertising (about 3.21 GRPs/1000) is associated with a predicted increase in accurately placing the candidates of about 3.0% (from a baseline of 67.4%, simulated 95% confidence intervals of 0.1 to 6.9%).[11] Apart from this finding, however, the only other statistically significant result appears to demonstrate that advertising exposure *decreases* citizen knowledge of the candidates' positions on tax cuts, although this effect does not also appear in the panel data.

Panel 2B examines whether advertising exposure facilitates the matching of personal and partisan positions. Because identifying the appropriate position required assessing a respondent's partisan affiliation, we included only those respondents who affiliated with or "leaned toward" either of the major political parties.[12] Individuals were scored as reinforced (1 = "Yes" or 0 = "No") if their expressed personal opinion on an issue matched their party's candidate's position (e.g., a Democrat who opposed personal Social Security accounts). Reinforcement is measured using six different items, the four policy areas used in the knowledge analysis, a simple measure of candidate preference, and a question that asked respondents which party they thought was better at managing the economy (we are unable to use the tax cut item because respondents were not asked their personal opinion on this issue).

We find little evidence that advertising facilitates the matching of personal and partisan positions. In only three instances (across 11 estimated models) is advertising associated with statistically significant increases in the likelihood a respondent adopts her party's position on an issue (in the panel analysis, opinions about personal Social Security accounts and funding universal health care for children, and expanding patient rights to sue HMOs in the cross-sectional analysis), but these findings are never repeated across both the panel and cross-sectional specifications. (There are also two negative and statistically significant effects, but again these are not robust across data sources.) We focus here on the Social Security item because it is the only area for which the prior analysis of citizen knowledge revealed that advertising was associated with greater knowledge of candidate positions—if

[9]Indeed, in a simple signaling model, voters can learn about what they do and do not support by observing the position of their nonpreferred candidate (Lupia 1994).

[10]In an earlier version of this article we considered additional issues that were secondary to the main thrusts of the 2000 campaign (e.g., giving the death penalty to terrorists, which both candidates supported) and found little evidence of learning or reinforcement. Additionally, we examined whether learning and reinforcement were conditional on the subject of the advertisements (as coded by the Wisconsin Advertising Project) and found no greater evidence of either effect. Finally, we have also explored whether learning and reinforcement were more prominent among individuals interviewed early in the campaign (e.g., the spring and summer of 2000) on the grounds that by the start of the fall election season both candidates' positions were already well known. These results, however, are no more favorable to the learning or reinforcement hypotheses.

[11]All marginal effects are calculated using Clarify (Tomz, Wittenberg, and King 2003) with categorical variables held constant at sample modes and continuous variables held constant at sample means. In both the panel and the cross-section this is a respondent living in California.

[12]Results are similar if we exclude independents who said they "leaned" toward one of the major political parties.

TABLE 2 Advertising Effects on Knowledge of Candidate Positions and Partisan Reinforcement

	Allow Personal Social Security Accounts (1)	Support School Vouchers (2)	Fund Universal Health Care for Children (3)	Expand Patient Rights to Sue HMOs (4)	Placement on Ideology Scale (5)	Which Candidate Favors Bigger Tax Cut? (6)
A Knowledge of Candidate Positions						
Knowledgeable response is:	(Bush = Yes, Gore = No)	(Bush = Yes, Gore = No)	(Bush = No, Gore = Yes)	(Bush = No, Gore = Yes)	(Bush to the right of Gore)	(Bush)
Panel						
Presidential Advertising Exposure (GRPs/1000)	0.027* (0.017)	−0.010 (0.014)	−0.003 (0.013)	−0.006 (0.016)	−0.005 (0.009)	0.008 (0.011)
Observations	1696	1761	1696	1696	3175	2531
Log-likelihood	−857.75	−903.27	−1004.93	−941.68	−1236.77	−1123.07
Cross-Section						
Presidential Advertising Exposure (GRPs/1000)	0.019* (0.010)	−0.003 (0.010)	0.006 (0.009)	0.013 (0.009)	−0.016 (0.011)	−0.015** (0.006)
Observations	3841	3991	3841	3841	5759	5513
Log-likelihood	−2347.64	−2405.52	−2351.99	−2154.86	−3146.54	−3223.21
	Allow Personal Social Security Accounts (1)	**Support School Vouchers (2)**	**Fund Universal Health Care for Children (3)**	**Expand Patient Rights to Sue HMOs (4)**	**Candidate Preference (5)**	**Which Party Manages Economy Better? (6)**
B Partisan Reinforcement (Alignment between respondent's party's candidate's positions and respondent's positions)						
Reinforced response is:	(Rep. = Yes, Dem. = No)	(Rep. = Yes, Dem. = No)	(Rep. = No, Dem. = Yes)	(Rep. = No, Dem. = Yes)	(Rep. = Bush, Dem. = Gore)	(Rep. = Rep., Dem. = Dem.)
Panel						
Presidential Advertising Exposure (GRPs/1000)	0.022*** (0.008)	0.010 (0.008)	0.022** (0.010)	−0.006 (0.009)	−0.035*** (0.012)	Insufficient data to estimate
Observations	2652	3758	2652	2652	2162	
Log-likelihood	−1296.27	−1666.91	−902.66	−1021.45	−666.72	
Cross-Section						
Presidential Advertising Exposure (GRPs/1000)	0.001 (0.012)	−0.016*** (0.006)	0.004 (0.014)	0.020* (0.011)	−0.001 (0.008)	0.013 (0.010)
Observations	3777	6958	3777	3777	6781	5101
Log-likelihood	−2471.68	−4604.84	−2365.87	−2485.64	−3241.15	−2717.26

Probit coefficients with robust standard errors in parentheses, clustered by state and media market combination. In panel analyses, advertising exposure is between first and second interview date. In cross-section, it is four weeks before interview date. Constant, control variables, state and week of (re)interview indicators, and, in panel only, days in panel, significant campaign event indicators, and prior responses, not reported to save space. See Appendix A2 for data coding and full model specification. *denotes $p < .10$, ** denotes $p < .05$, *** denotes $p < .01$, two-tailed tests.

advertising is to facilitate reinforcement, presumably it must first be the case that respondents can learn where the candidates stand on the issue. Per the column (1) estimate using panel data, and assuming an individual did not previously hold her party's position on Social Security, a one standard deviation increase in advertising is associated with a predicted increase in adopting her party's candidate's position by about 2.5% (from a baseline of 32.5%, simulated 95% confidence intervals of .2 to 5.0%).

In summary, there is weak and inconsistent evidence for the claim that advertising enhances citizen learning or the adoption of partisan positions. Examining Table 2 in its entirety, positive and statistically significant coefficients are about as common as negative and statistically significant effects. The most plausible evidence of learning and reinforcement concerns citizen opinions about Social Security reform, one of the key substantive issues of the 2000 campaign (JHJ 2004), where greater advertising is associated with better knowledge of the candidates' positions on this issue and (in the panel analysis) citizens adopting their party's position. 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 broadcast during the 2000 campaign had limited effects in this regard.

Does advertising persuade? Finally, we examine whether advertising directly alters evaluations of the candidates. While prior studies have generally found minimal evidence of persuasion, JHJ's recent work documenting a direct effect of partisan advertising on reported vote choice is a notable exception. In addition to this measure (coded so that 1 = "Bush," 0 = "Undecided," and −1 = "Gore"), we also examine whether partisan advertising affects respondents' affect toward the candidates, measured here as both the favorability of the candidates (measured on a standard 0–100 feeling thermometer) and their likeability (a 13-category scale created from candidate evaluations on four measures: caring, honesty, inspiration, and knowledge, higher values indicate greater likeability). Changes in affective evaluations may emerge because, as Brader (2005) and others have argued, advertising contains emotional imagery and language specifically crafted to alter people's views of the candidates.

Examining persuasion requires changes to the research approach we have taken so far. First, 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. Second, and most significantly, 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 advertisements advocating for each candidate separately as Pro-Bush and Pro-Gore advertising volume (in GRPs/1000). If advertising is persuasive we would expect Gore's advertising to improve his favorability, likeability, and chance of being voted for while diminishing Bush's. Likewise, Bush's advertising should have the opposite effects. Model estimates appear in Table 3.

In the panel and cross-sectional analysis, higher levels of Gore advertising are associated with a decline in Bush's favorability and these effects are statistically significant. Per the column (1) estimates using panel data, a one standard deviation increase in Gore's advertising (about 1.57 GRPs/1000) is associated with a decline of .70 in Bush's feeling thermometer score. While this effect initially seems small, it is relatively large given that the average shift in this score across interviews is a paltry .60. (In the cross-sectional analysis, the same shift in advertising is associated with a 1.73 unit decline in Bush's favorability.) We also find strong evidence in the cross-sectional analysis that each candidate's advertisements alter Gore's favorability, although these effects are not statistically significant in the panel analysis (and one, Bush's advertising, has the wrong sign). Per the column (2) specification using cross-sectional data, supplementing Bush's advertising by the same amount discussed above decreases Gore's favorability by 2.00 units while a similar increase in Gore's advertising increases his favorability by 2.98 units.

Turning next to candidate likeability, we find strong evidence that advertising alters the relative appeal of each candidate, but only in the cross-sectional data. Per the column (3) estimates using these data, a 1.57 GRPs/1000 increase in Bush's advertising is predicted to increase Bush's probability of obtaining an above-average likeability ranking by about 4.7% (from a baseline of 43.7%) while a similar shift in Gore's advertising decreases this probability by about 5.4%. The results for Gore's likeability are highly similar in the cross-section. Per the column (4) estimates, a 1.57 GRPs/1000 increase in Bush's advertising decreases the probability of obtaining an above-average likeability rating by 3.2% (from a baseline of 56.8%) while an increase of that magnitude in Gore's advertising increases the probability by 4.9%.

These effects are substantial, but it is somewhat disconcerting that they do not arise in both sets of data. Perhaps the effects in the panel are suppressed because many of the respondents were first interviewed during the early summer when the campaign was still relatively dormant

TABLE 3 Advertising Effects on Assessments of Candidate Traits and Vote Choice

	Bush Favorability (1)	Gore Favorability (2)	Bush Likes (3)	Gore Likes (4)	Bush, Undecided, Gore Vote Choice (5)
Panel					
Pro-Bush Advertising Exposure	0.280	0.299	−0.007	0.004	0.178***
(GRPs/1000)	(0.280)	(0.374)	(0.017)	(0.013)	(0.043)
Pro-Gore Advertising Exposure	−0.445*	0.150	−0.006	0.007	−0.132***
(GRPs/1000)	(0.238)	(0.443)	(0.015)	(0.018)	(0.036)
Observations	4036	4071	3741	3794	2312
R-squared/log-likelihood	0.63	0.64	−7288.51	−7684.68	−930.24
Cross-Section					
Pro-Bush Advertising Exposure	0.760	−1.271***	0.076***	−0.051***	0.046***
(GRPs/1000)	(0.591)	(0.341)	(0.015)	(0.017)	(0.016)
Pro-Gore Advertising Exposure	−1.101**	1.899***	−0.090***	0.079***	−0.097***
(GRPs/1000)	(0.511)	(0.347)	(0.014)	(0.015)	(0.017)
Observations	7689	7682	7261	7274	7422
R-squared/log-likelihood	0.38	0.40	−16294.93	−16356.31	−4532.88

Functional form is OLS in columns (1) and (2), and ordered probit in (3), (4), and (5). Coefficients with robust standard errors in parentheses, clustered by state and media market combination. In panel analyses, advertising exposure is between first and second interview date. In cross-section, it is four weeks before interview date. Constant, control variables, state and week of (re)interview indicators, and, in panel only, days in panel, significant campaign event indicators, and prior responses, not reported to save space. See Appendix A2 for data coding and full model specification. *denotes $p < .10$, **denotes $p < .05$, ***denotes $p < .01$, two-tailed tests.

or were interviewed long after the election when their perceptions were flavored by its outcome. In fact, if we separately restrict our analysis to individuals initially interviewed on or after September 1, 2000, or reinterviewed before November 14, 2000, and despite the substantial reductions in sample sizes ($N < 1{,}700$), we find greater evidence in the panel that advertising alters favorability and likeability (more statistically significant coefficients). Unfortunately, we lack sufficient data to impose both restrictions simultaneously.

Lastly, we examine whether advertising affects reported vote intention and find strong evidence that it does. In the panel, a respondent who was initially undecided is predicted to subsequently support Gore 36.1% of the time and Bush 45.9% of the time. Per the column (5) results with the panel data, increasing Bush's advertising above the average by 1.57 GRPs/1000 is associated with a 9.1% increase in the predicted probability of supporting Bush (95% confidence interval .5 to 18.2%) and a 7.8% decrease in the probability of supporting Gore (95% confidence interval −17.3 to −.3%). The same increase in Gore's advertising is predicted to increase the probability of supporting Gore by 6.6% (95% confidence interval −.2 to 15.4%) and decrease the probability of supporting Bush by 6.6% (95% confidence interval −15.2 to 0.3%). These results are particularly compelling because 85% of respondents in the panel express the same vote choice in both rounds of the survey.

These effects are also large in the cross-sectional analysis. The average respondent will support Gore 44.2% of the time and Bush 38.8% of the time. Increasing Bush's advertising by 1.57 GRPs/1000 is predicted to increase the probability of supporting Bush by 2.7% (−.3 to 5.9%) and decrease the probability of supporting Gore by 2.8% (−5.9 to .4%), while increasing Gore's advertising by that amount is predicted to increase the probability of supporting Gore by 5.9% (2.4 to 9.5%) and decrease the probability of supporting Bush by 5.6% (−8.9 to −2.3%). We note that the similarity of these effects across the panel and cross-sectional data, as well as the symmetry of the Bush and Gore advertising effects, suggest that these findings are not merely the result of this particular model specification coupled with observed patterns of advertising. Overall, these are substantively important and statistically significant persuasion effects that are far larger than those detected, if at all, in most previous research.

Assessing the mechanism of persuasion. The preceding analysis provides evidence that partisan presidential advertising can directly persuade voters to support a particular candidate. Alternatively, however, it might be the case that the informational and partisan-reinforcement effects of advertising, particularly for salient campaign issues like Social Security reform in the 2000 campaign, are substantively larger than these direct persuasion effects.[13] Here we consider the relative importance of the mechanisms by which advertising might change citizens' preferences for the candidates. Specifically, we compare the predictive power of changes in candidate affect, better knowledge of candidate positions, and the adoption of partisan positions (reinforcement), against a model in which advertising is also allowed to influence directly changes in vote choice (for a discussion of the reasons advertising might remain directly persuasive, see below). For this analysis, we rely on the panel data because it provides a window into changes in respondents' affect, knowledge, and issue positions. All model estimates appear in Table 4.

In column (1) we consider whether changes in candidate affect influence vote choice. (Note that in the first three models we exclude measures of advertising before considering whether advertising directly alters candidate preference. The basic statistical model is the same as in Table 3, column [5], excluding any measure of advertising.) The two variables measuring this are "Change in Bush Favorability Margin," calculated as the shift in the Bush minus Gore feeling thermometer score from the first to second interview (−200 to 200, positive values indicate Bush became more favorable) and "Change in Bush Likeability Margin," calculated as the shift in Bush's relative advantage on the likeability scales between interviews (−5 to 5, positive values indicate Bush became more likeable). (These measures are correlated at $\rho = .32$, $p < .0001$. In separate models the size and margin of statistical significance of the coefficients increases.) If changes in affect explain changes in candidate preference, we would expect positive coefficients on both variables. The data confirm this prediction. A one standard deviation increase in Bush's favorability margin increases the probability of supporting Bush by 2.7% (95% confidence interval −.7 to 7.3%), and a one standard deviation increase in Bush's

[13] A note on terminology is appropriate here: We consider changes in candidate affect as distinct from factual information about policy positions. While emotional appeals may persuade by conveying factual information (e.g., by demonstrating that "candidate X is a mean person"), this sort of persuasion may also occur in the absence of learning factual information. Additionally, insofar as appeals to emotion are harder to verify than those rooted in policy, it is more difficult to assess whether they are accurate relative to an external metric.

TABLE 4 Identifying the Mechanism of Persuasion

	Bush, Undecided, Gore Vote Choice				
	(1)	(2)	(3)	(4)	(5)
Prior Vote for Gore	−1.165***	−1.275***	−1.255***	−1.207***	−1.155***
	(0.097)	(0.156)	(0.135)	(0.093)	(0.081)
Prior Vote for Bush	1.322***	1.477***	1.104***	1.319***	1.276***
	(0.106)	(0.156)	(0.152)	(0.104)	(0.097)
Party ID (Scale, +2 Strong R to −2 Strong D)	0.391***	0.339***	0.386***	0.392***	0.409***
	(0.051)	(0.044)	(0.052)	(0.051)	(0.050)
Change in Bush Favorability Margin	0.003			0.002	
	(0.002)			(0.002)	
Change in Bush Likeability Margin	0.274***			0.280***	
	(0.068)			(0.070)	
Became Accurate on Social Security Positions		−0.051			
		(0.142)			
Support "Personal" Social Security Accounts		0.113			
		(0.071)			
Became Accurate on Social Security Positions* Support "Personal" Social Security Accounts		−0.212			
		(0.141)			
Adopted Party's Position on Social Security			−0.030		
			(0.136)		
Adopted Party's Position on Social Security*Partisanship			−0.101		
			(0.098)		
Pro-Bush Advertising Exposure (GRPs/1000)				0.238***	0.244***
				(0.057)	(0.055)
Pro-Gore Advertising Exposure (GRPs/1000)				−0.100*	−0.118**
				(0.053)	(0.050)
Observations	1983	1031	1440	1983	1983
Log-likelihood	−683.98	−363.88	−522.42	−678.00	−696.87

Ordered probit coefficients with robust standard errors in parentheses, clustered by state and media market combination. Advertising exposure is between first and second interview date. Constant, control variables, state and week of (re)interview indicators, days in panel, and significant campaign event indicators not reported to save space. See Appendix A2 for data coding and full model specification. *denotes $p < .10$, **denotes $p < .05$, ***denotes $p < .01$, two-tailed tests.

likeability margin increases that probability by 7.8% (95% confidence interval 2.0 to 12.8%).

Next, in column (2) we examine the effect on reported vote choice of changes in respondents' knowledge of the candidates' positions on allowing personal Social Security accounts. The variable *Became Accurate on Social Security Positions* is coded 1 if respondents could accurately place both candidates in the second interview, but could not do so in the first interview. *Support "Personal" Social Security Accounts* is coded 1 if the respondent favored them at the first interview, −1 if they opposed them, and 0 if they were undecided. Finally, the interaction term *Became Accurate* Support Personal Accounts* is simply the product of the two component variables. If it is knowledge of where the candidates stood on the issue of Social Security reform that explains vote choice, then the effect of prior opinions should be larger for respondents who became aware of the candidates' positions on the issue over the course of the campaign (e.g., positive coefficients). In fact, however, we observe the opposite effect: Respondents who supported (opposed) reform are *less (more)* likely to support Bush after learning of his and Gore's positions than they were before. In short, underlying opinions about Social Security reform coupled with learning where the candidates stood on that issue do not explain changes in reported vote choice.

Column (3) considers one possible explanation for this somewhat puzzling finding. Here we test if candidate support is explained by whether respondents *Adopted Party's Position on Social Security* (1 if Yes for Republicans or No for Democrats, 0 otherwise) after not having previously done so, and whether adopting this position enhanced the effect of *Partisanship* (positive values are Republican, negative values are Democratic) by interacting *Adopted Party's Position on Social Security* with *Partisanship*. If reinforcement is at work, we would expect that the conditional effect of partisanship would increase among respondents who took their party's position on this important issue.[14] The results do not support this hypothesis, however. The effect of partisanship is actually mitigated for those respondents who changed their position on Social Security reform to mirror those of their party, although this effect is not statistically significant.

Finally, the model estimated in column (4) pits the most promising explanation for the indirect effect of advertising—changes in candidate affect—against the pure (unmediated) effect of advertising. Substantively, the mediated (through affect) effect of advertising remains. The magnitude of the direct effects of advertising is also reduced somewhat. Comparing the results from column (4) with those from column (5), in which advertising effects are estimated in isolation on the same sample as is analyzed in column (4), the coefficient for the Bush advertising variable is about 3% smaller in column (4) and the coefficient for Gore advertising is about 13% smaller. Cumulatively, this finding allows us to state that at least some of the persuasive effects of advertising originate in each candidate's partisan advertising messages and their effects on candidate affect. Once we account for these indirect effects of advertising, however, direct persuasive effects of advertising remain.

Unfortunately we cannot identify the exact mechanism by which advertising is still persuasive. This is likely due, in part, to our inability to measure the emotional effects of advertising (e.g., on feelings of friendship, love, fear, hate). Additionally, the NAES measures of candidate likeability are relatively coarse. Alternatively, there may be some other mechanism by which advertising alters respondents' support for the candidates. These limits aside, we have identified one means by which advertising is persuasive through changes in candidate affect.

[14]It could also be the case that changes in candidate choice lead to changes in positions on Social Security reform. Because we do not find evidence that changes in opinions on Social Security are correlated with candidate choice, we need not confront the question of whether issue positions explain candidate choice or vice versa.

Who is most persuadable? Heretofore we have documented the persuasive effects of advertising and considered the means by which that persuasion takes place. A related and important theoretical concern is whether the effects of advertising are moderated by individual characteristics that determine whether individuals are exposed to campaign advertising and, conditional on being exposed, whether they are receptive to those messages. Most prominent in this regard is Zaller's (1992) Receive-Accept-Sample (RAS) model, which predicts that political awareness increases exposure to potentially persuasive messages, but that at high levels this awareness also allows individual to reject messages that are incongruent with their (often well-formed) opinions. When applied to data on opinion change over time, this suggests that both the least and most aware are relatively unaffected by communication, the former because they do not receive it, the latter because they resist it, while individuals lying between these extremes are more susceptible to political communication. To test this argument, we rely again on our panel data and use two common proxies of awareness (Price and Zaller 1993), education (broken here into four categories) and prior (first interview) political interest (three categories), to test for the moderated effect of advertising. Specifically, we create separate indicator variables for each measure of awareness and allow the effect of Bush and Gore's advertising to differ across these categories. Model results appear in Table 5.

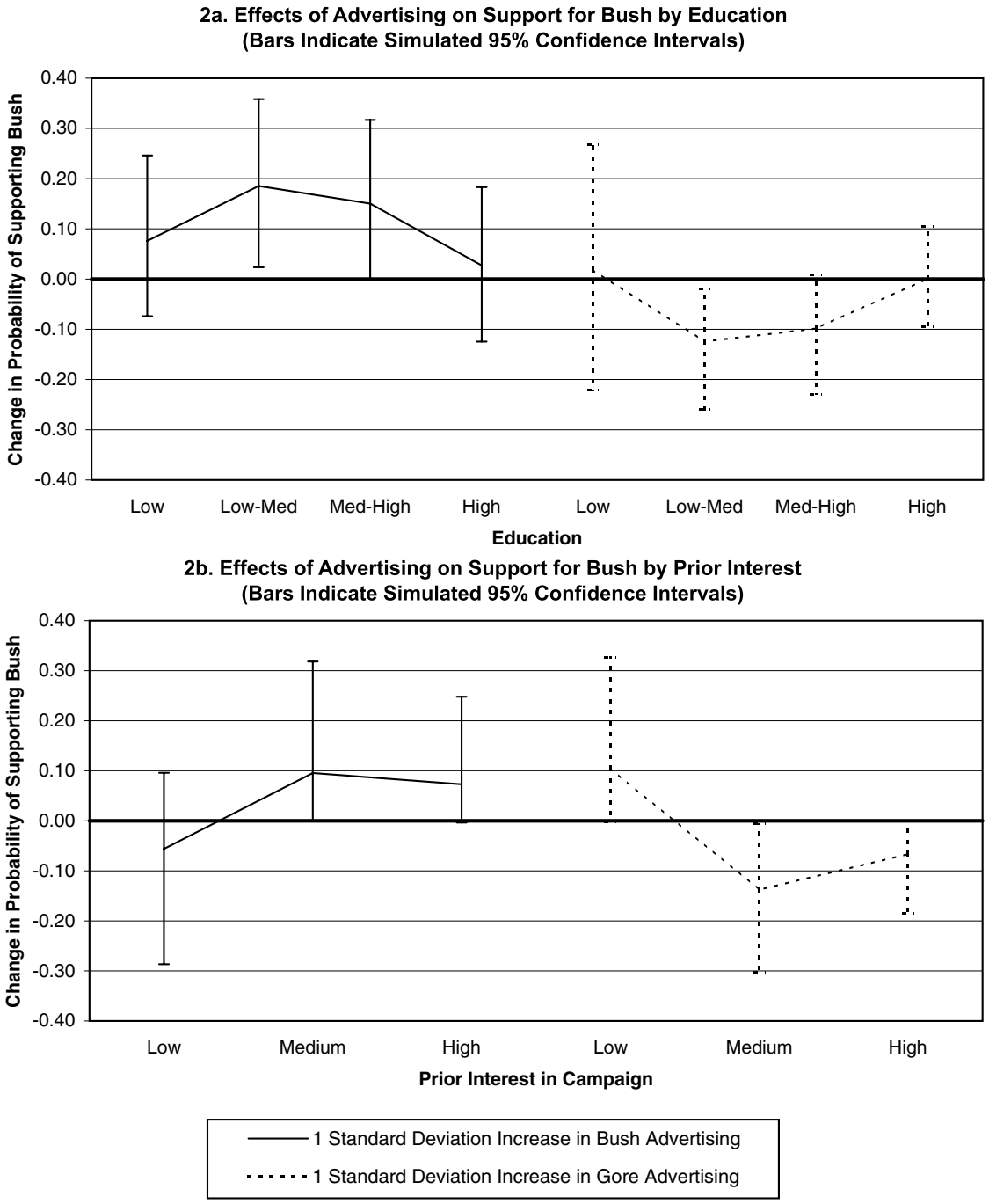
Coefficient estimates, in column (1) using education as a proxy for awareness and in column (2) employing political interest, are highly supportive of the RAS model: The greatest evidence of persuasion appears among moderately aware respondents. To ease interpretation, we present marginal effects for a one standard deviation increase in Bush and Gore advertising in Figure 2. In the top panel, we observe that increasing either Bush or Gore's advertising has almost no effect on the predicted likelihood of supporting Bush among both the least and most educated. Among those with moderate levels of education, however, a one standard deviation increase in Bush's advertising helps him (by about 17%) and a similar shift in Gore's advertising hurts Bush (by about 11%). These effects are all statistically significant at $p < .10$, and for the Low-Medium education category they are statistically significant at $p < .05$. Similarly, in the lower panel we observe the effect of advertising conditioned by prior political interest. Among those with little interest in the campaign, advertising effects are small and very imprecisely estimated. Those with moderate levels of interest are most affected (per the above assumptions, an increase in Bush's advertising increases the likelihood a respondent supports him by 10%, while an increase in Gore's

Table 5 How Education and Campaign Interest Moderate Advertising Effects

	Bush, Undecided, Gore Vote Choice	
	(1)	(2)
Pro-Bush Advertising Exposure (GRPs/1000)*Lowest Education Categories (1,2,3)	0.120	
	(0.152)	
Pro-Bush Advertising Exposure (GRPs/1000)*Low-Middle Education Categories (4,5,6)	0.312**	
	(0.150)	
Pro-Bush Advertising Exposure (GRPs/1000)*High-Middle Education Category (7)	0.247*	
	(0.129)	
Pro-Bush Advertising Exposure (GRPs/1000)*Highest Education Categories (8,9)	0.041	
	(0.113)	
Pro-Gore Advertising Exposure (GRPs/1000)*Lowest Education Categories (1,2,3)	0.036	
	(0.194)	
Pro-Gore Advertising Exposure (GRPs/1000)*Low-Middle Education Categories (4,5,6)	−0.268**	
	(0.126)	
Pro-Gore Advertising Exposure (GRPs/1000)*High-Middle Education Category (7)	−0.214**	
	(0.107)	
Pro-Gore Advertising Exposure (GRPs/1000)*Highest Education Categories (8,9)	0.002	
	(0.084)	
Pro-Bush Advertising Exposure (GRPs/1000)*Interest in Campaign = "Not Interested"		−0.084
		(0.089)
Pro-Bush Advertising Exposure (GRPs/1000)*Interest in Campaign = "Somewhat"		0.297**
		(0.146)
Pro-Bush Advertising Exposure (GRPs/1000)*Interest in Campaign = "Very"		0.208**
		(0.102)
Pro-Gore Advertising Exposure (GRPs/1000)*Interest in Campaign = "Not Interested"		0.360***
		(0.107)
Pro-Gore Advertising Exposure (GRPs/1000)*Interest in Campaign = "Somewhat"		−0.325***
		(0.126)
Pro-Gore Advertising Exposure (GRPs/1000)*Interest in Campaign = "Very"		−0.171**
		(0.068)
Education Category 2 (Some high school, 5%)	0.747**	
	(0.331)	
Education Category 3 (High school, 22%)	0.802***	
	(0.287)	
Education Category 4 (Post high school, vocational or technical, 3%)	0.382	
	(0.327)	
Education Category 5 (Post high school, some college, 17%)	0.631**	
	(0.290)	
Education Category 6 (Associate/2-year college degree, 10%)	0.627**	
	(0.263)	
Education Category 7 (College degree, 22%)	0.612**	
	(0.289)	
Education Category 8 (Some graduate or professional school, 5%)	0.491*	
	(0.290)	
Education Category 9 (Graduate or professional degree, 15%)	0.575**	
	(0.271)	
Interest in Campaign = "Somewhat"		−0.002
		(0.137)
Interest in Campaign = "Very"		0.196
		(0.176)
Observations	2312	1163
Log-likelihood	−922.35	−430.58

Ordered probit coefficients with robust standard errors in parentheses, clustered by state and media market combination. Advertising exposure is between first and second interview date. Constant, control variables, state and week of (re)interview indicators, days in panel, and significant campaign event indicators not reported to save space. See Appendix A2 for data coding and full model specification. *denotes $p < .10$, **denotes $p < .05$, ***denotes $p < .01$, two-tailed tests.

FIGURE 2 Awareness-Moderated Effects of Political Advertising



advertising decreases that probability by 14%), and these effects are statistically significant at $p < .05$. Finally, highly interested respondents seem to be somewhat persuadable, but these point estimates are smaller than for those with medium interest and the results are statistically significant only at $p < .10$. We note that the symmetry of the observed

results is not induced by model specification. The models allow Bush and Gore's advertising to have different effects within awareness categories; instead the symmetry arises because of effects observed in the data.

Overall, this research provides a new means for testing the RAS model of differential campaign effects. Unlike previous observational studies in which advertising exposure (or reported exposure) and resistance might be correlated with exposure to other campaign field activity or national events coverage, our analysis allows us to isolate the effect of changes in partisan advertising. We find strong evidence confirming the basic prediction of the RAS model. Advertising effects are not uniform throughout the population, but are instead higher among those individuals sufficiently aware to be exposed to campaign communication but not so aware that they either hold strong prior beliefs or can resist messages contrary to those beliefs.

Discussion

Campaign advertisements appear to 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 starkest portrayal, 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 after controlling for other campaign events and underlying differences across voters, these microlevel 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

We consider a number of extensions to our basic model to tests its robustness. First, our earlier analysis assumes that self-reported measures of individual characteristics (including ideology, partisanship, etc.) are uncontaminated by earlier advertising and are uncorrelated with the observed advertising stream. To confirm this assumption is innocuous, we reanalyzed our data excluding all individual characteristics while including, in addition to the advertising measures, only reliable measures of exogenous political conditions (state of residence, week of interview, significant campaign events, and whether a respondent lived in a competitive House district). In this context, which fully exploits the natural experiment, we continue to find statistically significant persuasion effects. We also test for the sensitivity of our results to the exclusion of either the week of interview indicators or the state of residence indicators. With either exclusion we continue to find that each candidate's advertising has significant effects.

Alternatively, 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 would diminish its effect. Similarly, it may be that advertising is persuasive, but that once one is exposed to a particular message, its subsequent marginal effects are diminished. To resolve this uncertainty, we conducted two additional tests. First, we repeated our earlier analysis of the effect of partisan advertising on candidate choice, but this time included both each candidate's advertising and a measure of that advertising volume squared. If advertising diminishes in effectiveness with repetition, we would expect that the quadratic transformation of advertising volume would have the opposite sign of each candidate's simple (untransformed) measure of advertising advantage. Relying on the cross-sectional data, this argument is confirmed. Bush is helped by his advertising and hurt by Gore's, but each effect is mitigated with additional advertising volume. More precisely, the net effect of Bush's advertising is 0 by the time volume reaches about 6 GRPs/1000 (the maximum Bush advantage observed in the cross-section is only 5.24 GRPs/1000). The effect of Gore's advertising also diminishes, but is still nonzero for GRPs/1000$>$200, far outside the observed advertising range.

Our second specification tested whether advertising effects were diminishing in the overall volume of presidential advertising. Here, we again repeated our earlier analysis, but this time interacted each candidate's advertising with the overall volume of presidential advertising.

These results also suggest advertising effects diminish with volume. The effect of a 1 GRP/1000 increase in Bush advertising is reduced to zero by the time overall advertising volume reaches about 6.5 GRPs/1000 (the maximum volume observed in the cross-section is 11.4 GRPs/1000), while for Gore that limit is not reached until overall advertising volume is about 12 GRPs/1000. In general, these results suggest that a candidate does best when her messages are not competing with those of the other candidate.

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 differ substantially from how voters behave when deciding how (and if) to vote on Election Day. Thus, it may be the case that the persuasion effects we observe in our analysis of the NAES would not manifest in actual voting data. If they do, however, this is compelling evidence of the external validity of these findings.

We therefore examine election data to test whether advertising changes actual voter behavior. 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 nonbattleground states served by the nation's 75 largest media markets (Mean .074, S.D. .045, Min. −.093, Max. .250). We again measure the partisan balance of the advertising stream using the CMAG data, and calculate Bush and Gore's advertising (GRPs/1000) both for the period September 1, 2000, to Election Day and for the period between October 1, 2000, and Election Day. To account for other differences across states, including whether Perot was on the ballot in 1996, we include state fixed effects in our analysis. (This forces us to discard nonbattleground states served by only a single media market because, unlike in our analysis of survey data, we have only single time-invariant observations of county election outcomes and advertising.) Results of the models are shown in Table 6.

The coefficient estimates shown in columns (1) and (2) validate our survey-based analysis. Both more Bush advertisements and fewer Gore advertisements are associated with a statistically significant increase in Bush's share of the two-party vote at the county level. We note that the larger magnitude of the coefficients in column (2) (about twice the size of those in column [1]) suggests that advertising broadcast closer to the election has a larger effect than campaign messages received earlier in the contest. Overall, by relying on the natural experiment of accidental treatment of individuals in nonbattleground states, we continue to find strong evidence of the persuasive effects of advertising even in aggregate vote returns.

Conclusion

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 nonbattleground states to presidential

TABLE 6 Robustness of Advertising Effects, Advertising and County Election Results in Nonbattleground States

	1996 to 2000 Shift in Republican Proportion of Two-Party Vote in County	
	(1)	(2)
Pro-Bush Advertising Exposure (GRPs/1000), September 1–November 7, 2000	0.0040***	
	(0.0012)	
Pro-Gore Advertising Exposure (GRPs/1000), September 1–November 7, 2000	−0.0025**	
	(0.0011)	
Pro-Bush Advertising Exposure (GRPs/1000), October 1–November 7, 2000		0.0075***
		(0.0024)
Pro-Gore Advertising Exposure (GRPs/1000), October 1–November 7, 2000		−0.0055**
		(0.0022)
Observations	824	824
R-squared	0.53	0.53

OLS coefficients with robust standard errors in parentheses, clustered by state and media market combination. Includes all states with multiple media markets. Constant and state indicators not reported to save space. See Appendix A2 for data coding and full model specification. **denotes $p < .05$, ***denotes $p < .01$, two-tailed tests.

advertising during the 2000 campaign, we are able to generate unbiased estimates of the effects of campaign advertising on citizens. This design allows us to isolate the effects of advertising from other campaign activities, events surrounding the campaign, differences in voter characteristics, and the misreporting of advertising exposure.

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. Instead, like Johnston, Hagen, and Jamieson (2004), we find evidence that advertising is persuasive. But comparing our approach to theirs, most directly by repeating our analysis of persuasion within battleground states in county-level vote returns (Table 6), illustrates the danger of assuming that nonadvertising 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 22–68% *larger* than those shown in Table 6, likely indicative of the fact that the campaigns are strategic about how they target both advertising and other campaign resources within battleground states. A research design that does not account for this targeting within battleground states cannot overcome this simultaneity bias, however. Only an appropriate natural experiment (or field or laboratory experiments with direct random assignment) can disentangle these factors, as we do by looking at variation within uncontested 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 educated voters and drew them to the polls, the sanguine tone of earlier scholarship might be appropriate. In that 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, we find a strong persuasive effect independent of underlying policy opinions on the key issues in the 2000 campaign (e.g., Social Security reform). It therefore seems clear that more advertising alone will not produce a "better" democratic result. By manipulating voters' expressed candidate preferences, the partisan balance of the advertising stream has a direct, important, and underdocumented effect on election outcomes. Coupled with the unequal distribution of wealth in society and relatively lax rules for campaign spending, our findings may validate the fears of many campaign finance reform advocates that television advertising has the potential to distort the democratic process.

We close by considering the generalizability of our findings to contexts beyond the 2000 presidential campaign. In some respects, this race between two viable candidates, a well-known and relatively liberal vice-president (Gore) and a big-state governor with a known conservative reputation (Bush), was remarkable given the candidates' historical records on the issues, for its relative lack of policy focus and downplaying of stark ideological contrasts. In this respect, the race resembled to a greater degree many House or Senate races than recent presidential contests where ideological (e.g., Bush I versus Dukakis) or numerous policy issues were dominant (e.g., Bush I versus Clinton). This alone suggests that the mechanism of persuasion we identify, changes in candidate affect, is likely important in many more electoral contests than one might infer from presidential races alone. Additionally, while one might imagine that in either of those earlier presidential races partisan reinforcement or issue-based voting might have been more apparent and more clearly related to campaign advertising, both races were also characterized by advertising that sought to muddle the issues and define the other candidate as dishonest, uncaring, out of touch, or similarly unlikable. Our research provides evidence that these sources of changes in voter preferences for the candidates, which originate in the nonpolicy focused effects of campaign advertising, cannot be ignored.

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