An Experimental Investigation of Advertising Persuasiveness:
Is Impact in the Eye of the Beholder?[1]

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Comments Welcome

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Abstract:

Using an experimental design of actual ads shown to a representative sample of viewers rivaling the size of many "large-n" observational studies during the course of the 2000 U.S. Presidential election, we examine the extent to which candidate produced messages affect individuals' decisions in the ballot box and whether these messages are more persuasive for some individuals than others. We find clear evidence that an ad's persuasiveness depends critically on the characteristics of its viewers. We show: 1] voters with an initial preference for either Bush or Gore but who are politically unengaged move together in response to ads, 2] the response of highly engaged voters with an initial candidate preference depends on their initial preference – there is some evidence that highly engaged voters polarize when exposed to political ads, and 3] undecided voters are the least responsive to ads and exposure results in increased indecision.

Elections are arguably the most essential means by which citizens interact with elected officials and they are the primary mechanism by which politicians are made accountable to the electorate. Considering the stakes involved, it is unsurprising that a tremendous amount of resources – both public and private – are spent during elections. Given that televised campaign advertising is a leading recipient of these resources in contemporary campaigns, understanding how candidates' televised messages affect their support in the electorate is critical for any assessment for the ability of citizens to exercise informed oversight in the electoral process.

While the effects of political ads on voter learning and turnout have been the subject of considerable debate and research,[4] we know comparatively little about the effect of political advertising on actual vote choice.[5] We lack a definitive understanding of the extent to which candidate produced messages affect individuals' decisions in the ballot box and whether candidate messages are more influential on some individuals' decisions than others.

The state of the literature on advertising effects reflects a broader challenge facing scholars of political media effects. Despite some important recent advances, Bartels' decade old assertion that "the state of research on media effects is one of the most notable embarrassments of modern social science" continues to ring true in part due to the considerable complications confronting observational and experimental research (Bartels 1993). The large sample sizes of observational studies provide the ability to examine media impact on different subsets of the electorate, but such studies face difficulties in measuring exposure to political advertising. In contrast, experimental studies provide the ability to manipulate exposure, but small sample sizes

[4] On voter learning see, for example: Patterson & McClure 1976; Faber & Storey 1984; Zhao & Chaffee 1995; Brians & Wattenberg 1996; Pfau et al 2002; Freedman et al 2004; Ridout et al 2004. On turnout see, for example: Garramone et al 1990; Ansolabehere & Iyengar 1995; Finkel and Geer 1998; Goldstein and Freedman (1999 & 2002a); Ansolabehere et al 1999; Kahn & Kenney 1999; Wattenberg and Brians 1999; Clinton and Lapinski 2004

[5] In an extensive review of the political science literature on campaign ads, Goldstein and Ridout (2004) discuss the familiar debate surrounding the existence of campaign effects but identify only a handful of studies which explore the actual effect of political advertising on campaign outcomes.

typically force scholars to assume that ads have a constant impact across individuals and focus instead on how ad characteristics affect ads' persuasiveness.[6]

By uniting the inferential power of experimental studies with important theoretical advancements probed in observational research, we offer a significant contribution to efforts focused on understanding the persuasiveness of political advertising. Our research offers an experimental test of the predominant political science model of attitude change because the experimental design and sample size we employ provides the ability to control (and therefore accurately measure) advertising exposure and assess the effect of this exposure on different subsets of potential voters. Focusing on how voters with varying levels of cognitive engagement in politics and with differing initial candidate preferences respond to ads reveals important differences. For the ads we examine, we show: 1] voters with an initial preference for either Bush or Gore but who are relatively unengaged in politics move together in response to ads, 2] the response of highly engaged voters with an initial candidate preference depends on their initial preference – there is some evidence that highly engaged voters polarize when exposed to political ads, and 3] undecided voters are the least responsive to ads (especially ads by competing candidates); exposure actually results in increased indecision.

1. The moderating effect of viewer characteristics:

Our examination departs from most previous experimental research by paying particular attention to the possibility that the effectiveness of political advertising varies among different types of individuals. We explore whether viewer characteristics moderate the effectiveness of political ads and discuss the implications of our results for the model of opinion change which

[6] For example, investigations have examined how tone (e.g. Kahn & Geer 1994), the difficulty of the argument (e.g., Carmines and Stimson 1980; Cobb and Kuklinski 1997), the use of verifiable facts (e.g., Geer 2006, "racialized" messages and images (e.g., Mendelberg 2001; Valentino et. al. 2002), and emotional appeals (Brader 2005) affect the persuasiveness of ads. In addition, see: Rosenberg et al 1986; Ansolabehere & Iyengar 1995; Pinkleton 1998; Noggle and Kaid 2000; Pfau et al 2002.

animates most observational research. Observational studies have convincingly demonstrated that cognitive engagement with politics and political predispositions interact to moderate the effectiveness of persuasive political communications[7]. We rely on the superior inferential power of an experimental research design (Green and Gerber 2003) to offer a more focused investigation of the moderating effect of these variables.

Most experimental research on the effectiveness of political ads assumes that advertising is equally persuasive conditional on a set of covariates.[8] As Achen (1992) notes "the statistical models [of behavior] assumed what everyone knew to be false, namely that each causal effect operated independently of the values of the other variable" (pg 196). In other words, the impact of advertising on the probability of respondent *i* voting for a given candidate at time *t* is typically assessed using a regression equation of the type:

$$Vote_{t,i} = f(\beta\ exposure_i + \gamma' \mathbf{x}_i) + e_i \qquad (1)$$

where $\mathbf{x}_i$ is vector of relevant demographics and the effect of exposure to advertising β is assumed to be constant across all individuals.[9] While few might accept that political communications have an identical effect on all individuals, by estimating regression specifications of the type given in equation (1), a considerable amount of observational research implicitly assumes precisely this when estimating the effectiveness of advertising (see, for example: Goldstein & Feldman 2002, Lau & Pomper 2002; Wlezien & Erikson 2001; West 1994).

More recently, however, survey based studies have taken seriously the idea that the size and direction of media and campaign effects depends on the characteristics of the recipient of

[7] See, for example, Iyengar and Kinder 1987; Miller and Krosnick 2000; Johnston et al 1996; Bartels 1996b.
[8] For an overview of this research, see Table B1 in Appendix B.
[9] Clearly if equation 1 is estimated using a non-linear estimator -- probit and logit are most common in the literature – the marginal effect of advertising is not held constant across the entire sample. Nonetheless, the moderating effects we investigate are not sufficiently addressed.

these communications and some viewers may be more persuaded by a given ad than others (e.g. Zaller 1992, Hillygus and Jackman 2003; Freedman, Franz and Goldstein 2004; Holbrook and McClurg 2005; Arceneaux 2005; Stevens 2005). In this paper we focus on two characteristics which have been shown to moderate media effects in observational studies: prior political beliefs and cognitive engagement with politics.[10] If so, the correct specification is not equation (1), but rather a specification of the form:

$$\text{Vote}_{t,i} = f(\beta\ \text{exposure}_i * \text{vote}_{t-1,i} * \text{political engagement}_i + \gamma' \mathbf{x}_i) + e_i \qquad (2)$$

Equation (2) specifies that the effect of an ad depends on the level of initial support for the candidate running the ad and the viewer's level of political engagement.

Cognitive engagement with politics and prior political beliefs are the characteristics which figure most importantly in the predominant model of attitude change employed by political scientists (Converse 1962, 1975; MacKuen 1984; Zaller 1992).[11] In this framework, the process of attitude change boils down to *reception* and *acceptance* of a persuasive message. Cognitive political engagement, which has elsewhere been referred to as political awareness or sophistication, and prior political beliefs interact to determine the relative susceptibility to persuasion and scholars have suggested a non-monotonic relationship between cognitive engagement with politics and attitude change (Zaller 1992).

In terms of the reception of persuasive appeals, more engaged individuals are more likely to expose themselves to candidate appeals as a consequence of the choices they make (see, for

[10] Our focus on prior vote intention and interest in politics is not meant to suggest that they are the only possible moderators of advertising effectiveness. Instead, we choose to focus our attention on two highly plausible potential moderators. For examples of other moderators see, for example: Valentino et al (2002) for an exploration of racial predispositions as moderators of ads containing racial cues and King and McConnell (2003) who argue that gender moderates the impact of negative ads.

[11] A similar model informs much psychological research (McGuire 1968, Sears & Whitney 1973).

example, Prior's (2005) work on the preference for news among individuals). In contrast, politically unengaged members may be more unlikely to receive exposure to political ads.

However, whereas highly engaged individuals are more likely to receive ads than unengaged individuals, they are arguably less likely to accept the ads' messages. Individuals with poorly developed political belief systems are less likely to experience inconsistencies between political messages and prior beliefs. In contrast, politically sophisticated individuals with well-defined beliefs may be able to resist persuasive messages contrary to their existing beliefs using the information they possess.

Considered jointly, the account suggests that politically unengaged individuals are rarely influenced because they are unlikely to receive a political message. Although highly engaged individuals are likely to receive political messages, they are also likely to only accept messages consistent with prior political beliefs. The most persuadable are presumably "moderately" engaged individuals – they are more likely than the unengaged to encounter messages and less likely to resist the persuasive messages than the highly engaged.

Our research provides a focused consideration of the acceptance step in an experimental setting. Because we manipulate exposure levels, we control for the impact of cognitive political engagement on reception. Even if observational studies were able to accurately measure exposure, careful consideration of the relationship between cognitive engagement and message acceptance is hindered by the predicted correlation between engagement and exposure noted above. Since our research design ensures no correlation between exposure and political engagement, we can more carefully consider how engagement and prior beliefs interact to the affect the acceptance of messages contained in political ads.

There are several ways in which prior political beliefs may interact with cognitive political engagement to produce varying levels of message acceptance. First, if citizens use existing candidate preference to assess the quality of information presented from different sources, initial candidate preferences may determine receptiveness to information.[12] Viewers may use their evaluations of a message source when deciding whether to accept or reject the content of a message. 'Partisan resistance' to persuasive appeals may be related to cognitive engagement because the more politically informed viewers are better able to identify (and therefore resist) messages inconsistent with prior beliefs (Zaller 1992).

Second, viewers may be equally receptive to information from the two candidates, but prior vote intention may indicate the viewer's balance of information favoring one candidate. For example, if voters are Bayesians, the probability that an ad changes a voter's opinion depends on the voter's level of initial support; even if an ad is equally informative to all voters. If a viewer's political information overwhelmingly supports one candidate then a small amount of new information is unlikely to change the viewer's candidate preference. Furthermore, the more information a viewer possesses, the less likely it is that an ad will provide the viewer with information relevant for changing the viewer's preferences. Political engagement likely moderates this 'inertial resistance' because the more engaged also tend to hold more consistent political beliefs (resulting in stable political opinions) (Zaller 1992).

[12] Although several studies test for differential ad effects conditional on party identification (Ansolabehere & Iyengar 1995; Chang 2003; Pfau et al 2002), we build on these studies by employing a more theoretically relevant moderator: prior vote intention. Consistent with Popkin's (1991) argument that party affiliation shapes voters' initial intentions absent candidate-specific information, to the extent that viewers with the same party identification support different candidates, accounting for a voter's initial intention accounts for heterogeneity which an analysis focused on partisanship misses. Conditioning on voters' intention rather than party affiliation accounts for the fact that an ongoing campaign reveals candidate specific information that can be used to disentangle candidate and party evaluations. Party identification assumes, for example, that the impact of a Gore ad is the same for a Democrat who supports Gore and a Democrat who supports Bush.

These two sorts of resistance to received messages offer clear expectations about the relative effectiveness of advertising on different types of individuals. The possibility of partisan and inertial resistances implies that ads should have little impact among politically engaged individuals with a preference for the sponsoring candidate's opponent. While engaged viewers should be willing to accept messages in ads sponsored by their preferred candidate, we may see only limited persuasive effects because these individuals likely possess information which already significantly favors the initially preferred candidate.

Finally, the theory suggests that ads are especially effective among individuals who are undecided prior to exposure. A lack of preference may reflect the fact that undecided individuals hold conflicting (and balanced) information about the candidates. If so, new information may tip the balance in favor of one candidate. The absence of a pre-exposure preference may also prevent undecided individuals from engaging in biased processing and therefore accept all persuasive appeals (resulting in substantial advertising effects). As Zaller (1992) notes, individuals not sufficiently informed to reject messages based on partisan cues "tend to uncritically accept whatever ideas they encounter" (p.45).[13]

2. Studying the Persuasive Impact of Advertising

Scholars have devoted considerable resources toward the task of disproving early claims about the 'myth' of media effects and identifying an accurate and empirically supported theory of these effects. Efforts to identify the impact of campaigns as a whole and their various constituent parts are a key subject within the broader media effects literature and the study of political advertising effects is itself a central element of this campaign effects literature. Perhaps

[13] However, if undecided voters are less engaged than less engaged voters with an initial candidate preference there might be reason to expect much smaller advertising effects among undecided voters. Individuals with such underdeveloped political belief systems might also lack the ability and motivation to use the information contained in ads to update their beliefs. It is unclear which account best describes the nature of initially undecided voters.

the most unassailable conclusion reached over the past fifty plus years of research on these topics is that no single study or method can provide a complete picture of media effects. Instead, our understanding of media effects improves incrementally through innovations in research designs, measurement strategies and theory development.

If media effects are indeed "more fugitive than minimal" (Bartels 1993), then heterogeneous effects is potentially one of the most consequential explanations for the fugitive nature of media effects.

Observational studies are promising in this regard due to the large sample sizes they employ. Large samples provide the ability to examine the behavior of various respondent groupings to investigate the existence of heterogeneous responses. The downside of this approach is the difficulty of accurately measuring exposure to ads. Recent efforts have built on past work, which relied exclusively on self-reported exposure (Faber, Tims, and Schmitt 1993; West 1994), by constructing a measure using respondent viewing habits and candidate advertising behavior (Goldstein and Freedman 2000; Johnston, Hagen, and Jamieson 2004). While this innovation is important, the improvement is only relative; survey based measures are inherently noisy measures of an individual's exposure to ads because actual exposure is unknown. Noisy measures produce inferential difficulties for regression analysis even if they are unbiased (Achen 1983).

Survey methods are also limited in their ability to investigate how characteristics correlated with exposure and recall, such as cognitive engagement with politics, affects the persuasiveness of ads. This is a more important concern. As outlined in the prior section, there is reason to believe that political engagement increases exposure but decreases receptiveness to persuasive appeals and we know from empirical work that political interest is positively

correlated with recalled and actual exposure (Johnston, Hagen, and Jamieson 2004) and may contaminate recall measures of exposure (e.g., Ansolabehere, Iyengar, and Simon 1999, Vavreck 2005). If so, characterizing the relationship between political engagement and the persuasiveness of ads using observational data becomes extremely difficult since survey based studies cannot easily account for potentially offsetting mechanisms correlated with respondent characteristics (i.e., the fact that political engagement increases exposure but decreases receptivity).

Controlled experiments allow researchers to resolve the endogeneity and measurement problems affecting survey studies by directly controlling media exposure (see, for example, Ansolabehere and Iyengar 1995; Green and Gerber 2000a; 2000b; 2003; Iyengar and Kinder 1987). Experimental designs can accurately measure exposure, but they typically face difficulties when assessing the potential moderating influence of viewer characteristics because of limited sample sizes (Chang 2003; Meirick 2002; Pfau et al. 2002). With a few important exceptions (e.g., Ansolabehere and Iyengar 1995), the bulk of experimental studies ignore the possibility of heterogeneous treatment effects.[14]

Existing experimental work yields uncertain conclusions about the impact of political interest. Ansolabehere and Iyengar (1995) argue ads have little effect among independents and opposition party identifiers irrespective of political interest and ads impact less interested people who identify with the party of the ad's sponsor. The lab experiment of Valentino et. al. (2004)

[14] Our study builds upon Ansolabehere and Iyengar's work in several important ways. First, our focus is slightly different – we examine the impact of presidential ads using actual candidate produced ads. Ansolabehere and Iyengar use constructed ads for the 1990 Senate election between Feinstein and Seymore for the investigation most proximate to the one we conduct. Second, our sample is larger and arguably more representative – we expose over 2,400 randomly selected, RDD recruited respondents to ads. In contrast, Ansolabehere and Iyengar conduct their investigation on a sample of 1716 recruited residents of California. Third, we control for initial predisposition using respondents' initial support for Gore and Bush rather than party affiliation. This distinction is important because, as Hillygus (2005) and Hillygus and Jackman (2003) have demonstrated, initial support is a cleaner measure of support and a better baseline.

reveals an impact among the least aware, but almost none among the most aware. Finally, the observational study of Johnston et al (2004) suggests that "news interest neutralizes impact from mass media," with less interested people being more susceptible to advertising.

Experimental work also faces external validity concerns – to what extent do the circumstances and sample of the experiment approximate the realities of a political campaign? Although several threats exist, one threat concerns the use of non-random samples.[15] Of thirteen recent experimental studies on advertising effectiveness (see Table B1 for details), nine rely entirely on student samples and one uses both students and adults recruited from the surrounding area. Convenience samples do not yield the inferential benefits of random sampling because it is unknown how the sample differs from the population.[16]

Given the costs and benefits of observational and experimental methods, we combine the benefits that a large sample size provides to observational studies with the ability to accurately and exogenously control exposure that experiments provide using a "large-n" experiment conducted on a representative sample of the American population.

3. Experimental Design

We conduct our experiment using the panel of Knowledge Networks (hereafter KN). Our sample respondents closely approximate a national Random Digit Dialing (RDD) sample because KN panelists are randomly selected using list-assisted RDD sampling techniques from a quarterly updated sample frame of the United States telephone population in the Microsoft Web TV network (87% of the population). When the experiment was conducted, all KN Panelists were provided a Microsoft WebTV and an Internet connection in exchange for participation.

[15] This is clearly not the only threat to external validity. We discuss other threats in sections 3 and 5.
[16] For example, the study of Valentino, et. al. (2004) uses a highly educated (> 50 % college educated), highly Democratic sample (> 64 %), and finds that exposure to a single ad from either Gore or Bush makes viewers more likely to vote for Gore and the magnitude of the average effect ranges from -.53 to -.98.

External validity is always a concern for experiments, but our study improves upon the external validity of existing studies in two ways. First, our subjects are close to a random sample of the United States adult population.[17] Second, respondents were exposed to experimental treatments via a WebTV device that instantly displayed high-quality videos on their television. Respondents were exposed to ads on their own television, in their own home at a time of their choosing – an experimental environment that closely replicates actual viewing experience.

The experience departs from actual experience in that the ads were embedded in the respondent's weekly survey. Respondents were sent an email informing them that their next survey was ready to be taken and a hyperlink in the email led to our survey. Randomly selected participants in a treatment group were shown a full-screen ad and asked several follow-up questions. Participants never knew that they were participating in an experiment, but they were aware that they were taking a survey with political content.

We investigate the persuasiveness of ads using three ads aired during the 2000 presidential election. Although using real ads increases the external validity of the study – we investigate the impact of ads actually employed by the competing candidates – the cost of using real ads is that we cannot explain the reasons that some ads are more persuasive than others. The ads differ in several respects and some ads may be more effective even conditional on audience composition (e.g., better visuals, better sound, more catching slogan).[18] Differences in ad

[17] Strictly speaking, although the KN panel is roughly comparable to the U.S. population, nothing ensures that the differences that do exist are inconsequential for interpolating the causal impact (see, for example, Horiuchi, ` and Tanigucki (2005)). We have no way of accounting for possible biases between the KN panel and the U.S. population (e.g., we do not know how people not in the Web TV network would react). Consequently, the most accurate statement of our investigation is that the causal impact we identify is restricted to the set of individuals willing and able to participate in the KN panel. Nonetheless, this is a much larger population than existing subject pools of ad experiments.

[18] Although scholars have identified a lengthy list of characteristics that likely affect whether an ad is effective for at least a subset of the electorate (e.g., tone, the use of images, emotion, and referenced facts), our ability to adjudicate between these possibilities is limited because we lack the required degrees of freedom (the ads differ in many respects and we cannot isolate the precise causal mechanism). Although we can certainly rationalize the

"quality" may yield differing results across treatments (i.e., not all Gore ads may be equally persuasive) and our results may reflect the possibility that we chose "bad" (i.e., unpersuasive) ads. Appendix A provides the content of the ads we use.

Table 1 summarizes our experimental design. The response rate of the experimental surveys was 77 % -- 7,743 completions out of 10,050 assignments.

[INSERT TABLE 1 ABOUT HERE]

It was known in advance that some respondents would be unable to view the ads because their WebTV unit lacked the hard drive required for providing on-demand video content. Which respondents had video capable systems was unknown because both devices were distributed simultaneously. Since the distribution of video-eligible WebTV to respondents was random, we use the subset of respondents who were unable to view the ads as the control.

For example, 2,850 respondents were assigned a survey containing just the Gore negative "Siding" ad. The 634 respondents able to view the ad are used to calculate the effect relative to those respondents who completed the survey but who could not view the video.[19] Although not a control group in the strict sense, because the respondents unable to receive the ad were "selected" by the exogenous allocation of WebTV devices by KN, the causes for concern are limited. Table 2 presents the demographic characteristics of the treatment groups.

[INSERT TABLE 2 ABOUT HERE]

The random assignment to the four treatment groups produced reassuringly similar groups and the similarity evident between the treatment groups and the "control" group suggests

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findings ex post, we instead focus exclusively on the more general question of whether the examined ads are: 1) effective at influencing the support of viewers and 2) whether viewer characteristics affect the impact of ads. Bartels (1996) raises the same point in reviewing Ansolabehere and Iyengar's experimental work.

[19] Survey non-response does not affect the estimation of the causal effect because the receipt of the treatment cannot affect non-response due to the fact that the treatment was administered during the survey (i.e., there was no opportunity to terminate participation as a consequence of being exposed to the treatment).

that the allocation of video capable WebTV units is independent of respondent characteristics; sizable demographic differences are absent despite the non-standard control group.[20]

There are three critical variables of interest for our analysis: the respondent's initial support for the presidential candidates, the respondent's level of cognitive political engagement, and the respondent's support for the presidential candidate post-treatment. To measure initial candidate support pre-treatment we use the respondent's answer to question administered almost immediately after the respondent was recruited to the KN panel and prior to the most intense campaigning.[21] Our measure of cognitive engagement with politics is a dichotomous measure (*% with high political engagement)* based on subjects' responses to questions about general political interest, overall attention paid to the campaign, attention paid to the campaign in the last week, and frequency of discussion about the campaign in the past week. Those in the top third on our interest index are identified as highly engaged. We dichotomize the measure because our sample size prevents a more nuanced investigation without imposing considerable assumptions (e.g., a linear effect) and prior research documents a non-monotonic relationship between political engagement and media effects (e.g. Zaller, 1992). To measure preferences post-treatment we use the responses to a question that was asked shortly after the ads were shown.[22]

As noted in section 1, we expect a non-monotonic relationship between prior beliefs, political engagement and attitude change because of the offsetting effects on exposure and acceptance. By using an experimental design we avoid this complication and focus directly on the predictions regarding viewer acceptance of persuasive appeals..

[20] The one obvious outlier is the fact that 56 % of treatment group 3 is female.
[21] The question asked who the respondents would support if the election were held today and provided the opportunity to choose Bush, Gore, Buchanan or indicate that they did not yet know.
[22] The question asked was "If the presidential election were held today, for whom would you vote? George W. Bush and Dick Cheney; Al Gore and Joe Lieberman; Other; Don't know/Not Sure." The first two responses were randomized across respondents.

According to the account of section 1, ads should be most persuasive among undecided viewers, followed by viewers with an initial preference, but who are politically unengaged. Relative to highly engaged viewers, both sets of viewers are more likely to accept a message for two reasons. First, unengaged and undecided viewers may be less likely (or capable) of engaging in biased processing aimed at preventing acceptance of messages inconsistent with political predispositions relative to engaged viewers Second, the pre-exposure opinions of these individuals may be more open to revision because their existing information does not overwhelmingly favor one candidate.

Our experiment also allows us to distinguish between these two explanations of ad effectiveness among less engaged individuals with a pre-exposure candidate preference. Because our experiment occurred in the final weeks of a closely fought Presidential election campaign and our advertisements are clearly sponsored by the candidates' campaigns, it is unlikely that any individuals would fail to realize which candidate a given ad supported. As we expect viewers with an initial candidate preference have sufficient information to enable biased processing based on the sponsor of the ad, if we find that a candidate's ad influences less-engaged supporters of the sponsor's opponent, it would be reasonable to conclude that less-engaged individuals do not engage in biased processing even when they are able to.

4. Results

As previously noted, our primary purpose is to determine whether prior vote intention and political engagement moderate the persuasiveness of political ads as some accounts suggest. However, given the uncertainty regarding the direction of the expected impacts – for example, does an initial predisposition for a candidate make a viewer more receptive to persuasion by that

candidate's ads or less so (perhaps because they have already been "persuaded" to agree with the candidate)? -- our tests are primarily against the null hypothesis that predispositions and political engagement do not moderate ads' persuasiveness.

An inevitable complication with investigations into the effects of advertising, and a complication we cannot fully avoid even with the relative large samples we employ is that the effects we are looking for are likely small. Like all investigations into campaign effectiveness, we face a "Catch-22" situation – unless the differences are large (or very precise), simple statistical methods we will be unable to detect statistically significant differences. However, if the differences are large we are likely to discount them as unrealistic (Zaller 2002) and due to the experimental condition. Because our treatment consists of exposure to one or two ads in the context of a robust and ongoing presidential election campaign, large differences are unlikely.

[INSERT TABLE 3 ABOUT HERE]

Given the random assignment to treatment groups and the similarity in sample characteristics reported in Table 2, Table 3 reports the distribution of post-treatment vote intentions for the control and treatment groups assuming that the persuasiveness of ads is unaffected by prior dispositions (as equation (1) suggests).[23] Comparing the distribution of each treatment group with the control group reveals scant evidence that the four treatments affect subject's vote intentions. The distribution most dissimilar to the control group (Treatment 3) suggests that exposure to conflicting ads decreased support and increased indecision. While most of the differences for the Gore only treatments are in the expected direction (for instance the apparent negative effect on Bush support in Treatment groups 2 and 4) we cannot reject the null hypothesis that distribution of vote intentions for the control and treatment group are

[23] Given the covariate balance evident in Table 2, employing matching estimators does not appreciable change the results we report. For expositional clarity we focus on the simple difference of means tests.

identical for treatments 1 and 4. There are two plausible explanations for the null findings. First, it may be that these ads simply did not have a causal effect on viewer's vote intentions.[24] Alternatively, it is possible that the modeling assumptions underlying Table 3 prevent us from observing these effects.

[INSERT TABLE 4 ABOUT HERE]

Table 4 relaxes the assumption that ads are equally persuasive regardless of initial predispositions implicit in Table 3. It examines whether the preferences expressed in the experimental survey differ between those exposed to treatments and those who were not among initial Bush supporters (for example). Comparing post-treatment vote intention among individuals with different pre-treatment vote intentions clearly demonstrates the moderating effect of prior candidate preferences for the persuasiveness of the ads we use.

The top section of Table 4, indicates that, with the exception of Treatment 1, exposure to the pro-Gore treatments 2 and 4 and the Gore and Bush ads in Treatment 3 increases the probability that initial supporters of Gore continue to support Gore relative to the control group and decreases the probability that they change to support Bush. For example, whereas 86 % of initial Gore supporters continue to prefer Gore in the experimental survey, the percentage is 88 % for Gore supporters exposed to two Gore ads (Treatment 4). Furthermore, while 4 % of the initial Gore supporters not exposed to any of the treatments indicate a preference for Bush, only 1 % of those exposed to Treatment 4 do so.

[24] In fact, the ad displayed in Treatment 1 does seem to have been ineffective. In none of our analyses was there any statistically significant effect of Treatment 1 on any group of subjects. This result is consistent with findings from interviews with campaign strategists who recalled that their own research suggested that this ad 'Siding' was particularly ineffective (Clinton and Lapinski 2004). This highlights our cautionary note that our results are based on a small sample of ads.

Among respondents initially intending to vote for Bush, exposure to the treatments decreases Bush's support and increases indecision for Treatments 2 and 3.[25] Among undecided voters, there is little evidence that the ads were effective in changing vote intentions. Non-zero differences with the control group are evident only among undecided voters exposed to the conflicting ads in Treatment 3. Furthermore, the non-zero effect of Treatment 3 results because of increased indecision. While 47 % of those without an initial preference remain without a preference in the control group, 59 % of the undecided voters exposed to the conflictual ads of Treatment 3 indicate no preference.

To highlight the important differences in Table 4, Table 5 summarizes whether the percentage changing their initial preference depends on the treatments for each set of initial preferences and each treatment. In other words, among initial Bush supporters, how does the percentage continuing to support Bush in the control group compare to the percentages in each of the treatment groups? The difference-in-means estimator is an unbiased, consistent and asymptotically normal estimate of the average treatment effect (Rosenbaum and Rubin 1983).

With the exception of Treatment 1, the mean differences are all of modest statistical significance. Focusing on those with an initial preference, we find evidence that ads appear to have a common effect – initial Bush supporters are less likely to support Bush (relative to initial Bush supporters in the control group) and initial Gore supporters are more likely to continue to support Gore (relative to initial Gore supporters in the control group) after exposure to treatments 2, 3 and 4. Movement among Bush supporters exposed to pro-Gore ads suggests that at least some Bush supporters did not engage in biased processing of the message.

In contrast to decided viewers, the same treatments (and ads) increase the indecision of undecided voters (relative to undecided voters in the control group). This finding stands in

[25] Treatment 4 has no effect and Treatment 1 curiously increases Bush's support.

contrast to the theoretical and common sense expectations that undecided voters should be most susceptible to advertising effects. Since undecided individuals are considerably less engaged in politics, the reception-acceptance model of attitude change implies that these undecided subjects should be open to persuasion when exposed to political ads. Our results do not match this expectation; the ads increased indecision among undecided subjects rather than persuading them to support the sponsoring candidate.

Tables 4 and 5 reveals that not only are there important differences in treatment effects depending on initial preferences, there are also important differences between voters with an initial preference and those that are initially undecided. Failing to account for the possibility that initial preferences might affect the persuasiveness of ads and the possibility that voters with an initial preference might behave differently from those without an initial preference would lead to very different conclusions. In fact, comparing the conclusions suggested by Table 3 and 5 highlights the importance: Table 3 suggests that ads are unpersuasive whereas Table 5 suggests that the persuasiveness of ads depends critically on the political predispositions of the viewer. Failing to account for the individuals' pre-treatment candidate preferences yields appreciably different conclusions than when initial support is allowed to moderate an ad's impact.

The evident differences in the estimated treatment effects between voters with an initial preference and those without an initial preference suggest that further consequential differences may exist. To probe this in more detail, we allow for the possibility that the persuasive impact of advertising also varies by levels of cognitive political engagement.

An individual's political engagement is clearly correlated with her ability to express a preference for a candidate, as the mere existence of a pre-treatment candidate preference indicates at least some cognitive engagement with the campaign. Accordingly, those with an

initial candidate preference are more engaged than initially undecided respondents.[26] However, the question of primary interest is whether the symmetry evident in Table 5 is due to incorrectly assuming that all Bush supporters and Gore supporters are equally responsive.

Because the mean of the distribution of political engagement is much lower among subjects who were initially undecided, there are relatively few highly engaged, yet initially undecided individuals.[27] Although it is certainly realistic to suggest that there are fairly few highly interested individuals who would be undecided about their preferred presidential candidate, the small size of this group in the population is reflected in our sample and our ability to draw inferences about the susceptibility of advertising effects to highly engaged undecided voters is quite limited.

[INSERT TABLE 6 ABOUT HERE]

The results presented in Table 6 highlight the important moderating role political engagement plays in the effectiveness of political ads. Ignoring political engagement suggests that initial Gore and Bush supporters both move toward Gore in response to treatments 2, 3, and 4. However, the first row of Table 6 suggests that this movement among Bush supporters was entirely the result of less engaged individuals. In other words, only the relatively unengaged Bush supporters were likely to respond to the ads (by up to 12 % when confronted with the two pro-Gore ads in Treatment 4). These results nicely reflect the claim that those moderately interested in politics are most likely to respond to political communication. In addition, these results suggest that despite being able to engage in biased processing (since the ads are clearly pro-Gore), some of these less engaged Bush supporters accepted the messages contained in the Gore ads.

[26] The mean of our interest index, which ranges from 0 to 1, is .66 for initial Gore supporters and .68 for initial Bush supporters. In contrast among the initially undecided mean interest is .48

[27] Among initially undecided subjects, 87% are classified as less interested with 13 % identified as more interested.

Among highly engaged Bush supporters, we find no evidence that exposure to Gore ads decreases support for Bush. In fact, when exposed to two pro-Gore ads (Treatment 4), highly engaged Bush supporters were actually more likely to continue to support Bush. Although it is difficult to generalize from a single finding, this suggests that respondents reacted to the one-sided information in exactly the opposite manner.

For undecided voters, the effects of ads are also largest among those with lower levels of political engagement, but these effects are surely unintended. Initially undecided voters with low levels of political engagement are more likely to remain undecided when exposed to treatments 2, 3 and 4 than undecided voters in the control group. Although the extremely small samples make definitive inference difficult, highly engaged undecided voters are less responsive than their less engaged counterparts.

Among those initially intending to vote for Gore, the impact between political engagement and persuasiveness is unclear: highly engaged respondents are more (Treatment 4), less (Treatment 1 and Treatment 3) and identically (Treatment 2) responsive to the ads relative to Gore supporters with lower levels of political engagement.

Putting the results together clearly reveals that ads are not equally persuasive for all voters and the extent to which a voter is persuadable depends on both the voters' initial preference and the voters' level of political engagement. Only some voters – notably those voters willing to identify a candidate preference but who are nonetheless relatively politically disengaged – appear responsive to political advertisements in anticipated ways. In other words, unengaged initial Bush supporters are less likely to support Bush when exposed to ads supportive of Gore. However, the preferences of voters with high levels of political engagement and an initial candidate preference are unresponsive to ads, and may even respond in ways counter to

the ads' intent. Finally, undecided voters are the least responsive to advertising – and exposure only increases their indecision.

Relating these results to the popular account of cognitive processing presented in section 1, we find that the account is only partially confirmed. Consistent with the account, we find no persuasive effects among highly engaged viewers with an initial candidate preference. We also confirm that the less engaged viewers with a preference are most susceptible to persuasion even though they arguably possess enough information to engage in biased processing.

However, the account is critically wrong about the automatic acceptance of persuasive appeals among the less engaged. If a lack of initial candidate preference indicates a lack of engagement (rather than a lack of ability or capacity to process the information), we find that exposure *increases* viewer indecision.

5. Caveats

Although the results shed novel insights on the nature of the persuasiveness of ads by employing an experimental design that can control exposure administered to a nationwide sample of sufficient size so as to investigate the impact on particular groupings of respondents, it is useful to highlight some caveats that restrict the conclusions that can be drawn from our study. We highlight these limitations not to denigrate the important set of results we find, but rather to suggest that extrapolating from our experimental condition to statements of general impact must be conditional. We hasten to note that all observational and experimental studies face important limitations and we seek to acknowledge the limitations evident in our study.

First, we expose respondents to ads relatively late in the campaign and the maximum exposure consists of two ads. Consequently, we might think that the design makes it unlikely to find large effects. In light of these facts, we think the evident findings are particularly notable.

Second, it is also possible to interpret the effects we document as an upper bound for ads shown late in the campaign. Voters normally encounter ads while watching television. Although our ads are shown to potential voters on the same television and in the same room that they would normally be exposed to ads, a notable difference is that the ads we use appear in the context of a survey and respondents control when the ad plays. Whereas ads are usually a "distraction" for voters interested in watching television, our ads are the main attraction. Focusing respondents' attention to the ad may overstate the true impact of the ad as voters may not normally notice the ads they are exposed to.

Third, the conclusions are only really valid for the sample of ads we examine. It is entirely possible that different ads would produce different effects. Despite this possibility, we find the consistency across the three ads used in treatments 2, 3 and 4 reassuring.

Finally, it is possible that the effects we find are transient – a temporary aberration due to the immediate exposure to an ad rather than a reflection of a persistent change in voter preferences. We wholly acknowledge this possibility, but we think this does not mitigate the importance of our results. Even if voters are only temporarily responsive to advertising, it does not follow that advertising is inconsequential. In fact, given the large amount of advertising that appears in the final weeks of the campaign, the fact that voters are temporarily responsive may mean that voters are especially responsive to ads and that the timing of ads can have large impacts on the electoral outcome by shifting the support of relatively unengaged supporters exposed to the advertising.

6. Conclusion

Given the central role campaign ads play in modern Presidential campaigns, how individuals respond to ads – and the extent to which ads shape the preferences of the electorate

and the outcome of the election – is an undeniably important question for assessing how candidate actions affect electoral outcomes.

It is also a difficult question. Difficulty in examining existing accounts of individual receptiveness to ads results because of the need to measure exposure in ways unconfounded with possible moderators of effectiveness using sufficiently sized samples to examine the impact on particular sets of viewers. As a consequence, progress has been limited.

We use an experimental study rivaling most observational studies to demonstrate that assessing the impact of political advertising on voter preferences depends critically on who watches the ads. Although scholars have made considerable progress in identifying how the characteristics of ads influence their effectiveness, focusing on ad characteristics alone is half the story. We demonstrate that the persuasiveness of ads depends on viewers' initial predispositions and level of political engagement.

Beyond clearly demonstrating the inferential problems stemming from a failure to account for these important viewer characteristics, our results speak to theoretical questions about what sorts of people are influenced by ads. Viewed from the perspective of past research, the fact that advertisements can actually prolong indecision among those without a candidate preference is perhaps our most surprising result. In contrast to the common sense notion that undecided voters ought to be most responsive to political ads, we find no evidence that ads increase support for sponsoring candidates among the undecided. In fact, we find that initially undecided voters are actually less likely to state a candidate preference following exposure to ads than they would be absent this exposure. These results, especially those related to Treatment 3 which contained opposing ads, may indicate that consideration of the content of ads generates confusion about which candidate deserves support.

The fact that undecided voters are not influenced by political ads raises questions for the assumptions of the reception-acceptance model of attitude change; these less engaged individuals do not uncritically accept all political messages to which they are exposed.[28] However, our results do appear to confirm other central elements of the model. Among those with an initial preference for Bush, pro-Gore advertising was only effective among those viewers who were less cognitively engaged with the campaign. Recalling that the willingness to offer an initial preference suggests that these 'low engagement' Bush supporters are not among the least politically engaged segment of society, this suggests that moderately engaged individuals may be most susceptible to advertising effects. That more engaged initial Bush supporters resisted the persuasive appeals of the Gore ads suggests that biased processing of information may occur, but only among those who are quite cognitively involved in the campaign.

By controlling exposure and exploring advertising effects among different types of individuals we are able to draw on and combine the strengths of previous observational and experimental studies to provide insight into the micro foundations of advertising effects. In the on going and collective effort to understand political advertising effectiveness and media effects in general, our study confirms and refines a prominent account of how individuals react to ads. Although more work is required to confirm and expand upon our findings given the caveats we note, our work provides an important investigation of the individual level behavior underlying macro-level studies of recent prominence which have reasserted that "campaigns matter."

[28] As their indecision further suggests that this resistance is not the result of biased processing, an alternative explanation for this result seems warranted. One plausible explanation is that less engaged individuals may face limitations of cognitive ability and or motivation to incorporate information contained in these ads. The possibility that less engaged citizens are unable to 'use' political information even when exposure is forced should raise doubts for those who argue that increased availability of quality political information will result in a more informed citizenry.

Table 1: Description of Treatments

Treatment Group	Associated Sponsoring Candidate	Ads	Experiment Dates	Respondents Assigned	Size
1	Gore	"Siding"	10/10 – 11/7	2,850	634
2	Gore	"Bean Counter"	10/9 – 11/7	2,850	689
3	Gore Bush	"Siding" "Priority"	9/29 – 10/21	1,500	516
4	Gore Gore	"Siding" "Bean Counter"	10/9 – 11/7	2,850	606
Control					5,298

Table 2: Summary Statistics for Treatment Groups

	Sample	Treat 1	Treat 2	Treat 3	Treat 4	Control
% supporting Gore (post-treatment)		.35	.35	.33	.35	.34
% supporting Bush (post-treatment)		.40	.40	.42	.42	.40
% undecided (post-treatment)		.25	.25	.25	.24	.25
% supporting Gore (pre-treatment)		.41	.43	.38	.42	.41
% supporting Bush (pre-treatment)		.41	.36	.38	.38	.40
% undecided (pre-treatment)		.18	.21	.24	.20	.20
% with high political engagement		.26	.31	.30	.34	.28
Mean education		2.08	2.06	2.09	2.07	2.08
Mean age		44.7	44.5	46.3	45.9	46.3
% reside in a "battleground" state		.38	.43	.40	.40	.40
% female		.47	.48	.55	.46	.48
% black		.08	.07	.08	.07	.07
Mean party identification		.51	.51	.49	.51	.51
N		564	612	391	555	5,298

Table 3: Pooled Candidate Support Post Treatment:

Post Treatment Vote	Control	Treat 1	Treat 2	Treat 3	Treat 4
Bush	.40	.41	.36	.38	.38
Undecided	.19	.18	.21	.24	.20
Gore	.41	.41	.43	.38	.42
N	5,298	564	612	391	555
Chi-Squared (2 dof)		.53	2.82	4.35	.91

Note: Chi-squared statistic is for the test that the treatment distribution is the same as the control group distribution.

Table 4: Conditional Candidate Support (and Average Treatment Effect) Post Treatment

	Post Treatment Vote	Control	Treat 1	Treat 2	Treat 3	Treat 4
Gore Supporters	**Bush**	.04	+.01	-.02	-.03	-.03
	Undecided	.10	.00	.00	-.01	+.01
	Gore	.86	-.01	+.03	+.03	+.02
	N	1,827	197	212	130	193
	Chi-Squared (2 df)		.80	.28	.33	.22
Undecided	**Bush**	.20	.00	-.04	-.04	-.05
	Undecided	.47	.00	+.04	+.12	+.03
	Gore	.33	-.01	.00	-.07	+.02
	N	1,342	141	153	96	131
	Chi-Squared (2 df)		.99	.50	.09	.41
Bush Supporters	**Bush**	.83	+.02	-.04	-.03	-.02
	Undecided	.11	-.03	+.02	+.05	+.02
	Gore	.07	+.01	+.02	-.02	+.01
	N	2,129	226	247	165	231
	Chi-Squared (2 df)		.3	.21	.15	.72

Note: Chi-squared statistic is for the test that the treatment distribution is the same as the control group distribution. P-value in parentheses.

Table 5: Average Treatment Effect for Selected Comparisons of Interest

	Treat 1	Treat 2	Treat 3	Treat 4
Stay Bush	+.02 (.24)	-.04 (.04)	-.03 (.20)	-.02 (.21)
Stay Undecided	.00 (.49)	+.04 (.19)	+.12 (.01)	+.03 (.26)
Stay Gore	-0.01 (.31)	+.03 (.15)	+.03 (.16)	+.02 (.22)

Note: Difference of Means Test for the reported comparison. P-value in parenthesis.

Table 6: Average Treatment Effect for Selected Comparisons of Interest

	Treat 1		Treat 2		Treat 3		Treat 4	
Interest	**Higher**	**Lower**	**Higher**	**Lower**	**Higher**	**Lower**	**Higher**	**Lower**
Stay Bush	**.00**	**+.03**	**+.01**	**-.07**	**.00**	**-.05**	**+.07**	**-.12**
	(.42)	(.22)	(.37)	(.02)	(.46)	(.12)	(.00)	(.00)
	77	149	82	164	62	103	103	127
Stay Undecided	**+.16**	**-.02**	**-.08**	**+.09**	**+.09**	**+.13**	**.00**	**+.03**
	(.07)	(.32)	(.17)	(.03)	(.23)	(.01)	(.49)	(.25)
	17	122	30	122	15	81	16	114
Stay Gore	**-.04**	**.00**	**+.02**	**+.02**	**.00**	**+.05**	**+.04**	**.00**
	(.14)	(.46)	(.26)	(.27)	(.45)	(.12)	(.10)	(.48)
	54	142	79	133	40	90	69	124

Note: Difference of means test for the reported sample and the associated control group. Cells also reports the number of respondents in each treatment category and the p-value (in parenthesis) for the test.

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Appendix A: Ads Used

The following scripts (with links to the ads) are from the online *National Journal* advertising archives.

Script of "Bean Counter"[29]

AL GORE: If your doctor says you need a particular specialist or some treatment, if you've got an HMO or an insurance company, a lot of times some bean counter behind a computer terminal, who doesn't have a license to practice medicine and doesn't have a right to play God, will overrule the doctor's orders. I'm telling you we need a patients' bill of rights to take the medical decisions away from the HMOs and insurance companies and give them back to the doctors and the nurses.
(On screen: Al Gore, Fighting for us; www.algore.com; Paid for by Gore/Lieberman Inc.)

Script of "Priority"[30]

ANNOUNCER [v/o]: Under Clinton-Gore, prescription drug prices have skyrocketed, and nothing's been done. George Bush has a plan: Add a prescription drug benefit to Medicare.
GEORGE W. BUSH: Every senior will have access to prescription drug benefits.
ANNOUNCER [v/o]: And Al Gore? Gore opposed bipartisan reform. He's pushing a big government plan that lets Washington bureaucrats interfere with what your doctors prescribe. The Gore prescription plan: Bureaucrats decide. The Bush prescription plan: Seniors choose.
(On screen: Paid for the Republican National Committee)

Script of "Siding"[31]

ANNOUNCER [v/o]: The issue: prescription drugs. George Bush's approach leaves millions of seniors with no prescription drug coverage -- none. (On screen: Source: CBO Analysis H.R. 4680 6/28/00; U.S. House DPC, 4/14/00)
And Bush forces seniors he does include to go to HMOs and insurance companies for coverage.
(On screen: Source: Blue Cross/Blue Shield Assoc. Letter, 4/24/00; National Journal, 4/1/00)
The National Council of Senior Citizens says, "The Bush approach is favored by big drug companies and leaves millions with no help."
(On screen: Source: National Council of Senior Citizens press release)

[29] Link: http://nationaljournal.com/members/adspotlight/2000/09/0905ag1.htm .
[30] Link: http://nationaljournal.com/members/adspotlight/2000/09/0905rnc1.htm .
[31] Link: http://nationaljournal.com/members/adspotlight/2000/09/0905dnc1.htm .

Appendix B
Table B1: Experimental Studies of Ad Effectiveness

Study	Dependent Variable	Treatment	Interacted Treatment	N	Sample composition
Rosenberg et al (1986)	Vote intention in hypothetical election	Candidate appearance	--	104	Student
Thorson et al (1991)	Candidate eval. Vote intention	Ad tone; Ad type; Ad content	--	161	Students
Ansolabehere & Iyengar (1995)	Vote intention.	Party of ad sponsor	Party id.; Interest in politics	1716	Non-students, recruitment not random
Ansolabehere & Iyengar (1994)	Vote intention.	Party 'ownership' of ad issues	--	141/445	Non-students, recruitment not random
Kahn & Geer (1994)	Candidate eval.	Ad tone; Ad content	--	303	Students
Pinkleton (1997)	Candidate eval.	Ad tone	--	165	Students
Pinkleton (1998)	Vote intention	Ad tone	--	165	Students
Chang (2001)	Candidate eval.	Ad tone	--	165	Student
Meirick (2002)	Candidate eval.	Ad tone	--	60	Students
Pfau et al (2002)	Candidate eval.	Ad sponsor; Ad tone	Party id.	311	Students
King & Mcconnell (2003)	Candidate eval. Vote intention	Num. of neg. ads	Gender	121	Students
Chang (2003)	Candidate eval. Vote intention	Ad tone; Ad sponsor	Party id.	754	65% students, 35% adults; recruitment not random
Valentino et al (2004)	Vote intention	Ad sponsor	Political knowledge	320	Non-students, recruitment not random